Distributional Consequences of Taxation: Evidence from Business Taxes, Wealth Transfers, and Employment Protection Legislation

Inauguraldissertation zur Erlangung des akademischen Grades eines Doktors der Wirtschaftswissenschaften der Universität Mannheim

vorgelegt von

Richard Winter

Mannheim, 2025

Dekan: Joachim Lutz

Referent: Prof. Dr. Philipp Dörrenberg

Koreferent: Prof. Dr. Holger Stichnoth

Verteidigt am 03.06.2025

Acknowledgements

There are many individuals without whom this dissertation would not have been possible, and to whom I am deeply grateful. First, I extend my heartfelt thanks to the tax team at the University of Mannheim. Experiencing such a level of empathy, camaraderie, and mutual support in a professional setting is rare, and I truly value the strong bonds and friendships that have grown from our time together. I am especially thankful to Jessica Müller, Alina Pfrang, and Katharina Schmidt for their support and the many treasured memories we shared.

I am profoundly grateful to my supervisors, Philipp Doerrenberg, Christoph Spengel, and Johannes Voget. Your continued guidance, encouragement, and insightful advice have been instrumental in shaping both my academic journey and personal growth. I have benefited immensely from your mentorship, through your teaching and through countless meaningful discussions over the years. Thank you for believing in me and standing by me throughout this journey. I am also grateful to Christine Bürner and Sandra Dumann for your invaluable support and encouragement.

My sincere thanks also go to my co-authors Philipp Doerrenberg, Fabian Eble, Davud Rostam-Afschar, Johannes Voget, and Jan Zental. Your collaboration was crucial to the success of this dissertation, and I am truly grateful for the stimulating, productive, and enjoyable work we accomplished together. I learned a great deal from each of you and will always value the experience.

I would also like to extend my thanks to the student research assistants who supported me throughout my doctoral studies. I am especially grateful to Victoria Ahrens, Hendrik Schmidt, Juliane Ott, Arthur Scholz, and Sergen Keskincelik for their dedication and assistance.

I gratefully acknowledge the financial support provided by the Leibniz Science Campus MannheimTaxation, the TRR 266 Accounting for Transparency cluster, and the Graduate School of Economic and Social Sciences at the University of Mannheim. Their support

enabled me to fully dedicate myself to this work.

Finally, and most importantly, I wish to thank my family and friends—without whom none of this would have had meaning in the first place. To my parents, who have always supported me with their wisdom, encouragement, and love, and remain a steady anchor in my life. To my brothers, Johannes and Stephan, who have shaped many of the best parts of who I am. To my grandmother Renate, who always believed in me, though sadly, she could not witness this achievement. To my friends Christoph, Sascha, Svenja, Yasemin, and Kevin, thank you for your understanding and support.

A very special thank you goes to Julia Edelmann, the love of my life. Your unwavering support carried me through difficult times, and you inspired me to give my all to reach this goal. I look forward with excitement and hope for all that lies ahead of us.

Mannheim, March 2025 *Richard Winter*

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List of Abbreviations

AJCA American Jobs Creation Act.

BvD Bureau van Dijk.

CIT Corporate Income Tax.

DPAD Domestic Production Activities Deduction.

EU European Union.

GBP German Business Panel.

IV Instrumental Variable.

LBT Local Business Tax.

MFL Multivariate Fractional Logit.

OLS Ordinary Least Squares.

PIT Personal Income Tax.

PPML Poisson Pseudo Maximum Likelihood.

RDC Research Data Centres.

TCJA Tax Cuts and Jobs Act.

US United States.

VAT Value Added Tax.

1 Introduction

Inequality of income and wealth is a global phenomenon that spurs a heated public debate in many countries. In recent years, the top 10 % of the income distribution owns 52% of total income, while the bottom 50% only owns 8.5%. Wealth concentration has reached particularly high levels, with the top 10% of the world owning 76% of all wealth, while the bottom 50% only owns 2% (Chancel et al., 2022). Although some economists have argued that a certain level of inequality in a society can foster economic growth (Kaldor, 1957; Mirrlees, 1971; Barro, 2000), there is wide consensus that high concentration of income and wealth reduces economic efficiency, weakens democracy, and fuels social unrest (Acemoglu and Robinson, 2006; Stiglitz, 2012). While it is difficult to determine an optimal level of inequality in society, evidence suggests that extreme inequality is undesirable from a social welfare perspective.

Addressing extreme inequality requires targeted policy interventions. Although some level of inequality arises naturally from economic factors, such as differences in skill, education, or effort, its extent is significantly shaped by policy choices, including taxes, social spending, and labor protections (Piketty, 2014; Chancel et al., 2022). Among these, taxation plays a central role in redistribution (Avi-Yonah, 2006). In principle, progressive income and wealth taxes are well suited to combat inequality (Saez and Zucman, 2024). Within this broader framework, two particularly promising instruments are taxes on corporate profits and transfers of wealth, such as inheritance and gift taxes. These forms of taxation affect individuals at the very top of the income and wealth distribution disproportionately and therefore have a strong potential to curb inequality (Piketty and Saez, 2007; Piketty et al., 2018; Saez and Zucman, 2020).

While progressive taxation holds promise in reducing inequality, its real-world effectiveness hinges on a multitude of factors, such as tax compliance and enforcement, government spending efficiency, and behavioral responses and their resulting tax incidence (Fullerton and Metcalf, 2002). This dissertation focuses on the latter two aspects as they fundamentally

shape the redistributive potential of tax policy. Through behavioral responses, the legal responsibility of paying a tax does not always align with who ultimately bears the economic burden. Firms, for example, can shift part of the burden of profit taxes to workers through lower wages or to consumers through higher prices in response to a tax increase (Fuest et al., 2018; Baker et al., 2023, see Auerbach, 2006, or more recently Fuest and Neumeier, 2023 for a review). These changes in prices can reduce the progressivity of the tax by shifting a part of the economic incidence from capital owners to workers and consumers. Behavioral responses are also a critical factor to consider in the context of wealth and wealth transfer taxes. Wealthy individuals often engage in sophisticated tax planning to minimize their tax liabilities, allowing them to avoid a substantial portion of wealth taxes (Landier and Plantin, 2017).

Effective tax policy design requires an understanding of how firms and individuals react to taxation. This dissertation examines three critical dimensions of these responses, with important implications for the distributional consequences of taxation. Chapter 2 provides novel evidence on how managerial decisions shape the incidence of profit taxes, analyzing how the economic burden of these taxes varies depending on the sign and magnitude of tax changes. Chapter 3 examines behavioral responses to changes in wealth transfer tax legislation and assesses their implications for government revenue. Chapter 4 evaluates firm decision making in response to inheritance taxation, particularly in relation to labor protection provisions. By exploring these three dimensions, this dissertation enhances our understanding of tax incidence and behavioral responses to taxes, and also provides insights to inform a more effective and equitable tax policy design.

In Chapter 2[1], we explore how decision makers in German companies respond to changes in their profit tax burden. Firms' reactions to changes in taxes are key determinants of economic incidence, which is important for gauging their welfare and distributional implica-

[1] This is joint work with Philipp Doerrenberg, Fabian Eble, Davud Rostam-Afschar, and Johannes Voget and is currently in the revision process for resubmission at the American Economic Journal: Economic Policy.

tions. While there is a large body of empirical literature on this issue, most studies focus on a single or few dimensions of incidence, e.g., the incidence of corporate income taxes on wages (Fuest et al., 2018). It is difficult to identify the impact of a tax change on a large variety of adjustment margins using observational data, and even harder to investigate the impact of differentially signed or sized tax treatments in one setting.

We fill this gap in the literature by asking firm owners in a randomized survey experiment how they would respond to a hypothetical change in their profit tax burden. Respondents were assigned to a tax change that differed in its sign (increase versus decrease) and magnitude (1%, 10%, or 25%) and were subsequently asked how this shock would affect different adjustment margins in their company. The available margins allowed for effects on wages, employment, profit distributions, reserves, investment, consumer prices, and tax planning. We document a high level of asymmetry between the effects of tax increases and decreases, suggesting that workers benefit substantially more from a tax decrease than they are hurt by a tax hike. Consumers on the other hand bear a substantially higher burden in the event of a tax increase, while the pass-through to prices for a tax decrease is comparably modest. We conclude that the distributional consequences of profit tax changes are vastly different depending on the direction of the change.

Chapters 3 and 4 shift the focus to wealth transfer taxes. In Chapter 3[2], we examine how wealthy individuals respond to inheritance and gift taxation. More specifically, we focus on reactions of business asset transfers in response to legislative changes to the preferential treatment of this asset class for wealth transfer tax purposes in Germany. Quantifying the responsiveness of the wealthy is important to evaluate the effects of future tax reforms on the progressiveness of wealth transfer taxation and its potential for raising government revenue. If wealth transfer taxes can be easily avoided through strategic timing, their effectiveness in reducing wealth inequality is severely undermined.

We leverage two events in the legislative history of wealth transfer taxation in Germany,

[2] This is joint work with Jan Zental and available as CESifo working paper (Winter and Zental, 2025).

which were expected to reduce generous tax exemptions of business assets, to estimate the responsiveness of inter vivos giving to taxation. We find that business transfers respond strongly and in an extremely timely manner to expected tax changes. Our results suggest that this responsiveness increases in the volume of wealth at stake. Furthermore, we document heterogeneity in transfer characteristics consistent with tax avoidance motives. We estimate that the amount of foregone gift tax revenue due to timing responses is up to 2.8 times the annual inheritance and gift tax revenue, which has major implications for future reforms of inheritance and gift tax legislation.

Chapter 4[3] examines the impact of labor protection regulation on firm performance in the context of wealth transfer taxation. The tax treatment of business assets poses a key challenge for policymakers. Family businesses, which form the backbone of many economies, generate positive externalities such as employment and growth. To prevent excessive burdens from taxation during succession, some countries grant tax exemptions for businesses transferred through inheritance or as gifts. However, these exemptions raise constitutional and fairness concerns. To balance this issue, policymakers often impose conditions—such as a selling restriction or maintaining certain employment levels—to qualify for tax relief (OECD, 2021). While intended to protect workers, these requirements may distort firm decision-making and lead to unintended consequences.

We investigate this issue by building a unique dataset, which combines extensive firm ownership data, scraped information on individual death events, and administrative employment data for German firms. We explore how the requirement to maintain the firm-level payroll above a pre-specified threshold affects firm performance after an inheritance. We document that the requirement inhibits firm-level employment growth during the required holding period throughout which stable employment must be guaranteed. This free cash seems to be used for increased capital investment instead. Our findings suggest that tying exemptions from wealth transfer taxes to economic outcomes entails unintended consequences, which

[3] This is joint work with Philipp Doerrenberg and Jan Zental.

should be considered when navigating the contentious field of taxing business assets in the context of wealth transfers.

Chapter 5 concludes with a summary of the main points of each chapter.

2 The Asymmetric Incidence of Business Taxes: Survey Evidence from German Firms

Co-Authors: Philipp Doerrenberg, Fabian Eble, Davud Rostam-Afschar and Johannes Voget

Abstract: We provide novel evidence on the incidence of business taxes using comprehensive survey and experimental data from German firms. Leveraging randomized variation in hypothetical tax changes, we find that the incidence of profit taxes is highly asymmetric. Tax decreases are more likely to benefit workers and stimulate investment, whereas tax increases tend to be passed on to consumers through higher prices and absorbed by firm owners through reduced profit distributions. Moreover, by varying the magnitude of the tax changes, we demonstrate that worker incidence increases with the absolute size of the tax change, partially offsetting the burden on firm owners.

Keywords: Corporate tax, tax incidence, firm behavior, investment, payout, wages

Acknowledgements: Declarations of interest: none. We thank Anne Brockmeyer, Giacomo Brusco, Florian Buhlmann, Michael Devereux, Michelle Hanlon, Jeffrey Hoopes, Tomáš Lichard, Eric Ohrn, Jens Mueller, Marcel Olbert, Andreas Peichl, Sebastian Siegloch, Barbara Stage, as well as participants at several conferences, seminars, and workshops for valuable feedback. We thank the German Research Foundation (Deutsche Forschungsgemeinschaft, DFG) for financial support as part of the *TRR 266 Accounting for Transparency* cluster (Project-ID 403041268). The trial was registered with the AEA (RCT trial ID AEARCTR-0011379) and received ethical approval from the U. Mannheim Institutional Review Board. Richard Winter gratefully acknowledges financial support from the Graduate School of Economics and Social Sciences of the University of Mannheim.

2.1 Introduction

Taxes on business profits are important cost factors for firms (Jacob, 2022). They affect financing and investment decisions (Zwick and Mahon, 2017; Ohrn, 2018; Giroud and Rauh, 2019), price setting (Baker et al., 2023) as well as hiring policy and wage negotiations (Arulampalam et al., 2012; Fuest et al., 2018; Dwenger et al., 2019). Furthermore, changes in profit taxes can impact both the scale and composition of labor and capital inputs employed by a firm.

When a firm experiences a change in its profit tax burden, its manager has a variety of adjustment margins to respond to the change. Will the manager reduce wage growth or distributions to shareholders after an increase in the profit tax burden? Are output prices affected after a tax decrease, or are the additional funds funneled towards new investment projects? Whatever the manager decides, her choices will have consequences for the firm's stakeholders, namely, employees, owners, and customers. These questions then lead to the question of tax incidence, which is crucial for determining the welfare and distributional effects of taxes and has important implications for optimal tax policy.

Existing empirical literature using observational data usually studies one particular dimension of incidence at a time in one specific setting, e.g., the effect of taxes on wages in one particular country. Focusing on one margin is due to the challenge of finding adequate data and identification strategies to isolate the effect. While these studies are able to identify the effects of taxes on single adjustment margins in their respective setting, the variety of countries, tax types, time frames, reform types and identification strategies makes it difficult to combine the insights into one comprehensive picture (Hsieh et al., 2023). For example, consider two studies that estimate the effect of business taxes on investments, one exploiting a reform with a large tax increase in country X and the other one using a reform with a small tax increase in country Y. Obviously, it is very difficult to attribute differences across the two studies' results solely to differences in the size of the tax change. Similarly, combining the price effects of one study with the wage effects of another study does not allow conclusions

to be drawn about the relative burden on consumers and workers. Data availability and the scarcity of different types of tax reforms further limit the informative value of existing observational studies. For example, the literature does not provide an answer to the question of whether business tax increases and decreases have symmetric effects, and provides only limited evidence on the effects on firm returns and consumer prices.

However, for a complete understanding of the effects of profit taxes and their incidence implications, it is important to consider all dimensions along which taxes can exert effects in a comparable setting and to examine if different types of tax reforms have different incidence effects. This is where our paper comes in: we use data from a novel large-scale survey of German firms to provide evidence on the tax responses of firms along many different margins and for different types of tax reforms. Our aim is to improve the understanding of the *full* picture of profit tax incidence within one unified setting. A survey approach is well suited for this purpose, as it allows measuring a comprehensive set of adjustment margins within a unified framework while randomly varying the size and the sign of the tax change. While we acknowledge that surveys have some limitations in comparison with well-identified observational studies, our survey approach complements the existing literature by allowing to study important aspects of business tax incidence that are difficult to consider using non-survey approaches.[4]

Our starting point is the effect taxes have on the managerial decision margins. We focus on the short-run direct effects of the managers' adjustment decisions, abstracting from general equilibrium effects.[5] We take a straightforward approach and ask firms how profit

[4] Survey experiments have been successfully employed in similar contexts, e.g., Graham et al. (2017), and enjoy ever-increasing popularity in the social sciences (Stantcheva, 2023). Although survey research is based on self-reported actions, it has been shown that survey-reported behavior is comparable to revealed preference results in observational data (Parker and Souleles, 2019). However, we discuss potential caveats of our survey results, which are common to all survey data, in Section 2.3.4.

[5] Conceptually, there are several ways how incidence can be measured (Fullerton and Metcalf, 2002). Economic incidence is often measured by the change in welfare for a specific group induced by the tax relative to the sum of welfare changes of all groups considered. We do not measure welfare in terms of utilities directly, but express the relative burden of the tax attributable to a specific group in terms of its share in the tax burden change, thereby abstracting from the dead-weight losses of the tax (Fullerton and Metcalf, 2002; Suárez Serrato and Zidar, 2016; Fuest et al., 2018). Generally, Harberger (1962), which constitutes the seminal paper in the incidence field, developed a simple two-sector closed economy model and finds that

taxes affect a set of decision margins in their companies. For this purpose, we randomly assigned survey respondents to hypothetical permanent tax increases and decreases of varying magnitudes, and inquired either how the additional funds available after a tax cut would be used or from which sources funds would be diverted to pay for the increased tax burden. Respondents were presented with an exhaustive list of categories to which they could attribute shares of the change in tax burden, e.g., wages, prices, shareholder distributions, investments, etc. Each of the shares was required to be an integer between zero and 100, and shares needed to sum to 100 across categories. In order to make sure that no relevant category was missing, we included an open field, where firms could indicate the missing category and the respective share. This design allows us to infer the full distribution of a, say, EUR 100 change in tax burden and to determine the specific relative importance of each possible response margin.

Using this setup, we are able to examine the complete set of short-run effects of profit taxes on employees, firm owners and customers through the initial adjustment decisions by the manager (i.e., for a given level of pre-tax profits). At the same time, we also measure channels through which indirect effects materialize, e.g., changes in investment, which eventually feed back into future pre-tax profits and are thus important for total incidence. Random assignment of the sign of the tax change provides the opportunity to test for asymmetries in the stated incidence reported by survey participants, whereas experimental variation in the size of the tax change allows us to tease out the sensitivity of profit tax effects with respect to treatment intensity. To the best of our knowledge, our paper is the first to provide evidence on tax incidence from a large-scale survey of companies. A unique feature of our paper is that the survey-based approach allows us to distinguish the effects of differently signed tax changes as well as differences in treatment intensity.

under plausible parameter values capital owners bear the entire incidence of the tax. However, this central result does no longer hold once an open economy setting is considered, where capital mobility becomes a relevant factor. Gravelle (2013) provides an overview of several recent theoretical models and shows how their insights hinge on the underlying assumptions being made. The results critically depend on factor mobility, factor substitution, capital intensity, international product substitution elasticities, and country size.

Our main findings can be summarized as follows. First, we document that reactions to tax changes are highly asymmetric. For every EUR 100 of additional funds available due to a lower tax burden, EUR 32 are received by workers in the form of higher wages or new jobs, EUR 9 are distributed to firm owners and only EUR 2 are used to reduce output prices benefiting customers. Moreover, EUR 21 are used to build reserves and EUR 27 to finance new investment projects. This presents a stark contrast to the distribution of the burden between workers, owners, and customers in the case of a tax increase. Here we find that a hypothetical EUR 100 increase in the profit tax burden of a company is financed by workers (EUR 17), owners (EUR 24) and consumers (EUR 18) to a similar extent. The remaining EUR 41 are financed through indirect channels: EUR 15 of the tax increase is offset by a reduction in planned investments, while EUR 13 is absorbed by existing reserves. The roles of increased tax-saving opportunities and new debt acquisition are comparatively minor.

Second, we observe heterogeneous effects with regard to the size of the tax change. We find that larger tax changes increase the incidence on workers, mainly through the extensive employment adjustment channel. The results indicate that this increased worker incidence mainly stems from the owner payout and reserves categories. For tax increases, this implies that firm owners are more hesitant to shoulder a greater proportion of the tax burden as the increase gets larger. Conversely, with tax decreases, employees benefit proportionally more as the tax reduction becomes larger.

Finally, by exploiting the presence of a rich set of company characteristics in our survey data, we investigate heterogeneity in profit tax incidence. Our results suggest that the positive investment effects of tax cuts increase in company size, plausibly reflecting differences in investment opportunities and general growth prospects. We further document sector-specific differences. Incidence on consumers via price increases is substantially higher in the construction sector, which could be explained by relatively low profit margins and inelastic demand. These features have been shown to shift the incidence from firm owners to consumers (Fullerton and Metcalf, 2002). For tax decreases, we find that manufacturing firms

are most likely to utilize additional funds for new investment projects relative to other industries, which we attribute to the generally higher degree of capital intensity in that sector. Our results further suggest that the legal form of the company has a substantial impact on the incidence on its owners. We find that a higher share of incidence attains to partnerships compared to corporations and sole proprietors. This finding may reflect differing levels of profitability across legal forms, which we are unable to measure with our data.

Our survey design enables an examination of how a vast array of potential adjustment margins is affected by tax changes in a unified setup. This approach extends beyond the scope of existing observational studies. However, it is also subject to the typical limitations inherent to survey data. Rather than relying on observed behavior, our methodology is based on self-reported responses to hypothetical tax changes. A potential drawback of using hypothetical treatments is that they might lead to reduced effort from respondents or give rise to experimenter demand effects (Haaland et al., 2023). For example, when facing a hypothetical tax increase, managers may hesitate to report lower wages or lay off employees, particularly if they aim to be perceived as more socially responsible by the experimenter.

We argue that these issues are of limited importance in our setting for three reasons. First, experimenter demand effects are likely less pronounced in online surveys compared to face-to-face interviews due to the increased anonymity experienced by participants (De Quidt et al., 2018; Haaland et al., 2023; Stantcheva, 2023). Additionally, the neutral framing of our survey regarding taxation further reduces the likelihood of experimenter demand effects (Haaland et al., 2023; Stantcheva, 2023). Since we employ a between-subject design, experimenter demand effects are also likely to be less problematic compared to those in within-subject designs (De Quidt et al., 2019). Moreover, if companies were to provide socially acceptable answers or attempt to influence the survey's outcome with exaggerated or untruthful statements, we would not expect to find significant differences based on the magnitude of the tax change. However, we do observe such differences. Second, we cross-validate our estimates of initial incidence on workers, firm owners, and consumers by comparing them

to prior literature relying on observational data sources. By carefully taking into account differences in the tax variation used to identify incidence parameters, we find similar results for those margins for which empirical evidence exists. This comparison bolsters our confidence in the validity of our survey results. Third, we investigate the predictive power of the respondents hypothetical answers by comparing their stated actions to realized actions in two distinct settings. In the first test, we merge our survey responses to Orbis financial data and information on changes in statutory Local Business Tax (LBT) rates. We then correlate the stated impact of a tax change on employment with actual employment changes after a change in the LBT rate. In the second test, we exploit two questions about planned employment adjustments in the year after the survey by also correlating them with employment changes observed in Orbis over the same time horizon. Both tests confirm the predictive power of the stated actions for actual behavior, despite high levels of measurement error and uncertainty.

We identify several contributions of our paper. First, while most studies in the literature focus on only one adjustment margin (and thus one group of stakeholders), we consider all possible different adjustment margins and study the distributional effect on all stakeholders in one unified setting.[6] Our results for tax increases are in line with studies using observational data, suggesting that the survey answers are reliable and reasonable. Arulampalam et al. (2012), Fuest et al. (2018), Dwenger et al. (2019) and Risch (2024), among others, estimate the incidence of LBT, Personal Income Tax (PIT) and the Corporate Income Tax (CIT) on workers through wage adjustments and employment, finding varying incidence estimates of 11-50%. Recent studies investigating the effects of corporate profit taxation on consumer prices find incidence estimates in the range of 30-60% (Dedola et al., 2022; Baker et al., 2023). The few studies that examine multiple adjustment margins differ from ours in that they either focus on different margins which all matter for only one single stakeholder group, like workers (Giroud and Rauh, 2019; Risch, 2024) or customers (Kosonen, 2015; Jacob

[6] For reviews of the empirical literature on corporate tax incidence, see Hanlon and Heitzman (2010), Clausing (2013), or Jacob (2022).

and Zerwer, 2024), or use general equilibrium models and structural estimations to assess the impact of taxes on various groups (Suárez Serrato and Zidar, 2016). The study by Suárez Serrato and Zidar (2023) is noteworthy because they provide incidence estimates for several groups simultaneously, finding that the incidence of the US state corporate income tax falls to 40% on capital, 30-35% on workers, and to 25-30% on landowners. Their study uses structural estimations and a general equilbrium framework with second-round effects, whereas we consider first-round effects in a partial equilibrium setting to study how managers initially respond to tax changes (including indirect effects). As in many other studies, returns to firm owners are unobservable in Suárez Serrato & Zidar (2016) and determined through a structural model, whereas we measure firm-owner returns directly. We thus add a different angle and approach to the incidence question and do not rest on assumptions commonly invoked in structural estimations and general equilibrium models.[7]

Second, we contribute to the question whether the sign of a business tax change matters for its effects and incidence. While this complements a few studies in the Value Added Tax (VAT) context finding that prices react stronger to VAT increases than to decreases, e.g., Benzarti et al. (2020), we are not aware of studies that evaluate this asymmetry systematically in the context of business profit taxes. Third, adjustment costs may imply that tax changes of different size have different effects. We provide a systematic evaluation of this question based on randomized variation in the tax size, thereby complementing a small set of papers that compare small and large tax reforms/kinks in other contexts (Chetty et al., 2011; Fuest et al., 2018).

Fourth, given the limitations of observational data in measuring prices, empirical evidence on the extent to which taxes are passed through to customers via prices is notably scarce.

[7] We contribute to a large set of papers that examine the effect of business taxes on single indirect margins by studying in one approach how managerial decision-making in response to business taxes influences the major stakeholder groups (owners, employees, customers) indirectly and showing that the usage of these indirect margins strongly depends on the sign of the tax change, . This for example includes papers on investment effects (Hanlon et al., 2015; Zwick and Mahon, 2017; Ohrn, 2018; Chen et al., 2023; Jacob and Zerwer, 2024), tax avoidance (Dyreng et al., 2022) or CEO compensation (De Simone et al., 2022; Bornemann et al., 2023).

A few recent papers show that the burden is with customers (Kosonen, 2015; Benzarti and Carloni, 2019; Kang et al., 2021; Dedola et al., 2022; Baker et al., 2023; Jacob et al., 2023). We measure prices directly and are the first to study price effects of business taxes along with other adjustment margins. Finally, we find that, on average, over all survey answers our incidence findings are comparable to those in the existing archival literature, at least for those margins for which we have archival evidence. Hence, policymakers can use surveys like ours to form ex-ante expectations about the effects of taxes on variables that are not examined in existing empirical work or in situations/settings without exogenous tax variation and credible identification.

2.2 Survey Design and Data

2.2.1 Survey and Sampling

Our tax incidence questions were fielded in the second wave of the German Business Panel (GBP), which launched on November 16, 2020 and closed on June 24, 2021. The GBP constitutes a large-scale survey of executives and high-level decision makers of companies operating in Germany, which periodically assesses their views and expectations regarding topics in accounting and tax policy. A detailed overview of the survey methodology and content is provided by Bischof et al. (2024). Firms participating in the GBP closely align with the target population in terms of industry affiliation. However, there is a slight under-representation of small firms and sole proprietors, and a corresponding over-representation of larger firms when contrasted to the universe of German firms.[8] To address this issue, we construct survey weights to make our sample representative of the broader German firm population.[9]

[8] It should be noted that firms in our population are on average naturally considerably smaller than listed US firms from the Compustat Northamerica population.

[9] Appendix A.4 details the weighting procedure and demonstrates that our survey weights effectively enhance the representativeness of our sample, bringing it closer to the broader German firm population. Furthermore, Table 3 and Figure A.14 show that the unweighted and weighted results are comparable, reinforcing confidence that our findings are representative of the broader German firm population. Additionally, Table A.2 in the Appendix A.5 compares firms that completed the survey with those in the Orbis database

The contact information of firms was obtained from the Bureau van Dijk Orbis database. The subsample of firms that participated in our survey was drawn randomly from the overall address pool and invited to participate in our online survey via email. A total of 8,392 respondents completed the questionnaire used for this paper. We sent invitation e-mails for the online survey on 45 work days between November 16, 2020 and January 22, 2021. Firms were randomly assigned to one of the 45 days. After 7, 14, and 28 days, we sent a reminder e-mail. We collected survey responses from November 16, 2020 through June 24, 2021. The overall response rate in the survey was 2%, and about 83% of respondents completed the survey.[10] Approximately 87% of survey respondents are the owner or CEO of the corresponding firm.[11]

2.2.2 Tax Incidence Survey Questions

The survey experiment started with the following question:

> *"Assume that your company has a (1%/10%/25%) permanently higher profit tax burden as a result of a tax increase. How do you finance the additional burden?"*

Figures A.2 and A.5 provide examples of the tax incidence questions as appearing in the online interface of the survey in German. Respondent companies were randomly assigned to one of the six different treatment groups defined by the combination of i) direction of tax change, either increase or decrease; and ii) magnitude of the tax change, either 1%, 10% or

that did not participate or did not complete the survey, showing similarity in terms of total assets, number of employees, operating revenue, and employee costs. This indicates that firms self-selecting into the survey (and completing it) do not appear to systematically differ in key financial characteristics from those in the Orbis database that are not part of our sample.

[10] Figure A.1 in Appendix A.1 illustrates the distribution of the completion share in our data. We exclude responses with a completion share of less than 90% from our analysis sample.

[11] For some of the larger firms, the CFO might be better equipped to provide an answer, even though the CEO ultimately is responsible for decision-making. Consistent with this notion, we see that the share of responding CEOs is significantly lower for the larger companies, whereas the share of respondents from the finance, controlling or accounting department increases as we move through the size distribution. This suggests that our survey is redirected to the appropriate decision-maker within the firm, who is capable of providing relevant answers to our survey questions.

25%.[12] We opted to assign percentage changes in tax burden over percentage point changes in statutory tax rates, as German firms face different tax rates depending on their legal form and hence are at different baseline levels of tax rates. These differences in applicable tax rates also motivated us to choose the term profit tax for our question over something more specific such as the corporate income tax, as respondent firms might be subject to different taxes. The term profit tax is inclusive of the German LBT, which has different effects on pass-through entities and corporate firms, the PIT, as well as the CIT.[13] Hence, respondents likely interpreted the treatment as a one-time change in their profit tax burden due to an adjustment of the statutory tax rate relevant for them. This implies that the tax shock is distortionary (as the firm may implement responses to react to the change in taxes) and interpreted in a way that it no only affects the firm itself but also its competitors.

After receiving the treatment, firms were presented with an exhaustive list of categories and could select shares attributable to each of them, either by using the slider next to each category, or by entering them directly via the boxes on the far right. Entered shares had to be non-negative and were required to add up to 100.[14]

Table 1 contrasts the available categories for the tax increase and decrease groups. Respondents could attribute the additional burden (in the case of a tax increase) or additional funds (in the case of a tax decrease) of the profit tax change to the following adjustment

[12] The tax decrease treatment was worded correspondingly: *"Assume that your company has a (1%/10%/25%) permanently **lower** profit tax burden as a result of a tax **cut**. How do you **distribute the additional funds**?"*

[13] The German corporate tax is levied on the income of incorporated firms. The LBT is payable by both pass-through firms and corporations, and is also applied as a tax on the profits of a business. The PIT is levied on the income earned by sole proprietors or partners in business partnerships. In the case of partnerships, partners are taxed at their respective PIT rates.

[14] This design choice effectively abstracts from the possibility of over-shifting, as only the full amount of the tax burden change can be distributed. This assumption is benign under perfect competition, as over-shifting can only occur under imperfect competition in certain circumstances (Fullerton and Metcalf, 2002). We note that even if over-shifting occurs, it is not necessarily the case that profits increase, which is the only instance our design would not be able to capture (as this would imply financing more than 100% of the tax increase through price changes). Hence, even in light of this limitation, we view our approach to be valid for many contexts and markets relevant in practice. Another potential limitation of this approach is the restriction of shares to be positive. Some theoretical models produce opposite-sign adjustments. One example of this phenomenon occurs in Dwenger et al. (2019), where a tax cut decreases employment through a wage bargaining channel. We traded off this limitation with the possibility that respondents might view negative shares as unintuitive and decided the latter to be more severe.

margins: wages and salaries, employment, distributed profits[15], retained earnings or reserves, consumer prices, investments, use of tax saving opportunities, and other categories (in the form of an open field question).[16]

In the following, we motivate the choice of our set of adjustment margins and how they affect the three stakeholder groups we consider. We distinguish between adjustment margins that have a direct effect on stakeholder groups and those with more indirect implications. For the direct impact on wages and employment, profit distributions and prices, the affected stakeholders are straightforward (workers, owners and consumers, respectively). However, managers may also choose adjustment margins that influence future pre-tax profits, thereby indirectly affecting stakeholder incidence. Numerous studies have documented the influence of tax changes on investment decisions (Hanlon et al., 2015; Zwick and Mahon, 2017; Ohrn, 2018; Giroud and Rauh, 2019; Chen et al., 2023). An increase in profit taxes might prompt managers to curtail capital investments. This reduction could lead to diminished labor productivity and lower *future* wages (Arulampalam et al., 2012). According to classical tax incidence literature (Harberger, 1962; Fuest et al., 2018), higher taxes can also lead to increased product prices for customers due to lower output as firms reduce investment. This often results in a shrinkage at both firm and industry levels (Djankov et al., 2010; Brekke et al., 2017; Giroud and Rauh, 2019), driven by marginally profitable firms exiting the market or downsizing at the firm level due to rising marginal costs of capital and labor (Jacob et al., 2023). Thus, tax-induced investment changes can significantly impact the incidence on firm owners, employees, and customers in the long run.

A higher tax burden may also incentivize firms to exploit tax saving opportunities more extensively. Successfully leveraging these opportunities allows firms to moderate the need for adjustments in investments, wages, or output prices, thereby lessening the impact on capital and labor (Jacob et al., 2023). The propensity to utilize such tax saving strategies may hinge

[15] Respondents only saw the category matching their stated legal form.

[16] In addition, the tax increase treatment groups had the option to select increases in debt capital in order to reflect the possibility that there might not be resources in the company to finance the additional burden.

on factors like the labor supply elasticity, tax deductibility options, or the degree to which higher tax incidence affects shareholders versus employees (Fuest et al., 2018; Dyreng et al., 2022). Additionally, higher taxes could lead to an increase in debt financing or a decrease in retained earnings (Djankov et al., 2010). Such shifts may complicate financing of investment or exacerbate principal-agent problems, particularly when a larger proportion of investment is externally financed (Ohrn, 2018).[17] Changes in financing structures, coupled with negative investment effects, can therefore result in greater tax incidence on either workers or firm owners, contingent upon factors like the labor supply elasticity and capital mobility.

The *Others* category was added to ensure that no relevant incidence category was missing. If a respondent selected a positive share, she could give a free-text answer indicating the missing category or categories. Figures A.10 and A.11 illustrate the text answers given in the *Others* category prompt for the tax decrease and tax increase treatments, respectively. The most frequently stated missing category in the decrease treatment seems to be the use of the additional funds for debt repayment, as indicated by the high frequency of responses referring to liabilities, repayment, or loans. Another factor seems to be that several companies were not making any profits, rendering a reduction in the profit tax burden impossible.

The order, in which the answer options were presented to the participants, was not randomized. While this could theoretically introduce some ordering effects, we are confident that this is not a major concern in our setting for two reasons. First, as the entered shares had to sum to 100, respondents could not consider the options in isolation but in the context of the full picture. Moreover, respondents could only proceed to the next screen once the sum constraint was satisfied. Second, the descriptive survey results presented below do not reveal a pecking-order pattern, in the sense that the first few categories are chosen to a larger degree than the others. Furthermore, we acknowledge that in the final implementation of the online survey by the GBP, the order of the second and third categories was switched across the increase and decrease treatment groups. This is illustrated in Figure A.5 in combination

[17] This effect is reversed in the case of tax decreases. For example, see Ohrn (2018).

Table 1: Incidence Categories

Tax Increase	Tax Decrease
Decreased payment to employees	Increased payment to employees
Reduction of jobs	Creation of additional jobs
Lower distributions to partners	Higher distributions to partners
Lower distributions to shareholders	Higher distributions to shareholders
Decrease in retained earnings/reserves	Increase in retained earnings/reserves
Price increases (for customers)	Price reductions (for customers)
Lower investments	Higher investments
More use of tax saving opportunities	Less use of tax saving opportunities
Others(*)	Others

Note: Table 1 shows the different incidence categories available to respondents for the tax increase and decrease treatment arms, respectively. Based on participants being randomly assigned to either the tax increase or tax decrease group, they are presented with the following question: *Assume that your company has a (1%/10%/25%) permanently higher/lower profit tax burden as a result of a tax increase/cut. How do you finance the additional burden/distribute the additional funds?* (*): Note that the category *Decrease in Debt Capital* was not available. We therefore integrated the *Increase in Debt Capital* category into the category *Others*.

with Figure A.2. This implementation issue does not affect the within-sign experimental design, i.e., the different tax increase treatments are consistent with each other. For the comparison of effects between tax increases and decreases, on the other hand, we cannot rule out that the differential ordering has an effect. However, it is unlikely that this inconsistency drives our results, for the same reasons mentioned above.

2.2.3 Summary Statistics and Covariate Balance

The survey collects data on fundamental company characteristics such as legal form, industry affiliation, as well as revenue and number of employees in the previous year. Table 2 provides some insights about the distribution of company characteristics in our sample.[18] The companies in our data are mostly corporations, with a share of about 73%, followed by sole proprietors and partnerships with shares of 13% and 14%, respectively. On average,

[18] As the set of survey respondents is based on available contact information in Bureau van Dijk's Orbis database, we have in principle a much larger set of available variables. However, we can only merge this information to the survey responses if the respective respondent agreed to linking the responses to external data sources. As the share of data linkage agreements is only about 50% and potentially constitutes a non-random subset of responses, we refrain from only using linkable data in our main analysis.

our sample firms have EUR 20 million revenues and employ 68 workers. The majority of companies operate in the services, manufacturing, and retail sectors, with shares of 33%, 17%, and 16% respectively.

In order to investigate how well the randomization procedure worked, we conducted multiple balance tests utilizing the available characteristics of the survey respondents in our data. Figure A.12 summarizes the results of our balancing tests. The figure shows the p-values for difference-in-means tests for each characteristic across every combination of treatments. The overall share of significant differences is 2.8%, which is substantially below the chosen significance level of 5%. The adjusted p-value using the Benjamini and Yekutieli (2001) correction is equal to one for every test, which gives us confidence that treatment assignment was successfully randomized.

Table 2: Descriptive Statistics

	N	Mean	SD	P10	Median	P90
Revenue	5,259	19,831,465	486,011,410	90,000	720,000	6,500,000
Num. Emp.	6,749	68	2,221	1	5	37
Corporation	6,749	0.73	0.44	0	1	1
Sole Prop.	6,749	0.13	0.33	0	0	1
Partnership	6,749	0.14	0.35	0	0	1
Manufacturing	6,749	0.17	0.38	0	0	1
Construction	6,749	0.071	0.26	0	0	0
Trade	6,749	0.16	0.36	0	0	1
Services	6,749	0.33	0.47	0	0	1
Other	6,749	0.27	0.45	0	0	1
CEO	6,749	0.7	0.46	0	1	1

Note: Table 2 shows descriptive statistics for our analysis sample. The sample includes responses with a completion rate of 90 percent or more and non-missing observations for all control and weighting variables.

2.3 Full Distribution of Tax Changes

In this section, we present our main results on the incidence of profit taxes. We exploit both the direction and intensity of our hypothetical treatment to investigate how factor-specific responses depend on the nature of the tax change.

2.3.1 Empirical Strategy

For each of our incidence categories, we estimate the following equation using Ordinary Least Squares (OLS):[19]

$$\begin{aligned} y_i = \beta_0 + \beta_1 \text{Increase}_i + \beta_2 \text{Medium Change}_i + \beta_3 \text{Large Change}_i \\ + \beta_4 \text{Increase}_i \times \text{Medium Change}_i + \beta_5 \text{Increase}_i \times \text{Large Change}_i + \varepsilon_i, \end{aligned} \tag{1}$$

where the dependent variable y_i is the share attributed to the respective category. The independent variables of interest are Increase_i, Medium Change_i, and Large Change_i and their interactions, which are indicator variables for the respective sign and magnitude (10% and 25%, respectively). The set of coefficient estimates, $\beta_i, i = 0, \ldots, 5$, allows us to empirically test for asymmetry between tax increases and decreases, as well as the incremental effects of the magnitude of the tax change.[20]

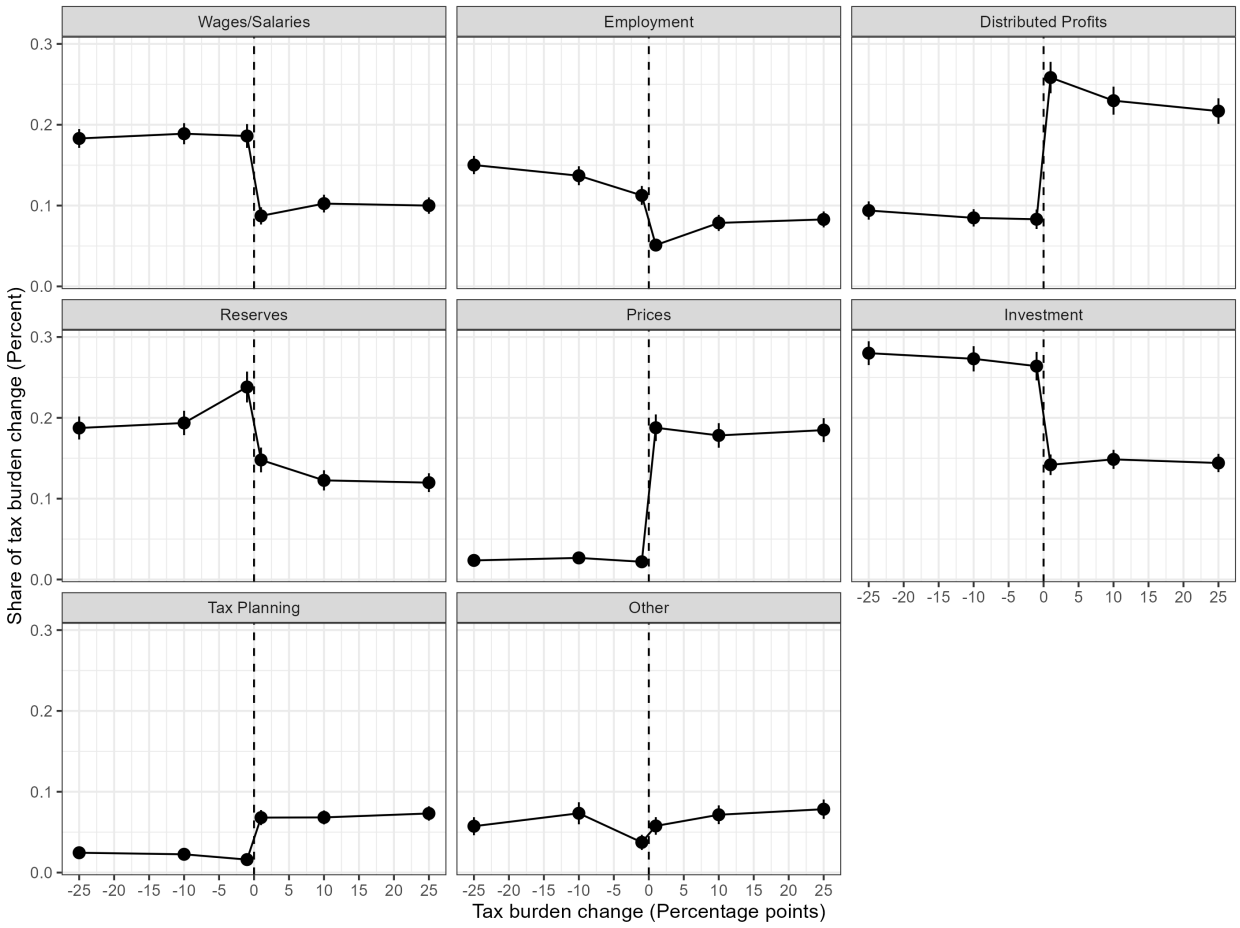
2.3.2 Results

We begin our analysis by plotting the aggregated coefficients for different treatment groups across the intensity of our treatment. Figure 1 presents incidence curves that illustrate how the average usage of categories varies with the dose of our tax treatment. These incidence curves reveal a substantial asymmetry in the effects of our treatment. For almost all cate-

[19] In addition to estimating Equation (1) using OLS, Section A.7.2 in the Appendix presents results from a Multivariate Fractional Logit (MFL) model to evaluate the robustness of our findings. Unlike OLS, which ignores the bounded nature of the outcome variables and the unit-sum constraint, the MFL model explicitly accounts for the fractional structure of the response variables. The comparison of average partial effects between the two methods shows a high degree of consistency, further strengthening confidence in our main results.

[20] In a robustness exercise, we include additional controls to improve the precision of our estimates. These controls include economic sector dummies (Manufacturing, Construction, Trade, and Services), a set of dummies for the legal form of the company, a set of dummies for small, medium, and large firms (measured by their annual revenues), and two indicators for firms that experienced a significant impact from the COVID-19 pandemic on their revenues or net income. We define a firm as significantly impacted by COVID-19 if the respondent was in the lowest quartile with respect to the stated percentage change in revenues and net income due to COVID-19, respectively. The results are provided in Table A.5 in Appendix A.7.1.

Figure 1: Tax Change Effects across Outcome Margins and Treatment Groups



Note: Figure 1 shows the effects of tax burden changes on different decision margins. Each panel shows the estimated incidence share for the respective category across the six different treatments based on the specification in Equation 1. Robust confidence bounds are indicated by vertical lines.

gories, they exhibit pronounced discontinuities at the zero line, where the treatment shifts from a tax decrease to a tax increase. Interestingly, aside from this discontinuity at zero, the incidence curves remain relatively flat across treatment doses, with some exceptions. For most categories, there appears to be little difference between a 1%, 10%, or 25% tax change. As a first key takeaway, we conclude that, in our setting, the asymmetry between tax increases and decreases seems to play a major role, whereas the treatment dose exhibit less pronounced variation. Building on this insight, we now explore the results for the individual categories in greater depth.

Next, we examine the frequency with which categories are chosen. Table 3 presents

summary statistics for the outcome variables across the distinct sign treatment arms, i.e., for tax increases and decreases, pooled over the three tax change magnitudes: 1%, 10%, and 25%. The fourth and fifth columns display the unweighted and weighted averages of category usage, respectively, while the three rightmost columns show the sample percentages of shares that are equal to zero, one, or fall within the open interval $(0, 1)$. We observe only small differences in means between the unweighted and weighted incidence shares. None of these differences are substantial.[21] The sample percentages indicate that companies made extensive use of most categories. Only the categories *Prices* and *Tax Planning* were used by less than 10% of respondents in the tax decrease group.

Table 3: Descriptive Statistics Incidence Shares

			Mean		Sample Percentages		
Outcome	Treat. Sign	Obs	Unwghtd.	Weighted	$s_{im} = 0$	$s_{im} = 1$	$s_{im} \in (0, 1)$
Wages/Salaries	Decrease	3348	0.186	0.179	0.393	0.020	0.587
	Increase	3401	0.097	0.092	0.637	0.009	0.354
Employment	Decrease	3348	0.133	0.121	0.567	0.007	0.426
	Increase	3401	0.071	0.064	0.734	0.008	0.258
Distributed Profits	Decrease	3348	0.087	0.093	0.701	0.020	0.279
	Increase	3401	0.235	0.219	0.463	0.056	0.480
Reserves	Decrease	3348	0.206	0.212	0.459	0.054	0.487
	Increase	3401	0.130	0.133	0.605	0.026	0.368
Prices	Decrease	3348	0.024	0.027	0.892	0.002	0.105
	Increase	3401	0.184	0.199	0.457	0.047	0.495
Investment	Decrease	3348	0.272	0.264	0.318	0.047	0.636
	Increase	3401	0.145	0.149	0.495	0.012	0.492
Tax Planning	Decrease	3348	0.021	0.021	0.908	0.004	0.088
	Increase	3401	0.070	0.067	0.707	0.009	0.284
Other	Decrease	3348	0.056	0.067	0.881	0.032	0.087
	Increase	3401	0.069	0.076	0.798	0.030	0.172

Note: Table 3 shows descriptive statistics for the outcome variables of the experiment. We require a completion rate of at least 90% and non-missing values for all control variables to be included in the final sample. The survey weights are calibrated for representativeness of the German firm population (see Appendix A.4).

Next, we present exact figures and statistical tests for the patterns shown in Figure 1,

[21] We explore the sensitivity of our findings with respect to the applied weighting scheme in Appendix Section A.7. Our treatment effects are robust to applying survey weights. Due to the higher level of precision in the unweighted estimates, we use unweighted regressions for the rest of the paper.

summarized in Table 4. It reports the level estimates of category usage for each treatment group, i.e., for each combination of tax change magnitude and sign, and compares these estimates across both dimensions. For corresponding tax change magnitudes, we provide test statistics for the difference in coefficients, with significant differences denoted by stars (* p $< 0.1, ^{**}$ p $< 0.05,$ *** p < 0.01). For corresponding tax signs, we report F-statistics for the joint test of coefficient equality, where a rejection indicates that at least one pair of estimates differs significantly.

Regarding the effects on workers, we find that for tax increases, workers bear about 17% of the profit tax incidence, with 10% channeled through reduced wages and 7% through reductions in employment. For wages, there is little difference between treatment doses, ranging from 8.7% to 10.2%, with an insignificant F-statistic for the joint test of coefficient differences. For employment, on the other hand, we find that the magnitude of the tax increase affects the share attributed to this category. While only 5% of a 1% tax burden increase is financed through reductions in employment, this share rises to 8% when the tax burden increase amounts to 25%.

For tax decreases, on the other hand, we find an incidence on workers that is almost twice as large as for tax increases, at 32%, with 19% resulting from higher wages and 13% from the creation of new jobs. One explanation for the lower impact on workers in the tax increase treatments compared to the tax decrease treatments could be the downward stickiness of wages as well as employment protection laws in Germany. With respect to magnitude, the pattern closely mirrors that of the tax increase treatments. While there are only minor differences in incidence across treatment doses for wages, employment shows significant differences between the lowest and higher treatment intensities, increasing from 11% for a 1% decrease in tax burden to 15% for a 25% cut in tax burden. The stronger employment response to larger tax changes—compared to smaller ones, and in contrast to the relatively stable wage response—is likely driven by wage rigidity resulting from collective bargaining agreements and minimum wage regulations (Fuest et al., 2018), which constrain

firms' ability to adjust wages. In contrast, firms adjust employment more strongly when tax changes are substantial enough to justify the costs of hiring or layoffs (e.g., severance payments, retraining, and administrative costs).

When turning to profit distributions, the survey data suggest that firm owners bear about 24% of the additional tax burden, compared to merely 9% of additional funds received in the case of a tax cut. Similar to the employment effects, the incidence seems to be affected by the magnitude of the tax change as well, however, this only holds true for tax increases. While firm owners bear about 26% of a small tax change directly through reduced profit distributions, this share decreases by about 4 percentage points for large profit tax burden increases. Thus, it becomes apparent that for higher tax increases, the incidence shifts from firm owners to workers. This pattern is consistent with firm owners covering modest increases in tax burden out of their own pockets, but are less willing or able to cope with the additional tax burden as the magnitude of the tax increase grows. For larger tax changes, more drastic measures become necessary, such as job cuts, to keep the company profitable.

For reserves, we also see some differences, with a sizable incidence of 13% in the case of increases, compared to 21% for the tax decrease treatment. This might at least partly be due to the prevailing economic conditions when the survey experiment was conducted, as companies were in financial distress due to the impact of the Corona crisis and in need of cash buffers as future developments were hard to predict. In terms of magnitude, reserves seem to be affected similarly to distributed profits, as the shares attributed to these categories decrease in the absolute value of the tax change. Reserve building decreases from 24 to 19% for large tax cuts, whereas a three percentage point lower share of reserves is used to cope with very large tax increases. This pattern would again be consistent with firms having limited buffers to cope with surprising cost changes and at some point have to adjust inputs in order to remain profitable.

The incidence on consumer prices features by far the highest asymmetry we detect. On average, the additional tax burden is passed on to consumers by 18%, whereas only 2%

Table 4: Level Estimates for Treatment Combinations

Sign	Low	Medium	High	F-statistic
Wages/Salaries				
Decrease	0.1861 (0.0075)	0.1889 (0.0067)	0.183 (0.0059)	0.22
Increase	0.0873 (0.0054)	0.1024 (0.0056)	0.1 (0.0052)	2.23
Difference	-0.099***	-0.086***	-0.083***	
Employment				
Decrease	0.1125 (0.006)	0.1369 (0.006)	0.1502 (0.0058)	10.53***
Increase	0.0511 (0.004)	0.0785 (0.0051)	0.0829 (0.005)	15.29***
Difference	-0.061***	-0.058***	-0.067***	
Distributed Profits				
Decrease	0.0831 (0.0061)	0.0848 (0.0055)	0.0939 (0.0058)	1
Increase	0.2584 (0.0099)	0.2298 (0.0088)	0.2169 (0.0081)	5.33***
Difference	0.175***	0.145***	0.123***	
Reserves				
Decrease	0.2381 (0.0097)	0.1936 (0.0077)	0.1874 (0.0073)	9.47***
Increase	0.1479 (0.0078)	0.1226 (0.0064)	0.1198 (0.0059)	4.53**
Difference	-0.090***	-0.071***	-0.068***	
Prices				
Decrease	0.0219 (0.0029)	0.0267 (0.0032)	0.0236 (0.0025)	0.63
Increase	0.1878 (0.0084)	0.1782 (0.0078)	0.1848 (0.0076)	0.38
Difference	0.166***	0.152***	0.161***	
Investment				
Decrease	0.2639 (0.009)	0.273 (0.008)	0.28 (0.0075)	0.94
Increase	0.1419 (0.0065)	0.1486 (0.006)	0.1441 (0.0058)	0.3
Difference	-0.122***	-0.124***	-0.136***	
Tax Planning				
Decrease	0.0161 (0.0027)	0.0226 (0.0029)	0.0245 (0.0029)	2.61*
Increase	0.068 (0.0048)	0.0682 (0.0044)	0.0731 (0.0045)	0.4
Difference	0.052***	0.046***	0.049***	
Other				
Decrease	0.0374 (0.0048)	0.0734 (0.007)	0.0574 (0.0057)	9.82***
Increase	0.0577 (0.0056)	0.0715 (0.0059)	0.0784 (0.0061)	3.35**
Difference	0.020***	-0.002	0.021**	

Note: Table 4 shows the incidence level estimates for the different treatment cells estimated from Equation (1). Test statistics for differences between tax increases and decreases are given below the coefficient estimates for each intensity pair. F-statistics for the joint test of equality of coefficients are given in the rightmost column, where a significant result indicates that at least one pair of coefficients is different. Robust standard errors for the composite coefficients are given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

of the additional funds available after a tax decrease would be used to lower prices. This pass-through rate seems to be unaffected by the size of the tax change, as the F-statistic for differences in coefficients is insignificant for both treatment signs. While this result complements a few studies in the VAT context, which find that prices react more strongly to VAT increases than to decreases (e.g., Benzarti et al., 2020), we are not aware of studies that systematically evaluate this asymmetry in the context of business profit taxes.

Furthermore, we detect asymmetries for the responses of investment to tax changes. The averages suggest that investment levels are less affected by tax increases as they are by tax decreases. With 27%, investments are almost twice as responsive to tax decreases than increases (15%). This asymmetric response is implied by the heterogeneous impact of the tax change on workers and consumers, as different pass-through possibilities across the sign of the tax change directly affect the investment sensitivity (Jacob, 2022).

To better understand the underlying mechanisms that drive companies to adjust their investment behavior in response to a tax change, we asked respondents selecting shares for investment in excess of 5% about their reasoning for this choice. Figure A.8 presents an example of the question as appearing in the online interface of the survey. Participants could choose values on a scale from 0 to 100, where 0 indicated that more (less) funds would be available for investment after a tax decrease (increase), and 100 indicated that the investment was more (or less) worthwhile. A lower value selected therefore indicates that the company faces capital constraints and a higher value suggests that the effects of the tax change on the profitability of the investment matters more. Figure A.13 illustrates the results of these follow-up questions. We binned the possible responses into three categories. Answers below 25 on the scale were attributed to the category *Capital Restriction*, answers between 26 and 75 were classified as indicating that both reasons were equally important, and answers above 76 were taken as indication that the profitability aspect predominated.

Our results indicate that the majority of companies appear to exhibit an investment response due to capital constraints, rather than the profitability of investment projects changes

after a tax shock. This finding aligns well with the investment behavior of United States (US) firms following the American Jobs Creation Act (AJCA), which notably reduced the tax burden on US companies. Faulkender and Petersen (2012) observe that capital-constrained firms, in particular, significantly increased their investments after experiencing a positive cash flow shock due to the AJCA. Similarly, Zwick and Mahon (2017) find that tax incentives related to bonus depreciation lead to an increase in investment and that profitable firms respond more strongly to incentives when they receive immediate cash flows from the reform, compared to tax-loss firms, which must wait to benefit from these deductions in the future.

Finally, we also detect some differences in the use of tax-saving opportunities in response to our hypothetical treatments. There is a consistent 5-percentage-point difference in the effect on tax planning between tax increases and decreases across the various magnitudes of the treatment. Firms appear more reluctant to adjust their tax-saving strategies in response to tax decreases, as the potential benefits may not justify the effort and costs associated with restructuring financial or operational decisions. In contrast, tax increases create a stronger incentive for firms to engage in tax planning, as they seek to mitigate the additional burden, leading to a more pronounced response. From small to large tax changes, there is a slight increase in category usage for tax decreases, which is statistically significant; however, with a change of merely one percentage point, this difference is not economically meaningful.

2.3.3 Response Cross-Validation

After establishing our baseline results, we examine how they compare to prior studies on the incidence of corporate taxes using (non-survey) observational data. To assess the reliability of our survey estimates in predicting real economic behavior, we follow Colarieti et al. (2024) and apply a *cross-validation method*. This approach evaluates how well our incidence estimates from hypothetical tax changes align with those from previous research based on observational data.

For the cross-validation, we present incidence estimates from prior studies, distinguishing between tax increases, tax decreases, and studies that pool both types of tax changes. Table 5 summarizes key details for each cross-validation, including the reference study, the tax variation analyzed, the direction of the tax change, the country sample, the specific episode examined, the incidence estimate reported in the study, and our corresponding survey-based estimate.

We begin by outlining the general methodology used to derive corporate tax incidence estimates from our survey experiment and illustrate the process with an example. Next, we comparing our tax incidence estimates separately with findings from studies that analyze tax increases, tax decreases, or pool increases and decreases.

Incidence Calculation. The basis for the calculation of our tax incidence estimates is the results for the incidence shares presented in Table 3. We define the *initial* incidence of the profit tax as the short-term impact of the tax change on workers, firm owners, and consumers. Considering a given level of pre-tax profit, the initial incidence indicates how a change in the profit tax burden is shared across stakeholders at the margin through changes in wages, distributed profits, and prices. The second-round effect on the tax incidence, on the other hand, stems from general equilibrium effects caused by, for example, changes in firm investment behavior, which in turn affect the capital-labor ratio, the future level of pre-tax profits, as well as factor payments.

Several papers using observational data in this field (cf. Table 5) abstract from general equilibrium effects and provide evidence on the initial incidence. In our setup, the initial incidence corresponds to the categories *Wages and Salaries*, *Distributed Profits*, and *Price*, as these are the most commonly used categories in previous studies.[22] Although we observe some aspects of the mechanisms behind second-round incidence, such as changes in investment or tax planning behavior, deriving the total incidence of a profit tax change requires

[22] One could argue that changes in retained earnings or reserves could also be attributed to the owners of the company, in addition to distributed profits. However, this holds true only if the reserves are eventually distributed to the firm's owners rather than used for future investments.

a theoretical model that accounts for the feedback effects of second-round incidence on the initial incidence categories. While we abstract from such a general equilibrium model, our results for second-round incidence effects nevertheless provide valuable insights for future theoretical research.

Given our interpretation of the *Wages/Salaries*, *Distributed Profits*, and *Prices* categories as components of the initial incidence of the profit tax on workers, firm owners, and customers, we can compare our results with existing findings based on observational data. Since the impact on these categories is expressed as a percentage of the change in tax burden, they are measured in the same units and can be directly compared. For each cross-validation in Table 5, we determine the initial incidence for the relevant category based on the factors (*Wages and Salaries*[23], *Distributed Profits*, *Prices*) considered in the previous study, as well as the direction of the tax change (*increase*, *decrease*, or *increase and decrease (pooled)*).

For example, the incidence estimates in Fuest et al. (2018) are derived from a sample of tax increases in Germany, where the authors analyze the incidence of the German LBT on workers and firm owners. To compare their incidence estimates with ours, we use the incidence shares from Table 3. For our example, we define the initial incidence on workers as the ratio of the *Wages and Salaries* incidence share to the sum of the *Wages/Salaries* and *Distributed Profits* incidence shares for tax increases, i.e., we compute the incidence as $0.097/(0.097 + 0.235)$. The incidence on consumers is defined analogously.

This definition is related but not identical to the concept of initial incidence used in earlier literature. For instance, Fuest et al. (2018) estimate the incidence of the German LBT on workers and firm owners by calculating their respective welfare changes within a simple partial equilibrium model. In their framework, the economic incidence of the tax is defined as the welfare change for each group relative to the total welfare change across all groups. In contrast, our measure is based on the change in tax revenue, abstracting from

[23] We do not include the employment adjustment margin in our cross-validation exercise, as all the referenced studies use wage adjustments to measure worker incidence. To better compare our results with these previous studies, we therefore include only the wage margin in calculating worker incidence.

dead-weight losses and over-shifting—both of which can result in a tax burden exceeding the generated tax revenue (Fullerton and Metcalf, 2002).

Tax Increases. Regarding tax increases, we compare our incidence estimates with three observational studies, as presented in Table 5, which assess tax incidence in the context of corporate tax increases. Examining variations in LBT in Germany, Fuest et al. (2018) find that 51% of the tax burden falls on workers through lower wages, while the remaining 49% is borne by firm owners. In contrast, Risch (2024), using a panel of S corporations in the United States and variation in business income tax induced by changes in personal income taxes, estimates a smaller worker incidence of 11%–18%. This discrepancy may stem from differences in firm size between the samples: the average (median) firm in Fuest et al. (2018) has 265 (53) employees, whereas the firms analyzed by Risch (2024) are significantly smaller, with an average of 20 employees (7). Our estimate of worker incidence (29%) falls between these two studies. This aligns with our sample's firm size, which averages 68 employees (median: 5) and is hence also positioned between the samples in Fuest et al. (2018) and Risch (2024).

Regarding tax incidence on consumers and firm owners, Jacob et al. (2023) find that, based on gas price data and variations in corporate taxes in Germany, 64% of the tax burden is borne by consumers, with the remaining 36% by firm owners. The study assumes no burden falls on workers due to minimum wage regulations in Germany and missing data on wages. When considering only firm owners and consumers, our estimates indicate a more balanced distribution of the tax burden: firm owners bear 56% and consumers 44%. Our sample consists of firms from various industries in Germany, including sectors with higher price elasticity and, therefore, greater consumer power, such as restaurants and electronics, compared to the gasoline market. Consequently, our estimate of consumer tax incidence is somewhat lower.

Tax Decreases. Analyzing previous studies on corporate tax cuts, estimates of the

incidence on workers range from 40% to 80%.[24] Using U.S. worker-level filings linked to corporate tax returns, Dobridge et al. (2021) investigate the impact of the Domestic Production Activities Deduction (DPAD) on wages, finding that 80% of the tax burden is passed on to workers, with the highest earnings gains concentrated among high-income employees. Similarly, Carbonnier et al. (2022) analyze a corporate tax credit tied to the payroll share of workers earning less than 2.5 times the minimum wage and estimate a wage incidence of 40% to 60%. Kennedy et al. (2024) examine the effects of the Tax Cuts and Jobs Act (TCJA) on firms and workers' income distribution using an event study design that compares similarly sized C corporations and S corporations within the same industries. Their approach exploits the fact that C corporations received a significantly larger tax cut than S corporations. In terms of distributional effects, they estimate a short-run incidence of 51% on firm owners, with the remaining share accruing to workers. Expanding their analysis beyond factor incidence to account for earnings distribution and owner-workers, they find that 80% of tax cut gains benefit the top 10% of earners—many of whom are both workers and firm owners—while the remaining 20% flow to the bottom 90%. Likewise, Duan and Moon (2024) leverage variation in corporate tax rates resulting from a small business tax cut in Quebec (Canada) to examine its effect on worker earnings. Their findings suggest a strong tax incidence of 73% on workers, accounting for both those with and without ownership stakes.

Compared to these previous estimates of tax incidence from corporate tax reductions, our findings indicate a worker tax incidence of 68% when considering the categories wages and distributed profits. This aligns most closely with the results of Duan and Moon (2024). A key

[24] Two additional studies on corporate tax decreases, Dwenger et al. (2019) and Ohrn (2023), also analyze worker tax incidence but are not directly comparable to our estimates. Dwenger et al. (2019) exploit exogenous variation in effective corporate tax burdens resulting from two tax reforms in Germany and estimate a worker tax incidence between 19% and 28%. However, their estimate is based on a combination of positive wage effects and negative employment effects. Since our experimental design does not allow for negative factor adjustments for tax decreases, a direct comparison with their preferred estimate is challenging. Ohrn (2023) analyzes the effect of two corporate tax breaks in the U.S. on the compensation of the five highest-paid executives and finds that executive tax incidence ranges between 17% and 25%. In comparison, our worker incidence captures a broader measure of tax incidence, encompassing both high- and low-income workers, making a direct comparison less suitable.

factor explaining this similarity is the composition of our sample, which consists primarily of smaller firms, with 67% employing fewer than 10 workers. In such firms, the owner-worker incidence plays an important role in determining worker incidence, whereas in larger firms, owner-workers may be less prevalent.

Tax Increases and Decreases - Pooled. Finally, we compare our estimates with studies that pool tax increases and tax decreases to calculate corporate tax incidence.[25] To better align with the relevant incidence margins used in the referenced studies, we pool the incidence shares from Table 3 for tax increases and decreases.

Using state-level variation in corporate taxes over time in the United States, Baker et al. (2023) estimate the tax incidence on consumers, workers, and firm owners. They find that 28% to 36% of the tax incidence falls on workers, around 20% on firm owners, and 43% to 51% on consumers. Our estimate of the incidence on workers is comparable (35%), but we find a lower incidence on consumers (25%) and a higher incidence on firm owners (40%). Unlike Baker et al. (2023), whose sample focuses exclusively on retail goods (e.g., groceries and drug stores) and C corporations, our sample also includes firms from industries such as manufacturing, construction, and services, as well as S corporations (27%). In particular, a large share of firms in our sample comes from the service industry (33%). These service-based industries often face more elastic demand, as consumers can delay consumption, switch providers, or seek substitutes when prices increase. This difference in sample composition likely explains our lower estimate of consumer incidence.

Liu and Altshuler (2013) estimate a worker tax incidence of approximately 60%, with a lower bound of 42% in their most conservative specification, using variation in effective marginal tax rates. Using the wage and distributed profit shares from Table 3 for both tax

[25] Arulampalam et al. (2012) examine the direct wage tax incidence by analyzing within-company and cross-company differences in tax liabilities across nine European countries. Their findings indicate a short-run incidence of 64% and a long-run incidence of 49%. However, a direct comparison with our estimates is less suitable, as their study measures the direct incidence of corporate tax on workers through wage bargaining while keeping other firm adjustment margins fixed. In contrast, our survey design allows for adjustments in other margins, such as output prices or investments. Consequently, the comparability between their results and our estimates is limited.

increases and decreases, we estimate an incidence of approximately 47%, which falls well within the range identified by Liu and Altshuler (2013). Finally, using a general equilibrium model and a structural estimation approach to assess the impact of taxes on various groups, Suárez Serrato and Zidar (2016)—along with further refinements in Suárez Serrato and Zidar (2023) and Suárez Serrato and Zidar (2024)—estimate that the incidence of the U.S. state corporate income tax falls between 38.1% and 50% on capital, 25% to 40% on workers, and 10% to 30% on landowners. Although we are unable to measure the tax incidence on landowners, making a direct comparison with Suárez Serrato and Zidar (2016) challenging, our estimates align closely with their findings for workers and capital. Specifically, our estimated incidence shares for wages (47% incidence on workers) and distributed profits (53% incidence on capital) are close to the ranges identified by the three aforementioned papers.

To sum up, the key takeaway from the cross-validation is that our survey responses reliably indicate firms' behavioral patterns in response to hypothetical tax changes. This reliability stems from the fact that these scenarios closely mirror real-world decision-making processes. Rather than being abstract or unfamiliar, hypothetical tax changes reflect the strategic financial and operational considerations that firms regularly evaluate. As a result, firms' responses to potential tax policy shifts tend to align with the actual actions observed in observational data.

Table 5: Cross-Validation (Selected Studies)

Paper	Tax Variation	Tax Change	Country	Episode	Incidence on					
					Workers	Workers: Our estimate	Owners	Owners: Our estimate	Consumers	Consumers: Our estimate
Baker et al. (2023)	Variation in state corporate tax rates	Increases and Decreases (pooled)	USA	2006-2017	Range: 28%-36%	35%	Range: 20%-21%	40%	Range: 43%-51%	25%
Carbonnier et al. (2022)	Large French corporate income tax credit	Decrease	France	2009-2015	50%, Range: 40%-60%	68%	50%	32%	-	-
Dobridge et al. (2021)	Variation in the Domestic Production Activities Deduction	Decrease	USA	1999-2015	80%	68%	20%	32%	0%	-
Duan and Moon (2024)	Corporate tax cuts	Decrease	Canada	2001-2017	73%	68%	27%	32%	-	-
Fuest et al. (2018)	Variation in local business tax changes	Increase (93% increases)	Germany	1993-2012	51%	29%	49%	71%	0%	-
Jacob et al. (2023)	Variation in local business tax rate	Increase (98% increases)	Germany	2014–2017	0%	-	36%, Range: 28%-39%	56%	64% , Range: 61%-72%	44%
Kennedy et al. (2024)	US corporate tax change (TCJA)	Decrease	USA	2013-2019	48%	68%	51%	32%	0%	-
Liu and Altshuler (2013)	Variation in corporate income tax across industry and time	Increase and Decrease (pooled)	USA	1982, 1992, 1997	60%, Lower bound: 42%	47%	40%, Upper bound: 58%	53%	-	-
Risch (2024)	Variation in top marginal personal tax rate in the United States	Increase	USA	2008-2016	11-18%	29%	80%	71%	0%	-
Suárez Serrato and Zidar (2016)	Variation in US state taxes and apportionment rules	Increase and Decrease (pooled)	USA	1980–2012	30-35%	47%	40%	53%	0%	-
Suárez Serrato and Zidar (2023)	Variation in US state taxes and apportionment rules	Increase and Decrease (pooled)	USA	1980–2012	35%	47%	38.1%	53%	0%	-
Suárez Serrato and Zidar (2024)	Variation in US state taxes and apportionment rules	Increase and Decrease (pooled)	USA	1980–2012	25-40%	47%	50%	53%	0%	-

Note: Table 5 summarizes previous estimates of tax incidence found in the literature on workers, capital/firm owners, and consumers. Own estimates are calculated based on margin adjustments in Table 3. The table highlights selected recent studies that are most suitable for comparison with our incidence estimates. A more comprehensive overview, including incidence estimates from further research, can be found in Appendix A.9.

2.3.4 Reliability of Survey Responses

The validity of our findings crucially depends on the reliability of our survey responses. Even though our results compare well to the findings of prior literature, as shown in Section 2.3.3, one might be concerned that they may be subject to behavioral or cognitive biases. More specifically, the effects we document could be driven by one or more of the following sources of bias, which have been documented by a large literature in the fields of experimental and behavioral economics (De Quidt et al., 2018; De Quidt et al., 2019; Haaland et al., 2023; Stantcheva, 2023): (i) social desirability, (ii) hypothetical nature of the treatment, and (iii) representative agent assumption. If these biases are present in our setting, our survey results may have little predictive power of actual behavior of firms, which we are ultimately interested in. After discussing each source of bias and the accuracy of manager surveys in general, we conduct several validation exercises to mitigate remaining concerns and bolster the confidence in our results. First, we show that respondents offer a high degree of reliability in stating characteristics of their firms. Second, we show that participants' stated actions are predictive of actually implemented actions.

Biases in Survey Responses. Regarding desirability bias and experimenter demand (i), managers may, for instance, hesitate to report lower wages or layoffs, particularly if they seek to be perceived as socially responsible by the experimenter (Haaland et al., 2023). However, we argue that these concerns are of limited relevance in our setting for three reasons. First, experimenter demand effects are likely less pronounced in online surveys compared to face-to-face interviews due to the increased anonymity afforded to participants (De Quidt et al., 2018; Haaland et al., 2023; Stantcheva, 2023). Second, the neutral framing of our survey regarding taxation further reduces the likelihood of experimenter demand effects (Haaland et al., 2023; Stantcheva, 2023). Third, since we employ a between-subject design, experimenter demand effects are likely less problematic than in within-subject designs (De Quidt et al., 2019). Moreover, if firms were systematically providing socially desirable answers or

attempting to influence the survey outcome with exaggerated or untruthful statements, we would not expect to observe significant differences based on the magnitude of the tax change. However, such differences are indeed present in our results. Finally, if a CEO faces unexpected tax increases and has to distribute the costs, also the actual decision making involves social aspects. Hence, the eventual decisions may also reflect social desirability concerns.

Another potential concern is the use of hypothetical tax changes in our treatments (ii). Their hypothetical nature may reduce respondent effort, as such scenarios can be difficult to translate into real-world decision-making (Haaland et al., 2023). To assess whether such bias is present, we implement several validation checks. First, we cross-validate our estimates of initial incidence on workers, firm owners, and consumers by comparing them to prior literature based on observational data sources (Section 2.3.3). Our findings align closely with existing empirical evidence, reinforcing our confidence in the validity of our survey results. Second, we evaluate the predictive power of respondents' hypothetical answers by comparing their stated actions to actual realized behavior in two distinct settings (Section 2.3.4). In the first test, we merge our survey responses with Orbis financial data and information on changes in statutory LBT rates. We then examine the correlation between managers' stated employment responses to a tax change and actual employment adjustments following LBT rate changes. In the second test, we exploit survey questions on planned employment adjustments for the following year, correlating them with observed employment changes in Orbis over the same time horizon. Both tests confirm the predictive accuracy of stated actions, strengthening the credibility of our survey approach.

Finally, the assumption that firm managers act as representative agents of their respective firms implies that a single decision-maker accurately reflects the firm's overall behavior (iii). However, this assumption becomes increasingly problematic as firm size grows, since decision-making in larger firms is typically distributed across multiple departments, stakeholders, and strategic considerations. In particular, CEOs and top executives in large firms—especially multinational corporations—operate within complex organizational struc-

tures that limit their ability to unilaterally implement their stated tax incidence shares. Unlike small business owners, who may have direct control over pricing and wage-setting, CEOs must navigate internal bureaucracy, shareholder interests, financial constraints, and competitive market pressures, all of which influence how tax burdens are ultimately distributed. However, in our sample, the majority of firms are relatively small: 81% of firms have fewer than 20 employees. Given this firm size distribution, we argue that the assumption of firm managers as representative agents is reasonable in our case.

Accuracy of Managers' Behavioral Forecasts. Besides potential biases in survey responses, another concern relates to the extent to which firm managers can accurately forecast their own behavioral responses to tax changes. Prior research using business survey data suggests that firm managers generally provide accurate forecasts of firm behavior concerning investment, firm characteristics, employment levels, and price-setting. Link et al. (2024) show that firms' planned investment volumes serve as strong predictors of realized investment levels in the subsequent year (based on survey responses). Regarding pricing behavior, survey evidence indicates that planned price changes align well with actual price changes or subsequent price revisions, based on survey questions comparing expected and past price changes, as well as price data from a selected subgroup of firms with online price records (Coibion et al., 2018). Similarly, Coibion et al. (2020) find that reported employment levels in surveys closely correspond to employment figures in administrative data. Additionally, Kumar et al. (2023) demonstrate that firms' responses to hypothetical survey treatments closely match results from randomized control trials using non-hypothetical information, such as GDP forecasts from professional forecasters. Furthermore, firm and manager characteristics—such as firm age and managerial position—largely conform to administrative or official records (Coibion et al., 2018; Coibion et al., 2020; Kumar et al., 2023).

Overall, these findings reinforce our confidence that firm managers' stated plans serve as reliable predictors of their actual behavior. In the following sections, we show that this result also holds in our survey.

Correspondence of Firm Characteristics. We begin by establishing the degree of correspondence of firm characteristics as stated by the respondents to financial statement data as indicated by Orbis. For this exercise, we merge the subset of responses that allowed for a linkage with external data sources (2,435 firms) to Orbis and investigate to what degree the stated size categories measured by revenue and number of employees in 2019 correspond to the Orbis equivalents based on the firms financial statements. A similar test was conducted by Bischof et al. (2024) for the first wave of the GBP. We form four categories for revenues and the number of employees respectively and calculate the proportion of observations that are in the same size category between the survey and the Orbis data. Limited by the availability of revenue and employee count in Orbis, we can do this comparison for 606 observations for the revenue test and for 1,516 observations for the employee test. We additionally compute Cohen's Kappa and provide its 95% confidence interval. The results for revenues are depicted in Table 6. We find a share of corresponding revenue categories amounting to 88%, with a Cohen's Kappa of 0.73, indicating a high level of correspondence. We obtain similar results for the number of employees, as indicated by Table 7. The sum of the diagonal elements is 0.81, with a Cohen's Kappa of 0.62. These results mirror closely the findings of Bischof et al. (2024) for the first survey wave. It should be noted that deviations between the survey and Orbis categorization is not necessarily indicative of incorrect survey responses. In the survey, it was specifically asked how many full-time employees subject to social security the firm employs, whereas the number of employees variable in Orbis is defined as the total number of employees included in the company's payroll. As these definitions are not necessarily congruent (e.g., due to apprenticeships, part-time employment or parental leave), slight deviations can be expected (Bischof et al., 2024). Overall, the comparison shows that firms state easily verifiable company characteristics with a high degree of reliability, which provides a general level of confidence in the survey responses.

Stated versus Realized Actions. For the next two validation exercises, we go a step further and examine the firm-level association between stated and realized actions (as op-

Table 6: Correspondence Revenue

	Orbis			
Survey	EUR 0–2 Mio.	EUR 2–10 Mio.	EUR 10–50 Mio.	> EUR 50 Mio.
EUR 0–2 Mio.	0.68	0.03	0.01	0.00
EUR 2–10 Mio.	0.05	0.17	0.00	0.00
EUR 10–50 Mio.	0.01	0.00	0.03	0.00
> EUR 50 Mio.	0.00	0.00	0.00	0.01

Note: Table 6 shows the degree of correspondence in size between the survey responses and Orbis financial data. Results are based on 606 observations. The diagonal elements sum to 0.89. Cohen's Kappa is 0.73, with 95% confidence interval [0.68, 0.79].

Table 7: Correspondence Number of Employees

	Orbis			
Survey	0-9	10-49	50-249	> 250
0-9	0.55	0.09	0.00	0.00
10-49	0.06	0.21	0.01	0.00
50-249	0.02	0.01	0.03	0.00
> 250	0.00	0.00	0.00	0.01

Note: Table 7 shows the degree of correspondence in size between the survey responses and Orbis financial data. Results are based on 1,516 observations. The diagonal elements sum to 0.8. Cohen's Kappa is 0.62, with 95% confidence interval [0.58, 0.65].

posed to easily verifiable characteristics) using Orbis data. First, we establish the predictive power of the hypothetical responses to hypothetical tax changes for actual decisions in response to realized tax changes. We exploit changes in LBT to test for the association between realized employment adjustments after a tax change and the stated incidence of a hypothetical tax change on firm-level employment. The second exercise uses two questions of the same survey wave in order to test for the predictive power of stated employment decisions in response to the COVID-19 pandemic.

Responses to changes in LBT. Firms operating in Germany are in principle subject to three types of taxes on their income depending on the legal form: CIT, PIT, and LBT. LBT apply to both corporate and transparent entities (sole proprietors and partnerships) and are levied on the firm's operating profits.[26] Importantly for our setting, the applicable rate of the LBT can be set by the local governments on the municipality level, however, the tax base and criteria for liability are set at the federal level. The decentralized authority to set tax rates provides us with a substantial amount of tax changes, which we can use to put the stated actions of our survey respondents to the test.

The starting point for this exercise is the set of 2,435 respondent firms which we are allowed to link with external data sources (such as the Orbis data base). For these firms, we require at least two years of financial data in order to be able to examine changes in outcomes, which reduces the number of firms to 2,077. Furthermore, we can only look at the behavior of firms that were subject to the same hypothetical and realized treatment, i.e., respondent firms in the tax increase group are required to having experienced an increase in the LBT in the past, whereas respondent firms in the tax decrease group are required to having experienced a decrease in the LBT.[27] This requirement further reduces our sample to a total of 382 firms experiencing 588 changes in LBT. Finally, we require these firms to

[26] For a detailed description see e.g., Fuest et al. (2018).

[27] We did not require the hypothetical and realized treatments to also correspond in terms of magnitude. This is due to the small sample size as well as our lack of knowledge about the actual change in tax burden for the realized tax treatment.

Table 8: Overview LBT Changes

Year	Decrease	Increase	Sum
2007	1	1	2
2009	4	0	4
2010	0	2	2
2011	0	5	5
2012	0	2	2
2013	0	5	5
2014	0	8	8
2015	0	7	7
2016	0	17	17
2017	0	12	12
2018	1	12	13
2019	0	22	22
2020	9	25	34
2021	3	20	23
2022	5	31	36
Sum	23	169	192

Note: Table 8 shows the distribution of LBT increases and decreases for the firms in our sample which we are able to link to external data sources.

have a non-missing observation for the change in employment in the year of the tax change. Due to the poor coverage of employment in Orbis, this cuts our sample in half, yielding a total of 192 firm-year level tax changes, out of which 169 are increases and 23 are decreases. The sample selection process is summarized in Table 9. The resulting subset constitutes approximately 3% of our initial sample of respondent firms. Table 8 summarizes the number of realized LBT changes per year for tax increases and decreases. About 2/3 of the realized tax changes occurred within a five year window around the survey period.

For the final set of firm-years, we run cross-sectional regressions for the two treatment signs separately. Column (1) of Table 10 shows the result of regressing a dummy for a positive change in employment on a dummy indicating whether the respondent firm has assigned a margin of at least 10% to the employment category in the survey.[28] The large

[28] The results are robust to variations in this threshold. For tax decreases, we are unable to detect an association if firms that indicated that less than 5% of the tax burden decrease would be used to create new employment are classified as a substantial employment impact. For tax decreases, the association

Table 9: Sample Selection LBT Validation

Restriction	Firms	Observations
Firms with Linking Agreement	2435	17576
More than one financial year	2077	14187
Any tax change	732	1202
Equal signs of treatment	382	588
Non-missing employees	165	192
Final increases	143	169
Final decreases	22	23

Note: Table 9 illustrates the sample selection process for the LBT change validation exercise.

positive coefficient indicates that firms which stated that they would hire new workers in response to a decrease in profit taxes are substantially more likely to having done so in the past in response to actual tax changes, compared to firms which did not indicate employment as a relevant margin. Column (2) of Table 10 shows the results of a similar exercise for firms in the tax increase group. Here, the outcome variable is a dummy for a negative change in employment, and the predictor is again a substantial share assigned to the employment margin in the survey. The coefficient estimate is smaller compared to the tax decrease group and lacks significance. This is to be expected, as some of the tax changes occurred in vastly different economic environments. Column (3) shows the results of restricting the realized tax changes to a five-year window around the survey date (2018-2022), where firms arguably were in similar economic circumstances compared to the survey. We find a strong positive association between the survey indication and actual changes in employment. The associations might be even stronger, if we were able to more accurately identify actually treated firms. As we do not have establishment-level data in Orbis, it could be that some of the firms are not actually affected by a change in the LBT for at least three reasons. First, the applicability for multi-establishment firms follows an apportionment rule depending on, among other things, the number of workers of the firm in the respective municipality. Second, changes in profit taxes directly affect firms only when they incur positive profits. For non-

becomes marginally weaker but remains highly significant throughout.

profitable firms, a change in the LBT might not directly translate to a change in its tax burden. Third, partnerships and sole proprietors can credit LBT paid on their income taxes, reducing the impact of a LBT change.

Table 10: Firm-level Association Hypothetical vs. Realized Behavior

	Pos. Change	Neg. Change	Neg. Change
Intercept	0.136*	0.216***	0.222***
	(0.077)	(0.036)	(0.044)
Indicated in Survey	0.864***	0.126	0.228*
	(0.077)	(0.088)	(0.121)
Num.Obs.	23	169	110
R2 Adj.	0.179	0.008	0.031

Note: Table 10 shows results of OLS regressions of indicators for positive and negative changes in employment in response to changes in Local Business Taxes on dummy variables for extensive use of that category in the survey. Column (1) shows results for tax decreases, where the outcome is an indicator for a positive change in employment. Column (2) shows the same for tax increases, where the outcome is an indicator for a negative change in employment. Column (3) shows the result for tax increases in a five year window around the survey date. Robust standard errors are given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Responses to COVID-19 pandemic. As data availability restricts the sample usable for our validation test on a potentially selected subset of firms, we provide an alternative test, which can be performed for a larger subgroup. For this second test, we exploit two questions that were asked in the same wave of the survey. Both questions are of a similar nature compared to our tax incidence questions. The first question was stated as follows: *"Are you currently planning to hire additional employees in the short term (0-12 months)?"* The second question had a more direct connection to an exogenous shock to the economic environment of the firm, and read *"What measures are you taking in the short-term (0-12 months) to cope with the burden of the Corona crisis?"*. Respondents were provided with a variety of options to choose from, where one of the possible categories was to decrease the number of employees. Compared to our previous exercise, this setting offers some advantages, but also some drawbacks. The major upsides of this approach are the eased data requirements and therefore increased power and representativeness of the sample, as well as the clearly

defined window over which the realized action should take place. This direct correspondence in timing between stated and realized actions allows for a more direct comparison in contrast to realized tax changes that might have been several years in the past. The major downside is the fact that the questions might not necessarily be subject to the same sources of bias as the tax incidence questions, which might reduce their validity as proxies. We thus view the following results as complementary to the previous exercise.

We regress changes in employment in the year after the survey was conducted on indicators for whether the firm stated that it would increase or decrease employment over the next 12 months. For this exercise, we only require two years of employment data for our respondent firms, which is a much weaker restriction than corresponding signs of tax changes. Therefore, the regressions presented in Table 11 are based on 1,506 firms. The first column shows the result for a specification where one-year percentage changes in employment are regressed on dummies for firms indicating increases or decreases in employment respectively, whereas Columns (2) and (3) show results for indicator variables for positive and negative changes in employment respectively. We find highly significant coefficients on both dummy variables, indicating that survey responses are indeed predictive of actual behavior. When interpreting the magnitude of the coefficient, one should keep in mind that firms were operating in a high-uncertainty environment, where even short term developments were difficult to predict.

2.4 Treatment Effect Heterogeneity

In this section, we leverage the additional firm characteristics available in the GBP survey to explore potential sources of heterogeneity in treatment effects. Specifically, we examine whether the impact of the tax change differs based on firm size, economic sector, organizational form, and financial distress.

Firm size plays a central role in determining how businesses respond to external shocks, as larger firms often have more resources and established networks, while smaller firms

Table 11: Proxy Test COVID

	Perc. Change	Pos. Change	Neg. Change
Reduce Employment	-0.056**	-0.061**	0.205***
	(0.023)	(0.029)	(0.032)
Increase Employment	0.059**	0.170***	-0.049**
	(0.024)	(0.026)	(0.023)
Num.Obs.	1506	1506	1506
R2 Adj.	0.007	0.051	0.050
Sample Means	0.060	0.220	0.180

Note: Table 11 shows estimates from regressing changes in employment over the year after the survey was conducted on indicator variables for hiring and firing plans respectively. In Column (1), the dependent variable is the percentage change in employment over one year, whereas in Columns (2) and (3) the dependent variables are indicators for a positive or a negative change in employment respectively.

may be more vulnerable to disruptions. Likewise, economic sector differences may influence treatment effects due to variation in competition, profit margins, and factor input intensity. Organizational form can also shape a firm's response tax changes, particularly in terms of governance structures and risk sharing. Finally, we consider financial distress, as firms with pre-existing financial vulnerabilities may experience heightened sensitivity to policy interventions, credit constraints, or market conditions. By analyzing these dimensions of heterogeneity, we aim to provide a more nuanced understanding of the treatments' impact and shed light on which types of firms feature the highest sensitivity in different margins and under what conditions.

2.4.1 Empirical Strategy

We test for treatment effect heterogeneity by implementing a slight variation of our main specification. We pool the different treatment intensity groups together and only allow for differences in effects by treatment sign. As the assignment of treatment intensity is uncorrelated with any firm characteristics due to random assignment, this simplification is innocuous. We estimate differences in incidence through the following set of OLS regressions

$$y_i = \beta_0 + \beta_1 \text{Increase}_i + \gamma_1' X_i + \gamma_2' \text{Increase}_i \times X_i + \varepsilon_i \tag{2}$$

where X denotes the firm characteristic of interest. This specification allows us to easily calculate and test incidence shares for different subgroups of our data, allowing these characteristics to differentially affect incidence for tax increases and decreases.[29]

We measure firm size by reported revenue in 2019 and follow the definition by the European Commission by considering firms as microenterprises if they have annual revenues of less than EUR 2 million, small if their revenues are below 10 million, medium for revenue below 50 million and large for revenues exceeding 50 million. For economic sector, we utilize the provided self-classification of the company in our survey and assign them to either to manufacturing, construction, trade or services, with any firm not falling into those categories as belonging to the group *other*. We sort our firms into groups of legal forms, distinguishing between corporations, partnerships and sole proprietors.[30] Finally, we perform a split on whether the firm indicated that it was severely impacted by the COVID-19 pandemic. In the survey, respondents were asked about the impact of the pandemic on their net income, and could report changes in net income on a scale from -100 to +100. We construct a dummy based on this variable equal to one for firms below the median value.

2.4.2 Results

Firm size. Figure 2 shows results for heterogeneous effects by company size as measured by the firm's stated revenue in 2019. Panels 2a and 2b show level estimates for average category usage across the four size categories for tax decreases and increases respectively,

[29] We also explore effect heterogeneity in a specification that includes all control variables of our main specification (Equation 1). The results are shown in Appendix A.8.

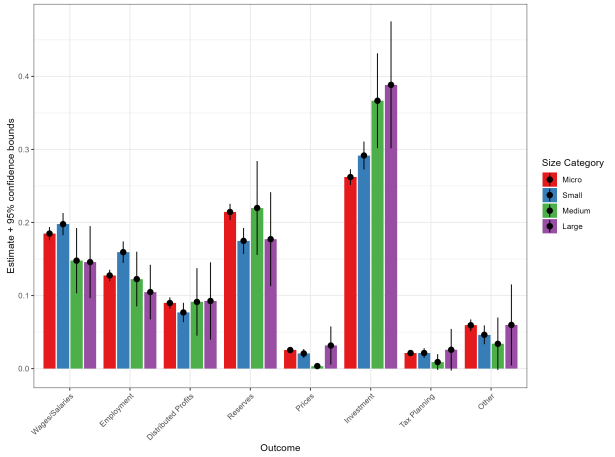
[30] In Germany, there exists a mixed company type called *GmbH & Co. KG*, which combines elements of a corporation and a pass-through entity. The strucutre offers limited liability as a GmbH, but at the same time, distributions to the owners are taxed with the personal income tax rate and not with the corporate income tax. For our classification, we assign this company type to the partnership group. The results remain unchanged if this legal form is removed from the sample.

while Panel 2c shows average partial effects of for the comparisons between each respective group and the baseline (microenterprises). We report the partial effects estimates and indicate significance by filled points, whereas hollow circles indicate that the adjusted p-value using the Benjamini and Yekutieli (2001) method exceed 5%. For most of the categories, the differences by company size are negligible in size and insignificant. The point estimates suggest that the impact of a tax cut on investment varies with company size, suggesting that a 10 percentage point larger share is attributed to funding new investment by medium companies compared to microenterprises, and even a 13 percentage point larger share by large companies. However, due to the small number of large companies in our survey, this result is insignificant after controlling for the false discovery rate. Investments of larger firms often benefit from larger economies of scale, which would be consistent with an increasing share attributed to investment when the tax burden decreases.

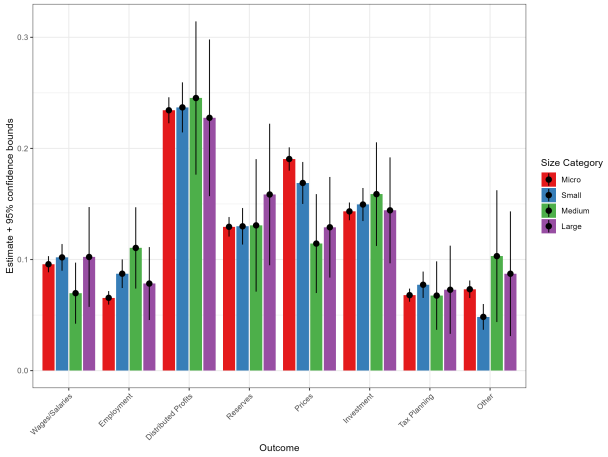
We also detect some evidence for varying pass-through to consumers for tax increases depending on the size of the firm, suggesting that larger firms finance 8 percentage points less via price adjustments compared to microenterprises. Though the adjusted p-values exceed the 5% threshold, our point estimates suggest that larger firms are more prone to adjusting employment at the extensive margin when faced with a tax hike instead. A possible driver of this effect might be that for smaller firms, employment is a rather discrete choice compared to larger companies. A firm with 4 employees can adjust its employment only by 25% at the margin, while a firm with 30 employees operates on a more continuous scale, where an adjustment by one employee would change the input factor labor only by about 3%. Furthermore, in smaller firms, each employee often fulfills multiple roles, making the decision to lay off a worker more impactful on the overall functioning of the business.

Economic Sector. Our results with respect to sector differences in incidence are summarized in Figure 3. Panels 3a and 3b again show the average category usage for each sector separated by the sign of the tax change, while Panel 3c illustrates the partial effects to test for differences between each sector and the baseline.

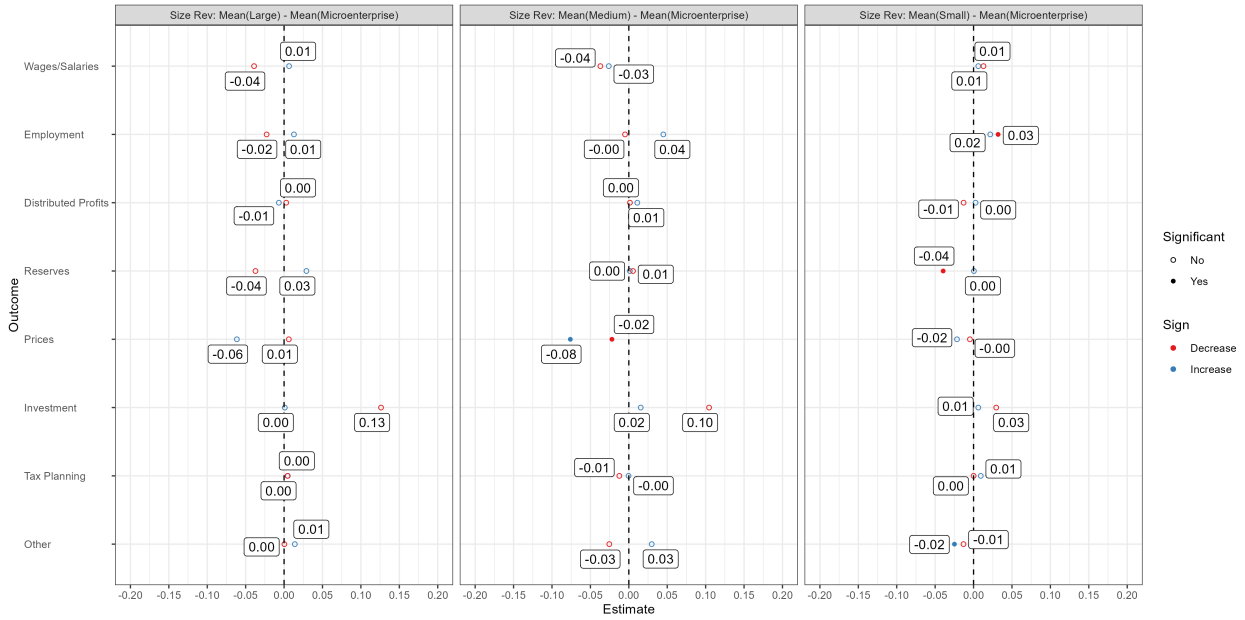
Figure 2: Incidence Heterogeneity by Firm Size



(a) Tax Decrease



(b) Tax Increase



(c) Average Partial Effects

Note: Figure 2 shows heterogeneity in incidence by company size measured by revenues. Panel 2a and 2b illustrate different levels and associated robust standard errors of category usage by treatment sign calculated from the estimated coefficients from Equation 2, whereas Panel 2c shows average partial effects for each comparison with the baseline. Average partial effects with a significant p-value after applying the Benjamini and Yekutieli (2001) correction are denoted by filled dots, whereas insignificant effects are illustrated by hollow circles.

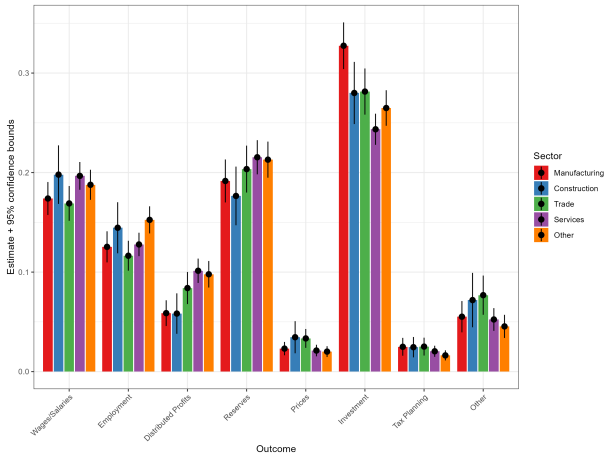
We find the most striking heterogeneity in the construction sector, which features a substantial 12 percentage point larger incidence on consumers in the case of a tax increase compared to the other sectors. The higher pass-through to prices seems to offset a lower incidence on the owners of construction companies, who are less affected by a tax increase compared to the other industries. Interestingly, this result does not materialize for tax decreases, where construction firms do not differ significantly from firms in other industries. One possible explanation for this could be the generally high level of competitiveness in the construction sector and thereby lower profit margins. Hence, firms in this sector have less wiggle room to absorb increased costs caused by tax hikes, which only leaves the option to pass them down to consumers. Additionally, the construction industry is characterized by inelastic demand compared to other industries, which further increases the pass-through of tax increases to consumers (Hillebrandt, 2000).

The partial effects estimates indicate that the impact of tax cuts on investment decisions by manufacturing firms is more pronounced than in other industries. This phenomenon is likely attributable to the high capital intensity characteristic of the manufacturing sector. Tax cuts, by reducing the user cost of capital, disproportionately benefit industries requiring substantial upfront investments, such as machinery or factories. The results suggest that in capital-intensive industries, funds are more likely to be allocated towards new investment opportunities rather than being distributed to shareholders.

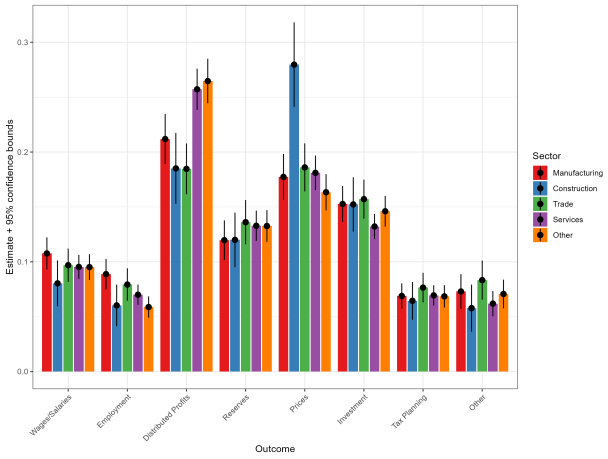
Our findings for the trade sector provide an additional indication that the level of competition is a significant driver of incidence. Similarly to construction, competition in the trade sector is relatively high as opposed to manufacturing and services, hence low profit margins cannot cushion the impact of a tax increase. In contrast to the construction sector, however, this does not lead to a substantially higher incidence on consumer prices. Instead, we observe marginally larger usage across the board for the other categories.

Organizational Form. We detect interesting heterogeneities by legal form of the respondent firm. Figure 4 shows level estimates of category usage (Panel 4a and 4b) as well as

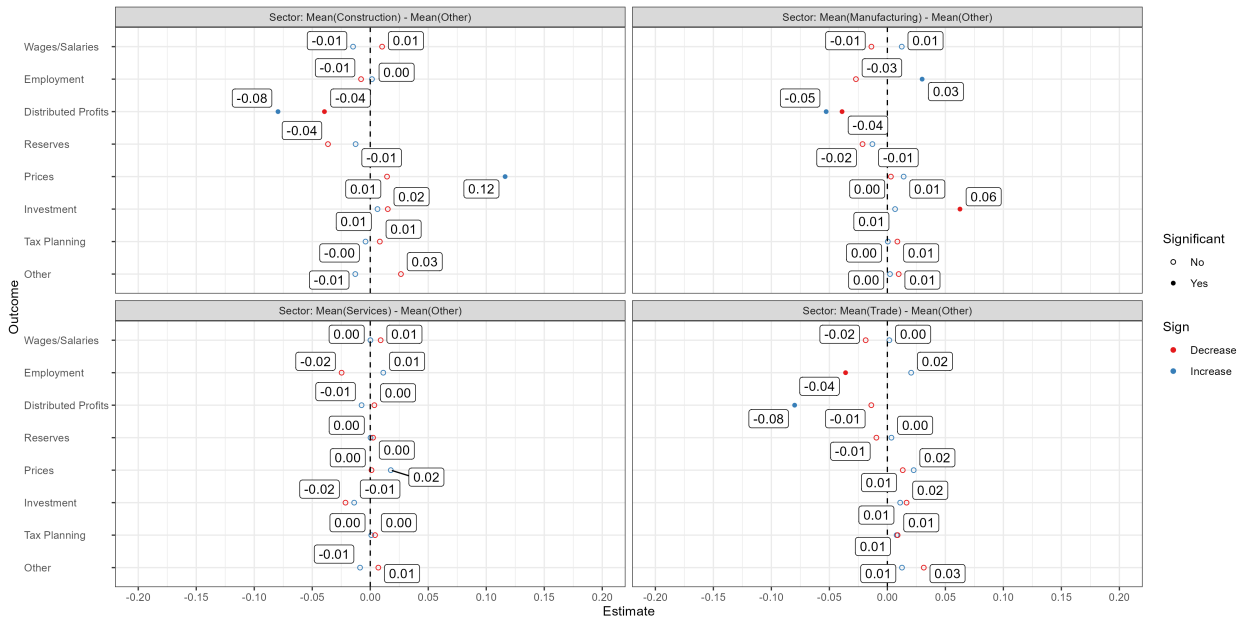
Figure 3: Incidence Heterogeneity by Economic Sector



(a) Tax Decrease



(b) Tax Increase



(c) Average Partial Effects

Note: Figure 2 shows heterogeneity in incidence by economic sector. Panel 3a and 3b illustrate different levels and associated robust standard errors of category usage by treatment sign calculated from the estimated coefficients from Equation 2, whereas Panel 3c shows average partial effects for each comparison with the baseline. Average partial effects with a significant p-value after applying the Benjamini and Yekutieli (2001) correction are denoted by filled dots, whereas insignificant effects are illustrated by hollow circles.

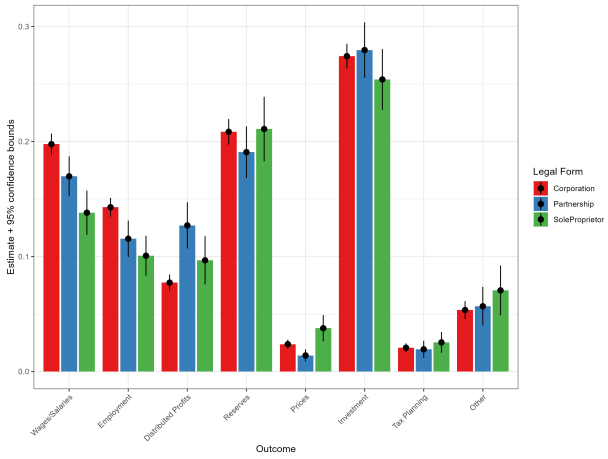
partial effects estimates (Panel 4c). First, the results indicate that worker incidence via wage adjustments is less prevalent in partnerships and sole proprietors compared to corporations, which can be observed for both tax increases and decreases. These effects remain significant even after controlling for differences in size and industry.

Second, the data suggest substantial differences in owner incidence depending on the organizational form of the companies. Partnerships state with 5 percentage points substantially larger effects on distributed profits for both decreases and increases compared to corporations. Sole proprietors, on the other hand, differ substantially only for tax increases, where the payout incidence is with a 11 percentage point lower share only half as large compared to corporations. Instead, sole proprietors seem to offset higher taxes through the use of reserves. Again, these differences persist even after accounting for differences in size and sector distribution across legal forms.

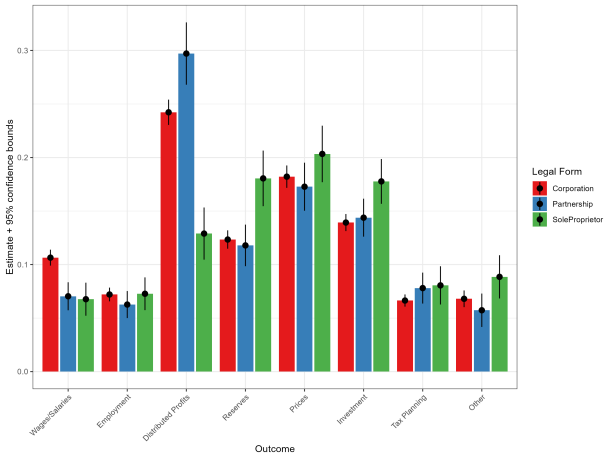
Financial Distress. Finally, we explore whether the economic condition a respondent company was in during the COVID-19 pandemic has an impact on its stated distribution. Figure 5 again shows level and partial effects estimates for each category for increases and decreases separately. For tax decreases, we observe that owners benefit more from the additional funds if their company wasn't severely affected by the pandemic as indicated by its impact on the companies net income. Companies with a substantial drop in net income due to lock-down or supply chain disruptions likely experienced a severe tightening of liquidity constraints. Hence, a decrease in taxes would then be used to pay off debt or short-term liabilities. This notion is supported by the opposite-sign partial effect on the category *Other* in combination with Figure A.10, which suggests that a substantial share of the free text answers alluded to repayment of debt.

For tax increases, we also see a pronounced difference in payout incidence depending on the net income impact of the pandemic. A similar logic can be applied here as for the tax decrease treatment. Firms with a substantial negative impact on their net income might be in a precarious situation where no profits are available to be distributed to shareholders or

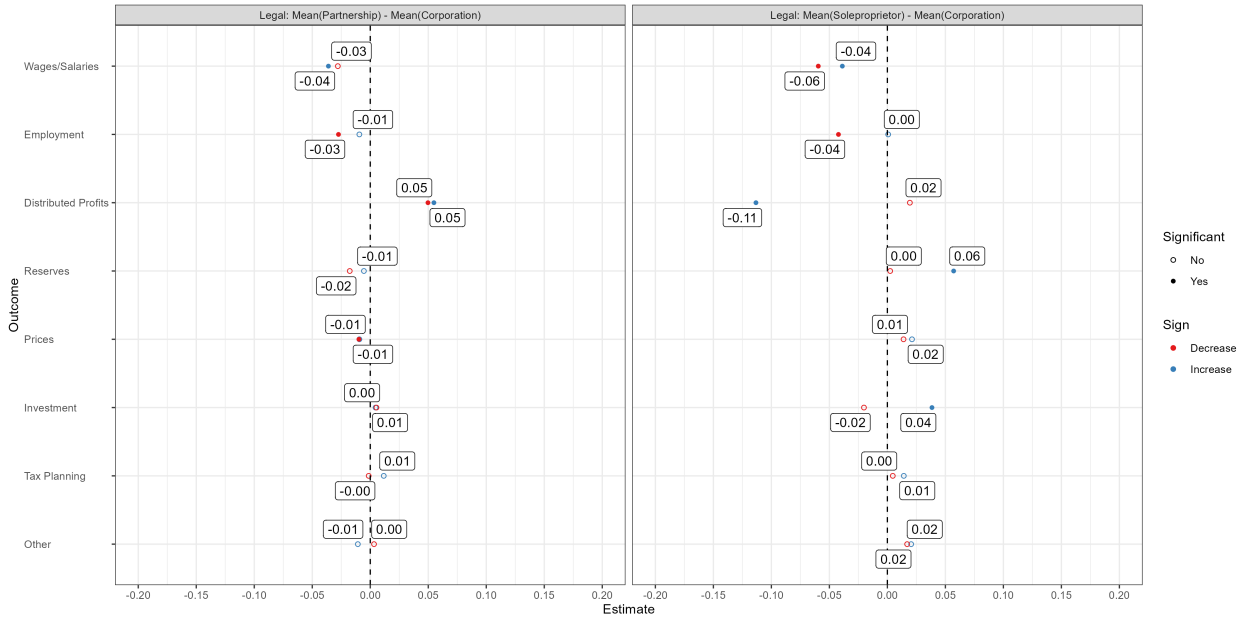
Figure 4: Incidence Heterogeneity by Organizational Form



(a) Tax Decrease



(b) Tax Increase



(c) Average Partial Effects

Note: Figure 2 shows heterogeneity in incidence by company legal form. Panel 4a and 4b illustrate different levels and associated robust standard errors of category usage by treatment sign calculated from the estimated coefficients from Equation 2, whereas Panel 4c shows average partial effects for each comparison with the baseline. Average partial effects with a significant p-value after applying the Benjamini and Yekutieli (2001) correction are denoted by filled dots, whereas insignificant effects are illustrated by hollow circles.

partners, which requires the funds to come from other channels.

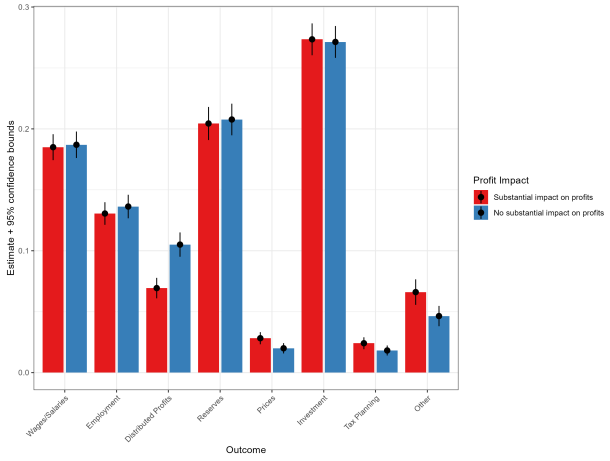
We further detect a lower pass-through of tax hikes to consumers from firms that experienced a substantial net-income impact of COVID-19. This effect may indicate differences in demand elasticities across these subgroups, as the impact on net income arguably stems from a decrease in revenue. As distributed profits and price adjustments are less available for firms severely impacted by the pandemic, we observe that these firms instead push the burden onto workers, which are proportionally more affected in this particular subgroup compared to firms that experienced no substantial impact of the crisis.

2.5 Conclusion

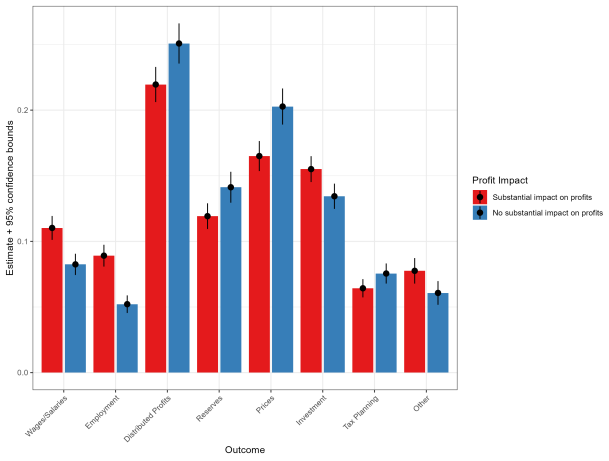
The question of who bears the economic incidence of taxes on company profits is a first-order question in the literature and remains an active area of research. We contribute to this debate by pursuing a novel empirical strategy based on stated incidence in a large firm survey. In contrast to existing studies, this empirical approach allows us to shed light on the effect of business taxes on a large set of possible adjustment margins and affected groups in a unified setting. Moreover, our experimental approach enables us to test for asymmetric tax incidence in response to increases and decreases, as well as the influence of the magnitude of tax changes.

Our findings highlight a pronounced asymmetry in how tax increases and decreases affect economic agents. Consumers bear a substantial portion of tax hikes, as firms pass on a significant share of higher costs through price increases (18%), yet they benefit only marginally from tax reductions, with only 2% of tax cuts leading to lower prices. Similarly, capital owners experience a greater burden from tax increases (24% absorbed through reduced distributed profits) than they gain from tax cuts (9%). Conversely, employees experience an asymmetric effect in the opposite direction: while tax hikes have a limited impact on wages and employment (10% and 7%, respectively), tax reductions result in more substantial wage and employment gains (19% and 13%, respectively). Furthermore, firms exhibit a stronger

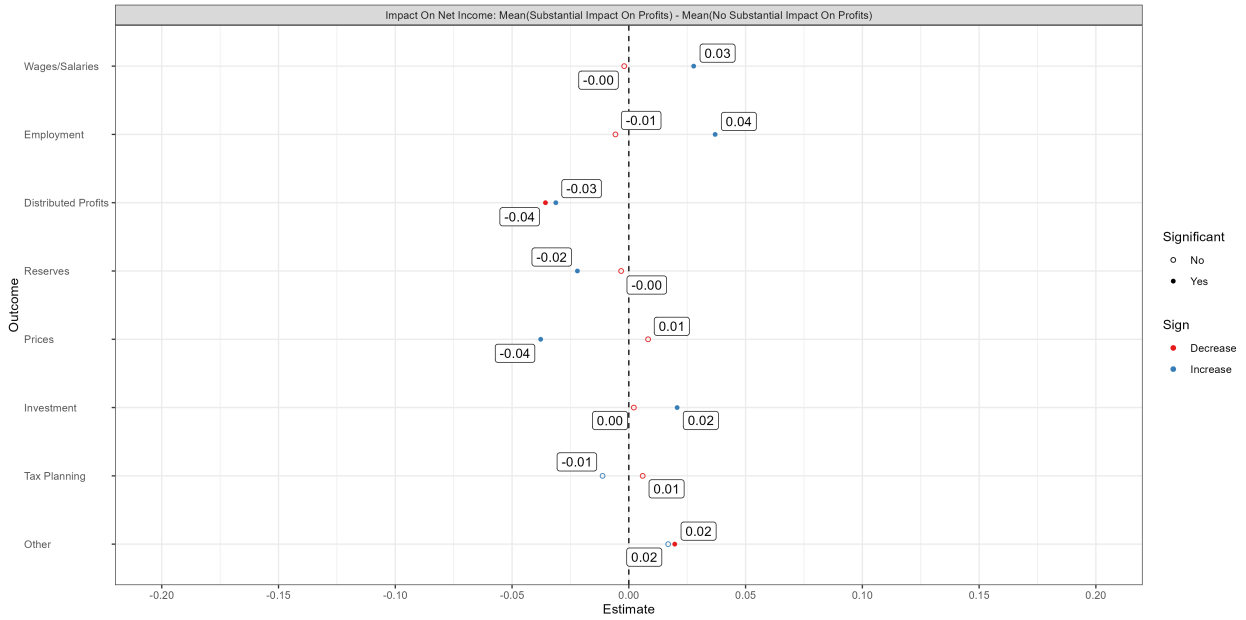
Figure 5: Incidence Heterogeneity by COVID-19 Profit Impact



(a) Tax Decrease



(b) Tax Increase



(c) Average Partial Effects

Note: Figure 2 shows heterogeneity in incidence by whether the company was substantially impacted by COVID-19. Panel 5a and 5b illustrate different levels and associated robust standard errors of category usage by treatment sign calculated from the estimated coefficients from Equation 2, whereas Panel 5c shows average partial effects for each comparison with the baseline. Average partial effects with a significant p-value after applying the Benjamini and Yekutieli (2001) correction are denoted by filled dots, whereas insignificant effects are illustrated by hollow circles.

investment response to tax cuts (27%) than to tax hikes (15%). These asymmetries underscore the importance of considering the direction of tax changes when evaluating their economic consequences. Our analysis of treatment intensity further reveals that larger tax changes have a stronger impact on employment than on firm owner payouts and retained earnings. By exploiting the presence of a rich set of company characteristics in our survey data, we further investigate heterogeneity in profit tax incidence. We find pronounced differences in tax effects based on firm size, economic sector and organizational form, as well as how the response differs when the firm experiences financial distress.

Our survey-based approach enables a comprehensive assessment of how firms adjust to tax changes across multiple margins. While this methodology offers insights beyond traditional observational studies, it also comes with inherent limitations. Since our findings rely on self-reported responses to hypothetical tax changes, they may be subject to reduced respondent effort or experimenter demand effects (Haaland et al., 2023). For instance, managers might underreport wage cuts or layoffs in response to tax hikes due to social desirability concerns. To mitigate these concerns, we conduct a comprehensive set of validation exercises. First, we find high levels of reliability of survey responses with respect to firm characteristics. Furthermore, we show that statements about intended actions are predictive of actually realized behavior. While we cannot exclude the possibility of slight biases in our results, these checks indicate that the survey responses are indeed reliable.

From a policy perspective, our findings have important implications. Since workers gain more from tax reductions than they lose from increases, while the opposite holds for firm owners, tax cuts targeting labor income may have progressive effects. Moreover, the weak pass-through of tax cuts to consumer prices suggests that reductions in corporate taxation may not directly translate into broad consumer benefits. These insights highlight the need for a nuanced tax policy that carefully accounts for asymmetries in tax incidence.

3 Better Early than Never – The Effects of Anticipated Gift Tax Changes on Business Transfers

Co-Author: Jan Zental

Abstract: Wealth transfer taxes can be important instruments to counter increasing wealth inequality. Yet, inter-generational business transfers, whose distribution is particularly concentrated at the top, are inherently difficult to tax. Many countries treat this asset class preferentially to avoid overburdening family firms, and sophisticated tax avoidance strategies by business owners exploit this preferential treatment to erode the tax base. We analyze how business transfers react to anticipated changes in such preferential tax treatment using administrative data at the individual-transfer level from the universe of German gift tax assessments. We find strong and rapid timing responses of business transfers to expected tax changes. We show that the response is stronger for higher-valued transfers and find heterogeneity in transfer characteristics consistent with a tax avoidance motive. We further estimate that the amount of foregone gift tax revenue due to timing responses is up to 2.8 times the size of actual annual inheritance and gift tax revenue.

Keywords: Wealth transfer tax avoidance, business owners, tax uncertainty

Acknowledgements: We thank Philipp Doerrenberg, Christoph Spengel, Dirk Foremny, Rainer Bräutigam, Heiko Vay, Svea Holtmann, Charlotte Bartels and Konrad Stahl for their comments. We owe special gratitude to discussants of earlier versions of this paper: Isabel Micó-Millán, Theresa Bührle, Johannes König, Cécile Gayet and Georg Thunecke. We also thank participants at various seminars, workshops and conferences for valuable feedback, comments and suggestions. We are thankful to the Research Data Centres (RDC) of the Federal Statistical Office and the Statistical Offices of the Federal States, in particular Kerstin Stockmayer and Kathrin Stief, for data provision and support. Victoria Ahrens, Hendrik Schmidt and Juliane Ott provided valuable research assistance. We acknowledge financial support by the Leibniz Science-Campus MannheimTaxation and the Graduate School of Economics and Social Sciences of the University of Mannheim. Both authors are affiliated with the University of Mannheim.

3.1 Introduction

Taxes on wealth transfers can be important instruments to counter increasing wealth inequality (Nekoei and Seim, 2022). A wealth class whose distribution is particularly concentrated at the top are business assets in the form of shares in partnerships and corporations (Saez and Zucman, 2016; Kopczuk and Zwick, 2020; Smith et al., 2023). The effective taxation of business wealth transfers, however, is prone to avoidance strategies by business owners (Henrekson and Waldenström, 2016). As a result, the effective tax rate structures for wealth transfers are often regressive in practice, which counteracts their intended purpose.[31] The measurement of business owners' avoidance responses to wealth transfer taxes is thus key in designing effective tax policies on wealth transfers.

The avoidance of wealth transfer taxes by business owners is facilitated by the existence of preferential tax regimes for business assets in many countries.[32] The intended purpose of these preferential taxation regimes is to secure employment and maintain firm liquidity by reducing the tax burden associated with inter-generational business succession. However, excessive privilege for a specific asset class, especially if it is more prominent at the top of the wealth distribution, undercuts the legitimacy of a tax mainly used as a re-distributive tool. As a political consequence, public pressure and judiciary decisions have led to reforms of inheritance and gift taxation in various countries in the past.[33] Business owners may anticipate such legislative changes and the adverse tax consequences for themselves and optimize the timing of their wealth transfers with respect to expected changes in the tax code.

In this study, we analyze how business owners respond to such anticipated changes in the preferential business transfer taxation, where re-timing responses may counteract policy intentions and reduce tax revenues. We exploit two anticipated events in the years 2012

[31] See, e.g., OTS (2018) for the case of the UK.
[32] See OECD (2021) for an overview.
[33] Consider Henrekson and Waldenström (2016) for a description of the Swedish case, which led to the abolishment of the Swedish inheritance tax.

and 2014 that threatened to negatively alter the preferential taxation of business transfers in Germany. We combine this institutional setting with a large administrative dataset containing the universe of German wealth transfers in the form of gifts and inheritances. We use a Differences-in-Bunching methodology to measure behavioral responses in the intra-year distribution of gifts on a weekly level preceding these event dates. As a counterfactual distribution, we use the years 2010 and 2011, where neither announcements nor legislative changes took place. This way, we can identify the excess mass of tax-motivated gift transfers of business assets. Furthermore, we explore the characteristics of transfers within the event windows to document heterogeneity consistent with differing motives underlying tax avoidance. Finally, we use the excess mass estimates from our bunching approach to provide back-of-the-envelope calculations of foregone gift tax revenue to the German State due to behavioral responses.

Our analysis begins with an investigation of the run-up period (Event Window I from hereon) to a debate in the German Parliament regarding the abolition of a well-known tax avoidance scheme called *Cash-GmbH*.[34] This scheme involved putting cash into a shell company prior to the transfer in order to benefit from the tax exemptions for business assets and allowed wealthy individuals to pass on basically unlimited amounts of wealth almost tax free. For instance, for an individual trying to pass-on a bank deposit of EUR 26 million, this scheme allowed unintended tax savings of up to EUR 8 million.[35]

Next, we consider the period leading up to the last major verdict of the German Constitutional Court in 2014 (Event Window II from here on), when it evaluated the conformity of the preferential treatment provisions with the German constitution. At the time, the provisions had been heavily criticized to be excessive and subject to no means-testing. They were thus argued to be in violation of the principle of equality granted by the German constitu-

[34] The German abbreviation *GmbH* stands for *Gesellschaft mit beschränkter Haftung*, which is a legal form akin to a limited liability company (LLC) in both commercial law and tax law. It is restricted to non-listed, privately-held companies.

[35] Assuming a 30% tax rate on the cash transfer and abstracting from the costs for setting up a corporation as well as fees for tax advisors and notaries.

tion. The precise tax consequences of the verdict were ex-ante unknown as there were several scenarios possible depending on the judgement of the court and the severity of subsequent legislative changes. However, the expectation of a significant increase of the tax burden on (large) businesses was predominant, due to which business owners were incentivized to conclude their succession before the verdict.

We find that transfers of business assets in the form of gifts react very strongly and in an extremely timely manner to the risk of a future tax rate increase. For our first event, which threatened an increase in the effective tax rate (ETR) of up to 30%, we find that the excess number of transfers is more than 9 times higher than the average number of transfers in the counterfactual period. This observation is unexpected given the empirically documented obstacles to timely inter vivos transfers of ownership in the literature (Schmalbeck, 2001; Kopczuk, 2007) and the short period of opportunity for behavioral responses, as the event could only be anticipated four months in advance. For the verdict of the Constitutional Court, where the exact tax implications were not clear *ex ante*, we still find an excess mass of transfers five times higher than what we would expect absent the event. The observed pattern is particularly pronounced for high-value transfers, with individuals in the highest wealth quartile being almost twice as responsive as the average transfer. This might be due to higher tax literacy, better-informed tax consultancy, or fixed costs of avoidance coupled with higher amounts of wealth at stake compared to lower-value transfers.

Our heterogeneity results with respect to transfer characteristics suggest that transfers made within Event Window I are more likely to benefit a minor or a daughter of the donor and that effective tax rates are significantly lower for these transfers. We fail to detect such heterogeneity in Event Window II, which suggests that business transfers in the first event window are fundamentally different from transfers in the second event window. The observed pattern is consistent with a factual transfer of cash rather than control of an actual company.

We further quantify the extent of gift tax avoidance implied by the re-timing of business transfers. To this end, we perform the following thought experiment. Suppose that the

excess transfers we identified were not taxed under the relevant schedule at that time, but under the provisions that were expected to be applicable after the respective point of the event windows. How much revenue did the German government lose in this hypothetical scenario due to re-timing responses? To provide a nuanced picture that takes into account the uncertainty individuals faced when forming their transfer decision, we simulate a variety of scenarios that differ in the tightness of applicable tax rules. We estimate the corresponding amount of foregone tax revenue to amount to up to EUR 12 billion, which exceeds the total tax revenue from gifts and inheritances of EUR 4.2 billion in the reference year 2011 by a factor of 2.8. Altogether, our findings suggest that gift tax policy design which disregards the behaviour of business owners has adverse fiscal and distributional consequences.

We contribute to two related strands of the literature. Our main contribution lies in showing the timeliness with which wealthy business owners respond to threats of (adverse) tax changes. Early empirical evidence on the general responsiveness of wealth transfers to taxation was gathered by Bernheim et al. (2004) and Joulfaian (2004). Bernheim et al. (2004) use cross-sectional data from the Survey of Consumer Finances (SCF) to show that inter-generational wealth transfers in the US are responsive to estate and gift tax changes.[36] They find weak evidence that behavioral responses are stronger for higher-value transfers. By utilizing administrative gift tax assessment data, we are able to overcome the incomplete coverage of the wealthiest individuals inherent to surveys such as the SCF. From a macroeconomic perspective, Joulfaian (2004) uses aggregate time series to show large responses of overall gift volume in anticipation to tax changes in the US. His analysis supports the notion that inter vivos giving responds to anticipated tax changes through inter-temporal substitution. Closer to our institutional setting and by using survey data of German firms, Hines et al. (2019) also find that wealth transfer taxes significantly influence the timing of gift transfers. More precisely, they show that German family firms are more likely to conduct

[36] Inheritance and gift taxes are levied at the level of the recipient of a wealth transfer whereas estate taxes target the wealth giver (i.e., the decedent). When referring to wealth transfer taxes, we relate to all three tax types. The term bequest taxes comprises both estate and inheritance taxes.

a succession after a tax reform in 2009 favorable for transfers of businesses. This finding is consistent with work by Glogowsky (2021), who, using tax return data on German inheritances and gifts, documents that tax optimization patterns quickly adapt to the post-reform tax rates. Our approach builds on these insights by exploiting the high-frequency nature of the administrative tax data. This allows us to demonstrate the enormous speed in which inter-temporal shifting among the wealthy occurs. In particular, for both of our events we provide evidence of a trade-off between tax minimization and retaining control: business owners wait until the very last moment before locking in their transfer decision.

Second, we shed new light on the response channels of wealthy individuals to tax policy. Wealth (transfer) tax avoidance channels include deliberately under- or overvaluing assets (Poterba and Weisbenner, 2003; Montserrat, 2019), shifting housing assets between family members to reduce property taxes (Di Porto et al., 2021) or geographic relocation (Brülhart et al., 2022; Moretti and Wilson, 2023). As being wealthy often coincides with owning a business, a growing number of studies specifically examine the role of closely held businesses in their owners' tax avoidance strategies. Alvaredo and Saez (2009) and Duran-Cabré et al. (2019) demonstrate that business owners responded to the Spanish wealth tax by shifting non-business assets into tax-exempt business shells . More recently, Micó-Millán (2024) finds evidence of the same behavior for a Catalan inheritance tax reform, emphasizing that the tax-induced change in asset composition alone accounted for half of the post-reform tax revenue decrease from inheritances. The use of (closely held) firms as tax shelters is further also documented in the context of income shifting between personal and corporate tax bases (Romanov, 2006; Alstadsæter et al., 2014) and the labeling of business owners' private consumption as tax-exempt business expenditures (Leite das Neves, 2024).

We contribute to this emerging field within the literature by considering a setting in which transferring assets in the form of (closely held) business was essential to avoid high tax rates (Event Window I). Further, by exploiting differences in transfer characteristics between our two events, we can distinguish between mere tax-motivated asset transfers in business

ownership (Event Window I), and transfers of actual business control (Event Window II).

In Section 3.2, we provide an overview about the preferential treatment of business assets under German inheritance and gift tax law. We also depict its development over time and define the two event windows that we use for our empirical approach. In Section 3.3, we describe our data, and in Section 3.4 we explain our methodology and provide our main results. We explore characteristics of the transfers within the bunching window in Section 3.5. The quantitative implications of our main results in terms of foregone tax revenue due to tax avoidance are shown in Section 3.6, followed by a discussion and conclusion in Section 3.7.

3.2 Institutional Background

3.2.1 Taxation of Wealth Transfers in Germany

Tax Treatment of Wealth Transfers. Germany levies an inheritance tax on bequests at death, i.e., a tax on the enrichment of the heir. Gifts are treated in the same way as inheritances under German tax law. From a tax perspective, it should not matter whether wealth is transferred during the lifetime of the donor or only after his demise.[37] The starting point for the tax base is the gross wealth transferred to the recipient. In the case of a gift, the donor is free to choose the amount and the recipient of the transfer.[38] In principle, all types of wealth are subject to the transfer tax.

However, there is an exhaustive list of personal and objective exemptions depending on the relationship between the donor and the recipient as well as the type of asset transferred. The tax liability is based on the gross value of the assets received, after deducting the liabilities of the estate and the exemptions. The tax base is increased by transfers received by the same donor in the ten years preceding the taxable event, i.e., the date of death or

[37] Differential tax treatment of *inter vivos* gifts and inheritances would create incentives to exploit one form of transfer in order to avoid taxation of the unfavourable means of transfer.

[38] In case of an inheritance, there is a default line of succession with fixed proportions depending on the degree of kinship. This can be overridden to some extent by specifying a last will.

the date of the gift. The inheritance and gift tax levied increases over seven tax brackets of taxable bequests. In general, tax rates are lower for close family members and increase as the degree of kinship decreases. Table B.1 shows the tax schedules for the three tax regimes during our sample period. The highly progressive rates range from 7% in tax class I up to 50% for transfers higher than EUR 13 million to unrelated persons.[39]

Preferentially treated asset classes. As in most European Union (EU) countries, the German inheritance and gift tax law grants preferential treatment to certain asset classes, namely, agricultural assets, business assets and substantial shareholdings in corporations.[40] The preferential treatment of these asset classes is generally justified with the notion that the continuance of companies is in the public interest. As productive enterprises secure jobs and foster economic growth, they are argued to benefit society as a whole. However, the distinction between justified exemptions in the public interest and excessive privilege is not always clear cut.

Since 2009, the German legislation distinguishes between *productive business assets* and so-called *administrative assets* (*Verwaltungsvermögen*). Stemming from the rationale that only productive business assets should be taxed preferentially, these administrative assets are defined as assets which are not strictly necessary to successfully run the business.[41]

Crucially, the legal definition of administrative assets at the time did not include cash holdings, which was subsequently exploited by wealthy individuals to transfer their private

[39] As the inheritance and gift tax is designed as a stepwise proportional tax, the legislator grants a special provision (*Härteausgleich*) that prevents the average tax rate to increase substantially at the bracket cut-off points. Instead, the law allows for transition areas characterized by marginal tax rates of 50% if the statutory tax rate is lower than 30%, and 75% if the statutory rate is higher than 30%, until the average tax rate has caught up to the higher level of the next bracket.

[40] The definition of business assets comprises shares in partnerships and sole proprietorships. The definition of substantial shareholding refers to the donor of the wealth transfer possessing a minimum share of 25% in a corporation. This minimum share can be reached individually or through joint ownership, under the condition that joint ownership entails joint action inside the firm. See Houben and Maiterth (2013) and Bräutigam and Spengel (2021) for a comprehensive description and critique of this preferential taxation regime. Further, the transfer of self-occupied real estate also receives preferential treatment from German inheritance and gift taxation, albeit in different sections of the tax code.

[41] E.g., properties granted for use by third parties, corporate shareholdings of 25% or less, art objects or financial asset shares.

wealth tax neutrally through use of a scheme called *Cash-GmbH*. This tax avoidance scheme involved setting up a corporation and depositing cash into the company assets. As cash was not deemed to be harmful administrative assets, the cash within the corporation qualified for preferential treatment and could benefit from the generous exemptions for productive assets.[42] If tax subjects were willing and able to set up a corporation, they could pass-on basically unlimited cash holdings without being subject to the gift tax law, by making use of the provisions for productive assets detailed below.

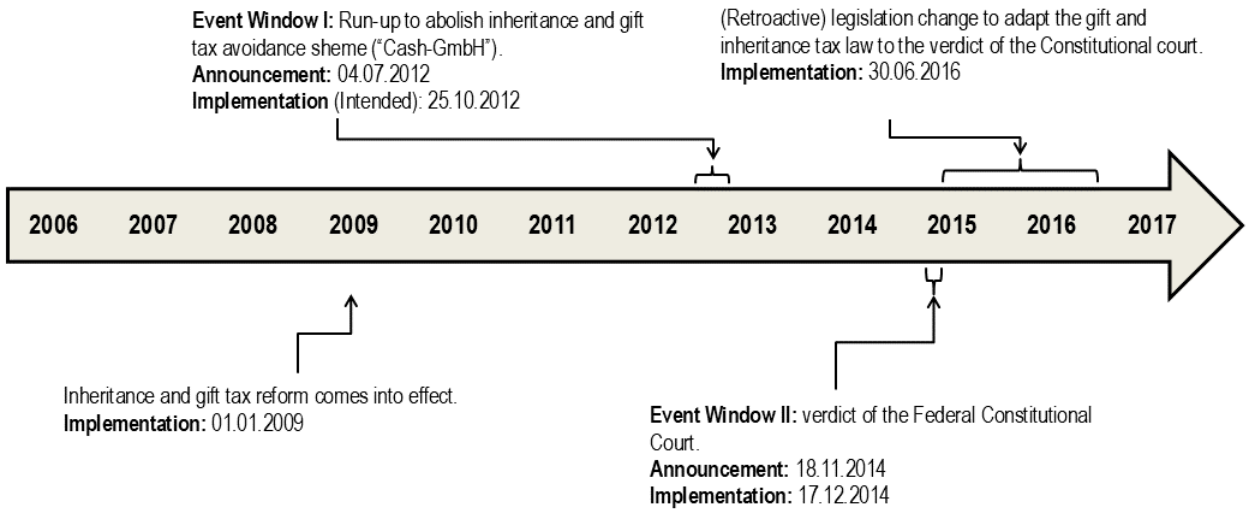
Exemptions for productive assets. Any agricultural assets, business assets and substantial corporate shareholdings that are not classified as administrative assets are in principle eligible for two possible modes of preferential taxation. First, the law allows for a regular exemption of 85% (*Regelverschonung*). This means that only 15% of the taxable transfer of this asset type is considered in the tax base. The main requirements for the exemption to be applicable are that the transferred business could not be sold in the five years following the transfer and that the average sum of wages over this holding period could not be reduced by more than 20%. This regular exemption could be replaced by a more generous but also more restrictive optional exemption of 100% (*Optionsverschonung*). In order to obtain a full exemption, the business should not be sold for a duration of seven years and the average sum of wages over the now seven year holding period could not be lowered. When filing her tax return, the recipient had to decide which exemption model should be applied. She was then locked into this decision, without the possibility to change to the less or more restrictive regime at a later point. If the requirements for the exemptions were violated at the end of the relevant period, the tax was proportionately re-levied. Importantly, the exemption was granted irrespective of the amount of wealth transferred or the economic situation of the business in question.

[42] An additional benefit of using cash as productive assets was that this cash increased the share of productive assets relative to administrative assets in the company. As the exemption was granted on the overall company value as long as administrative assets were not higher than 50% of the total company value, cash injections could be used to cover for a larger share of non-productive business assets, such as rental property.

3.2.2 Legislative Development and Leveraged Reforms

For our analysis, we exploit two events in the legislative development of the inheritance and gift tax law in Germany, which caused a substantial revision of the expectations of tax subjects with regard to their future tax liabilities. The respective windows for behavioural responses to these events had clearly defined end points, which were known to the public at the announcement date. This common feature allows us to cleanly identify transfer allocation within the event windows. Figure 6 illustrates the two event windows we will focus on in our analysis and in the exposition below.

Figure 6: Major Events during the Legislative Development of the Inheritance and Gift Tax



Note: Figure 6 shows the most important legislative events during our sample period. The last major reform which introduced the exemptions for business assets was implemented in 2009. Event Window I starts with the submission of the draft proposal from the German Federal Council regarding the effective abolition of the *Cash-GmbH* avoidance scheme, effectively banning masked cash transfers through shell corporations, and ends just before the parliament debate on 25 Oct 2012. Event Window II includes the time between the announcement of the verdict of the German Constitutional Court in November 2014 until the day of judgement on 17 December 2014. The court decided on whether the preferential treatment provisions in general were constitutional and had the power to set a retroactive implementation date. Source Troll/Gebel/Jülicher/Gottschalk: ErbStG, XI. Entwicklung der ErbSt von 2009 bis 2016, 2021.

Event Window I: (Failed) Ban of the Cash-GmbH. As noted in the previous section, an elementary flaw of the newly implemented provisions exempting business assets

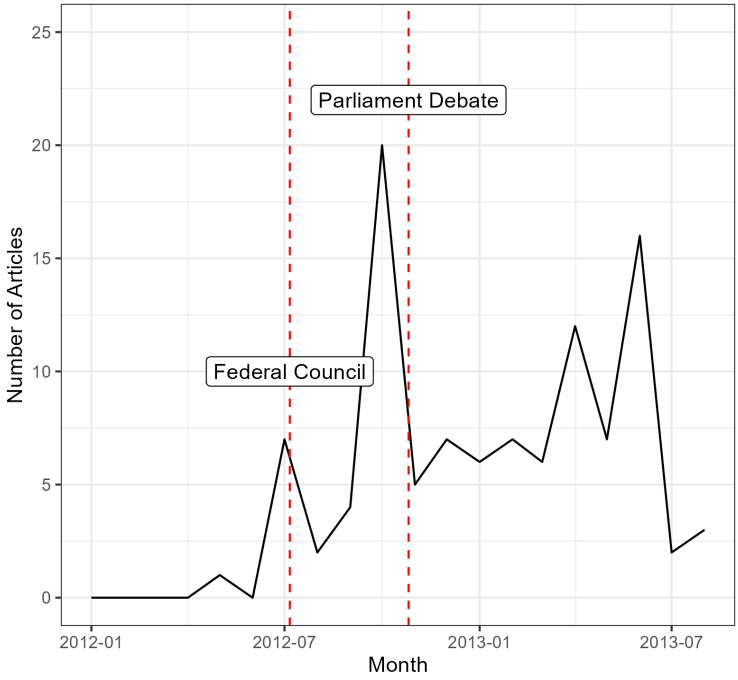
from inheritance and gift taxation was the administrative asset catalog. As cash was not explicitly named in the definition of an administrative asset, paying wealth transfer taxes could be circumvented rather easily. In order to close this gaping loophole in the tax code, the Federal Council (*Bundesrat*) submitted a draft proposal on 6 July 2012 to the German Parliament (*Bundestag*), which included a respective amendment of the administrative asset catalog. The parliament decided on 25 October 2012 on this provision change that would ban the transformation of cash into preferentially treated business assets. If the parliament had decided in favor of the legislative change on that day, the scheme would have been rendered impossible with immediate effect. This was also the expected outcome of the debate.

For individuals planning to make use of this tax saving vehicle, the abolition would entail the loss of a factual 100% exemption and in turn an increase in the effective tax rate from zero to about 30%.[43] Especially for wealthy individuals, this change in effective taxation would entail a massive loss of wealth to the family. For instance, a taxable amount of EUR 26 million passed on after the debate would be subject to an increase in the tax burden levied on the transfer by close to EUR 8 million. However, the parliament could not produce the required majority and the current provision stayed in place until 7 June 2013, when the provision was eventually adjusted. The public discussion of the planned changes to the tax code started on 4 July 2012 after the submission of the draft proposal and culminated in the weeks imminent to 25 October 2012.[44] This is indicated by Figure 7, which shows the monthly count of newspaper articles including the search term *Cash-GmbH* on the Dow Jones Factiva database. The figure shows a clear spike in media attention around the submission of the draft proposal in July 2012, with a surge just prior to the parliament debate in October of the same year and extended coverage afterwards until the loophole was finally closed in 2013.

[43] Assuming a transfer to a close family member in excess of EUR 26 million. Also for transfers of smaller wealth levels, the increase in effective tax rates was still substantially above 0%, see Table B.1.

[44] The introduction of the new law failed because the law change involved some more controversial provision changes such as tax benefits for same sex partnerships. For more details see Bundesrat 6 July 2012, 302/12.

Figure 7: Newspaper Articles including the Term *Cash-GmbH*



Note: Figure 7 shows the monthly count of German newspaper articles containing the search term *Cash-GmbH* from January 2012 until August 2013. The red vertical lines indicate the date of the submission of the draft proposal by the German Federal Council on 6 July 2012 as well as the date of the parliament debate in the German Parliament on 25 October 2012. *Source:* Dow Jones Factiva, accessed on 10 November 2022.

Event Window II: Verdict of the Federal Constitutional Court. In September 2012, the Federal Financial Court (*Bundesfinanzhof*) appealed to the Constitutional Court inquiring whether the preferential treatment of business assets was in violation of the constitution.[45] In its inquiry, the Financial Court argued that the far-reaching or even complete exemption of preferentially treated asset classes were excessive and unsubstantiated. Such an exemption would assume that the wealth transfer tax endangers business continuation. Yet, the actual exemption did not take into account the transferred value or the capacity of the recipient to bear the tax burden. For instance, there were no provisions to verify the existence of sufficient liquid funds to pay the tax, or whether such funds could be acquired in case that the tax would be deferred. It further stated that the notion of preserving jobs as an argument for exempting businesses was flimsy, as the vast majority of companies taxed

[45] Bundesfinanzhof, 27 Sep 2012, II R 9/11.

had less then 20 employees, which automatically excluded them from the job preservation requirement. After the inquiry of the Financial Court, individuals had to expect that the generous exemption provisions were destined to change for the worse (Mödinger and Kaiser, 2018). Tax consultants publicly urged business owners to conclude their succession before the verdict of the Constitutional Court.

At this point it is instructive to consider the possible scenarios business owners could be confronted with, depending on the outcome of the court's decision. The three scenarios are summarized in Table 12. One possible albeit improbable outcome was that the Constitutional Court would dismiss the critique of the Financial Court and approve the existing rules. Second and similarly improbable was the outcome that the court would find all provisions of the wealth transfer tax law in its current form (or only the provisions in question) to be void and in need of a fundamental reform. In that case, the provisions deemed as void would not have been applicable for any transfer occurring after the verdict. Note that the court has the power to declare certain provisions as void but cannot enact new legislation. In that case, the legislator would need to implement a new form of the law in accordance with the court's demands. This outcome would have had a similar effect as the abolition of the Cash-GmbH for companies, if only the provisions for preferential treatment of business assets was deemed to be void. In effect, this "worst case scenario" would have caused a substantial increase in effective tax rates as well. Finally, the court could reach a similar verdict as in 2006 and find that the law in its current form was incompatible with the constitution. This judgment would differ from the previous one in the sense that the law in its current form would remain applicable until the legislator has reformed the current provisions. However, even in that case the legislator could in principle backdate the reform to the day of judgment.

Hence, in order to hedge against Scenario 2 or 3, individuals would need to complete their transfers before the verdict. Despite some outcomes being more probable than others, each one was a possibility and remained so until judgment day.

The Constitutional Court decided on the case in 2014, which was announced at the

beginning of the same year. The oral hearing took place on 8 July 2014 and the passing of verdict was announced on 18 November and promulgated on 17 December.[46] Hence, the public knew that the court would decide on the case at some point during 2014 at the beginning of the year. The ruling of the Constitutional Court was salient in the media and public interest was high. Figure 8 shows trends in the Google search index for the term inheritance tax (*Erbschaftsteuer*) during 2014.[47] The figure features two sizeable spikes around the week of the oral hearing in July as well as the day of judgment in December. The first senate of the court declared the provisions granting preferential treatment to business assets to be incompatible with the constitution, i.e., scenario three was realized. In its verdict, the court deemed the exemptions to be excessive and for that reason unconstitutional and demanded an adjustment of the law until end of June 2016.[48]

3.3 Data and Descriptive Statistics

3.3.1 Data

We use German inheritance and gift tax return data provided by the Research Data Centres (RDC) of the Federal Statistical Office and the Statistical Offices of the Federal States for the years from 2007 until 2019.[49] This dataset covers all wealth transfers whose tax determination dates fell into this period.[50] The data consist of repeated cross-sections,

[46] See the corresponding press release no. 102/2014 from 18 November 2014.

[47] Interestingly, the Google trends index for the term *Cash-GmbH* did not show substantial variation during 2012. We view this as indication that Event Window I was less relevant for the general public but rather for a specific subgroup that was advised by professional tax consultants.

[48] After a lengthy legislative process, the provisions in question were adjusted on 4 November 2016 with retroactive application for taxable events since 1 July of that year. Instead of a fundamental reform of the law, the legislator adjusted the provisions selectively to comply with the demands of the court verdict. The main changes included a melt-down of the exemption percentage for major acquisitions, a means test of the recipient, as well as a tightening of the job preservation requirements to include companies with five or more employees.

[49] *Erbschaft- und Schenkungsteuer-Panel* (EVAS 73611).

[50] As inheritance and gift tax returns are usually filed and administered at least one year after the taxable event and with an average time lag of three years (see Figure B.2 in the appendix), our dataset effectively covers earlier periods than 2007. On the one hand, the data also contain transfers where the taxable event occurred already in 2006. On the other hand, coverage of taxable events in the years 2018 and 2019 is likely incomplete, as a substantial share of same-year tax cases has not been assessed yet. Both years are (potentially) missing those gifts and inheritances that were filed late or took a long time to administer

Table 12: Possible Decisions of the Constitutional Court and Implications

Decision	Meaning	Implication
Amnesty	Court finds provisions to be in line with the constitution	No change
Void	Inheritance and gift tax law (or just preferential treatment) is void	Day of judgment terminates preferential treatment immediately
Incompatible	Not void but incompatible with the constitution	For the time being, law remains applicable but provision change might be applied retroactively to transfers since judgment day.

Note: This table shows the different decisions the German Constitutional Court could have reached in response to evaluating the preferential treatment provisions in 2014, together with the implications each decision would have had on the applicability of the law. The implications of an incompatibility decision were highly uncertain, as neither the degree nor precise timing of tightened provisions were known beforehand.

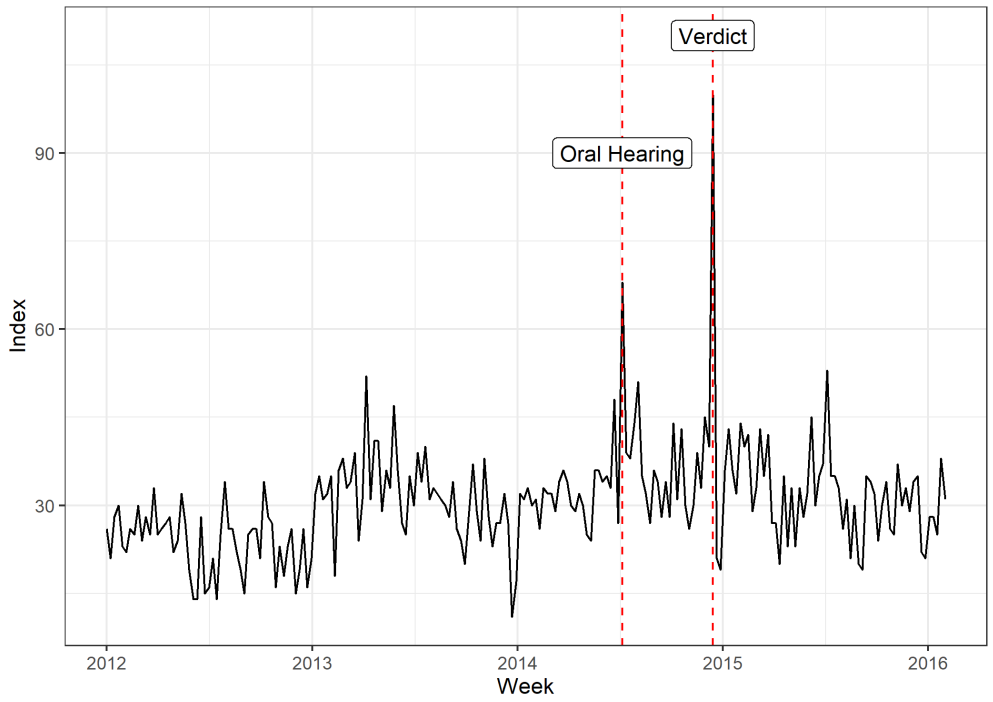
where the unit of observation is an individual tax assessment. As the records are heavily anonymized, we can only track assessments connected to the same transfer, however, there is no identifier for individuals.

The data contain information about giver and recipient characteristics (i.e. birth dates, sex, state, responsible financial office) and relational degree, tax base details and the tax rate as well as taxes paid. They further include three different variables relating to the relevant dates: the date of taxable event, the date of tax generation and the date of tax assessment. For our purposes, the date of taxable event is of main interest, as it allows to track the transmission of wealth at a daily frequency. It coincides in most cases with the date of tax generation, which constitutes the date of the legal recognition for tax purposes.[51] For our analysis, we always keep the most recent date of tax assessment, as deviations between initial and final tax assessments can be substantial. Apart from these time-related variables, our analysis uses different elements of the tax base. Primarily, these are the different asset

(for instance due to family conflicts).

[51] The date of tax assessment on the other hand is important to understand the data structure and the different steps of the administrative process from initial to final tax determination. Moreover, it is essential to establish uniqueness of observations per transfer date and recipient.

Figure 8: Google Trends Index for *Erbschaftsteuer* between 2012 and 2016



Note: Figure 8 shows a search index for the term "Erbschaftsteuer" (=*Inheritance Tax*) for the period between January 2012 and January 2016. The dashed vertical lines indicate points in time of public interest, namely the day of the oral hearing of the German Constitutional Court on 8 July 2014 as well as the day of verdict on 17 December 2014. *Source:* Google Trends, accessed on 5 May 2022.

classes provided for in the inheritance and gift tax returns, i.e. agricultural assets, business assets and company shares (see Section 3.2 for an elucidation).

Overall, the data provided by the RDC cover about 3 million assessments over the whole sample period from 2007 to 2019. Table 13 shows the sample selection process for our final sample. We first eliminate transfer types that are generally not of interest for our analysis, such as erroneously calculated tax amounts or special cases of inheritance taxation. Second, we eliminate transfers with missing birth dates or missing age information. We then establish uniqueness of transfers by always keeping the last tax assessment for a given transfer case, which additionally ensures that our values correspond to the latest update to the values in the tax returns.[52] These initial selection steps leave us with roughly 2 million

[52] Unfortunately, the identifier for a given inheritance or gift case is not reliable as a panel identifier. The original tax return number is given by the responsible financial office, which may already be used by another financial office. Furthermore, the tax number is not kept when an individual moves and challenges a tax

Table 13: Sample Selection

Selection Step	Obs.	Δ Obs.
Overall transfers assessed between 2007 and 2019	3,025,788	
Regular transfer types	2,650,154	−375,634
No birth date nor age for nat. pers.	2,480,155	−169,999
Establish uniqueness	2,094,912	−385,243
Restricting to non-negative asset transfers	2,080,392	−14,520
Restricting to transfer years 2009 until 2017	1,324,195	−756,197
Restricting to gift transfers	281,545	−1,042,650

Note: This table shows the selection steps taken for our final analysis. Before selection step four, the unit of observation is an individual tax assessment. Therefore, a specific inheritance or gift transfer can occur multiple times, with one observation for each tax re-assessment. Regular transfer types refers to the exclusion of special transfer types such as pre- and post-inheritances, cross-border cases, taxation of family foundations at fixed time intervals, cases with erroneously calculated tax amounts and intended use.
Source: Authors' calculations based on data from the RDC of the Federal Statistical Office and the Statistical Offices of the Federal States.

unique transfers. We further exclude negative asset transfers and transfers before 2009 and after 2017. Transfer values before 2009 where based on different valuation principles and the preferential treatment provisions were only introduced with the 2009 reform. We exclude observations after 2016 because of the lag between taxable event and tax assessment, which leads to these periods not being representative of overall transfers. The initial sample of unique transfers consists of 1.3 million receipts, the vast majority of which (79%) are inheritances. Our period of interest includes 281,545 unique gifts.

3.3.2 Descriptives

Table 14 provides an overview of the gift sample separated by the major asset class of the transfer.[53] The table illustrates several interesting features of the data. First, the preferentially treated asset classes (agricultural property, business assets, and company shares) are

assessment later. As we are provided with an anonymized identifier based on the tax number, we are unable to distinguish these cases and produce our own identifier based on gender and birth dates of the donor and recipient, their relational degree and the responsible financial office. Our results are robust to relying on the tax return number as the initial identifier.

[53] The major asset class is defined as the asset class that constitutes the largest share of the overall gift value. For instance, a transfer where EUR 10 million of business assets and 5 million of real estate are gifted, the major asset type of the transfer is defined as business asset.

on average of higher overall transfer value compared to transfers of cash and real estate. The distributions of all asset classes are highly skewed to the right, with mean values being several orders of magnitude higher than the median. Second, recipients of preferentially treated assets are on average five to eight years younger when receiving a gift when compared to the recipients of real estate or cash and financial assets.

Third, because of preferential treatment, effective tax rates are substantially lower for agricultural property, business assets and company shares, with an average effective tax rate of less than one percent. We can also see that not all transfers of favoured asset classes are fully exempt from taxation. Incomplete take-up of preferential taxation does not come as surprise. Especially for transfers of low-value assets, applying for preferential taxation came at a cost (restrictions to minimum payroll sums and holding periods, see section 3.2) while the amounts of assets transferred were below the generally high allowance values. This could incentivize owners of small businesses and especially agricultural asset owners to refrain from receiving preferential tax treatment.

Fourth, cash and financial assets have the highest share of recipients below legal age. Finally, gifts of preferentially treated assets are twice as likely to go to a son than to a daughter of the donor. In contrast, the gender distribution of receipts of cash and real estate is close to equality. For comparison, we show the same descriptive table for bequests in Table B.2 in the appendix.

For our analysis, we restrict this initial sample to all transfers that include (non-negative) values for the sum of agricultural assets, business assets and company shares.[54] This definition includes roughly 12,000 transfers in which none of the preferentially treated assets constitute the major asset type of the transfer. This final restriction reduces the sample size to 76,943 gift transfers over the period from 2009 to 2017.

[54] Tax law stipulates that the value sum across all three asset categories shall be considered for preferential taxation. Due to the application of the net principle, only positive sums are relevant for tax purposes.

Table 14: Descriptive Statistics of Gift Sample

	Obs.	Mean	Std. Dev.	P01	P50	P99
Agricultural property						
Overall value of receipt	11,561	294.02	527.52	0	160	1,918
Age of recipient at transfer	11,558	40.70	11.71	19	39	75
Effective tax rate (p.p.)	11,561	0.44	1.98	0	0	11
Share of fav. assets (p.p.)	11,526	63.40	38.64	0	77	100
Above allowance	11,561	0.32	0.46	0	0	1
Minor recipient	11,558	0.01	0.08	0	0	0
Son	11,561	0.52	0.50	0	1	1
Daughter	11,561	0.14	0.34	0	0	1
Female giver	11,561	0.35	0.48	0	0	1
Business assets						
Overall value of receipt	31,950	5,720.97	67,791.89	5	687	73,076
Age of recipient at Transfer	31,884	40.02	13.38	8	40	78
Effective tax rate (p.p.)	31,938	0.46	2.28	0	0	13
Share of fav. assets (p.p.)	31,894	82.21	34.83	0	100	100
Above allowance	31,950	0.71	0.45	0	1	1
Minor recipient	31,884	0.04	0.19	0	0	1
Son	31,950	0.55	0.50	0	1	1
Daughter	31,950	0.26	0.44	0	0	1
Female giver	31,950	0.31	0.46	0	0	1
Cash and financial assets						
Overall value of receipt	95,379	172.05	655.07	0	50	1,855
Age of recipient at Transfer	94,575	48.68	17.62	4	49	87
Effective tax rate (p.p.)	95,304	4.36	7.23	0	0	29
Share of fav. assets (p.p.)	520	48.39	48.89	0	26	100
Above allowance	95,379	0.62	0.49	0	1	1
Minor recipient	94,575	0.05	0.21	0	0	1
Son	95,379	0.23	0.42	0	0	1
Daughter	95,379	0.20	0.40	0	0	1
Female giver	95,379	0.50	0.50	0	1	1
Company shares						
Overall value of receipt	21,789	3,597.52	24,732.95	1	415	58,150
Age of recipient at Transfer	21,716	40.65	14.37	7	40	79
Effective tax rate (p.p.)	21,784	0.72	3.03	0	0	18
Share of fav. assets (p.p.)	21,756	71.92	42.45	0	100	100
Above allowance	21,789	0.65	0.48	0	1	1
Minor recipient	21,716	0.04	0.19	0	0	1
Son	21,789	0.44	0.50	0	0	1

Table 14: Descriptive Statistics of Gift Sample Continued

Daughter	21,789	0.22	0.42	0	0	1
Female giver	21,789	0.32	0.47	0	0	1
Real estate						
Overall value of receipt	120,866	266.30	510.92	6	129	1,757
Age of recipient at Transfer	120,820	45.47	15.07	13	45	81
Effective tax rate (p.p.)	120,850	3.47	5.64	0	0	24
Share of fav. assets (p.p.)	11,123	57.63	48.15	0	100	100
Above allowance	120,866	0.42	0.49	0	0	1
Minor recipient	120,820	0.02	0.13	0	0	1
Son	120,866	0.23	0.42	0	0	1
Daughter	120,866	0.19	0.39	0	0	1
Female giver	120,866	0.51	0.50	0	1	1

Note: Table 14 shows descriptive statistics for the sample of gifts after our selection process detailed in Table 13. The overall value of receipt is expressed in Thousand Euros. Above Allowance is an indicator for a transfer above the personal allowance of the recipient, Minor Recipient is a dummy variable equal to one if the recipient is below 18 years old at the time of transfer. Son and Daughter are indicator variables indicating the recipient gender and relation of the recipient to the donor. Female Giver is an indicator equal to one if the giver is female. Summary statistics are given for each asset type separately. For comparison purposes we also show asset classes that are not the main focus of our analysis, namely, cash and financial assets as well as real estate.
Source: Authors' calculations based on data from the RDC of the Federal Statistical Office and the Statistical Offices of the Federal States.

3.4 Excess Mass Estimation

3.4.1 Methodology

The shifts in expectations regarding effective tax rates for preferentially treated asset classes created large incentives for re-timing responses within the event windows described in Section 3.2. Individuals who expected their effective tax rate to change for the worse after the end point of the respective event window (25 Oct 2012 and 17 Dec 2014), were incentivized to conclude their wealth transfers of preferentially treated assets before these final deadlines.[55] Bunching in the distribution of transfers in the event windows over time allows us to estimate these short-term re-timing responses.

Bunching methods have been used extensively in the literature to estimate causal behavioral effects. The basic methodology was developed in the tax context by contributions of Saez (2010), Chetty et al. (2011) and Kleven and Waseem (2013) and has since then found many applications in the social sciences.[56] The basic idea of the bunching approach is to quantify the behavioral responses elicited by a discontinuity in incentives by estimating the excess mass in a distribution of interest. We derive adjustment responses by estimating the excess mass in the distribution of weekly wealth transfers around the event dates. In our particular case, we utilize an alternative to the classical polynomial based approach in which we use the unaffected 2010 to 2011 distribution as a counterfactual. This so-called difference-in-bunching approach has been applied in several recent studies, e.g., Brown (2013), Best and Kleven (2017) or Buhlmann et al. (2020), and relies on a suitable reference distribution as counterfactual to the distribution in the event window. The counterfactual distribution allows us to model how transfer behavior would have looked like absent the events and attribute the excess mass of transfers to behavioral responses. By relying on actual data rather than approximations based on polynomial extrapolation, the method avoids some of

[55] From a tax planning perspective it would be optimal to set the transfer date as close to the deadline as possible. One reason for this is the potential of new information coming in, potentially rendering the re-timing no longer necessary. Another reason to delay the transfer for as long as possible was that transferring a business is a complex endeavor, which takes time to plan and execute correctly.

[56] see Kleven (2016) for a recent overview of methods and applications.

Figure 9: Evolution of Preferentially Treated Asset Transfers by Type

Note: This figure depicts the evolution of assets transfers eligible for preferential treatment over time, split up between gifts and inheritances at the recipient-receipt level. Each point represents the number of transfers of transfers of preferentially treated assets (agricultural property, business assets and company shares) in a specific week. Authors' calculations based on data from the RDC of the Federal Statistical Office and the Statistical Offices of the Federal States.

the assumptions invoked, which we consider to be violated in our application.[57]

Figure 9 illustrates our approach graphically. It depicts the annual distributions of transfers of preferentially taxed asset classes in weekly bins throughout our sample period. The figure distinguishes between inheritances and gifts and covers taxable event years from 2009 to 2017. Several patterns are worth emphasizing. First, inheritances are distributed almost uniformly across years. This suggests that the date of death is not strategically chosen for tax planning purposes in our sample. It is also suggestive that the patterns we observe for gifts are not artifacts of some underlying trends in transfer behavior. In stark contrast, the distribution of gifts features sizable spikes at specific dates throughout the observed period.

Second, we observe substantial start- and end-of-year bunching as well as mid-year bunching, the latter albeit to a much smaller extent. These patterns can likely be explained by the end of the financial year. This is generally 31 December for most companies and 30 June for agricultural enterprises. The financial year may be relevant for a transfer decision, as valuation is generally based on the firms operating income.[58]

Third, there are irregularities in our event windows of interest, namely, in the third quarter of 2012, as well as in December 2014, as well as around mid-year of 2016, where we observe the highest mid-year spike of our sample period as well as a level-drop in the period afterwards. Finally, we detect a declining trend in inheritances starting in 2017, which we attribute to the administrative lag between taxable event and tax assessment, as well as the effect of the 2016 reform.

From our analysis of the legislative development and the graphical evidence just presented, we have identified the period from January 2010 until July 2012 as candidate for a

[57] For example, the polynomial approach generally assumes that observations farther away from the threshold are not affected by the discontinuity. In a robustness check, we also estimate the excess mass using the polynomial approach and find quantitatively similar results.

[58] Valuation of business assets for the purposes of the inheritance and gift tax is generally based on the market value. As this market value is commonly not available, alternative valuation methods such as the simplified income capitalization approach (*Vereinfachtes Ertragswertverfahren*) may be used (§§199,200 BewG). Under this approach, the average operating income over the last three years is multiplied with a capitalization factor to reflect future earnings prospects. Hence, valuation is easiest when the transfer occurs just after a financial year end.

"regular" transfer distribution without contamination from behavioural responses. By the onset of this period, enough time has passed since the 2009 reform for individuals to become familiar with the adjusted provisions. At this point, expectations for a stable legal framework could be formed. Importantly, the legislative discussions initiated in July 2012 could not be anticipated by the public beforehand. Hence, we have a stable legal framework during that time, and the gift transfer distribution only features the common start-, mid- and end-of-year spikes.

For illustration purposes and normalization, we consider the transfer distribution one year around the event date, i.e., 26 weeks before and after the respective end point of the event window (=normalization window). In order to be able to directly compare the distributions of transfers in our event windows with the counterfactual of the reference year, we have to account for level differences in transfers. Hence, we divide the weekly bin counts by the total sum of transfers in the normalization window. The normalized bin counts then represent the proportion of transfers in that week relative to the overall transfers occurring in that period.

More formally, let n_i denote the number of transfers in bin $i \in \{-26, \ldots, 26\}$ and $\tilde{n}_i \equiv n_i / \sum_{j=-26}^{26} n_i$ the normalized bin count. After visually identifying the bunching region, we calculate the excess mass $\hat{b}$ by the difference in bin counts between the transfer distribution in the event window and the counterfactual, normalized by the average bin count of the counterfactual distribution in the bunching region:

$$\hat{b} = \frac{\sum_{i=L}^{U} (\tilde{n}_i - \hat{\tilde{n}}_i)}{\sum_{i=L}^{U} \hat{\tilde{n}}_i / N_i} \tag{3}$$

where N_i is the number of bins in the bunching region, $\hat{\tilde{n}}_i$ is the normalized bin count for the counterfactual year, and L and U denote the lower and upper limit of the bunching window. Hence, $\hat{b}$ estimates the excess number of transfers in the bunching region relative to the average height of the counterfactual. Multiplying the excess mass with the bin width yields an estimate of the average timing response. Following Buhlmann et al. (2020), we construct

standard errors for our excess mass estimates using a non-parametric bootstrap procedure. We randomly sample individual transfers in our estimation sample with replacement and calculate a new sample of counts based on which we then re-estimate the excess mass. This process is repeated 1,000 times yielding a vector of excess mass estimates. We use the standard error of this vector as an estimate for the standard error of $\hat{b}$.

3.4.2 Difference-in-Bunching Estimates

We move on to quantify the observed behavioral responses using the difference-in-bunching framework. The results for our two main events of interest are presented in Figures 10a and 10b. Each graph shows weekly normalized bin counts centered around the deadline of interest and the corresponding date in the counterfactual period. The dashed lines at the zero mark indicate the deadline across all figures. The boxes next to the vertical lines display the estimated excess bunching with non-parametrically bootstrapped standard errors in parentheses. Outside the bunching regions, the distribution of the reference period and the counterfactual period appear to be remarkably similar. Our chosen reference period hence seems to constitute a suitable counterfactual for the event period.

Event Window I. Our first window of interest is the period leading up to the debate about the abolition of the *Cash-GmbH* on 25 October 2012 in the parliament. Recall that the general expectation regarding the outcome of the event was that the *Cash-GmbH* avoidance scheme would be prohibited with immediate effect by extending the definition of administrative assets to include also excessive cash holdings. Even though the parliament surprisingly did not reach the expected conclusion, expectations had changed beforehand and the transfer decision was already locked in. Therefore, we observe a significant timing response within the event window which is depicted in Figure 10a. The response to the threat of an effective tax rate increase that would entail an increase in tax burden in the millions is large in magnitude. The excess bunching detected constitutes 9.02 times the average size of the counterfactual normalized transfer distribution. As can be seen from the

counterfactual distribution, the month of October is not a month where a lot of transfers of preferentially treated assets occur normally, indicating that most of the transfers we observe in that window are purely motivated by tax considerations.

This excess mass of transfer counts also translates in a substantial spike in volume, which is illustrated in Figure 11a. The figure shows the gross value of transfers of agricultural assets, business assets and company shares in the year of 2012. In less than a month, business owners transferred more than EUR 40 billion in anticipation of an adverse tax policy change.

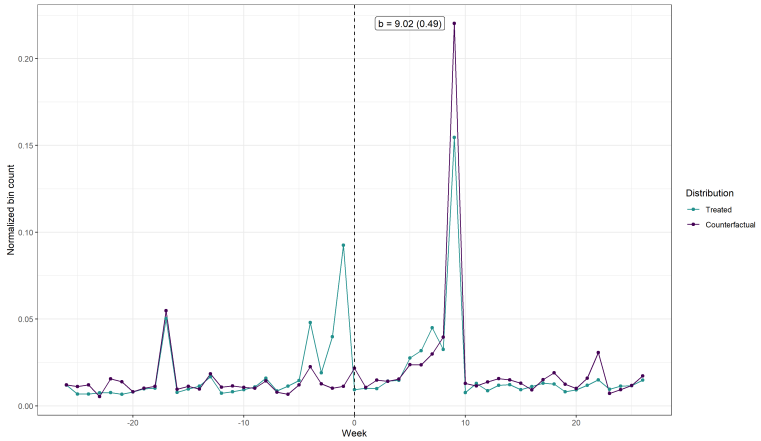
Event Window II. A particularly interesting case provides Event Window II, the period leading up to the verdict of the Constitutional court on 17 December 2014. As the concrete tax implications of the verdict of the court where ex ante not known, it provides us with some insight into the beliefs of responding individuals regarding the outcome. As we have illustrated in Section 3.2, there were three potential outcomes and only some variations of two of them featured immediate detrimental consequences. The risk of retroactive application of any change or the potential voiding of the exemption provisions led a substantial amount of individuals to transfer their assets in the three weeks before the verdict. The results from our estimation are presented in Figure 10b. Our excess mass indicates that the excess of normalized transfer counts is about 5.62 times the average size of the counterfactual in the bunching region and is strongly significant. When compared to the counterfactual distribution, it is apparent that the majority of the estimated excess mass comes from the start-of-year spike normally observed in the transfer distribution. Our results indicate that as a precautionary action to hedge against the possibility of an immediate law change, individuals planning to pass on their business at the beginning of 2015 pulled their transfer forward in time just before the verdict. Figure 11b shows that in terms of volume, the response in the second event window is much more moderate in comparison to the first event window. We see a spike in volume around the oral hearing of the Constitutional Court in July as well as a sharply increasing trend in the months leading up to the verdict, with a substantial spike in December. With an overall volume of business transfers of EUR 10

billion, the response size in December 2014 is equal to only about 25% of the October 2012 volume.

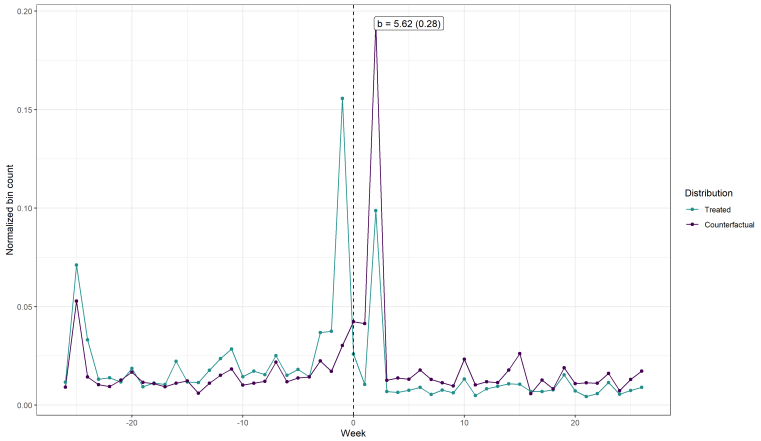
Overall, we detect sizeable timing responses of transfers by business owners preceding expected tax changes. This speaks to the extreme tax literacy of this particular set of individuals, which supports findings by Houben and Maiterth (2013), Mödinger and Kaiser (2018) and Glogowsky (2021) for the German context. Our evidence also shows that the responsiveness is extremely rapid. For our first event window, individuals had only four months to undertake all steps necessary to pass-on a fortune of wealth. In the following sections, we go a step further and investigate response heterogeneity, identify transfer characteristics to speak to the question who bunches, and provide a back-of-the-envelope calculation of the tax consequences.

Figure 10: Difference-in-Bunching around Event I and II

(a) Event Window I: 2012 Failed ban of the *Cash-GmbH*



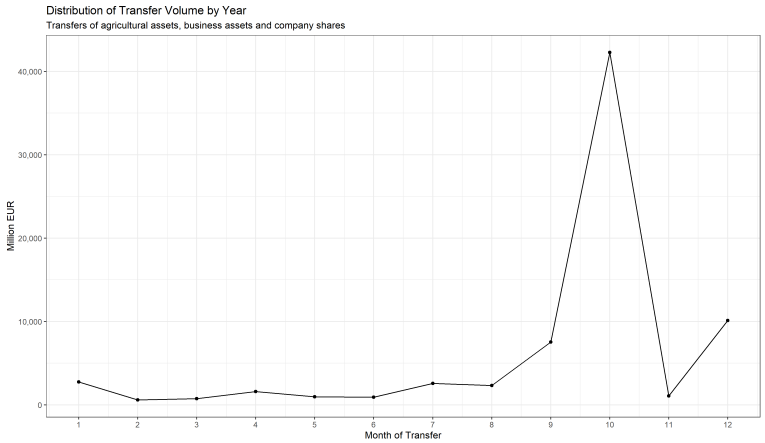
(b) Event II: 2014 Constitutional Court Verdict



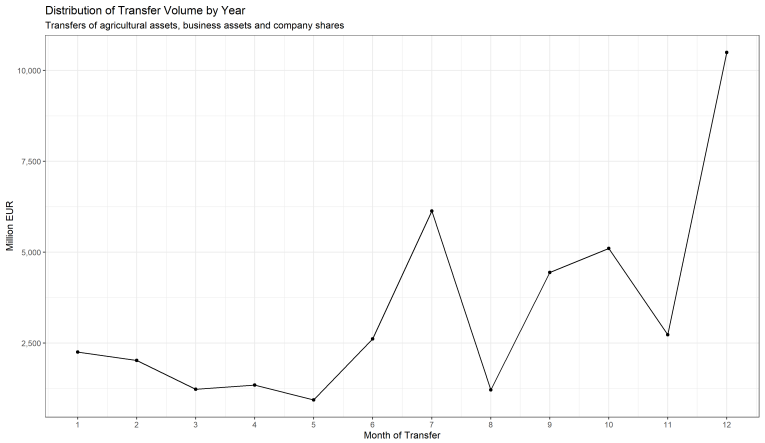
Note: Figures 10a and 10b display normalized weekly transfer counts for Event Windows I and II detailed in Section 3.2. The bins for each distribution are expressed as shares of overall transfers occurring around a one-year window around the end point of the event window. Weeks are centred around the end of the event window, where week zero starts with the end point date and includes the six days thereafter. All details are described in Section 3.4.1. The treated distribution for Figure 10a includes transfers in a one-year window around 25 October 2012 whereas the counterfactual distribution comprises transfers in the same window around 25 October 2011. The treated distribution for Figure 10b includes transfers in a one-year window around 17 December 2014 whereas the counterfactual distribution comprises transfers in the same window around 17 December 2011. The boxed numbers indicate the excess mass estimate in the three weeks before Event I and II with bootstrapped standard errors in parentheses. *Source:* Authors' calculations based on data from the RDC of the Federal Statistical Office and the Statistical Offices of the Federal States.

Figure 11: Monthly Gift Transfer Volumes

(a) Gift Transfer Volume 2012



(b) Gift Transfer Volume 2014



Note: Figures 11a and 11b display the monthly volumes of gift tax transfers for the years 2012 and 2014 respectively. Considered in the calculation are transfers that involve preferentially treated asset types, such as agricultural assets, business assets or shares in corporations. *Source:* Authors' calculations based on data from the RDC of the Federal Statistical Office and the Statistical Offices of the Federal States.

3.4.3 Heterogeneous Effects

We repeat the analysis conducted on the main sample on two sets of sample splits to tease out response heterogeneity in our estimates. Of particular interest to us is first, whether the asset classes eligible for preferential treatment respond differently and second, whether wealthy individuals are more responsive than less wealthy ones and whether there are differences for these groups across the two event windows.

Asset Class. In a first step, we investigate whether the behavioural response differs across types of preferentially treated asset classes. We subset our data to the three types of transfers, i.e., transfer of agricultural assets, business assets, and company shares and re-estimate the excess mass for each sub-sample. We present the estimated normalized excess masses and two standard error confidence intervals in Figure 12a.

We find that for the first event window, business assets appear to be the most responsive asset class, closely followed by company shares. In comparison, agricultural assets seem to be unresponsive to the abolition of the *Cash-GmbH*. One contributor to this result might be the fact that transfers of agricultural assets tend to be much smaller in magnitude of wealth transferred, which diminishes the gains from tax planning compared to its cost. It might also more frequently be the case for these assets to fall under the personal exemption thresholds, which makes the provisions for preferential treatment irrelevant. We also detect differences in responses between the two event windows of interest. For the failed ban on the *Cash-GmbH* scheme, business assets and company shares responded similarly strong, whereas for the verdict of the German Constitutional Court, transfers of business assets are twice as responsive as transfers of company shares.

Wealth Quartile. Second, we are interested in whether wealthy individuals were more responsive to the considered events compared to less wealthy individuals. Given the progressive nature of the tax schedule, more wealthy individuals had higher incentives to respond to the looming threats and given a fixed portion of the costs of avoidance, we expect them to

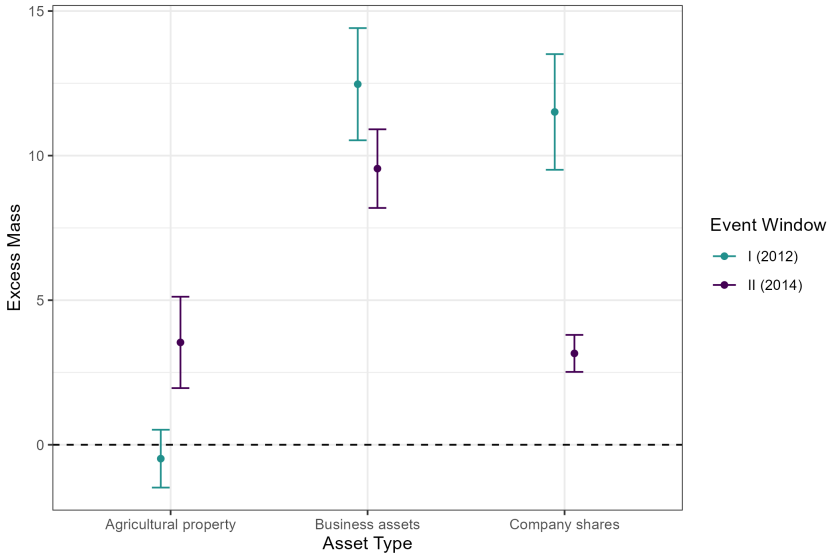
gain more compared to less wealthy ones. To get at this question, we sort individual transfers into quartiles using data between 2009 and 2017.[59] By considering the overall distribution of wealth, wealth quartiles are comparable across events and not sensitive to the distribution of transfers in the respective event window.

We find that across both events, the responsiveness of individuals seems to increase in the amount of wealth transferred. Where the amount of bunching is relatively negligible in the lowest quartile, the excess mass estimates increase by up to 10 times as we move through the wealth distribution. We detect the largest response for the highest quartile for Event Window I, which amounts to almost two times the average effect. This result has intuitive appeal: as setting up a corporation for use as a tax saving vehicle only is costly, we expect the largest benefits of pulling forward the transfer for high net-worth individuals. Furthermore, the prospect of losing preferential treatment was more likely ex ante, increasing the incentives to transfer before the event date at all costs. The response heterogeneity by levels of wealth at stake has distributional consequences for the progressivity of announced tax changes, which is an important consideration for policymakers.

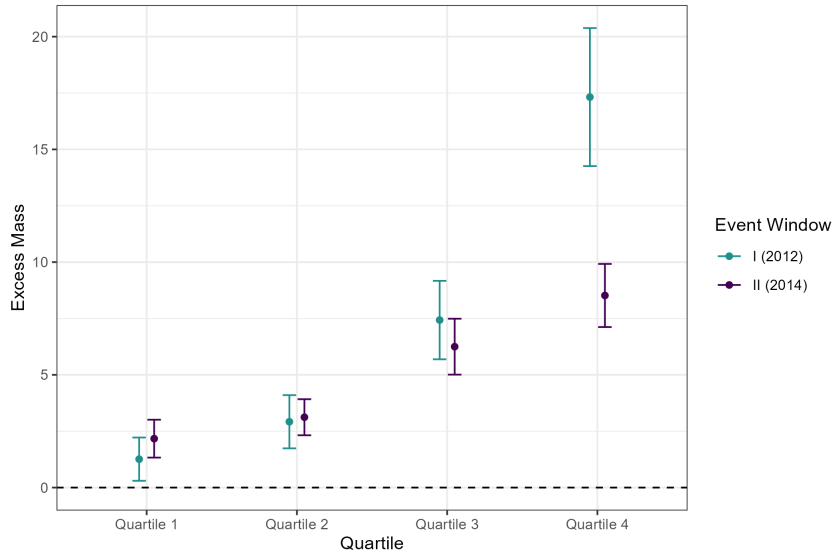
[59] We exclude the periods before 2009 for setting the wealth quartiles, as the determination of business value followed a different method before the 2009 reform, leading to apples to oranges comparisons.

Figure 12: Bunching Heterogeneity

(a) Asset Class



(b) Wealth Quartile



Note: Figure 12 shows excess mass estimates for different sample splits with two standard errors confidence bands. Figure 12a illustrates excess mass estimates for the different preferentially treated asset classes (agricultural property, business assets and company shares), while Figure 12b depicts excess mass estimates for each wealth quartile across the different event windows. *Source:* Authors' calculations based on data from the RDC of the Federal Statistical Office and the Statistical Offices of the Federal States.

3.5 Characteristics of Bunchers

After documenting significant behavioural responses to the legislative developments, we investigate whether transfers made inside the bunching windows identified in the previous section differ from transfers outside the bunching windows in terms of observable characteristics. The observed transfer characteristics might shed some light on the transfer motives and provide further indication of tax avoidance intent. Based on prior literature, we identify several characteristics which we expect do differ between transfers within the bunching windows and those outside of them.

First, we expect transfers within the bunching windows to be characterized by even lower effective tax rates and higher use of tax exemptions compared to the overall sample. As responding individuals show an acute awareness of legislative developments and threats, we expect them to be able to optimize the transfer from a tax perspective, making full use of any exemptions available.

Second, it is generally understood that family firm owners have difficulties relinquishing control, especially when they are the founder (Handler, 1994; Sharma, 2004). From this perspective, we would expect recipients of family businesses to be generally of a more mature age and ready to take over the business from their predecessor, even if inheritance tax planning is at play. As transfers of cash are less tied to a desire to retain factual control over the asset, we would, on average, expect more transfers to benefit a minor compared to regular business transfers. Consistent with this notion, our descriptive results in Table 14 show that the share of minors is the highest for gift transfers of cash and financial assets. Finding a significantly higher share of minor recipients within our event windows would be consistent either with the threat of the legislative change to be perceived as so detrimental that it outweighs the desire to retain control, or with a masked cash transfer.

Finally, Bennedsen et al. (2007) among others show that male descendants are favoured in obtaining control of the family business in a succession event. As pointed out by Kubíček and Machek (2019), this can be due to a multitude of reasons such as primogeniture, gender

stereotypes, or willingness to join and lead on the recipient side. Several studies show that male heirs are more likely to receive transfers of business assets in general (Ahrens et al., 2015; Kubíček and Machek, 2019; Tisch and Schechtl, 2023), which is consistent with the observed patterns in Table 14. Based on these findings, we expect transfers in the first bunching window in particular to more likely benefit a female recipient compared to transfers outside. To the extent that these transfers constitute cash transfers masked as business successions, we should see a higher incidence of female ownership more akin to transfers of non-business assets.

To investigate differential transfer characteristics, we estimate OLS regressions using the following specification:

$$\begin{aligned} y_i = \alpha + \beta_1 \,\text{BW Event I}_i + \beta_2 \,\text{BW Event II}_i + \sum_{k=2}^{4} \gamma_k \,\mathbb{1}\left\{\text{Wealth Transfer Quartile}_i = k\right\} \\ + \sum_{j=1}^{4} \delta_j \,\mathbb{1}\left\{\text{Age Quartile}_i = j\right\} + \eta_1 \,\text{Company Shares}_i + \eta_2 \,\text{Business Assets}_i + \varepsilon_i, \end{aligned} \tag{4}$$

where *BW Event I* and *BW Event II* are indicators for transfers located in Bunching Window I and II respectively. We control for other determinants of transfer characteristics such as quartiles of the wealth transfer amount and donor age based on the distribution of the respective variables between 2009 and 2017, as well as for the major asset type transferred. As outcomes, we consider the effective tax rate (ETR), the share of favourably treated assets, and a set of indicators equal to one if the recipient was of minor age, a son or daughter of the donor, and whether the gift donor was female. We show the result of this exercise in Table 15. The table shows the coefficient estimates for the two indicator variables of interest with robust standard errors in parentheses.

We find that transfers in the bunching windows feature a significantly lower effective tax rate. In comparison to the sample average of 0.08%, taxes on transfers made during

the first event window's bunching window were 50% lower, while transfers made during the second bunching window were about 13% lower on average. This suggests a higher level of tax expertise among individuals who choose to locate just before the event deadlines. Additionally, we document that the proportion of transferred assets within the bunching windows that is taxed preferentially is higher than that outside of them, which accounts for the lower effective tax rates.

Next, we consider the characteristics of the recipients as outcomes. First, we find differing results regarding transfers to recipients of minor age between the two bunching windows. In case of the first bunching window, transfers are more likely to go to a minor recipient. Compared to the sample average of 2.8%, transfers to a minor occur 39% more frequently in the bunching window, holding wealth, the age of the donor, and the asset type constant. This finding is in line with evidence from Finland that transfers of firm ownership to minor children are a common method to avoid later inheritance and gift taxes (Paukkeri et al., 2023). However, for the second bunching window we find the opposite effect: transfers to a minor occur about 29% less frequently compared to the sample mean. This suggests that transfers immediately before the abolition of the *Cash-GmbH* avoidance scheme were driven more by pure tax avoidance motives than transfers prior to the verdict of the German Constitutional Court in 2014. If we consider the transfer of business assets that exploits a loophole in the administrative asset catalogue as a mere transfer of liquid funds rather than the actual control of a business, these patterns become more intuitive.

Second, our results suggest that transfers preceding the abolition of the *Cash-GmbH* are more likely to go to a daughter compared to transfers outside the bunching window. The coefficient estimate suggests that after controlling for other transfer characteristics, transfers in the first bunching window are about 19% more likely to benefit a daughter of the donor compared to the average over the whole sample. Interestingly, there is no significant difference in gender recipient patterns between transfers in the second bunching window compared to transfers outside of it. There are two main explanations for this pattern.

On the one hand, this again could be indicative that the transfers during the first bunching window are more similar to a transfer of cash than a transfer of a company. Hence, viewing the first bunching window as an opportunity to pass on cash to the next generation could rationalize this result. Alternatively, it might also be the case that these quasi-cash transfers are more easily split among several descendants. In a succession case where the son receives the business and the daughter financial assets as compensation, we would not see the funds received by the daughter in our sample. However, if shares in a cash-holding corporation are transferred during the bunching window, this transfer will show up in our data and hence increase the share of female recipients in that period only.

Overall, these tests are consistent with the behavioural responses to the first legislative event being driven by individuals explicitly exploiting the tax loophole in the administrative asset catalogue, whereas for the second event, responses are generally more similar to regular business transfers and would therefore be consistent with a hedging motive regarding detrimental future developments.

Table 15: Characteristics of Bunchers

	Dependent Variable:					
	ETR	Share Fav. Assets	Minor Rec.	Son	Daughter	Female Giver
BW Event I	-0.004^{***}	0.101^{***}	0.011^{***}	-0.025^{*}	0.041^{***}	0.02
	(0.001)	(0.008)	(0.006)	(0.013)	(0.012)	(0.013)
BW Event II	-0.001^{*}	0.016^{**}	-0.008^{**}	0.01	0.009	0.005
	(0.001)	(0.007)	(0.003)	(0.011)	(0.009)	(0.01)
Wealth Controls	X	X	X	X	X	X
Age Controls	X	X	X	X	X	X
Sample mean	.008	.728	.028	.483	.218	.339
$R^2 Adj.$	0.08	0.14	0.01	0.1	0.03	0.02
Observations	76,917	76,687	76,798	76,937	76,937	76,937

Note: Table 15 presents the results of estimating Equation 4 using Ordinary Least Squares. The coefficients are displayed for regressing different outcome variables on indicators for transfers located within the bunching window of Event Windows I and II. All specifications control for wealth transfer quartile and age quartile as well as the major asset type. Robust standard errors are given in parentheses below the coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% significance level respectively. Results are robust to different specifications of the control variables.
Source: Authors' calculations based on data from the RDC of the Federal Statistical Office and the Statistical Offices of the Federal States.

3.6 Quantification of Tax Avoidance

Armed with an estimate of the normalized excess mass in the distribution of taxable transfers, we can quantify the extent of tax avoidance using a back-of-the-envelope calculation of the forgone tax revenue due to re-timed transfers. The quantification exercise is based on the following thought experiment. Suppose that the excess transfers we identified in the previous section were not taxed under the relevant schedule at that time, but under the provisions that were expected to be applicable after the respective end point of the event windows. How much revenue did the German government lose in this hypothetical scenario due to re-timing responses?

To answer this question, we back out the overall number of transfers that are due to tax planning in a first step. Let $\hat{b}_T$ be the estimated excess mass of the transfer count distribution for event window $T = 1, 2$. Based on the observed number of transfers in the bunching window $N_T = \sum_{t=L_T}^{U_T} n_t$, we can calculate the excess number of transfers using

$$N_T^E = N_T - \frac{N_T}{\hat{b}_T} = \frac{(\hat{b}_T - 1)}{\hat{b}_T} N_T. \tag{5}$$

Intuitively, we subtract the number of *regular* transfers ($1/\hat{b}_T \times N_T$) from the overall number of transfers. The number of transfers left after this transformation can be interpreted as excess transfers in the sense that they would not have occurred in the absence of the events.

In a second step, we exploit the detailed information about taxable transfers in our data to recalculate the tax base and final tax burden of each transfer in the bunching window under the hypothetical scenario. In order to reflect the uncertainty taxpayers faced when deciding on pulling forward their transfer, we entertain a variety of hypothetical scenarios. For the baseline scenario, we simply add back the full amount of tax exemptions for preferentially treated assets to the actual gift tax base. Afterwards, we conduct the same calculation steps that lead from the tax base to the actual amount of determined tax. This implies applying the (progressive) tax rate schedules as stated in the respective version of the tax code.[60]

[60] This means that actual tax rates varied by transfer size, albeit they increased relative to the initially

Conceptually, this approach assumes that for both event windows, the extreme case scenario realizes, i.e., the exemptions for preferentially treated asset classes are void, meaning that companies are fully taxed on their assets.

As the complete voiding of the preferential treatment provisions was a rather unrealistic outcome for the 2014 verdict of the Constitutional Court, we vary the negative tax consequences to provide a more nuanced picture. In our first set of alternative scenarios, we consider different exemption shares to be applied to the overall tax base. This would have been one way the legislator could have addressed concerns by the court that the privilege for business assets was excessive. Therefore, in addition to the complete voiding of the provision, which would correspond to an exemption share of 0, we calculate hypothetical scenarios based on an exemption share of 25, 50 and 75%.

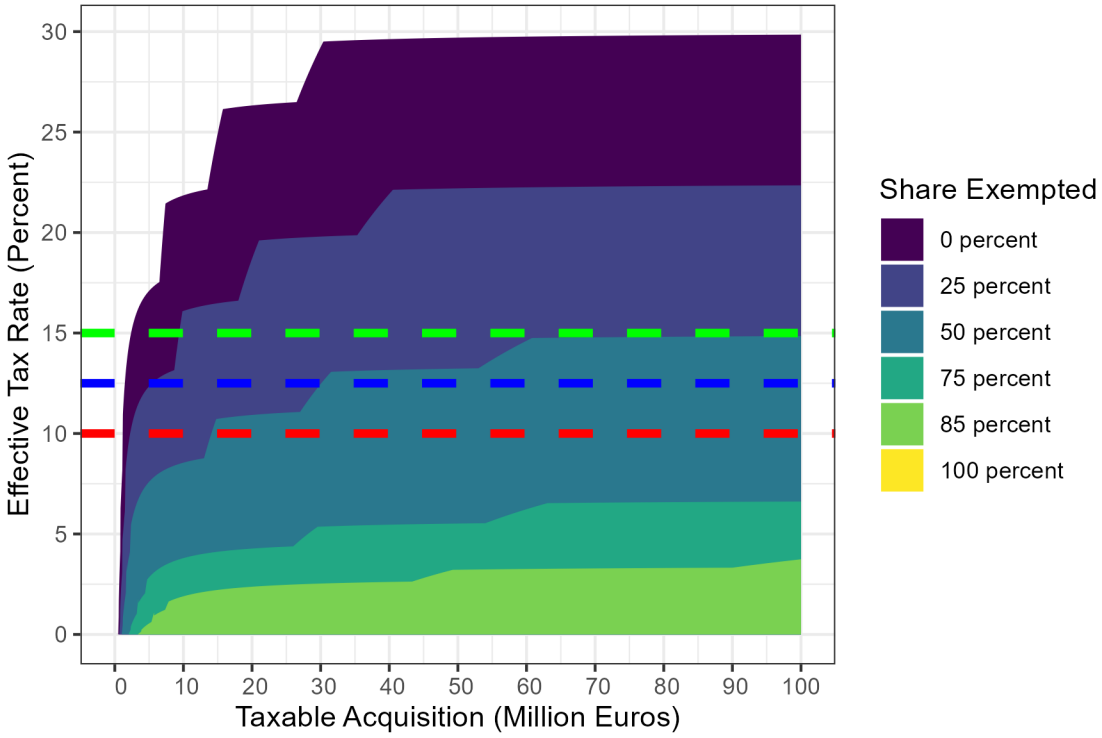
Another possibility, which was proposed by researchers and politicians in the aftermath of the verdict, is a so-called flat tax.[61] A low, flat tax rate in combination with a broad tax base has the potential to raise equal if not more revenue while simultaneously curbing tax avoidance opportunities exploiting tax exemptions. For this alternative set of scenarios, we implement flat tax rates of 10, 12.5, and 15% while simultaneously reducing the exemption share for preferentially treated asset classes to zero. Note that we still allow for personal exemptions that would also apply to other types of assets, such as real estate or cash.

Figure 13 illustrates the tax consequences of the hypothetical scenarios in terms of the effective tax rate, based on the taxable acquisition of the transfer. The figure displays effective tax rates for an exemplary transfer to a spouse (tax class I), resulting from applying the tax schedule for different values for the exemption share. It is apparent that removing parts of the exemption share leads to substantial increases in effective tax rates across the wealth distribution. For very large inheritances, a flat tax of 15 percent would correspond to a decrease of the exemption share to 50% of the taxable acquisition.

applicable tax rates (especially for large transfers of business assets and with little amounts of other transfers). The highest applicable tax rate was 30%.

[61] See, for example, Bach and Thiemann, 2016, or the plenary protocol 18/180 of the Bundestag from 24 June 2016, available under `https://dserver.bundestag.de/btp/18/18180.pdf#P.17773`.

Figure 13: Effective Tax Rates in Hypothetical Scenarios



Note: Figure 13 shows the effective tax rates in the hypothetical scenarios we consider for our revenue loss calculations, exemplary shown for tax class I. The shaded areas visualize effective tax rate structures for different shares of exemption for qualifying asset types based on the value of taxable acquisition. The dashed lines on the other hand visualize the alternative flat tax rates of 10, 12.5 and 15% we employ. Effective tax rates are calculated assuming a transfer to a spouse, implying the maximum personal deduction of EUR 500,000 and tax class I. The calculated rates further accommodate the equitable compensation provided by the law in the region around an increase in the average statutory tax rate (*Härteausgleich*) as well as the deduction amount for low acquisitions (*Abzugsbetrag*).

By computing the difference between the recalculated hypothetical burden and the actual taxes paid and aggregating the individual changes over the bunching interval, we get a measure of the overall change in tax revenue if every transfer would have been subject to the hypothetical scenario tax schedule. We then compute the share of this revenue change due to tax planning by multiplying the overall revenue change with the share of excess transfers in total transfers. Hence, our estimate of foregone tax revenue R^F can be expressed as:

$$\begin{aligned} R^F &= \sum_{i \in I_T} \left[TB_i^h - TB_i^r \right] \times \frac{N_T^E}{N_T} \\ &= \sum_{i \in I_T} \left[TB_i^h - TB_i^r \right] \times \frac{(\hat{b}_T - 1)}{\hat{b}_T}, \end{aligned} \tag{6}$$

where TB_i^h and TB_i^r denote the hypothetical and real tax burden of individual i and I_T denotes the set of individual transfers in the bunching window.

Figure 14 shows the result of this exercise. For the two event windows, the foregone tax revenue is plotted for each of the seven hypothetical scenarios. The dashed horizontal lines depict the average foregone revenue across scenarios for each event, while the solid horizontal line corresponds to the overall revenue from inheritance and gift taxation in Germany in 2011 as a reference point.

For each scenario, the computed revenue loss constitutes a lower bound, as it is based on the assumption that behavioural responses only take place within the bunching windows. However, *Cash-GmbHs* were possible from 2010 until mid-2013, which means that the revenue loss is likely to be even higher. The different scenarios allow us to provide some bounds on the revenue effect.

The amounts of foregone tax revenue differ substantially across events, which is both due to very different estimated amounts of hypothetical tax revenues as well as a higher *share of bunchers*.[62] A lower-bound estimate of the foregone tax revenue is provided by the least strict tightening of the preferential treatment provision, granting only a 75% exemption as

[62] See Table B.3 in the Appendix.

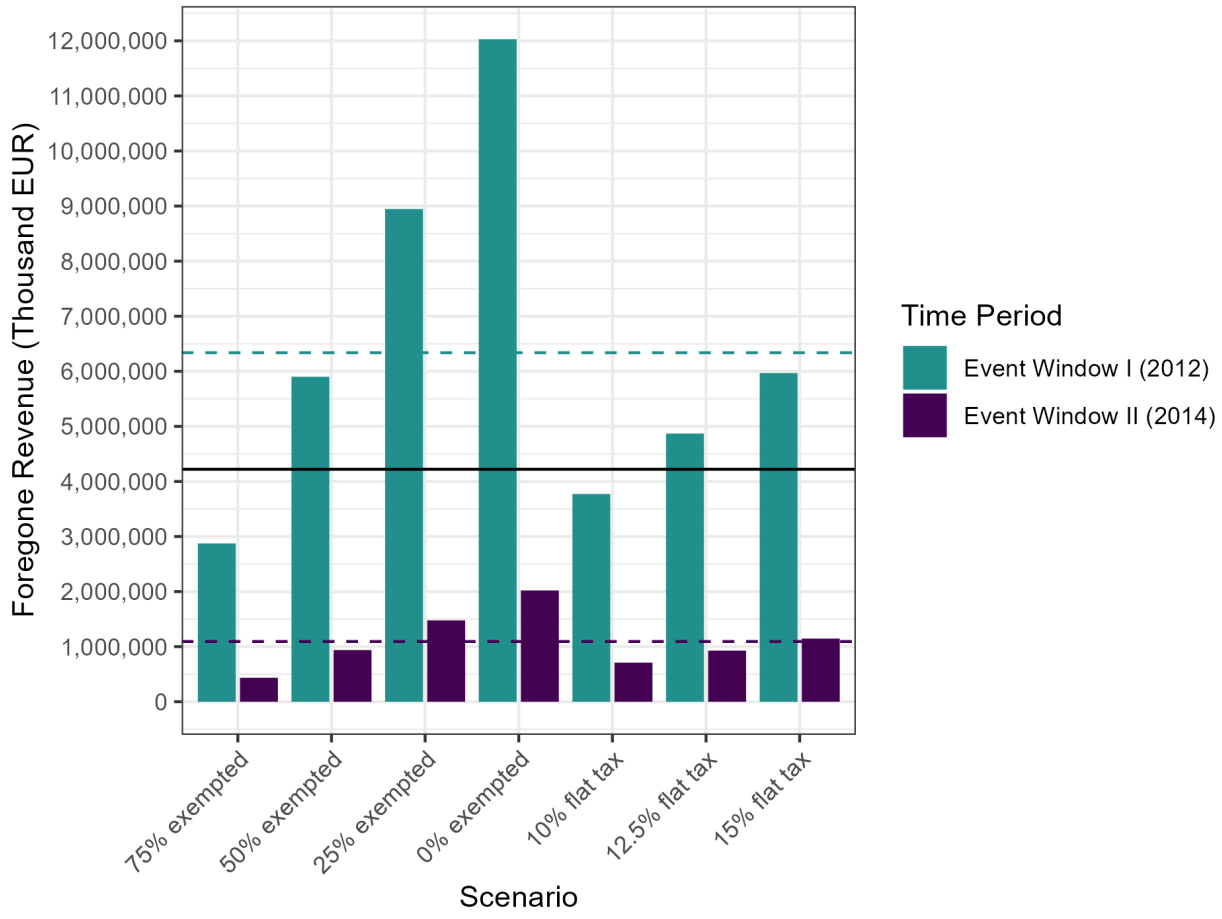
opposed to a factual full exemption in most cases. In this case, the foregone revenue from tax avoidance of EUR 3.2 billion amounts to 76% of the total revenue from inheritance and gift taxation in Germany (from all transfers) in 2011. If these transfers had instead been taxed without preferential treatment exemption, the German government would have obtained additional revenues of EUR 12 billion, which constitutes 2.8 times the actual tax revenue from 2011. For Event Window I, this is a reasonable scenario, as the excess mass is very likely to stem from tax planning vehicles only. This is because a major reason for the bunching right before the event date was to prevent administrative assets like cash and real estate from not being exempt from inheritance and gift taxes.[63] The average foregone revenue of roughly EUR 6 billion is still 1.5 times higher than the reference revenue, indicating substantial revenue losses due to tax planning. Note that while the initial purpose of the preferential treatment of certain assets was to alleviate concerns of overburdening firms with inheritance and gift taxes, the fiscal consequences of this tax avoidance scheme were enormous. This becomes evident when comparing the magnitude of our findings to evidence of tax expenditures for (family) businesses in other countries.[64]

For Event Window II, the foregone revenue estimates are much more moderate. The lower bound estimate of a reduced exemption of 75% would have implied a revenue gain of EUR 0.435 billion, which amounts to approximately 10% of the 2011 revenue. If the extreme scenario of a voiding of the preferential treatment provisions was realized, the additional revenue would have totalled EUR 2 billion or approximately 48% of the 2011 revenues. The average foregone revenue across the different scenarios amounts to EUR 1.093 billion, or 26% of the reference revenue.

[63] In effect, administrative assets were excluded from gift tax exemptions after the reform, with two special cases depending on their value relative to the transferred amount of business assets. Above 90%, the entire amount of transferred business assets became fully ineligible for gift tax exemptions. Below 10%, their amount was considered negligible and therefore did not lead to reduced gift tax exemptions.

[64] See Figure 3.17 in OECD (2021), where for Belgium 0.5% and for the Netherlands 8% of the actual tax base are foregone due to tax expenditures.

Figure 14: Foregone Tax Revenue Across Hypothetical Scenarios



Note: Figure 14 illustrates the foregone tax revenue to the German state under the different hypothetical scenarios considered. Each bar represents the difference in tax revenue in the respective scenario net of the taxes actually paid. The first four scenarios show differences in taxes for lower shares of preferential treatment, ranging from 75% to 0%. The last three scenarios consider the revenue effects of a flat tax between 10 and 15% on the taxable transfer with no exemption for preferentially treated asset types. The dashed horizontal lines constitute the average foregone revenue across the different scenarios for each event window. The solid black line represents the overall revenue from the inheritance and gift tax for the reference year 2011 (EUR 4,221,122 Thousand). Authors' calculations based on data from the RDC of the Federal Statistical Office and the Statistical Offices of the Federal States.

3.7 Conclusion

Our findings show that the anticipation of tax changes substantially influences the timing of business transfers to the next generation. The speed with which business transfers react to changes in the tax environment is surprising. This is particularly the case for the ban

of a favorable tax avoidance scheme (Event Window I), which induced business owners to transfer more than EUR 40 billion in less than a month, and only four months after the event could be anticipated. Prior literature suggests that successions within family businesses are prone to trigger conflicts within the family.[65] Against this background, we provide evidence that an external threat in the form of substantial increases in effective taxation leads to temporarily coordinated and timely family action. This has implications for the design and communication of tax policy changes, as anticipated tax changes will come at the loss of a large part of the potential tax base.

We further document heterogeneous behaviour across asset classes in Section 3.4.3. An apparent difference concerns the insensitivity of transfers of agricultural assets to the same events that induce enormous reactions for business assets and company shares. In principle, owners of agricultural property can apply to the same preferential tax treatment as the other two classes. The documented insensitivity might be explained by either missing awareness to the existence of these incentives, low general tax planning activities (because transfer values are low) or the deterrence effect that compliance with tax regulations would entail. Moreover, we show that the ability or willingness to exploit tax incentives increases substantially with business wealth. Business transfers in the highest quartile are more responsive to the threat of taxation than business transfers in the lowest quartile by a factor between 4 (Event Window II) and 15 (Event Window I). This has distributional consequences for the progressivity of the tax code, as the subjects deemed to pay a higher share of taxes are precisely the ones most able to circumvent taxation.

Event Window I most clearly allows the identification of tax avoidance, given that bunching at the unusual mid-year date (25 October 2012) is hardly explainable by non-tax-planning reasons. Our results from Section 3.5 lend further support to this notion, as transfers during the reaction window differ from regular transfers in ways that are consistent with a tax avoidance motive. We quantify the amount of foregone tax revenue to an upper bound of

[65] See Kubíček and Machek (2020) for a recent overview.

approximately EUR 12 billion or 2.8-times the actual amount of overall tax revenues from gifts and inheritances in 2011. Our results imply that the conventional argument which disregards the importance of wealth transfer taxes as fiscal instruments due to their low quantitative relevance (compared to other tax sources like income or consumption) neglects the shifts in the underlying tax base due to avoidance behavior.[66]

However, our quantification of foregone tax revenues only partially captures the overall welfare loss due to the extreme timeliness of tax planning responses. Two other sources of welfare loss are conceivable: Non-productive rent-seeking and rushed company succession. First, the (short-term) use of tax advisors and lawyers to minimize the tax burden associated with a business succession is costly. Tax advisory cost comprises a time component for the preparatory tax advisory process, and a share of the transfer value when the actual tax declaration is prepared.[67] Legal costs arise with, for instance, notary fees associated with setting up a new firm (as it was likely the case during Event Window I). Taken together, legal and advisory fees for a single tax-motivated business succession range from five-digit to small six-digit numbers. Moreover, in such situations, the tax advisory cost does not reflect an activity that creates economic value.[68] Instead, it shifts the incidence of wealth transfer taxes to individuals with a lower tax base plasticity, causing dead-weight losses to society.[69]

Second, business successions ought to be carefully prepared in advance. Otherwise, the decision to transfer control and ownership could pertain long-lasting negative consequences for the firm and its stakeholders. From a welfare perspective, the efficient allocation and management of capital is key. Surveying all German chambers of industry and commerce reveals that the recommended time span for a business succession ranges from one to up to

[66] See also Escobar et al. (2019) for another example in the Swedish setting of how the tax elasticity of tax-favoured gift transfers reduces the inheritance tax base.

[67] Both cost components are subject to the German tax advisor fee regulation, although the latter component can be reduced by up to 90% on the tax advisor's discretion. Figure B.3 in the Appendix depicts the value-based component.

[68] Weisbach (2002) argues that *tax planning, ..., produces nothing of value.*

[69] Scheuer and Slemrod (2020) introduce the concept of plasticity to describe the ease with which the super-rich can change between different tax bases.

ten years, with the majority suggesting that a succession duration of five years is optimal.[70] Our results show that the motivation to avoid increases in wealth transfer taxes drastically shortens the succession period of businesses. Albeit we cannot directly observe the transfer of mere ownership versus actual company control, our identification of the characteristics of bunching business owners reveals that at least during Event Window II, actual company control was passed onto the next generation. This raises the question whether such transfers had been planned long in advance, and are only executed at short notice due to tax reasons. If this is not the case, immediate tax risks may motivate a hastened succession in some firms. Future research might answer the question whether these firms subsequently suffer from inferior performance or prolonged intra-family conflict.

Our results yield two major policy implications: taxing wealth transfers at uniform rates and considering wealth taxes as a backstop to wealth transfer tax avoidance. First, our findings for Event Window I shed new light on the exploitation of business shells for tax purposes in the German context. Our findings are also in line with a growing body of international evidence that ranges from the avoidance of wealth (transfer) taxation to the evasion of consumption taxation using businesses as a vehicle. In our setting, we expect this avoidance response to be driven by a very progressive nominal tax rate structure, with the highest tax rate being equal to 50% of the transfer value. In the presence of ample tax planning opportunities, a higher difference between ordinary tax rates and tax rates under preferential taxation increases the incentives for wealth-transferring individuals to exploit a business shell.[71] This provides a strong argument for policy makers to refrain from (inherently difficult) tax discrimination between business and non-business assets. In other words, a reduction of tax rate differentials across assets classes, possibly accompanied by a lowering of the overall tax rate structure, would reduce behavioral distortions due to tax code progression. By simulating the foregone tax revenues for both events using a flat tax

[70] Figure B.4 in the Appendix provides the distribution of recommended succession periods.

[71] Note that the short-term timing responses documented in this work merely provide one example among manifold options to avoid German inheritance and gift taxes as a business owner.

rate of 15% in Section 3.6, we find that such approaches, despite the likely reduction in administrative costs, significantly reduce the negative fiscal consequences of tax avoidance.

The second policy implication of our results is that unified systems of wealth transfer taxation, which target both inheritances and gifts, suffer from a high elasticity of the gift tax base. Our estimates of foregone tax revenue show how timing responses strongly reduce overall tax revenues from wealth transfers. Policy instruments that mitigate the negative fiscal consequences exist. The retroactive closure of loop holes in the tax code would be an option to limit avoidance responses and the actual regressivity of the wealth transfer tax system. One could, for instance, date back the applicability of the revised tax regulation to the date of announcement. Yet, in the context of the responsiveness of the wealth transfer tax base, Hey (2010) argues that *...mere budget effects cannot be considered to be an announcement effect justifying retroactivity.* Another, more implementable policy instrument to mitigate tax-motivated wealth transfer tax avoidance could be the taxation of (net) wealth. As the taxation of wealth is known to also evoke avoidance reactions, particularly by wealthy individuals, certain provisions would hence need to be met.[72] First, our results lend support to a tax that targets only very wealthy individuals through sizable individual tax allowances, which could increase political support and public legitimacy. Second, due to the geographic mobility of wealthy tax payers and the difficulty to enforce cross-border direct taxation of wealth, taxation of (net) wealth ought to take place within a coordinated, multinational framework.[73] Finally, tax rate differentiation between different asset classes should be avoided to reduce the type of asset shifting documented in our work. We argue that, despite the known limitations of and obstacles to (net) wealth taxation as an individual policy instrument, our results support viewing wealth taxes as a backstop to the virtual non-taxation of wealth transfers by wealthy individuals through re-timing of transfers and the use of tax-advantaged business shells.

[72] Advani and Tarrant (2021) provide a recent overview of behavioral responses to wealth taxes.

[73] An example would be the global minimum tax on billionaires proposed by Zucman (2024), critically discussed by Amaddeo (2024).

4 The Real Effects of Job Protection Legislation on Firm Performance – Evidence From German Inheritance Tax

Co-Authors: Philipp Doerrenberg, Jan Zental

Abstract: This paper examines the real effects of employment protection measures on firm performance by leveraging a unique feature of German inheritance and gift tax law. Specifically, we exploit the preferential tax treatment granted to gratuitous business transfers, which is contingent on meeting minimum holding periods and payroll sum requirements. To study these effects, we identify firm ownership changes triggered by the death of the owner, utilizing Orbis ownership data and publicly available death records. We merge this data with administrative employment data and employ a stacked difference-in-differences design, exploiting a size-dependent applicability threshold. By comparing firms subject to payroll sum requirements to those exempted, we isolate the causal impact of these provisions, as both treatment and control group undergo an exogenous succession event. Our findings indicate that the payroll sum requirements significantly reduce employment growth, with affected firms experiencing up to 20% slower growth relative to the control group.

Keywords: Inheritance taxation, firm ownership succession, employment protection

Acknowledgements: We thank Christoph Spengel, Felix Vetter, Ulrich Glogowsky, Dominika Langenmayr, Dirk Foremny, Jordi Jofre, Jan Kemper, Christin Schmidt, Emilia Gschossmann and Katharina Schmidt as well as participants of various seminars, workshops and conferences for valuable feedback, comments and suggestions. We owe special gratitude to discussants of earlier versions of this paper: Theresa Bührle and Linda Wu. We thank the Institute for Employment Research (IAB) and in particular Manfred Antoni for data provision. Sergen Keskincelik provided excellent research assistance. All authors are affiliated with the University of Mannheim. Richard Winter gratefully acknowledges financial support from the Graduate School of Economics and Social Sciences of the University of Mannheim.

4.1 Introduction

Taxes on wealth transfers can be important instruments to combat increasing wealth inequality (Nekoei and Seim, 2022). In this context, the tax treatment of business assets is the cause of heated public debate in many countries. Almost all developed countries treat business assets preferentially, for example, through lower rates or exemptions (OECD, 2021). This preferential tax treatment is granted on grounds of the notion that productive businesses provide employment and growth, which benefits society as a whole. Therefore, it is argued that it is essential not to overburden these companies during firm succession. Paying wealth transfer taxes may lead to a lack of liquidity in transferred businesses and, in the worst case, could induce company liquidation and threaten employment.

Tax policies designed to shield transferred businesses from harmful taxation should ensure that preferential treatment achieves its intended purpose. In many countries, this requires recipients to meet specific conditions to qualify for tax benefits. Such conditions may include restrictions on selling the business within a set period or mandates to preserve jobs (OECD, 2021). Ideally, these requirements incentivize companies to continue generating positive externalities. However, they could also negatively impact firm performance for two main reasons. First, these conditions may hinder the efficient allocation of labor and capital within and between firms (Akcigit et al., 2023). In such cases, well-meaning restrictions on restructuring decisions could inadvertently exacerbate crises. Firms with declining employment trends face a difficult trade-off between complying with the requirements on the one hand and optimizing their resource allocation on the other. Second, conditional exemptions introduce uncertainty about future tax obligations, which could affect risk taking (Astrachan and Tutterow, 1996). Inexperienced heirs in particular may respond to this uncertainty by adopting a risk-averse, "business-as-usual" approach to avoid potential retroactive tax liabilities. Given these risks, it is worth exploring whether tying tax benefits to constraints on factor inputs could lead to unintended consequences.

We provide an empirical answer to this question by examining the case of Germany. Since

2009, German inheritance tax legislation has required firms to meet input factor requirements to qualify for generous tax exemptions on the transfer of business assets. Specifically, firms must maintain their cumulative payroll above a pre-defined threshold and are prohibited from selling operationally essential parts of the business during a designated holding period. During this time, neither capital nor labor inputs can be significantly reduced without risking a retroactive revocation of the tax exemption. This requirement effectively increases the layoff costs incurred by the firm by the proportional withdrawal of the tax exemption. Between 2010 and 2016, the cumulative payroll requirement applied only to firms with more than 20 employees at the time of transfer. Firms with 20 or fewer employees could benefit from the exemptions without the requirement.[74] We exploit this policy variation by focusing on firms that experienced exogenous, death-related ownership transfers. Using a stacked difference-in-differences design, we compare firms that underwent a succession while being subject to the payroll sum requirement with comparable firms that experienced the same event at the same point in time, but without the requirement. Our approach allows us to eliminate a variety of potential confounders (such as the succession itself) and is robust to treatment effect heterogeneity (Cengiz et al., 2019; Baker et al., 2022), thus isolating the impact of the labor protection element of the provision.

We combine three data sources to conduct our analysis. First, we use the company database Orbis from the commercial data provider Bureau van Dijk (BvD) to identify all German firms that underwent ownership changes between 2010 and 2015.[75] Second, we link these ownership changes to publicly available individual-level death data to identify death-related successions. This data mainly consist of two databases sourced from newspaper death notices and tombstones, hosted by CompGen, a German non-profit association for Computer Genealogy. We enrich this information by scraping obituary sections from regional newspaper websites. In order to ensure that we are capturing ownership changes subject to inheritance

[74] These firms only had to adhere to the capital requirement.

[75] We focus on this period because the requirements were adjusted in 2009 and mid-2016, without retroactive application.

taxation, we restrict our sample to unique matches within our database and focus on family firms where the incoming owner has the same family name compared to the outgoing owner. Third, we merge the firm-level data with administrative employer-employee records, which provide us with a rich set of labor market outcomes. To further limit differences between our treatment and control group, we require firms to be in a window of 10 employees above or below the threshold.

Our findings show that the payroll sum requirement reduces growth of the firm-level payroll among affected firms facing uncertainty about their future tax obligations. This decline is driven by both lower increases in nominal wages and reduced expansion of employment, consistent with more cautious employment strategies imposed by incoming heirs. Our results are robust to zooming in close to the threshold. Moreover, using this more-fine grained identification approach, we find a significant positive positive effect on investment during the holding period, indicating that firms substitute labor with capital. We further validate our design with a placebo test that assumes that the treatment takes effect at ten employees. Both tests suggest that size differences are unlikely to explain our findings. Finally, we detect a muted impact of the requirement on highly leveraged firms, suggesting that the responsiveness to the treatment hinges on the financial situation of the firm.

We contribute to several strands of literature. First, our work relates to an established literature on the effects of wealth transfer taxes on firms. Utilizing German firm survey data, Hines et al. (2019) show that inter vivos gift transfers occur more often during economically viable circumstances, which speaks to the importance of liquidity concerns when liable to wealth transfer taxation. Using data from Greek firms and an instrumental variable approach, Tsoutsoura (2015) finds that inheritance taxation has a negative effect on firm investment. In a similar vein, Ellul et al. (2010) find that inheritance law as such can already have a negative effect on firm investment by dispersing ownership within families. In the German setting, Glogowsky (2021) finds that tax incentives are actively exploited, particularly for testaments, which is a conventional way to organize firm succession in an an-

ticipatory manner. Franke et al. (2016) evaluate the payroll sum policy using cross-sectional data from the inheritance and gift tax statistic and simulations. Their findings suggest that the requirements for preferential treatment are likely to exert only negligible effects on firms. We add to this literature by showing the distortionary effects of tying inheritance tax exemptions to real economic outcomes.

Second, we add to the literature on the effects of labor market regulation on firm outcomes. In particular, we provide evidence on the role of firing costs and employment targets on firm-level employment. Theory predicts that employment protection in the form of increased lay-off costs has a stabilizing effect on employment by reducing job turnover through reduced hiring and firing (Bentolila and Bertola, 1990; Lazear, 1990). There is some empirical support for this prediction. Autor et al. (2007) exploit state-level variation in wrongful-discharge legislation in the US to provide evidence on the link between dismissal costs and productivity. They find that wrongful-discharge protection reduces employment flows and firm entry rates. Adhvaryu et al. (2013) use fluctuations in rainfall in rural India to instrument local labor demand, finding that industrial employment is more sensitive to shocks where labor regulation is less restrictive. Related to Autor et al. (2007), Kugler and Pica (2008) examine the effects of an Italian reform that lead to an increased cost of unjust dismissals. The authors find that this increased cost decreased new hires as well as separations of workers in affected relative to unaffected firms. Additional evidence from the Italian setting is provided by Sestito and Viviano (2018), who leverage an Italian reform lowering firing costs for newly signed open-ended job contracts. Utilizing differential exposure to the reform, the authors show modest hiring responses to a reduction in dismissal costs. We complement these findings by showing that these effects also materialize in a setting with uncertainty with respect to dismissal costs. In a closely related setting Akcigit et al. (2023) investigate the effects of binding employment maintenance commitments in the context of the privatization of East German firms in the aftermath of the fall of the Iron Curtain. They find that firms with binding employment commitments experienced a 22 percentage point

higher annual employment growth rate but also had a 3.6 percentage point higher probability of exit. We complement their findings by showing how targets imposed without regard for the economic situation of the firm can reverse the well-intended effects of the regulation in the context of firm ownership successions.

Third, we offer new insights to the literature on the impact of uncertainty on firm performance. It is long recognized that firms experiencing a temporary increase in uncertainty with respect to their business environment become more cautious in their hiring and firing decisions.[76] This prediction is based on the notion that hiring and firing costs increase the option value of waiting (Bloom, 2014; Bamieh et al., 2025). Another explanation for this result could be that the mere act of hiring new personnel could itself entail additional business risk, which a firm in an especially uncertain business environment might be reluctant to take on (Bamieh et al., 2025). The empirical literature in this field mostly focuses on the impact of macroeconomic shocks to uncertainty that affect entire countries or industries, see Bloom (2014) for a review. A clear limitation of relying on this type of variation is that it is difficult to isolate the impact of changes in a firm's own belief about uncertainty from the behavior of others or the realizations of these uncertainty shocks (Berger et al., 2020). To the best of our knowledge, the only other study using firm-level variation in uncertainty to investigate the effects of uncertainty on firm outcomes is Bamieh et al. (2025), who leverage quasi-experimental variation in business uncertainty induced by variation in trial duration for wrongful dismissal litigation in Italy. The authors find that uncertainty has a curbing effect on job turnover, hiring, and separations during the period of uncertainty, which vanishes as soon as the source of uncertainty is removed. We find similar employment effects in our setting.

Finally, we relate to a literature that utilizes death events as exogenous variation for identification. This approach was pioneered by Jones and Olken (2005), who use deaths of national leaders while in office to investigate whether changes in country leadership have

[76] For a leading seminal paper in this regard, see e.g. Bernanke (1983).

effects on economic growth. Similarly, Fadlon and Nielsen (2021) use Danish administrative data on fatal health shocks to provide evidence on their impact on households' short- and medium-run labor supply. Finally, Jäger and Heining (2022) use exogenous, death-related worker exits to examine how these exits affect firms' demand for incumbent workers and new hires. Studies on the effects of wealth transfer taxes typically face challenges due to the endogeneity of the transfer decision. A few papers implemented Instrumental Variable (IV) approaches to isolate exogeneous variation in transfers, e.g., Bennedsen et al. (2007) or Tsoutsoura (2015), who both use the gender of the donor's first-born child as an instrument for within-family firm succession. We leverage surprising deaths as exogenous changes in firm ownership to identify the causal effect of the payroll sum requirement on affected family firms.

The paper proceeds as follows. In Section 4.2, we lay out the institutional background and develop our hypotheses. Section 4.3 introduces our data sources and presents descriptive statistics. In Section 4.4, we provide our empirical strategy, while we present and discuss our results in Section 4.5. Section 4.6 concludes.

4.2 Institutional Background and Hypotheses Development

4.2.1 Institutional Background

Gratuitous Business transfers that involve a German recipient, donor or firm are subject to German wealth transfer taxes. The German transfer tax system is organized as an inheritance tax, which taxes the recipient of a wealth transfer rather than the total estate.[77] Inter vivos giving is integrated into the inheritance tax law in order to harmonize the two means of gratuitous wealth transfers. Hence, gifts and bequests *a priori* yield the same tax consequences. The tax base is the net wealth transferred to the recipient, defined as gross wealth received net of liabilities associated to the receipt. It encompasses all transfers that the recipient has received by the same donor within a ten-year period prior to the gift or

[77] This is in contrast to the estate taxes levied for example in the United States or the United Kingdom.

bequest. Applicable rates increase in wealth transfer amount and decrease in the degree of kinship to the donor (see Table C.1a in Appendix C). Importantly for our setting, business assets are always treated as transfers within the most favorable tax class I. Additionally, the amount of tax allowances also decreases in the degree of kinship (see Table C.1b in Appendix C). In case of bequests of business assets, the inheritance tax due may be deferred up to seven years with zero-rated interest.

In principle, all types of wealth are subject to the transfer tax. However, various personal and objective exemptions exist. Most prominently, certain types of assets are taxed at reduced rates, such as housing or business assets. Business assets in this definition comprise closely-held shares in limited liability companies, shares in partnerships and assets of sole proprietors as well as agricultural assets. These asset classes are eligible to the exemptions elucidated below, provided they contain predominantly productive and not so-called administrative assets. The latter category includes cash, other financial assets and rented housing. This owes to the notion that only economically productive values such as machines and production buildings should benefit from the tax exemptions. During the considered time period, administrative assets within an asset transfer are considered not eligible for the business asset exemptions, if their share in the total transferred business asset exceeds 50%.

When an individual firm owner transfers her assets gratuitously, the recipient may choose between two alternative exemption models: the regular exemption (*Regelverschonung*) or the optional exemption (*Optionsverschonung*). Both schemes differ in the generosity of the exemption granted and the requirements that must be met by the recipient of the business in the years after the transfer.

Under the regular exemption, the recipient benefits from an 85% exemption of the taxable bequest. This exemption is tied to two requirements. First, the recipient may not sell or liquidate the business over a period of five years. Second, if the company has more than 20 employees at the date of the taxable event, it must adhere to the payroll sum requirement. This requirement compares the initial payroll sum, defined as the average payroll sum over

the five years prior to the transfer, with the cumulative payroll sum over the holding period. The cumulative payroll must at least equal 400% of the initial payroll, which allows for an overall reduction in labor of 20%. If either of the requirements is violated at the end of the holding period, the exemption is proportionally re-levied depending on the degree of violation. For instance, if the cumulative payroll after five years only reaches 200% of the initial payroll sum, the exemption of 85% is reduced by 50% to 42.5%.

Under certain conditions the heir may choose the optional exemption, which increases the exemption to 100%, i.e., the tax base is fully exempt. This more generous exemption mode is however also tied to stricter requirements.[78] The period during which the business cannot be sold nor liquidated increases to seven years, and the cumulative payroll sum over this period may not fall below 700% of the initial payroll. This would require the annual payroll sum to stay at least at the level of the initial payroll sum on average. A violation of either requirement leads to the same tax consequences as for the regular exemption.

Figure 15 illustrates the tax consequences from violating either the holding or the payroll sum requirement for the two exemption models. Both examples are based on a company value of EUR 6.5 million and assume a transfer to a child of the donor subject to a EUR 400 thousand personal allowance. Figure 15a shows the total tax liability for the recipient if she sells or forecloses the business at specific points in time after the receipt. Figure 15b below shows the total tax liability based on the cumulative payroll sum at the end of the respective holding period relative to the initial payroll sum. What becomes apparent when comparing the tax consequences across the two requirements is that violating the holding requirement has a lot more bite compared to the payroll sum restriction. For example, falling short of the holding requirement by one year (20%) entails an increase in the overall tax burden of 200% for the regular exemption, whereas decreasing the payroll sum by an average of 20% entails no tax consequences at all, and only by reducing the payroll sum by an average of 40% do the same tax consequences arise.

[78] In addition to a stricter eligibility criterion of administrative assets not exceeding 10% of the overall company value.

Both holding and payroll sum requirements have been in place since 2009. The take-up rates of these exemptions for bequests across the years 2010 until 2015 varies between 71% and 75%, implying a strong attractiveness of the tax exemptions.[79] Despite adjustments in both 2009 and 2016, their basic functioning has remained the same since then. Since reductions in employment have a larger relative impact on the payroll sum for smaller firms, the payroll sum requirement was imposed only on firms with more than 20 employees in the year of transfer, whereas firms with 20 employees or less only had to comply with the holding requirement.[80]

Severe sector-specific economic downturns do not generally lead to a relaxation of the payroll sum requirements.[81] Further, various loopholes to the regulation exist.[82] However, these are only relevant for active tax planning of wealth transfers through *inter vivos* gifts. Given our focus on surprising inheritances of small and medium-sized firms with remunerated employees, such exceptions to the policy applicability do not interfere with our later analysis.

More relevant in our setting is the question whether family firms could try to manage the payroll by paying themselves or employed family members higher wages on paper to fulfill the requirement while reducing the number of employees at the same time. This concern is alleviated by strict monitoring and safeguards in place. First, compensation of managers of closely-held corporations is closely monitored by the financial offices. Once the manager holds more than 25% of shares, the compensation has to withstand an arms-length

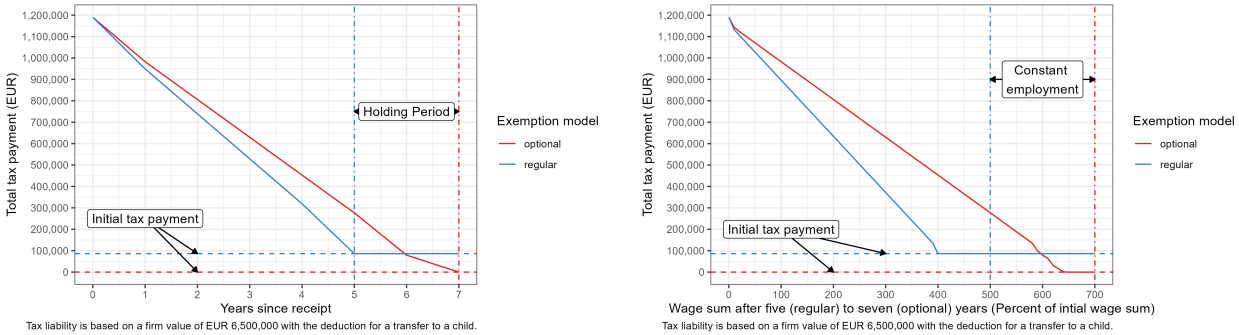
[79] Source: own calculations based on the inheritance and gift tax statistics.

[80] This headcount requirement was softened after the last major reform in 2016, where a payroll sum requirement was imposed also for firms between five to 20 employees.

[81] An exception occurred during the COVID-19-pandemic, when a government program partially replaced the wage bill of firms in sectors of the economy that were hit particularly by mandatory measures, such as store closings. Subsequently, concerns were voiced about the harsh consequences concerning the payroll sum requirement regulation, as total payroll sums paid by firms substantially decreased during this period. As a result, the payroll sum requirement was relaxed from 1.3.2020 until 30.6.2022.

[82] First, firms without any employees can still fully benefit from tax exemptions, without any need to contribute to the positive externalities of providing employment. Second, until 2013 (larger) firms with more than one dependency could avoid the payroll sum requirements through holding companies. Due to a lack of legal clarification, payroll sum requirements only targeted the employees in the holding company itself. The payroll sum in the subsidiaries was disregarded. Third, temporary workers are excluded from the payroll sum definition, creating incentives for maintaining flexible shares of non-permanent labor force under the constraint of labor laws.

Figure 15: Example Tax Consequences



(a) Violation of holding requirement

(b) Violation of payroll sum requirement

Note: For the calculation of effective tax rates, we assume transferred business assets to have a value of EUR 6.5 million. The personal deduction for a child of the donor is EUR 400,000. The tax liability calculation abstracts from additional personal circumstances of the recipient and assumes that the overall transfer is fully eligible for the regular and optional tax exemption.

comparison with the earnings of other managing directors in comparable positions. If this test fails, the manager is at risk to be liable to a back taxes, as the excess compensation is deemed as a hidden profit distribution, which is subject to CIT and LBT. Second, similar safeguards are in place for employment of close family members. Employment contracts have to fulfill certain requirements in order to qualify as dependent employment (instead of co-entrepreneurship). Among other things, these contracts also have to satisfy an arms-length principle. If this test fails and the illegitimate arrangement is detected, any accrued entitlement to social security benefits is revoked and potentially back taxes are levied. Hence, trying to circumvent the payroll sum requirement through family employment arrangements is subject to hefty financial penalties and therefore not an attractive option.

4.2.2 Hypothesis Development

In this section, we develop our hypotheses on how the payroll sum might affect firm outcomes. Firms that fail to meet the payroll sum thresholds face significant financial penalties proportional to the reduction in labor input when the inheritance tax is reapplied, effectively increasing costs of layoffs, wage cuts, and voluntary separations. The extent to which these

adjustment costs actually impact firms depends on their development during the holding period and the uncertainty associated with employment expectations. If the firm subject to the requirement is on a positive employment trend prior to the succession, the requirement is likely to be immaterial for two reasons. First, the initial average payroll, based on which the cumulative payroll is evaluated, is lower than the current payroll, which gives the company some leeway in adjusting employment downward. Second, to the extent that the positive growth continues after succession, the firm would actually increase its labor input, which is not affected by a downward restriction.

In contrast, for firms with a declining trend in employment (e.g., during a recession), the payroll sum requirement is likely to be binding. Such firms may want to *reduce* employment without the requirement. Under these circumstances, firms subject to the payroll sum requirement are expected to maintain higher payroll sums during the holding period than comparable firms not required to maintain a certain level of employment. This artificial stabilization is expected to vanish once the holding period ends, as treated firms are then free to optimize labor input.

We visualize the outcome trajectory for treated and control units in the recession scenario in Figure 16a. The graph shows the actual and counterfactual outcome evolution of treated and control firms around the succession event under the assumption that the optional exemption was chosen. In this case, the average payroll sum over seven years is not allowed to drop below the initial payroll sum. When the firm is on a declining trend, that means that it actually has to increase employment relative to the year before the succession during the holding period to fulfill the requirement. After the holding period, the firm is then free to adjust its labor input to the optimal level, which would entail a substantial decrease in employment. As the descriptive results of Section 4.5 will show, it is unlikely that during our period of study any firm in the sample was facing such circumstances. During the period 2010 to 2015, economic growth was generally positive in Germany and therefore declining employment trends such as during the COVID-19 pandemic were not prevalent.

However, in addition to these direct effects, the payroll sum requirement may influence firms even in the absence of a recession. Incoming owners, particularly those with limited experience, may adopt conservative labor strategies due to the requirement, as firing costs increase the option value of waiting (Bloom, 2014; Bamieh et al., 2025). In addition, the mere act of hiring new personnel could itself entail additional business risk, which a firm in an especially uncertain business environment might be reluctant to take on (Bamieh et al., 2025). Thus, this tendency is likely to be more pronounced in cases of unexpected ownership transitions, such as those resulting from the sudden death of a predecessor. In such scenarios, inexperienced owners might avoid taking risks in order to avoid potential tax penalties, leading to slower employment growth. This mechanism would give rise to the dynamic effects illustrated in Figure 16b. We formalize this mechanism in the following hypothesis.

Hypothesis 1. *The payroll in firms that experience a surprising ownership transition due to an early death of the previous owner grows slower when subject to the payroll sum requirement.*

To the extent that labor and capital are substitutable, we expect the free resources not used for an expansion of labor to be used for capital investment instead. This is due to the expected tax penalty increasing the cost of labor relative to the user cost of capital, which gives rise to substitution effects (Caballero and Hammour, 1998). This prediction would also be in line with empirical evidence on the effects of employment protection on capital investment (Autor et al., 2007; Cingano et al., 2016). Furthermore, unlike labor, which represents an ongoing expense and commitment, capital investments (e.g., machinery, equipment, or real estate) are tangible assets that can be resold if the firm is liquidated. If the heir is uncertain about their long-term commitment to the business, they may see capital investments as a way to preserve or even enhance the firm's liquidation value. Based on these considerations, we formulate the following prediction regarding the effect of the payroll sum requirement on investment.

Hypothesis 2. *Capital investment in firms that experience a surprising ownership transition due to an early death of the previous owner increases during the holding period when subject to the payroll sum requirement.*

These opposite effects of the payroll sum regulation on adjustment of capital and labor are driven by the elasticity of substitution between capital and labor. The more easily a firm can invest into new machinery to replace labor (higher elasticity of substitution), the stronger the positive effect on investment and the stronger the negative effect on employment growth following an increase in labor adjustment costs.

However, financially constrained firms, whose ability to acquire capital instead of labor is limited, might exhibit a lower effective elasticity of substitution. This would result in more attenuated responses to such regulations - these firms neither reduce employment nor increase capital investment as strongly as their financially healthy counterparts. Against this background, we consider heterogeneity in responses due to differing financial conditions as a potential explanation why identical labor market policies can have varying effects across firms and sectors. Our third hypothesis hence becomes:

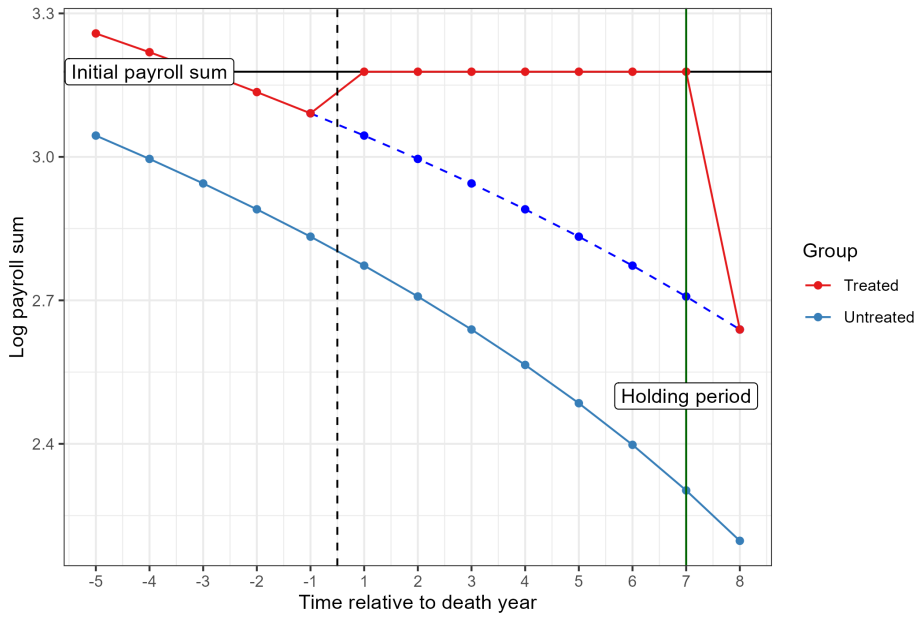
Hypothesis 3. *Capital investment (employment) in treated firms with limited access to external financial funds prior to ownership transition increases (decreases) less than capital investment (employment) in treated firms without financial constraints.*

4.3 Data and Descriptive Statistics

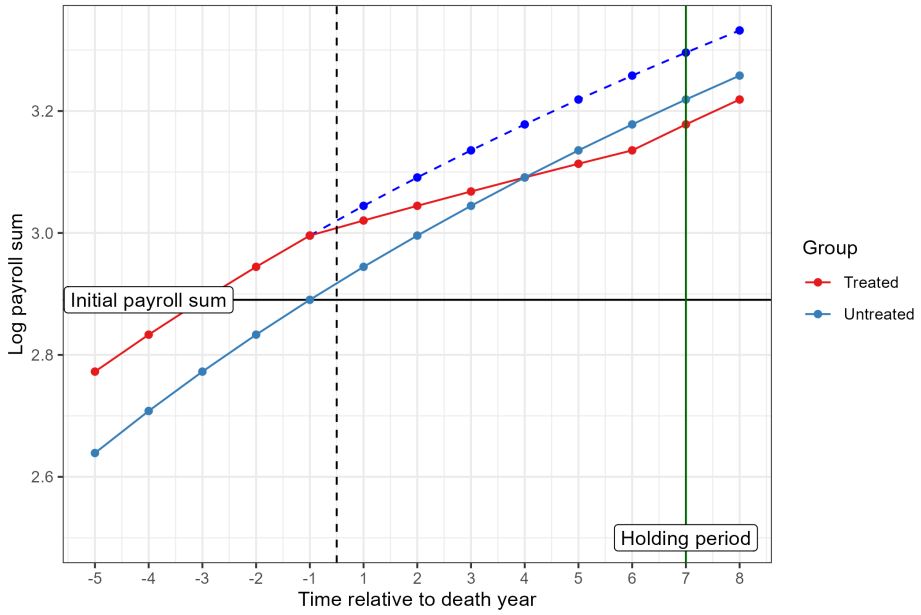
4.3.1 Data sources

Our analysis builds on information from a variety of data sources, which we describe in the following. Our starting point is firm ownership information from BvD's Orbis data base. We focus on changes in the global ultimate owner (GUO), defined as an individual with a controlling influence of more than 25%, to identify changes of firm ownership during the

Figure 16: Illustration of hypothesized effects.



(a) Recession scenario



(b) Uncertainty scenario

Note: Figure 16 shows the potential development of treatment and control firms in two distinct scenarios. In the first scenario (Figure 16a), both firms are on declining employment trends before the succession, but one firm needs to maintain a sub-optimally high payroll sum. In the second scenario (Figure 16b), both firms are on the same positive employment growth path prior to succession, but the firm facing the restriction becomes more conservative in its employment strategy during the holding period.

period 2007 to 2022. In addition, we gather information on firm characteristics and financial data.

In order to identify changes in firm ownership that are due to an inheritance, we retrieve death information from three different data sources.[83] First, we use death notice data provided by the CompGen family announcements project.[84] CompGen is a non-profit and non-governmental association for computer genealogy, which hosts several projects related to researching ancestors and family history. The data for the death notice project is collected by volunteers browsing through the obituaries sections of local newspapers and digitalizing the information on the deceased in a comprehensive database. Figure 17a illustrates the spatial coverage of this data and indicates that most of Germany is well covered in the database, with a higher concentration of observations in the West and South. Figure 17c shows the time coverage of the death notice database from 2000 to 2022. Over our period of interest, the coverage is on a declining trend, starting from about 110 thousand observations in 2010 to about 65 thousand in 2015. Overall, the data holds death records for about 556 thousand individuals in that time window.

Second, we employ digitalized tombstone data provided by the CompGen tombstone project. In order to preserve the information on the deceased, voluntary contributors take pictures of all tombstones on a given graveyard and digitalize the inscriptions in a database. The included information generally comprises the full name as well as the date of birth and death. Figure 17b shows the distribution of inscriptions aggregated on the zip code level. As is apparent from the figure, the spatial distribution of the tombstone database is less dispersed than the death notice database. Its coverage is particularly high in the north and east of Germany, whereas it is only sporadic in the southeast. Our period of interest covers recorded deaths of 595,977 individuals. Similarly to the death notice database, the number of observations decreases during our period of interest, albeit to a smaller extent. The number

[83] More extensive detail on data collection and preparation is provided in Appendix C.2

[84] Data by the *CompGen-Projekt Familienanzeigen in Tageszeitungen* available at `http://familienanzeigen.genealogy.net/`

of deaths steadily decreases from a total of 105 thousand in 2010 to about 95 thousand in 2015.

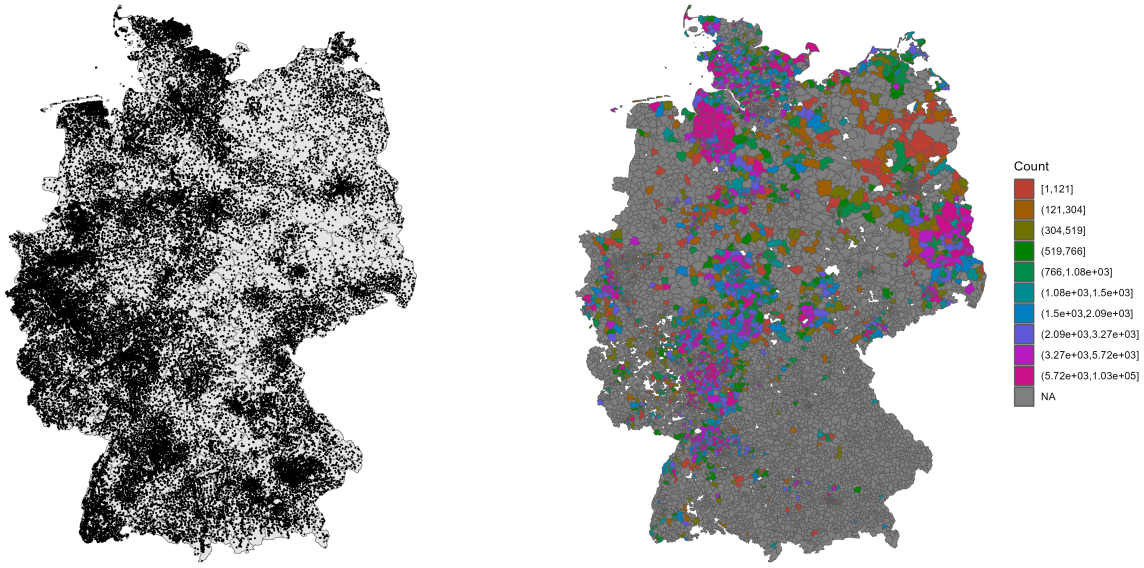
Third, we augment the obituary information of the death notice data base by web scraping the obituaries sections of a total of 46 regional newspapers.[85] Using optical character recognition trained on German texts, we recover a total of 1,321,583 obituaries. Figure 17c shows the number of obituaries by death year over time, illustrating that coverage improves for our scraped newspapers as we move closer to the present, which counteracts the declining trend in the other two sources. The upward trend starts to pick up with the onset of our period of interest, where we observe about 10 thousand recorded deaths. At the end of our sample period in 2015, coverage increased to more than 60,000, surpassing the CompGen death notice database by the next year.

Figure 17d compares the inter-temporal coverage of our data to the official data on recorded deaths by the German Federal Statistical Office. Due to the opposing trends in coverage between our different data sources, we attain a relatively constant set of about 200 thousand death records per year, which constitutes roughly a quarter of the overall number of deaths in Germany. Given the nature of our data sources, we expect business owners to be over-represented in the available death records. For the death notice data, we deem it more likely that a departed business owner receives an obituary posted by either his or her family or the company. For the tombstone data on the other hand, large graveyards in bigger cities are better covered, which is where most economic activity takes place.

Finally, to enable the analysis of labor outcomes of the inheritance tax policy, we require reliable and detailed firm-level data on wages and employees. Firm databases containing firms' financial accounts cannot reliably provide such information reliably, as especially small and medium-sized firms in Germany are not required to report these numbers in their external accounting. To overcome this challenge, we complement our firm-level dataset with administrative information on establishment-level employment and wages provided by

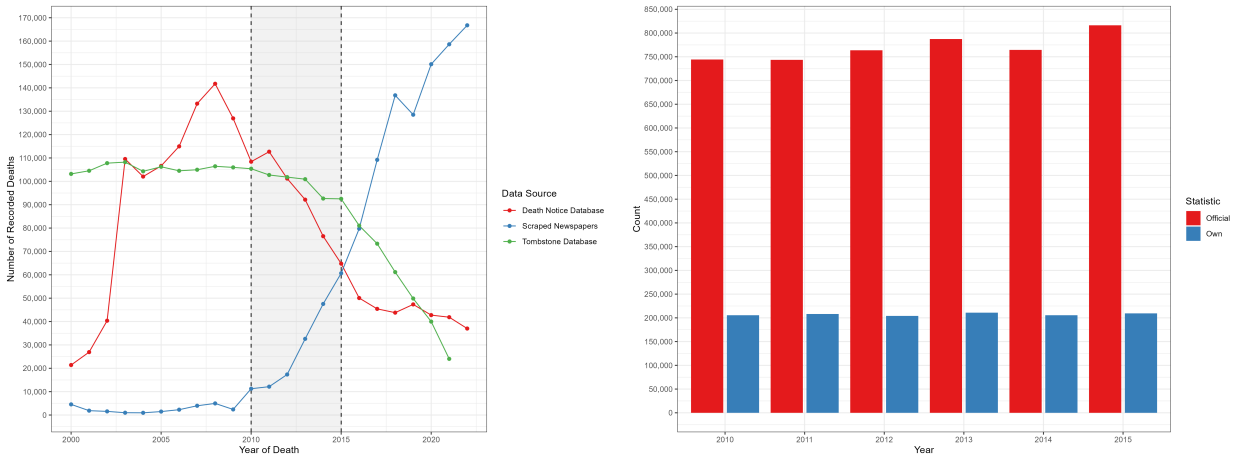
[85] List of newspapers available upon request from the authors.

Figure 17: Spatial and inter-temporal coverage of death data



(a) Spatial coverage of death notice data.

(b) Spatial coverage of tombstone data.



(c) Coverage over time.

(d) Comparison with official statistics.

Note: Figure 17 shows the spatial and absolute coverage of our different death data sources. Figure 17a shows the spatial coverage of recorded newspaper obituaries across Germany. Figure 17b shows the spatial coverage of the Tombstone data. As we only have information about the zip code a graveyard is located in, observations are aggregated on the zip code level. Figure 17c shows the intertemporal coverage of each data source. Figure 17d compares the count of death events in our three databases to the official death statistics provided by the German Federal Statistical Office (Destatis).

the Research Institute of the Federal Employment Agency (*Institut für Arbeitsmarkt- und Berufsforschung, IAB*). We use a project-specific sample based on a link of the Mannheim Enterprise Panel to the Establishment History Panel.[86] These data include the number of full-time and part-time employees and their compensation at the level of the establishment.[87]. This excludes temporary and seasonal workers, which coincides with the tax policy definition we consider. Employee compensation is measured as the (imputed) sum of all wages.[88]

4.3.2 Sample Selection

The sample selection process is summarized in Table 16. Our starting point is the set of first-level ownership links between a German company and another entity covered by Orbis, augmented by information on the global ultimate owners with at least 25% total ownership share. From 2007 until 2023, these data include about 65 million links involving 5 million unique firms and 6 million owners. From this initial sample, we keep firms with less than five owners at a time, reducing the number of firms and individuals slightly. Next, we only retain firms with at least one substantial ownership change over the observed period. That is, we retain firms with a first-level shareholding change of at least 25% or a change in the GUO. After these pre-selection steps we are left with 817 thousand firms belonging to 1.04 million individuals between 2007 and 2022.

At this point, an observed exit of an individual GUO could be due to a variety of reasons. For instance, the owner could have sold their share in the company, or transfer their share via a gift or inheritance to another individual. In order to isolate exogenous changes in firm ownership due to an inheritance, we match the ownership data against the death data

[86] The resulting data product is called *Projektspezifische Stichprobe aus dem Mannheimer Unternehmenspanel verknüpft mit administrativen Daten des IAB.*. For more details on the linkage process underlying the administrative data see Diegmann et al. (2024).

[87] A business number, centrally assigned by the federal employment agency, defines an establishment. This definition moves the level of observation down, from the firm-level to the actual place of production. However, multiple places of production may share a common business number if, for instance, they perform a similar task within the company and are located within the same municipality.

[88] The underlying database originated from reports of social security contributions, which are capped at approximately 160% of the average gross wage. The imputation procedure is described by Drechsler et al. (2023).

Table 16: Sample selection

Description	Observations	Establishments	Firms	Individuals
All first-level shareholder links of German firms	64,867,881		5,238,125	6,303,703
Less than 5 owners in a year	53,177,761		5,077,543	5,539,569
Collapse to firm-year panel	37,031,115		5,077,543	3,799,643
At least one substantial ownership change	10,043,190		817,074	1,044,849
Add financial Years 2005 and 2006	10,506,534		817,074	1,044,849
Matched with death records	696,477		47,147	38,853
Merged to yearly establishment-level IAB data	602,317	41,219	31,418	29,024
Keep unique or local name matches during 2010-2015	152,063	13,779	10,495	9,826
Keep firm when incoming/outgoing owner share family name	60,963	5,393	4,208	3,949
Keep non-exiting establishments	40,122	3,455	2,945	2,792
Single-owner-firm-establishments with 10-30 pre-death employees	7,237	611	611	611
Owner is younger than 70 at death	1,733	165	165	165

Note: Table 16 shows the sample selection process from the Orbis ownership data until our match with the IAB data. We start with all ownership links from a German individual to a company during the available period from 2007 until 2022.

sources introduced in Section 4.3.1. A death record observed in the death data is matched with a GUO exit if the records share the same first name and last name. Additionally, we allow for a lag of up to one year between the recorded death and the observed change in ownership in Orbis.[89]

During the period between 2007 and 2022, we match a total of 38 thousand individuals based on their last name, first name, and year of death, owning a total of 47 thousand firms. We subsequently merge this data to the establishment-level administrative employment data of the IAB. We are able to match about 67% of Orbis firms with a death record to the IAB data. While this reduces the number of firms to 31 thousand, we now observe a total of 41 thousand establishments of these firms.

We implement several steps to ensure that we correctly identify an ownership transition that is due to an inheritance and minimize any potential attenuation bias due to false positive matches.[90] As metrics for our baseline restriction, we use the number of matches as well as the headquarter location of the firm and the coverage area of our death sources. For the CompGen death notice data, which stems from newspaper archives, we geo-reference any location we find in the death record for each distinct newspaper.[91] Subsequently, we calculate the centroid of the mentioned locations and create a buffer zone based on the 80th percentile of the distance between each location and the centroid. This way, we obtain a coverage area for each newspaper, which is not sensitive to outliers. As we do not directly observe the location of an obituary in our scraped newspaper data, we use the coverage area for the newspapers identified from the CompGen death notice data also for our scraped death records. For the tombstone data on the other hand, we calculate the coverage area of a graveyard based on the zip code of the grave yard combined with a fixed buffer zone of

[89] In the Orbis ownership data, an individual is identified as a GUO in that year, if the information provider confirmed the ownership relation within the year of observation. Hence, it could be that a GUO was confirmed in that year and died in the same year, but the new incoming GUO was only confirmed in the following year.

[90] As we rely on a combination of last name, first name, and the year of ownership change vis-a-vis a death year, a chance match where these margins correspond for two different persons by coincidence could occur.

[91] This most frequently refers to the place of death. Alternatively, it includes place of last residence, place of funeral, place service, or place of birth.

300 km around the graveyard location.

Figure 18 provides an illustration of the match quality in the data and our first validation of match quality based on the number of matches and coverage area of our death records. The figure shows the number of individuals with the same combination of last name, first name, and death year, that are matched to a GUO exit in Orbis on the x-axis, and the number of occurrences on the y-axis. For the vast majority of cases, we either have a unique match within a given database or find at least one match that is in the coverage area of the respective death source.[92] As our baseline restriction, we omit all ownership transitions with a duplicate match or where neither of the matches is inside the coverage area of the newspaper or graveyard.

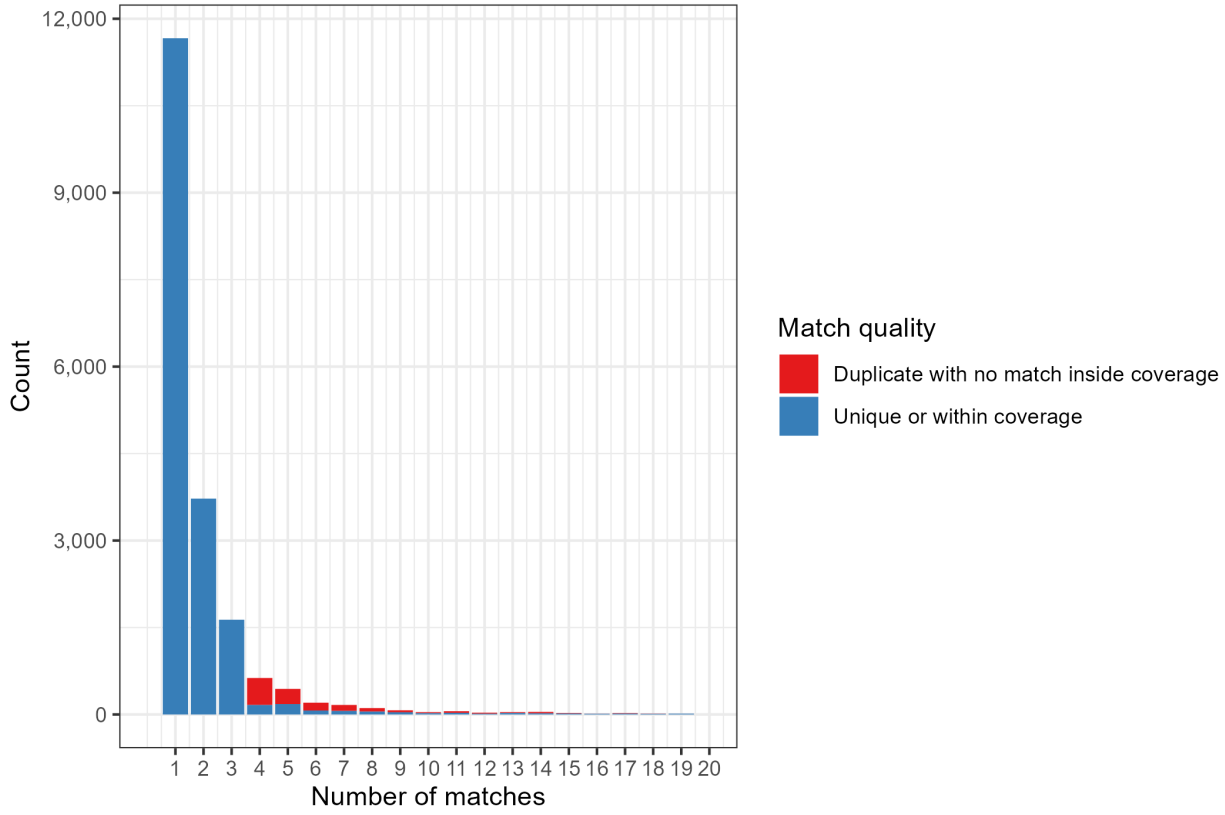
After restricting the sample further to our period of interest between 2010 and 2015, we are left with 14 thousand establishments of 10.5 thousand firms belonging to 9.8 thousand individuals. In order to further sharpen the identification, we implement several additional restrictions to isolate unexpected inheritances. First, we require the new incoming GUO to share the same last name as the deceased GUO, which is true for about half of the remaining sample (4,208 firms).[93] Second, we focus on non-exiting establishments, which leaves us with 3,455 establishments of 2,945 firms. Third, we consider single-owner-establishment firms with more than 10 and less than 30 employees in the year before the succession, which includes 611 firms. Finally, we only retain cases where the departed was less than 70 years old at the time of death.[94] This leaves us with a final sample of 165 firms.

[92] Note that in principle, a given death could be recorded multiple times. We might have a death notice from the CompGen death notice database, which was also retrieved from our scraper of the newspapers website. Additionally, we might have an observation for this individual from the tombstone data, when she was buried on a graveyard covered by the CompGen tombstone database.

[93] This restriction effectively trades off more credible identification of inheritances, as most inheritances occur within family, for coverage of female heirs, who are more likely to change their last name after marriage.

[94] This restriction enables us to focus on surprising changes in ownership and control. At 70 years, a German male can expect to live for another 14 years, while the life expectancy of a German female is even 17 years, which is considerably longer than the expected time to pass on a business to the next generation. The figures are taken from the mortality tables published by the German Federal Statistical Office (Destatis) for the years 2010 to 2015.

Figure 18: Number of Matches



Note: Figure 18 illustrates the number of matches between an individual shareholder exiting the sample with a deceased individual from our death data. The horizontal axis shows the number of matches with the cumulative proportion of observations in parentheses. Counts colored in blue indicate that the match was either unique across all data or unique within one database, whereas red counts indicate duplicate matches within a data base.

4.3.3 Descriptive Statistics

Table 18 shows descriptive statistics for our regression sample. The table compares key firm characteristics for treatment and control firms in the year prior to succession. Firms above and below the relevant size threshold of 20 employees are similar in most aspects but not identical. We (naturally) see differences in employee count and the payroll sum, as group assignment depends on these variables. On average, control firms employ 14 employees and have a payroll sum of EUR 864 thousand, compared to an average of 25 employees and a payroll sum of EUR 1.6 million among treated firms. Apart from these differences, treatment and control firms are remarkably similar. At death, the previous owners are on average 60 years old and the firm existed for 30 years in both treatment and control group. Total assets are with an average of EUR 1.3 million and EUR 1.9 million not identical but also not significantly different from each other.

Table 18: Descriptive Statistics and Difference-in-Means Test

	Control Group (10-20)		Treatment Group (21-30)		Difference-in-Means	
	Mean	Count	Mean	Count	Difference	t-Statistic
Age at death	60.44	128	60.78	37	-0.346	-0.25
Firm age	30.97	122	29.22	36	1.745	0.41
Total assets	1237.90	104	1856.58	31	-618.677	-1.55
Leverage	0.70	104	0.75	31	-0.049	-0.83
Investment	0.08	102	0.05	28	0.025	0.37
Cash Share	0.17	102	0.16	31	0.009	0.26
Employees	14.09	128	25.03	37	-10.933***	-19.61
Payroll sum	864.46	128	1647.11	37	-782.654***	-9.02
Median wage	81.17	125	79.09	37	2.081	0.56
Mean wage	83.59	125	83.45	37	0.148	0.04

Note: Table 18 shows descriptive statistics of our final dataset. The depicted averages and observation counts are split up by employee-size class (10-20 employees for the control group and 21-30 for the treatment group) and measured in the year before the succession. The two rightmost columns show differences in means between the two groups for the respective variables as well as t-statistics for the differences. Imbalances for the number of employees and payroll are by construction, as the assignment to the treatment and control group depends on these variables.

4.4 Empirical Strategy

Our identification strategy centers on the feature of the inheritance tax code that ownership transfers of establishments with more than 20 employees are subject to the payroll sum regulation, while ownership transfers of firms with less than 20 employees are not. We estimate the effect of the payroll sum restriction on affected firms using a cohort-based difference-in-differences design. Thereby, we compare two companies that experienced an inheritance event in the same year (cohort), where one of these companies has more than 20 employees and is therefore liable to the payroll sum requirement, and the other has less than 20 employees and is therefore out of scope.[95] Using this approach, our estimand is the intended treatment effect on the treated, as we cannot be certain whether the preferential treatment provisions were actually invoked, i.e., whether the firm actually took up the policy, and if yes, which mode of exemption was utilized.

In order to estimate treatment effects based on valid comparisons only, we employ a stacked regression approach. This approach addresses concerns related to staggered-adoption difference-in-differences designs that were raised by recent literature (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Baker et al., 2022). The stacked regression approach involves setting up separate datasets for each clean 2×2 treatment-comparison group. In our case, this boils down to a separate dataset for each event year cohort, that is, for transfers of ownership in each year from 2010 to 2015. The data is then stacked on top of each other and treatment effects are estimated from this stacked dataset with dataset-specific unit and time fixed effects using the following OLS regression (Cengiz et al., 2019; Baker et al., 2022):

$$y_{ijte} = \alpha + \beta_1 \text{Treatment}_{ie} \times \text{Post}_{te} + \gamma_{jt} + \mu_i + \delta_{te} + \varepsilon_{ite}, \tag{7}$$

[95] An obvious concern would be that the applicable employee size threshold underlying the payroll sum requirements also coincide with other relevant size-varying employment regulations. To the best of our knowledge, this is not the case in our setting. For instance, German protection against dismissals becomes mandatory with firms above 10 employees. Similarly, the implementation of work councils takes place for firms with at least 5 employees.

where i refers to the individual firm, j to the industry, t to the year of observation and e to the event cohort. As outcome variables, we consider the (logarithm of the) payroll sum, the (logarithm of the) number of employees, the logarithm of the median and mean wage, and the percentage change in fixed assets. In our baseline specification, we allow for industry-specific trends γ_{jt} and include unit-cohort fixed effects (which reduce to unit fixed effects) μ_i as well as time-cohort fixed effects δ_{te}. To investigate differential effects during and after the required holding period, we adjust Equation 7 by splitting the post indicator into a short-run (up to five years after the taxable event) and a long-run (at least 6 years after the taxable event).

We accommodate the non-negative nature of our outcomes and the fact that the number of employees is a count variable by also estimating the equivalent specifications using a Poisson Pseudo Maximum Likelihood (PPML) model:

$$E[y_{ijte}|X] = \exp\left\{\alpha + \beta_1 \text{Treatment}_{ie} \times \text{Post}_{te} + \gamma_{jt} + \mu_i + \delta_{te}\right\}, \tag{8}$$

where X denotes the set of control variables. Since our outcome variables are likely non-linear, PPML regressions are particularly well suited in this setting (see e.g., Wooldridge, 2023) and serve as a robustness check for functional form assumptions.

We check for pre-trends and dynamic effects by estimating an event study specification that includes leads and lags of the treatment indicator:

$$y_{ijte} = \alpha + \sum_{l=-4, l\neq -1}^{7} \beta_l \; D_{ie,t-l} + \gamma_{jt} + \mu_i + \delta_{te} + \varepsilon_{ite} \tag{9}$$

Again, we estimate this specification using OLS and the PPML equivalent:

$$E[y_{ijte}|X] = \exp\left\{\alpha + \sum_{l=-4, l\neq -1}^{7} \beta_l \; D_{ie,t-l} + \gamma_{jt} + \mu_i + \delta_{te}\right\}. \tag{10}$$

Significant coefficient estimates for the lead terms, $l = -4, \ldots, -2$ would cast doubt on

the parallel trends assumption, whereas the coefficients on treatment lags, $l \geq 0$ allow us to gauge the dynamic effect of the payroll sum.

4.5 Results

4.5.1 Labor and Investment Effects

We begin by investigating the effect of the payroll sum requirement on the payroll sum itself. Column (1) of Table 19 shows the results of estimating Equation 7 with the natural logarithm of the payroll sum as the dependent variable, while Column (2) depicts the results when using a Poisson model instead. We report the transformed coefficient estimates $\tilde{\beta} = \exp(\hat{\beta}) - 1$, with robust standard errors in parentheses such that the effects have a percentage change interpretation. We find that over a period of up to 8 years after the ownership transition, the payroll sum requirement has a negative effect on the overall payroll sum. On average, the payroll sum decreases by 11 to 14% in the treatment group relative to the control group.

We dissect this result by looking at whether this overall effect on the payroll is driven by the number of employees or wage setting. Columns (3) and (4) of Table 19 show results of estimating Equation 7 and the Poisson equivalent of 8 when the dependent variable is the (log of) number of employees, whereas Columns (5) and (6) show treatment effects for the log specification for median and mean wages. We find negative effects in most specifications, suggesting that both hiring and firing as well as wage-setting decisions are affected by the requirement.

As our hypotheses postulate dynamic effects that differ in the short- and long-run, we estimate a variation of our static baseline specification where we split the post dummy into a short-term (1-5 years after the event) and a long-term effect (6-8 years). The results are shown in Table 20 and suggest that the treatment effect increases dynamically over time, from a short-term effect of seven to 10% during the first five years of the requirement reaching 19 to 22% after eight years. This pattern is present both for the number of employees and median wages, however, the negative effect is much more substantial for the number of

Table 19: Effect of the payroll sum requirement.

	Payroll		Employment		Wages		Invest.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat x Post	-0.111	-0.142**	-0.121**	-0.09	-0.06**	-0.052**	0.07
	(0.076)	(0.068)	(0.052)	(0.057)	(0.027)	(0.025)	(0.233)
Ind.-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-years	1,733	1,733	1,733	1,733	1,696	1,696	1,296
Adj. R2	0.821	0.853	0.316	0.671	0.869	0.874	0.747

Note: Table 19 shows results for regressions of our dependent variables on our main treatment indicator. For the payroll sum and employment we present estimations from OLS estimations of the logged outcome in (1) and (3), and Poisson regressions in (2) and (4). For wages we consider the log of the median (5) and mean (6) wage for a given establishment. The investment regression in (7) considers the log of fixed assets. All specifications include the full set of fixed effects. *** p < 0.01, ** p < 0.05, * p < 0.1.

employees.

In order to speak to the nature of the effect, i.e., whether we see an actual decrease in the level of our outcomes or rather a hampered growth of treated firms, we investigate the raw outcome evolution for both treated and control firms. Figure 19 shows the average development of treated and control firms across the four outcome variables of interest around the event. We show differences in outcomes with respect to the year prior to the event. As the raw averages are not taking the fixed effects structure into account, slight deviations in trends are to be expected. For example, the industry composition is likely different between the treatment and control group. Nevertheless, the raw trends indicate that before the succession event, both treatment and control firms are on a positive growth path. After the succession, however, growth in the treatment group seems to decline relative to the control group, which is noticeable both in the evolution of the payroll as well as the number of employees, where the treatment group actually starts to reduce employment slightly over the medium term, whereas the control group continues its positive growth after about five years.

We check the validity of our parallel trends assumption and investigate dynamic effects

Table 20: Short-term and long-term effects.

	Payroll		Employment		Wages		Invest.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat x Post(1-5)	-0.069	-0.1*	-0.08*	-0.055	-0.05**	-0.041*	0.105
	(0.062)	(0.056)	(0.042)	(0.047)	(0.025)	(0.022)	(0.221)
Treat x Post(6-8)	-0.189*	-0.215**	-0.197**	-0.153*	-0.08**	-0.073**	0.006
	(0.114)	(0.1)	(0.083)	(0.09)	(0.034)	(0.032)	(0.326)
Ind.-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-years	1,733	1,733	1,733	1,733	1,696	1,696	1,296
Adj. R2	0.822	0.854	0.317	0.673	0.869	0.875	0.747

Note: Table 20 shows results for regressions of our dependent variables on separate indicators for the first five years and following three years of treatment. For the payroll sum and employment we present estimations from OLS estimations of the logged outcome in (1) and (3), and Poisson regressions in (2) and (4). For wages we consider the median (5) and mean (6) for a given establishment. The investment regression in (7) considers the log of fixed assets. All specifications include the full set of fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

using the event study design of Equation 9. The results of this approach are shown in Figure 20. The flat pre-trends in the four years prior to the succession suggest that, conditional on our fixed effects structure, there is no significant difference in the outcome evolution of our treated and control units prior to succession. In particular, we do not find evidence for managing the headcount in anticipation of an ownership transfer in order to either fall below the required 20 employees (leading to the firm not being subject to the requirement at all) or easing the requirement due to a lower initial payroll sum.[96] However, we observe a dynamically building negative treatment effect in the years after the payroll sum requirement becomes active. The pattern is similar for the payroll sum itself as well as the number of employees, with a rather muted response of median wages. We find no evidence for a substantial effect on investment, suggesting that labor is not substituted for capital in our main sample. This suggests that the saved labor costs are either kept as reserves or are paid out instead.

[96] Figure C.1 in Appendix C shows that there is no evidence for firms bunching at the threshold of 20 employees in the data.

The pre-succession outcome trajectories suggest that, on average, our sample firms are on positive employment trends prior to succession. Since the payroll sum requirement was not necessarily binding, we are unable to assess potential crisis-exacerbating effects. However, we find evidence supporting Hypothesis 1 suggesting that uncertainty regarding future tax obligations inhibits employment growth. This effect is particularly pronounced in our preferred specification of surprising ownership transitions.[97] Concerning Hypothesis 2, i.e. the effect of the payroll sum regulation on investment, we do not identify a significant effect in our main specification. The point estimates suggest a slightly positive effect, which is concentrated during the holding period as indicated by Column (7) of Table 20. When we zoom in closer to the threshold of 20 employees by considering control firms in the range of 15 to 20 employees (cf. Section 4.5.2 below and Figure C.3 in Appendix C), we find that the policy had a temporary positive investment effect. Due to the limited sample size for this particular subset, we cannot conclusively say whether this finding is due to statistical outliers or improved comparability of the control group with respect to investment trends. Therefore, we interpret this result with caution.

Our findings are consistent with prior literature that examines the role of dismissal costs and uncertainty on employment, which generally finds that after an increase in dismissal costs, firms tend to hire fewer new workers due to the anticipated costs of downward adjustment (Autor et al., 2007; Kugler and Pica, 2008; Sestito and Viviano, 2018) and potential increase in business risk by hiring new staff (Bamieh et al., 2025). As a failure to maintain employment levels above the pre-succession average effectively raises the cost of employee separation, we document similar effects in our setting. Unfortunately, the time period covered by our panel does not allow us to investigate long-run effects. Therefore, we cannot conclusively speak to whether treated firms catch up in the long run. We find suggestive evidence that the treatment effect indeed vanishes over time, as the dynamic treatment effect is no longer significantly different from zero by the last relative period, which corresponds

[97] In untabulated tests, we repeated the analysis for the full sample of identified death cases without restricting the age of the deceased owner at death. The effect is considerably weaker in the overall sample.

to the first year after the holding period of the optional exemption.

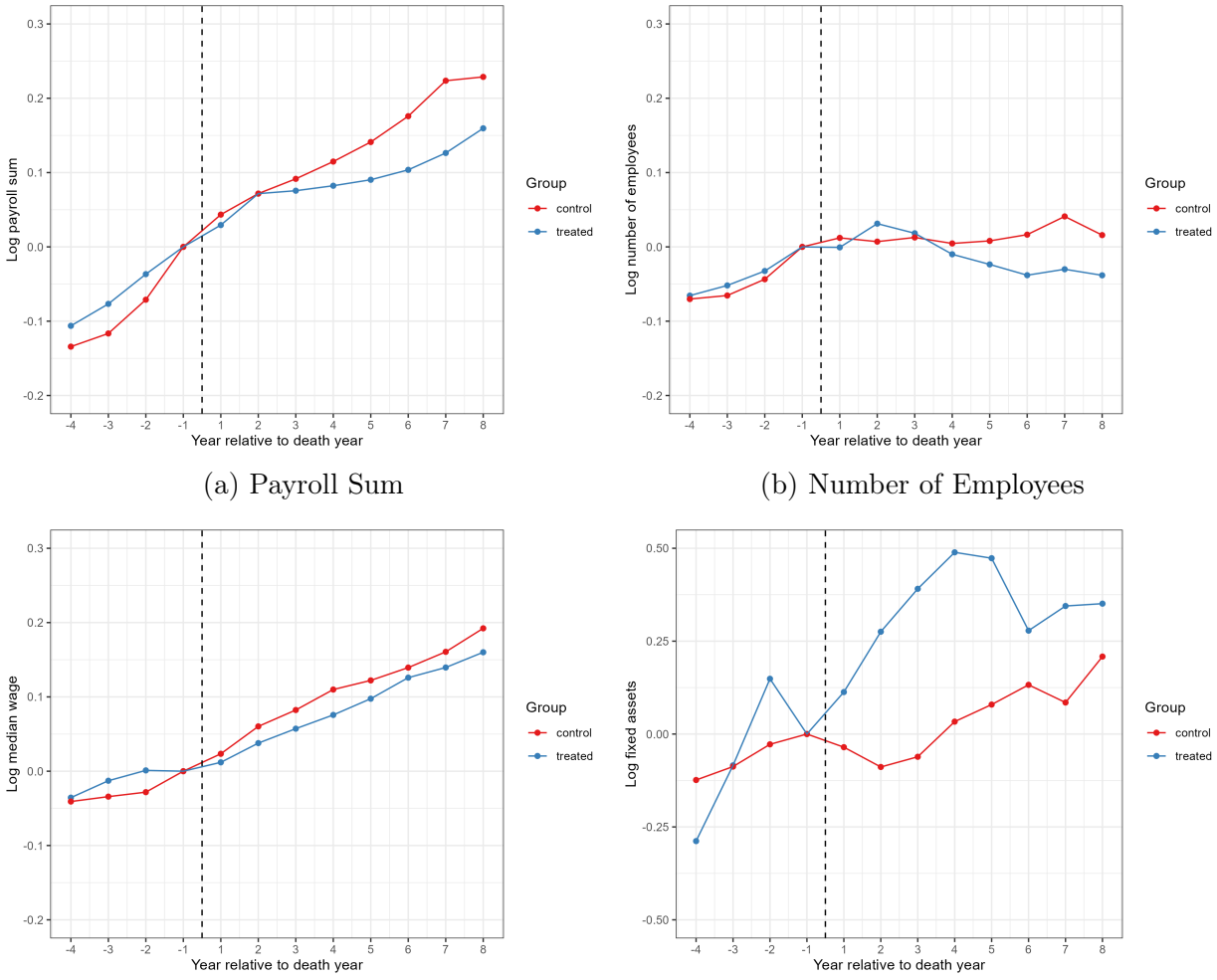
In a closely related study, Akcigit et al. (2023) examine the effects of employment maintenance requirements on firm growth, but they find markedly different outcomes. Their analysis focuses on a policy implemented during the privatization of East German firms after reunification, which mandated that new owners commit to employment targets, with penalties for non-compliance. Unlike our findings, they document a 22 percentage point higher annual employment growth rate for firms subject to binding requirements, alongside a 3.6 percentage point increase in the likelihood of firm exit.

These contrasting results likely stem from differences in policy design and implementation. In our setting, firms and their heirs were assigned to the policy exogenously, whereas in the East German case, individuals self-selected into the policy, and employment targets were negotiated individually at the firm level. Furthermore, in our setting, most firms were not directly constrained by the policy, which may further explain the divergence in outcomes.

4.5.2 Robustness Tests

The biggest threat to our identification strategy is arguably the size-dependent threshold of the policy. By construction, this threshold leads to imbalances between our treatment and control group, which could induce bias driven by size-specific trends post treatment. Even though there is early empirical evidence that smaller firms create more jobs compared to larger firms, thereby violating Gibrat's law (Birch, 1981; Birch, 1987), more recent studies have shown that these differences vanish once the age of the firm is controlled for (Haltiwanger et al., 2013; Criscuolo et al., 2014; Franke et al., 2016). As the firms in both our treatment and control group tend to be mature companies with an average age of 30 years, differential size trends violating Gibrat's law are unlikely to be present in our setting. However, in spite of this evidence and even though we restrict our treatment and control group to a narrow range of 10 employees above and below the threshold in the year prior to the taxable event, the possibility of this form of bias remains. Thus, we implement two tests that address this

Figure 19: Raw trends of main outcomes of interest



(a) Payroll Sum

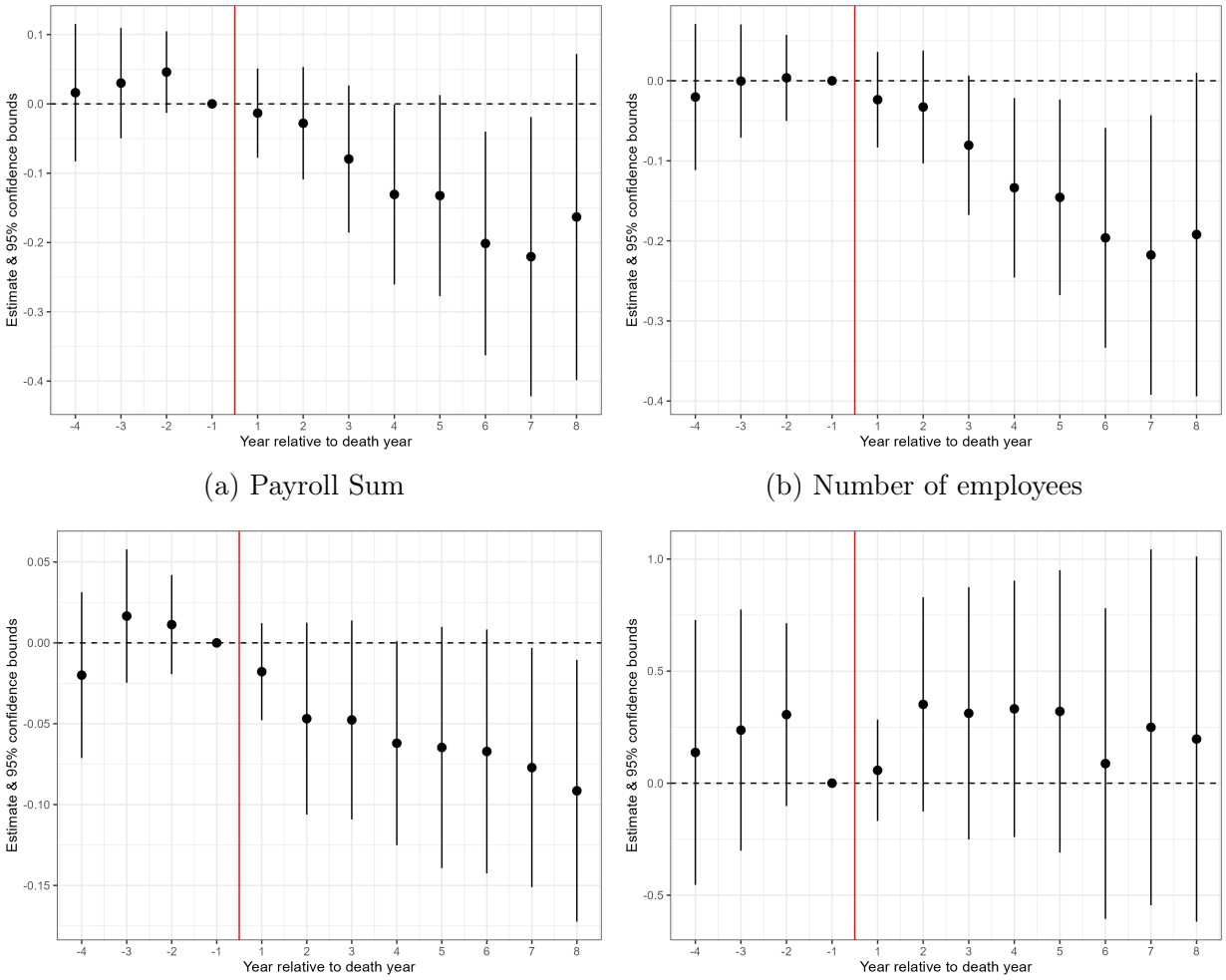
(b) Number of Employees

(c) Median Wage

(d) Fixed Assets

Note: Figure 19 shows raw averages of our main outcomes of interest for the treated and control group separately. Averages are taken over relative time to treatment across all cohorts with no adjustment for fixed effects.

Figure 20: Dynamic Effects of the Payroll Sum Requirement



(a) Payroll Sum

(b) Number of employees

(c) Median wage

(d) Fixed Assets

Note: Figure 20 shows the results of our event study specification Equation 9 for our four main outcomes. In each figure, we plot the coefficient estimates on the relative-time treatment dummy as well as the 95% confidence interval around the estimate. Standard errors are clustered on the individual firm level. The coefficients for the payroll sum and the number of employees are based on the PPML specification in Equation 10, while the estimates for median wage and fixed assets are from the OLS specification in Equation 9.

concern.

In our first test, we re-estimate the event study specification for a stricter size requirement on our control firms. As the firm size distribution provides for a larger mass of potential control firms, we restrict the comparison group to firms with at least 15 but less than 20 employees in the year before the death of the owner. That way, our control firms are even closer in terms of observable characteristics. Figure C.3 shows the results for this alternative specification. Our employment findings remain virtually unchanged in terms of magnitude and significance, suggesting that size is not a relevant factor driving our results. Moreover, zooming into a more comparable size range reveals a significant positive effect of the payroll sum requirement on investment during the first years of the holding period. The effect materializes with a delay of about one year and begins to flatten out in the fifth year after the succession. Hence, we find evidence for the substitution of labor for capital using the more comparable control group, supporting Hypothesis 2.

For our second test, we implement a placebo treatment, which assumes that the payroll sum requirement was applicable for firms exceeding a size threshold of 10 employees. If our approach was to capture only size effects, we would expect to find similar differences between treatment and control firms for this placebo treatment. The results of this exercise are shown in Figure C.4. We find no significant pre- or post-trends for employment outcomes, strengthening our confidence that the documented effects are indeed not driven by differences in size between our treatment and control group. This echoes previous findings of Franke et al. (2016), which also document that firms in different size groups in Germany do not show differential employment growth rates over time.

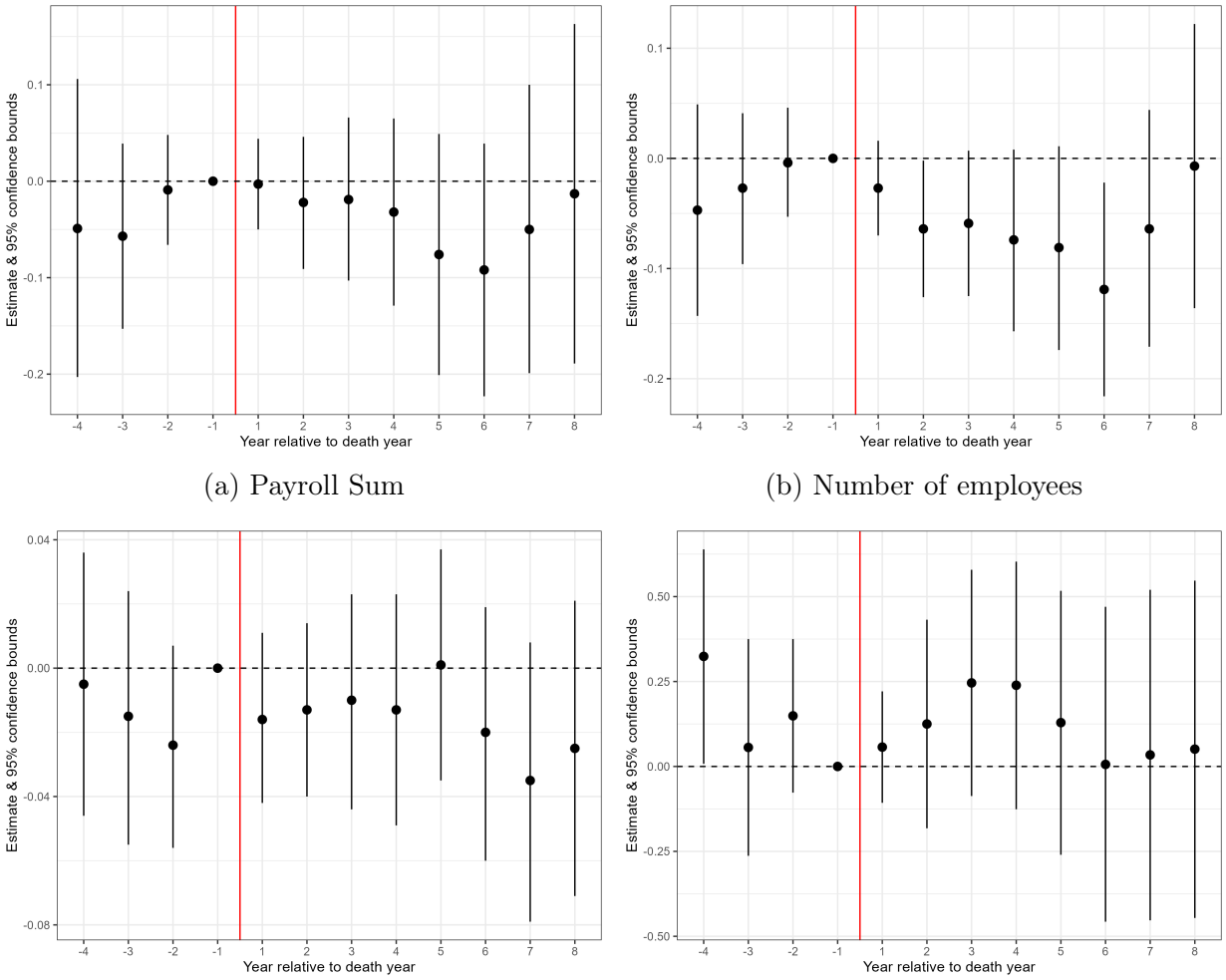
4.5.3 Effects on Highly Leveraged Firms

In order to test Hypothesis 3, we slightly alter our sample to focus on highly-leveraged firms with restricted access to external funding. As the small sample size of our main specification does not allow us to explore further heterogeneity, we ease the requirements of our preferred

identification approach by lifting the restriction on the age at death of the outgoing owner. In this way, we retain a larger set of firms in exchange for a potential loss in the exogeneity of the transfer. Table C.3 shows descriptive statistics for this alternative sample. As for our main sample, apart from size-related differences in employment and capital, the firms in the two samples are similar.

We explore the effects of the payroll requirement on highly leveraged firms by estimating our event study specification of Equation 9 on the subset of firms with above median current leverage prior to succession. Figure 21 shows the results of this exercise. We document that the payroll sum requirement has no significant negative impact on the overall payroll sum and only a weak negative effect on the number of employees at the end of the holding period of the regular exemption. Neither median daily wages nor investment seem to be significantly affected by the requirement as well, consistent with Hypothesis 3.

Figure 21: Heterogeneous Effects of the Payroll Sum Requirement for Firms in Financial Distress



(a) Payroll Sum

(b) Number of employees

(c) Median Daily wage

(d) Fixed Assets

Note: Figure 21 shows the results of our event study specification for firms with an above-median current leverage. In each figure, we plot the coefficient estimates on the relative-time treatment dummy as well as the 95% confidence interval around the estimate. Standard errors are clustered on the individual firm level. The coefficients for the payroll sum and the number of employees are based on the PPML specification in Equation 10, while the estimates for median wage and fixed assets are from the OLS specification in Equation 9.

4.6 Conclusion

In this paper, we exploit the setting of the German inheritance tax tying tax exemptions to payroll requirements to analyze the effects of such requirements on firm outcomes. We combine ownership information, publicly available information on death cases, and adminis-

trative employment data to estimate the causal impact of these requirements on employment, wages, and investment. By comparing firms that undergo an inheritance while being subject to a payroll target with unrestricted firms experiencing the same event, we are able to isolate the effect of the payroll sum requirement.

We document that the growth of employment and daily wages in treated firms is up to 20% smaller than in their untreated counterparts. This finding is contingent on surprise ownership successions, where heirs are arguably less prepared to assume the responsibilities of running a business. Zooming in close to the treatment threshold, we further find some evidence for a substitution from labor to capital in the form of a (short-term) increase in investment.

Our results highlight the unintended consequences of tying tax relief to rigid input factor conditions. This suggests that such policies, while aimed at preserving employment, may affect firm adaptability and growth. These findings raise important questions about the trade-off between safeguarding jobs in the short term and promoting efficient resource allocation in the long term. One possibility to reduce the distortions we document in the context of inheritance taxation could be to broaden the tax base by terminating preferential treatment of specific asset classes with a simultaneous reduction of the high statutory tax rates, possibly with extended options for tax deferral based on the companies' economic condition. This would eliminate the need to impose conditions on these exemptions to align differential treatment with constitutional and fairness considerations.

An important caveat of our analysis is that we provide evidence for a selected subset of all firms liable for these provisions. As a substantial share of firms are passed on already during the lifetime of its owner through inter vivos gifts, careful tax planning and preparation of incoming heirs for their new responsibilities can circumvent much of the potentially harmful effects of the policy.

5 Summary of Main Findings

What are the distributional consequences of taxes? This dissertation contributes a large literature that attempts to answer this question by providing empirical evidence from three distinct settings.

Chapter 2 investigates how changes in profit taxes affect managerial decision margins, which are of first-order importance for determining the economic incidence of these taxes. Considering the behavioral responses to a tax is crucial when evaluating its progressiveness and has implications for welfare analyses. Despite its relevance for public policy making, empirical evidence on the full distribution of profit tax changes among a firm's stakeholders is scarce.

Using experimental survey data, we show that the incidence of profit taxes on firm adjustment margins is highly asymmetric. While tax increases are mostly borne by firm owners and consumers, the benefits of a tax cut accrue to workers substantially more to workers and finance additional investment. We further document that incidence on workers increases in the absolute value of the size of the tax change, thereby offsetting incidence on firm owners.

Our findings suggest that profit tax changes are progressive in either direction, as firm owners are more affected by tax increases and workers benefit more from tax cuts. Still, further research is needed to determine how the initial adjustments feed back into future pre-tax profits, which is essential for evaluating the full incidence.

In Chapter 3, we shed light on the responsiveness of wealthy individuals to wealth transfer taxation. Tax avoidance behavior by the wealthy is a major concern for policymakers in the context of wealth taxes, as it has the potential to curb the redistributive effects of these taxes, leading to regressive taxation in practice.

By exploiting rich administrative data on the universe of German gift tax returns we document that inter vivos gift transfers are very sensitive to legislative changes. In response to mere threats of changes in preferential treatment of business assets for wealth transfer tax purposes, high-value gifts are pulled forward in time in an extraordinarily timely man-

ner to maximize the benefit from generous tax exemptions and hedge against detrimental developments.

These findings have important implications for the implementation of future tax changes. If reforms are not applied retroactively, which might not be politically feasible, the redistributive effects might only be realized with a considerable delay. Sophisticated tax planning by the wealthy will avoid much of the initial impact, and due to the infrequent nature of wealth transfers, the current tax base will be shielded from taxation for the foreseeable future.

In Chapter 4, we examine the real effects of job protection legislation on firm performance. This issue is especially important in the context of wealth transfer taxes, as many countries face difficulties in the tax treatment of business assets. The beneficial treatment of a particular asset class generally has to be justified with the provision of positive externalities to society (such as the provision of employment and growth). Therefore, it is important to determine whether provisions that tie preferential treatment to real firm outcomes that proxy these externalities have unintended consequences.

We leverage a unique dataset that combines information on ownership changes from Orbis, publicly available information on death events, and administrative employment data for German establishments to provide an empirical answer to this question. We find that requiring firms to guarantee a stable level of employment depresses growth in affected firms relative to firms not liable to this requirement. The free cash seems to be used for capital investments instead.

These findings suggest that tying preferential treatment to real economic outcomes might distort optimal factor input decisions of firms. Considering these unintended distortions it might be worthwhile to consider alternative provisions that align the conflicting policy goals.

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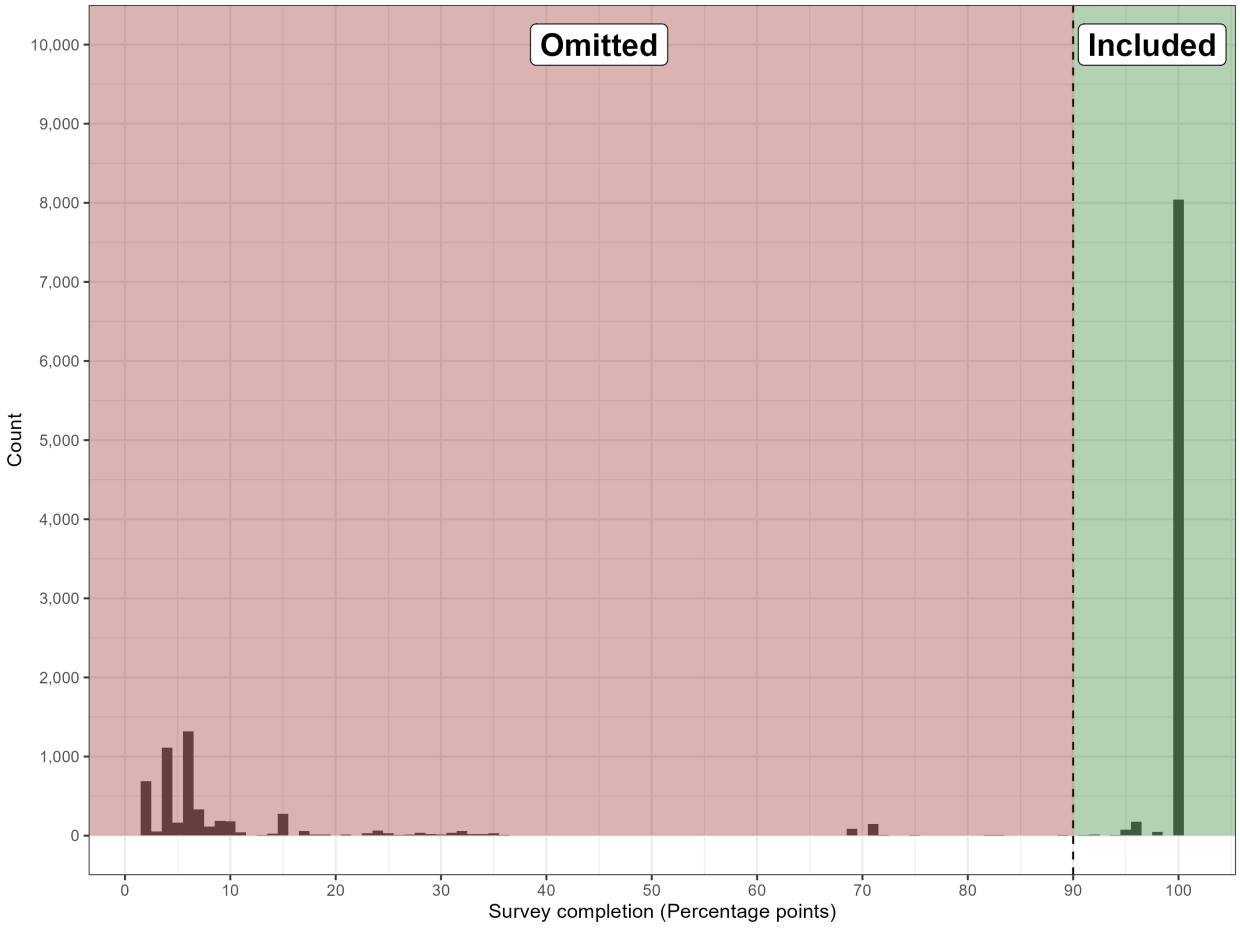
A Appendix to Chapter 2

A.1 Completion Rate

Figure A.1 illustrates the completion rates of survey respondents. Responses with a completion rate below 90% (shaded in red) are excluded from the analysis, while all responses meeting or exceeding the 90% threshold ($N = 8{,}392$) are retained (shaded in green). The final sample used in the main analysis ($N = 6{,}749$) consists of these high-completion responses, further refined to exclude observations with missing values for control and weighting variables.

To evaluate whether firms in our final survey sample ($N = 6{,}749$) – those with a completion rate of at least 90% and non-missing values for control and weighting variables – differ systematically in key financial characteristics, Section A.5 in the Appendix presents a balance table comparing their financial profiles (as recorded in Orbis) with those of German firms in the Orbis database that either did not participate or did not complete the survey (i.e., non-participants). The analysis finds no significant differences in key financial metrics between firms in our final survey sample and non-participants, suggesting that firms that completed the survey are not systematically different in their financial characteristics from those excluded from our sample.

Figure A.1: Completion Rate



Note: Figure A.1 depicts the distribution of the progress at which the respondent finished the survey. Responses that fall in the shaded red area are excluded from the survey.

A.2 Screenshots of Original Survey Questions

A.2.1 Tax Decrease Treatments

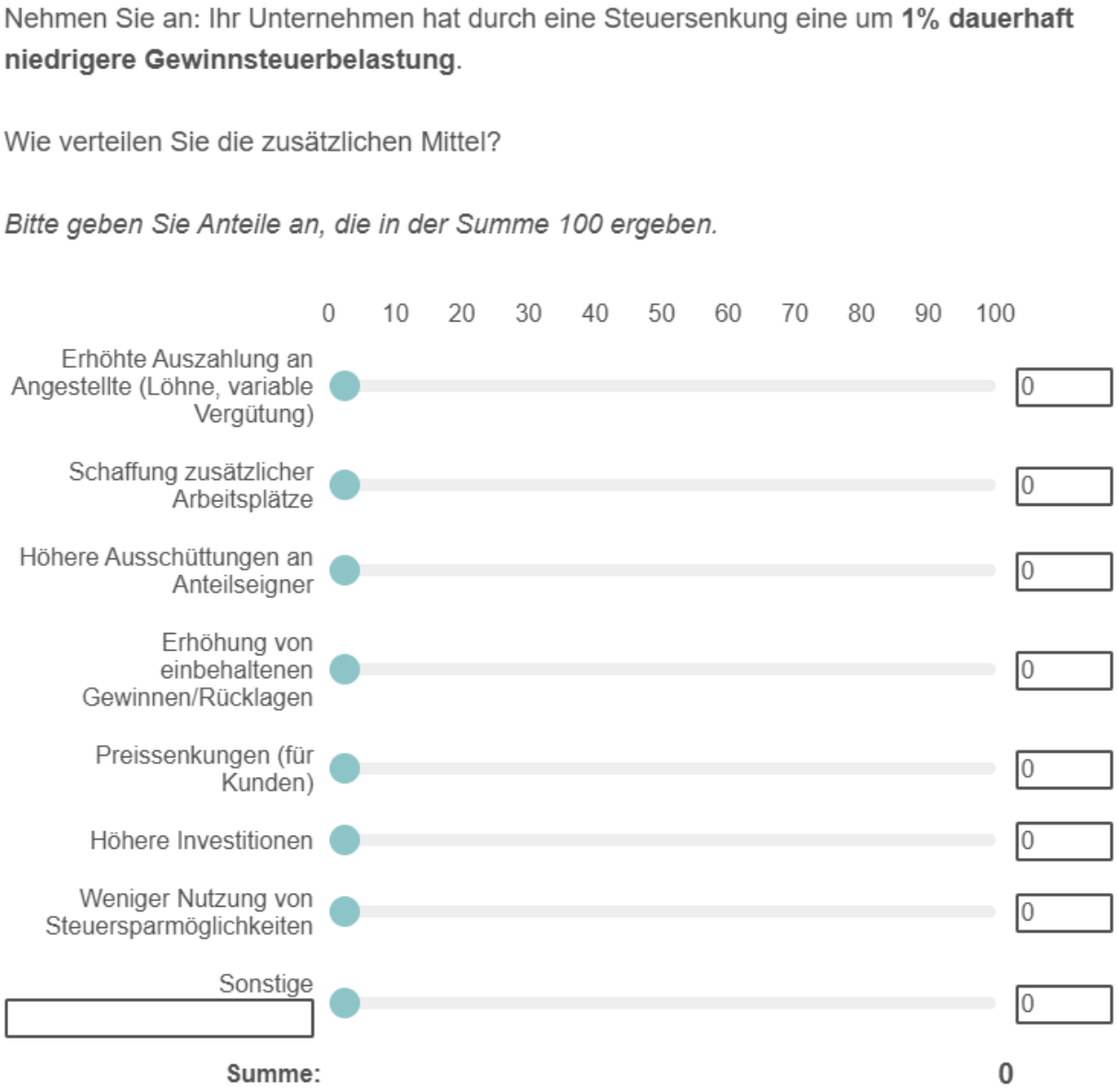
Question:

Assume that your company has a (1%/10%/25%) ***permanently lower profit tax burden*** *as a result of a tax decrease. How do you distribute the additional funds? Please enter shares that add up to 100.*

Answer Options:

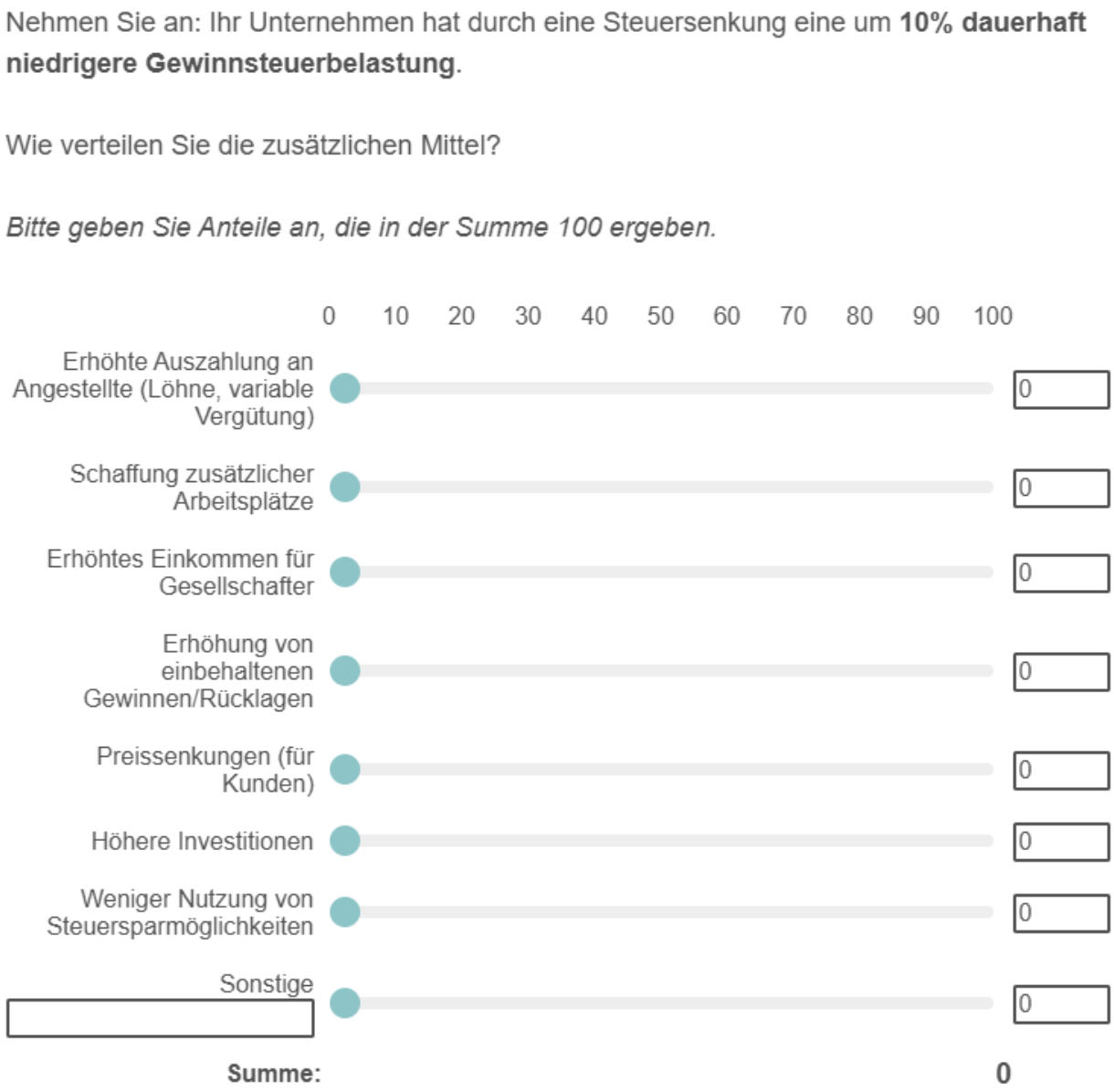
- Increased payment to employees
- Creation of additional jobs
- Higher distributions to partners (for non-corporations)
- Higher distributions to shareholders (for corporations)
- Increase in retained earnings/reserves
- Price reductions (for customers)
- Higher investments
- Less use of tax saving opportunities
- Others

Figure A.2: Example Survey Question Tax Decrease Treatment - 1%



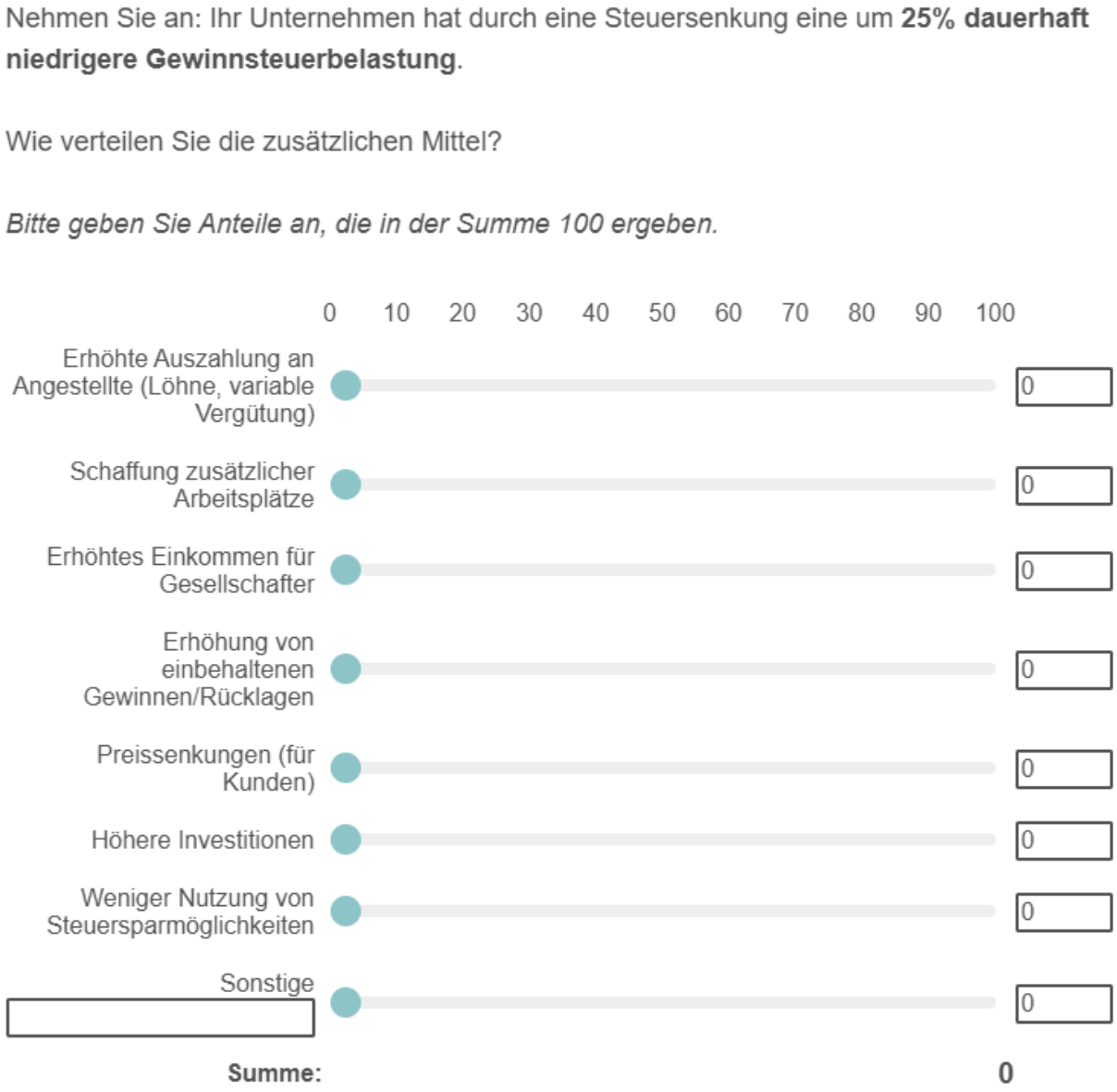
Note: Figure A.2 shows an example of the tax decrease survey experiment as appearing in the web survey of the GBP. After the hypothetical treatment (*"Assume that your company has a (1%/10%/25%)* ***permanently lower profit tax burden*** *as a result of a tax decrease. How do you distribute the additional funds?"*), the respondent was asked how the additional funds would be distributed and notified that entered shares must add up to 100. The respondent then could attribute shares to the categories listed in Table 1 either via adjusting the sliders or entering them directly in the boxes to the right. Shares were initially set to zero for all categories.

Figure A.3: Example Survey Question Tax Decrease Treatment - 10%



Note: Figure A.3 shows an example of the tax decrease survey experiment as appearing in the web survey of the GBP. After the hypothetical treatment (*"Assume that your company has a (1%/10%/25%)* ***permanently lower profit tax burden*** *as a result of a tax decrease. How do you distribute the additional funds?"*), the respondent was asked how the additional funds would be distributed and notified that entered shares must add up to 100. The respondent then could attribute shares to the categories listed in Table 1 either via adjusting the sliders or entering them directly in the boxes to the right. Shares were initially set to zero for all categories.

Figure A.4: Example Survey Question Tax Decrease Treatment - 25%



Note: Figure A.4 shows an example of the tax decrease survey experiment as appearing in the web survey of the GBP. After the hypothetical treatment (*"Assume that your company has a (1%/10%/25%)* ***permanently lower profit tax burden*** *as a result of a tax decrease. How do you distribute the additional funds?"*), the respondent was asked how the additional funds would be distributed and notified that entered shares must add up to 100. The respondent then could attribute shares to the categories listed in Table 1 either via adjusting the sliders or entering them directly in the boxes to the right. Shares were initially set to zero for all categories.

A.2.2 Tax Increase Treatment.

Question:

Assume that your company has a (1%/10%/25%) ***permanently higher profit tax burden*** *as a result of a tax increase. How do you finance the additional burden? Please enter shares that add up to 100.*

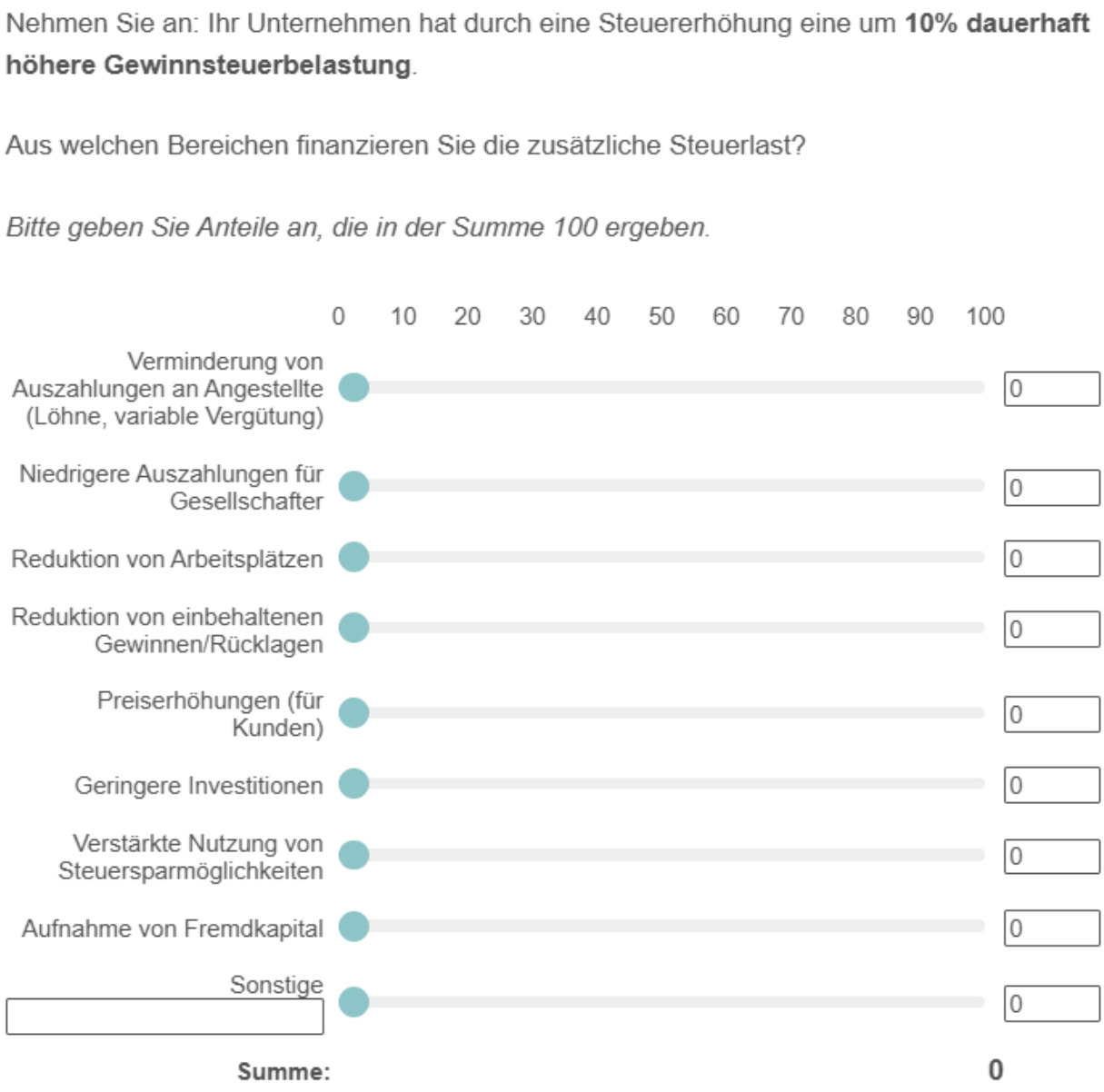
Answer Options:

- Decreased payment to employees
- Lower distributions to partners (for non-corporations)
- Lower distributions to shareholders (for corporations)
- Reduction of jobs
- Decrease in retained earnings/reserves
- Price increases (for customers)
- Lower investments
- More use of tax saving opportunities
- Increase in Debt Capital
- Others

Figure A.5: Example Survey Question Tax Increase Treatment - 1%

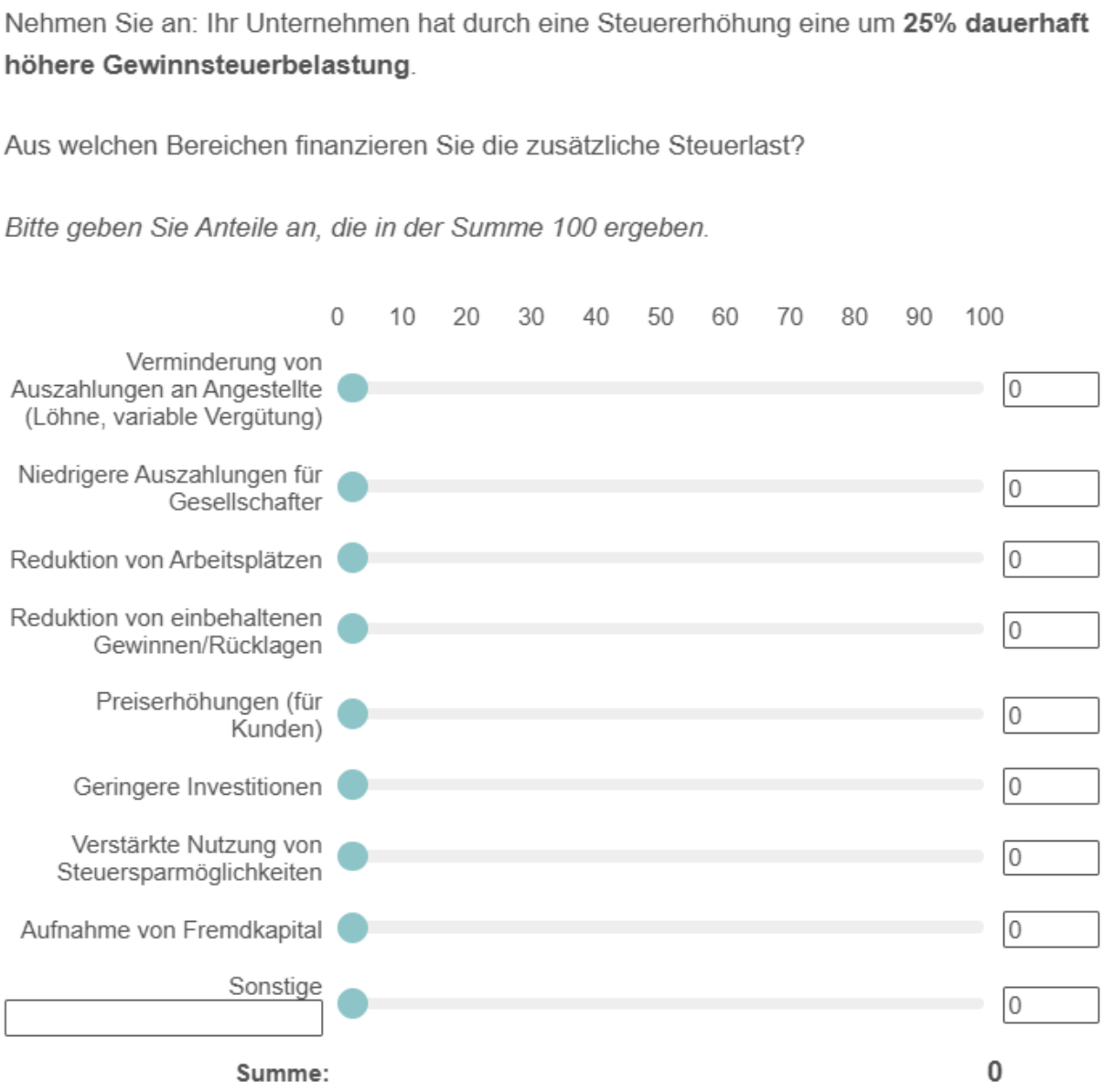
Note: Figure A.5 shows an example of the tax increase survey experiment as appearing in the web survey of the GBP. After the hypothetical treatment (*"Assume that your company has a (1%/10%/25%)* ***permanently higher profit tax burden*** *as a result of a tax increase. How do you finance the additional burden?"*), the respondent was asked how the additional funds would be distributed and notified that entered shares must add up to 100. The respondent then could attribute shares to the categories listed in Table 1 either via adjusting the sliders or entering them directly in the boxes to the right. Shares were initially set to zero for all categories.

Figure A.6: Example Survey Question Tax Increase Treatment - 10%



Note: Figure A.6 shows an example of the tax increase survey experiment as appearing in the web survey of the GBP. After the hypothetical treatment (*"Assume that your company has a (1%/10%/25%)* ***permanently higher profit tax burden*** *as a result of a tax increase. How do you finance the additional burden?"*), the respondent was asked how the additional funds would be distributed and notified that entered shares must add up to 100. The respondent then could attribute shares to the categories listed in Table 1 either via adjusting the sliders or entering them directly in the boxes to the right. Shares were initially set to zero for all categories.

Figure A.7: Example Survey Question Tax Increase Treatment - 25%



Note: Figure A.7 shows an example of the tax increase survey experiment as appearing in the web survey of the GBP. After the hypothetical treatment (*"Assume that your company has a (1%/10%/25%)* ***permanently higher profit tax burden*** *as a result of a tax increase. How do you finance the additional burden?"*), the respondent was asked how the additional funds would be distributed and notified that entered shares must add up to 100. The respondent then could attribute shares to the categories listed in Table 1 either via adjusting the sliders or entering them directly in the boxes to the right. Shares were initially set to zero for all categories.

A.2.3 Reasons for Change in Investment - Tax Decrease Treatments

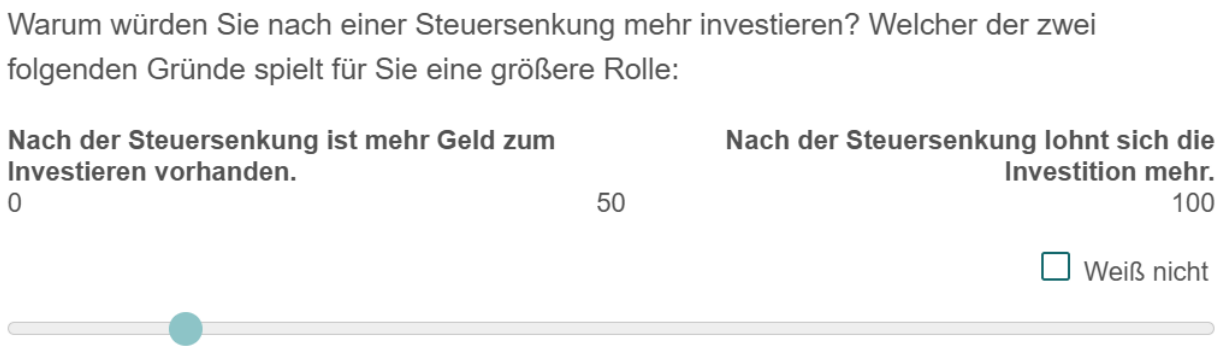
Question:

Why would you invest more after a tax cut? Which of the following two reasons plays a greater role for you?

Answer Options - Slider: [0,100]

- 0: After the tax cut, more funds are available
- 100: After the tax cut, the investment is more worthwhile

Figure A.8: Example Survey Question Reasons for Change in Investment - Tax Decrease



Note: Figure A.8 shows an example of the question eliciting the reasons for a substantial change in investment due to a tax change for the tax decrease treatment. **If the respondent had entered a share of at least 5 percent for the investment category, she was asked a follow-up question about the reason for this choice.** She could adjust the slider from 0 to 100, where a value of 0 indicates that more funds would be available after the tax decrease, and a value of 100 that the investment is more worthwhile after the tax decrease.

A.2.4 Reasons for Change in Investment - Tax Increase Treatments

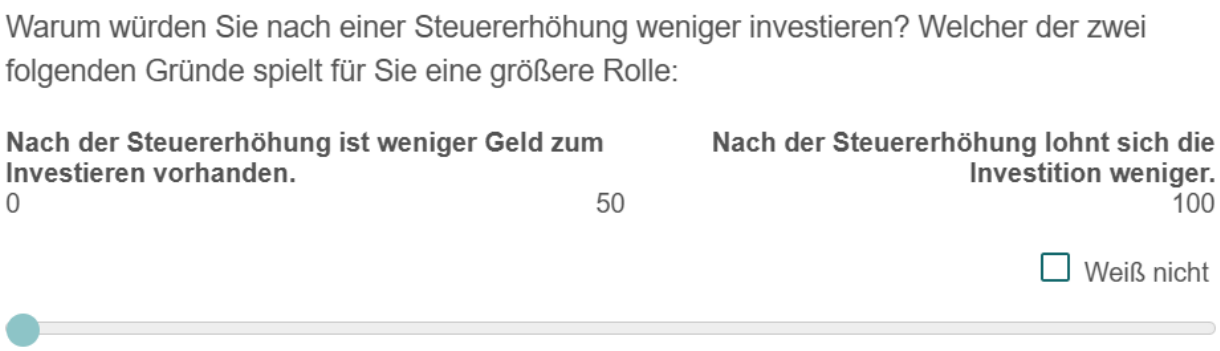
Question:

Why would you invest less after a tax increase? Which of the following two reasons plays a greater role for you?

Answer Options - Slider: [0,100]

- 0: After the tax increase, there is less money to invest
- 100: After the tax increase, the investment is less worthwhile

Figure A.9: Example Survey Question Reasons for Change in Investment - Tax Increase



Note: Figure A.9 shows an example of the question eliciting the reasons for a substantial change in investment due to a tax change for the tax increase treatment. **If the respondent had entered a share of at least 5 percent for the investment category, she was asked a follow-up question about the reason for this choice.** She could adjust the slider from 0 to 100, where a value of 0 indicates that there is less money to invest after the tax increase, and a value of 100 that the investment is less worthwhile after the tax increase.

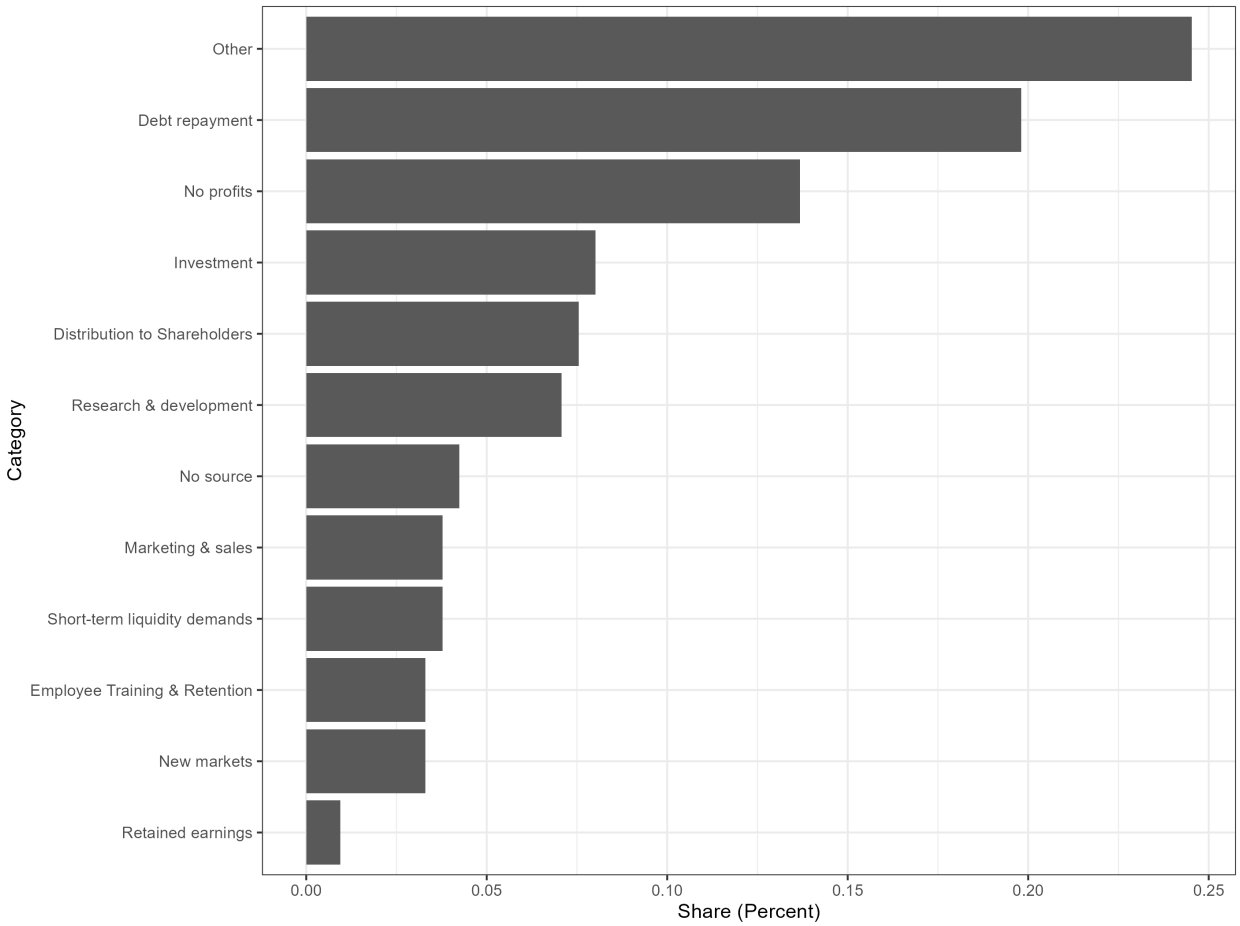
A.2.5 Text Entries - Other Category

After receiving the randomized tax decrease and increase treatments, participating firms selected from a comprehensive list of adjustment categories, detailed in Section 2.2.2 and Appendix A.2. Firms could allocate shares to each category either by adjusting the slider next to the respective option or by entering values directly in the input boxes on the far right. All entered shares had to be non-negative and sum to 100%. In addition to predefined adjustment margins – including wages, employment, distributed profits, retained earnings or reserves, consumer prices, investments, and tax-saving strategies – firms also had the option to select an *Others* category.

The *Others* category was included to ensure that no relevant incidence category was overlooked. If respondents allocated a positive share to this category, they were prompted to provide a free-text response specifying the missing category or categories. Figures A.10 and A.11 illustrate the text responses given in the *Others* category for the tax decrease and tax increase treatments, respectively.

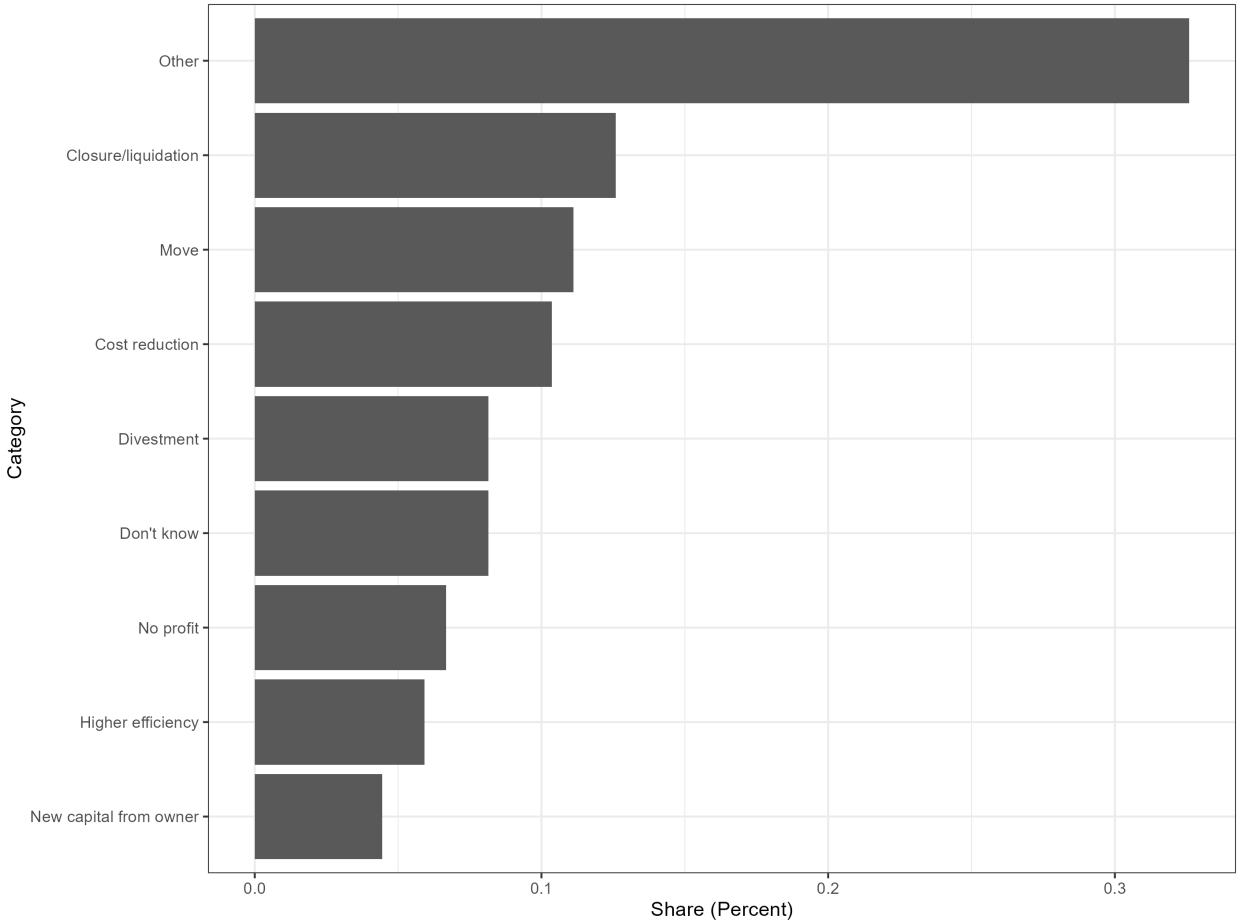
For the **tax decrease treatment**, the most frequently mentioned missing category appears to be the use of additional funds for debt repayment, as indicated by terms such as liabilities (Verbindlichkeiten), repayment (Rückzahlung), and loans (Kredite, Darlehen). Additionally, some firms noted that they were not generating profits, making a reduction in the profit tax burden irrelevant. In the **tax increase treatment**, respondents most commonly cited company liquidation, relocation, and cost-cutting measures as potential responses to a tax hike.

Figure A.10: Free Text Entries - Tax Decrease Treatment



Note: Figure A.10 displays the most common categories selected by respondents in the **tax decrease treatment** arm after indicating a positive share in the *Others* category.

Figure A.11: Free Text Entries - Tax Increase Treatment

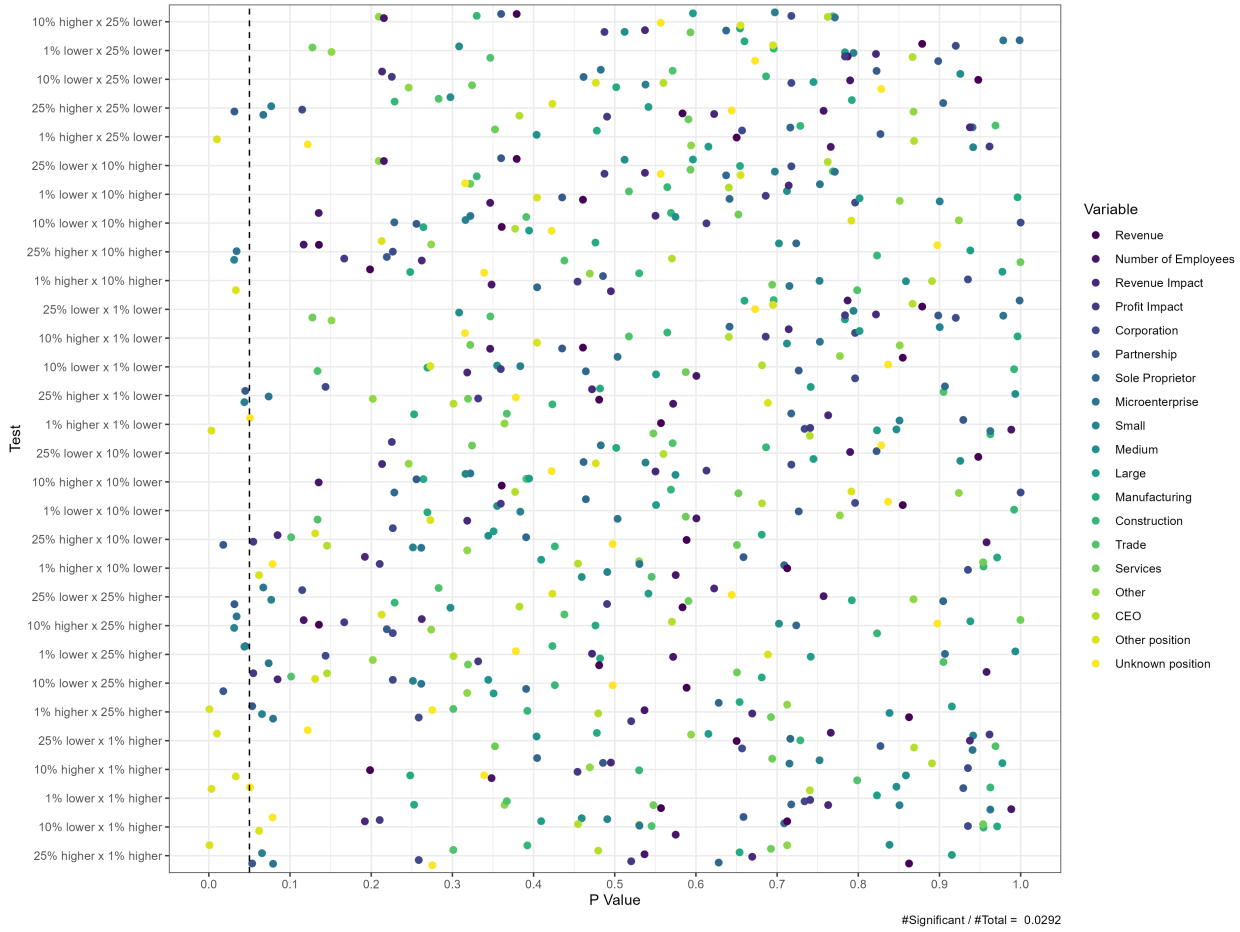


Note: Figure A.11 displays the most common categories selected by respondents in the **tax increase treatment** arm after indicating a positive share in the *Others* category.

A.3 Balance Tests

To assess the effectiveness of our randomization procedure, we conducted multiple balance tests using the available characteristics of survey respondents. Figure A.12 summarizes the results, displaying p-values from difference-in-means tests for each characteristic across all treatment combinations. The overall proportion of significant differences is 2.9%, well below the chosen significance threshold of 5%. Moreover, after applying the Benjamini and Yekutieli (2001) correction, the adjusted p-value for every test equals one, reinforcing our confidence that the treatment assignment was successfully randomized.

Figure A.12: Covariate Balance Tests Across Treatment Cells.



Note: Figure A.12 shows the results of difference-in-means tests for all firm and respondent characteristics across each combination of treatment sign and magnitude. Each point represents the p-value of a test. The dashed vertical line shows the 5% significance level. The proportion of significant tests out of the total number of tests conducted is only 2.9%, which is well below the chosen significance threshold of 5%. Therefore, we infer that there are no significant differences between our treatment groups.

A.4 Sample vs. German Firm Population

To derive insights that are generalizable to the entire German firm population, we construct weights to ensure our survey sample is as representative as possible across the following three dimensions: industry sector, number of employees, and revenues. These weights adjust for differences between the sample and the universe of active firms in Germany, allowing for more accurate estimations of population parameters.

Weighting survey data involves assigning each legally independent firm in the GBP a factor that reflects its relative importance in estimating population statistics, such as, e.g., the mean revenue of all German firms (Sand and Kunz, 2020). The objective is to compute firm-level weights, w_i, which serve as multiplicative factors for each observation i, ensuring that sample-based estimates closely approximate the true population values.

We employ the **raking method** of iterative proportional fitting (Kolenikov, 2014) to calculate survey weights, aligning the sample distribution with known population characteristics. The three key dimensions considered in this process are:

- **Industry sector** (1-digit WZ08 classification),[98]
- **Number of employees** (0–9, 10–49, 50–249, $\geq$ 250 employees, subject to social insurance contributions),
- **Revenue categories** (EUR 0–2 million, EUR 2–10 million, EUR 10–50 million, > EUR 50 million).

The calibration weights are constructed using the raking algorithm (Deming and Stephan, 1940; Kolenikov, 2014), which iteratively adjusts survey weights to align the sample's marginal distributions with those of the target population. Specifically, the algorithm ensures that the weighted distributions of industry sector (1-digit WZ08), number of employees (subject to

[98] The WZ 2008 classification of the German Federal Statistical Office, compatible with the NACE Rev. 2 classification used by the European Community.

social insurance contributions), and revenues in the sample closely mirror the corresponding distributions in the 2019 business register of the Federal Statistical Office. The year 2019 was chosen as the reference point as it was the most recent dataset available during the sample design phase.

To prevent distortions caused by excessively high survey weights in underrepresented cells, we apply a trimming procedure, capping weights at the 5th and 95th percentiles of the distribution while ensuring that the total sum of weights remains unchanged.[99] This approach helps stabilize the variance of the survey weights and minimizes the influence of extreme values in the analysis.

Table A.1 compares the distribution of firms in our sample with the overall German firm population in terms of revenue, number of employees, and industry classification (1-digit WZ08) for the reporting year 2019 (RY 2019). Overall, the weighting process effectively increases the representativeness of our sample, aligning it closely with the broader German firm population.

[99] We are grateful to Dr. Matthias Sand from GESIS (Department Survey Design and Methodology) for providing the weight-trimming algorithm.

Table A.1: Sample vs. German Firm Population

	Unweighted Sample	Weighted Sample	Population (RY 2019)
Panel A: Revenues (in EUR)			
Less than EUR 2 mn.	0.764	0.927	0.932
EUR 2-10 mn.	0.168	0.054	0.051
EUR 10-50 mn.	0.051	0.014	0.013
EUR 50 mn. or more	0.016	0.004	0.004
Panel B: Employees subject to social insurance			
0-9 employees	0.667	0.865	0.874
10-49 employees	0.255	0.108	0.101
50-249 employees	0.064	0.022	0.021
250 and more employees	0.014	0.005	0.005
Panel C: Economic Sector (1-digit WZ08 Classification)			
B - Mining and quarrying	0.004	0.001	0.001
C - Manufacturing	0.173	0.068	0.064
D - Energy supply	0.007	0.014	0.022
E - Water supply	0.005	0.003	0.003
F - Construction	0.071	0.111	0.110
G - Trade	0.156	0.182	0.171
H - Transport and storage	0.028	0.034	0.032
I - Hospitality	0.054	0.076	0.071
J - Information and communication	0.134	0.042	0.039
K - Provision of financial and insurance services	0.037	0.023	0.021
L - Real estate and housing	0.032	0.057	0.053
M - Provision of freelance, scientific and technical services	0.132	0.160	0.150
N - Provision of other commercial services	0.075	0.069	0.064
P - Education and teaching	0.015	0.024	0.023
Q - Health and social services	0.025	0.047	0.071
R - Art, entertainment and recreation	0.024	0.036	0.034
S - Provision of other services	0.027	0.053	0.069
N	6,749		

Note: Table A.1 presents the distribution of firms with respect to revenues, the number of employees, and the economic sector (1-digit WZ08 classification) for our sample of firms and the population of firms in Germany for the reporting year 2019 (RY 2019), based on the business register of the German Statistical Office. We use the reporting year 2019 for comparison, as it was the most recent year available at the time the sample pool was created.

A.5 Orbis Comparison: Participants vs. Non-participants

Section A.5 presents a balance table (Table A.2) comparing the observable financial characteristics of firms in our survey sample (i.e., participants), as obtained from Orbis, with those of German firms in the Orbis database that did not participate in the survey or did not complete our survey (i.e., non-participants), using financial data from the 2019 reporting year. The selection of 2019 as the reference year was based on its status as the most recent available dataset at the time of sample construction. The comparison encompasses key financial indicators, including total assets, number of employees, turnover, and cost of employees, with all variables constrained to non-negative values. For each variable, we report the number of available observations within Orbis, along with the corresponding mean and median values. Furthermore, we conduct a t-test to assess differences in means between survey participants and non-participants, presenting the associated p-values. The survey sample comprises firms that explicitly consented to linking their survey responses with external data sources (N = 2,435). Overall, we find no significant differences in key financial metrics between survey participants and non-participants, suggesting that firms opting to participate and complete the survey do not systematically differ in financial characteristics from those in the Orbis database that were not included in our sample.

Table A.2: Sample vs. Orbis Population

	Participants			Non-Participants			
	Obs.	Mean	p50	Obs.	Mean	p50	p-value
Total Assets	793	11,374,676.10	798,066.00	462,984	13,879,657.47	1,005,009.00	0.86
Number of Employees	1,516	23.18	6.00	901,927	28.88	3.00	0.75
Turnover/Operating Revenue	606	13,105,905.96	900,000.00	228,972	19,909,355.46	1,050,000.00	0.68
Costs of Employees	62	16,558,588.39	4,492,631.00	42,142	11,847,030.72	4,427,542.50	0.62

Note: Table A.2 compares the sample of firms participating in our survey with the reference group of German firms from Orbis that did not participate, using data from the reporting year 2019. The comparison includes total assets, number of employees, turnover, and cost of employees, with non-negative values required for all three variables. We report the number of available observations in Orbis for each variable, along with the mean and median of each firm characteristic. Additionally, we present the p-value of a t-test comparing the means between participants and non-participants. The survey sample consists of firms that consented to linking their survey data with external data sources (N = 2,435). The reporting year 2019 was chosen as it was the most recent year available when the sample pool was created.

Furthermore, we investigate whether firms in our survey sample that consented to linking their survey data with external databases systematically differ in key financial characteristics reported in the survey compared to those that declined the linking agreement. Table A.3 presents a comparative analysis based on key firm attributes, including revenue, number of employees, legal form, and sector. For each variable, we report the number of observations, along with the corresponding mean and median values. Additionally, we provide the p-value from a t-test comparing the means between firms that agreed to the data-linking arrangement (N = 2,435) and those that did not (N = 4,314). Once again, we find no systematic differences between these groups. The only statistically significant difference observed pertains to the share of firms in the construction sector (p-value: 2%). However, this difference is economically negligible, with 6% of firms in the linking group compared to 8% in the non-linking group.

Furthermore, an analysis of employee numbers in Table A.3 and Table A.2 reveals that firms that consented to data linking and have employee information available in Orbis (N = 1,516) tend to be smaller regarding employees than the overall survey sample. However, the median number of employees remains nearly identical between the two groups, with a median of 5 in Table A.3 (linked firms) and 6 in Table A.2 (survey participants). Additionally, firms in the linking group reported a lower number of employees in the survey itself (Mean: 43, Median: 6), reinforcing the reliability of our survey responses.

Table A.3: Linking Agreed vs. Linking Not Agreed

	Linking Agreed			Linking Not Agreed			
	Obs.	Mean	p50	Obs.	Mean	p50	p-value
Revenues	1,959	30,206,127.46	650,000.00	3,300	13,672,687.66	750,000.00	0.23
Number of Employees	2,435	68.46	5.00	4,314	68.42	5.00	1.00
Corporation	2,435	0.72	1.00	4,314	0.73	1.00	0.24
Sole Proprietor	2,435	0.13	0.00	4,314	0.13	0.00	0.94
Partnership	2,435	0.15	0.00	4,314	0.14	0.00	0.12
Manufacturing	2,435	0.18	0.00	4,314	0.17	0.00	0.30
Construction	2,435	0.06	0.00	4,314	0.08	0.00	0.02
Trade	2,435	0.15	0.00	4,314	0.16	0.00	0.66
Other Sector	2,435	0.28	0.00	4,314	0.27	0.00	0.18

Note: Table A.3 compares firms in our survey that consented to linking their survey data with external databases like Orbis to those that declined. The comparison is based on key firm characteristics, including revenue, number of employees, legal form, and sector. For each variable, we report the number of observations, as well as the mean and median values. Additionally, we present the p-value from a t-test comparing the means between firms that agreed to the linking agreement and those that did not.

A.6 Reasons for Investment Change

To better understand the factors driving companies' investment adjustments in response to tax changes, we asked respondents who allocated at least 5% of their adjustment shares to investment to explain their reasoning. Figure A.8 in Appendix A.2 provides an example of how this question appeared in the survey's online interface.

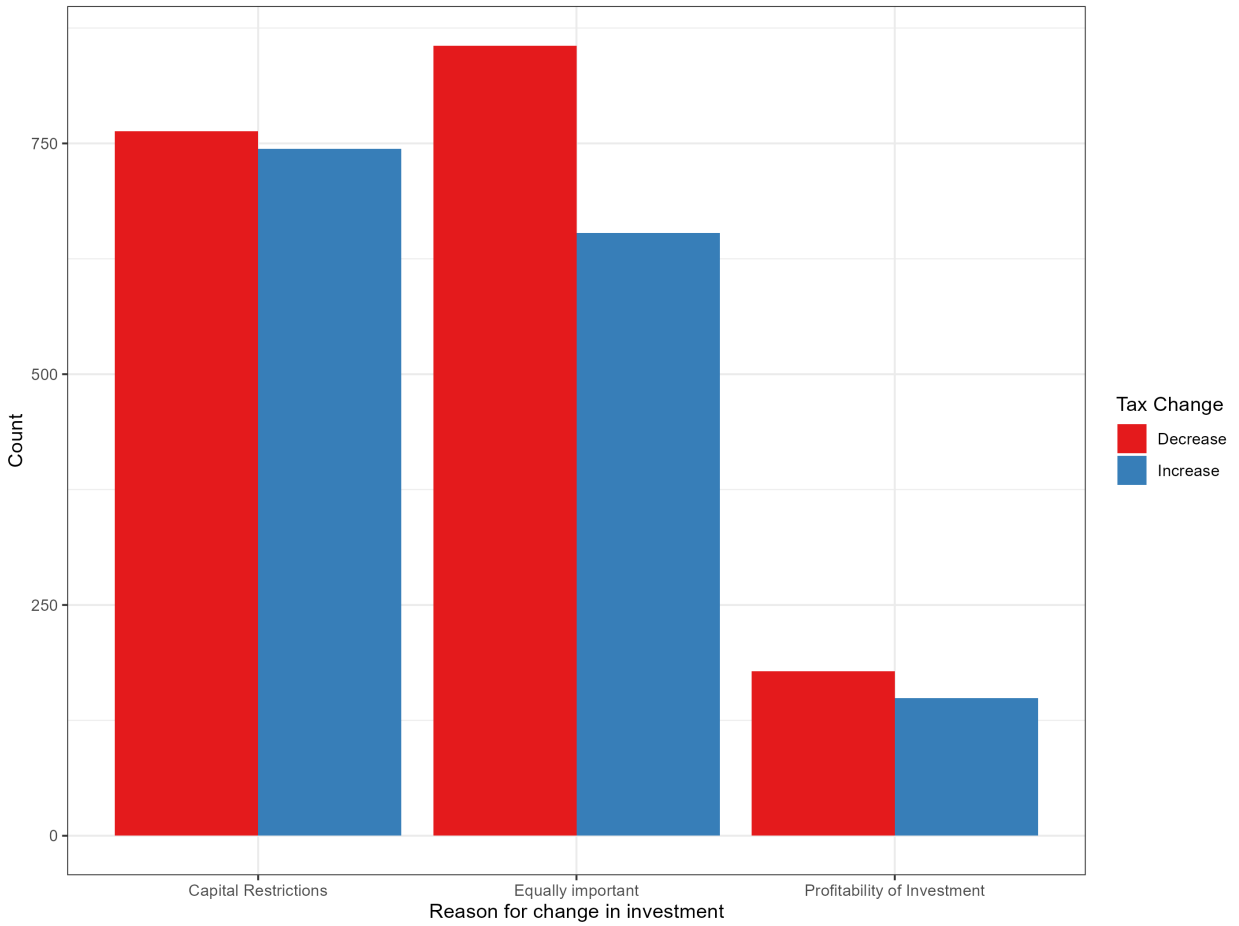
Participants rated their reasoning on a scale from 0 to 100, where 0 indicated that investment adjustments were primarily driven by changes in available funds following a tax decrease or increase, while 100 suggested that the perceived profitability of investments was the dominant factor. Lower values indicate that firms face capital constraints, whereas higher values suggest that the tax change primarily affects the profitability of investment opportunities.

Figure A.13 presents the results of these follow-up questions. We categorized responses into three groups:

- **Capital Restriction** (*values below 25*), indicating that firms adjust investment primarily due to liquidity constraints.
- **Mixed Reasons** (*values between 26 and 75*), suggesting that both capital availability and investment profitability play a role.
- **Profitability-Driven** (*values above 76*), meaning that firms primarily adjust investment in response to changes in its expected returns.

Our findings suggest that the majority of firms adjust investment behavior due to capital constraints rather than shifts in the profitability of investment projects following a tax change.

Figure A.13: Reasons for Change in Investment



Note: Figure A.13 shows the results of a follow-up question respondents were asked when selecting a share of investment incidence greater or equal to 5%. After being asked why they attributed a substantial share to the investment category, respondents could adjust a slider ranging from 0 to 100, where 0 indicated that more/less funds were available to invest, 100 that the investment was more (less) worthwhile, and 50 that the factors were equally important. We binned the responses into three categories related to the slider prompts, with responses lower than 25 and larger than 75 being assigned to the polar cases.

A.7 Robustness Tests - Main Results

A.7.1 Ordinary Least Squares (OLS) Estimation

In this section, we present the results of the OLS estimation of Equation (1). These results, shown in Table A.4, form the basis of the main findings discussed in Section 2.3.2.

For each category of incidence (i.e., the dependent variables listed in Table A.4), we apply

OLS estimation to Equation (1):

$$y_i = \beta_0 + \beta_1 \text{Increase}_i + \beta_2 \text{Medium Change}_i + \beta_3 \text{Large Change}_i + \beta_4 \text{Increase}_i \times \text{Medium Change}_i + \beta_5 \text{Increase}_i \times \text{Large Change}_i + \varepsilon_i, \quad (1)$$

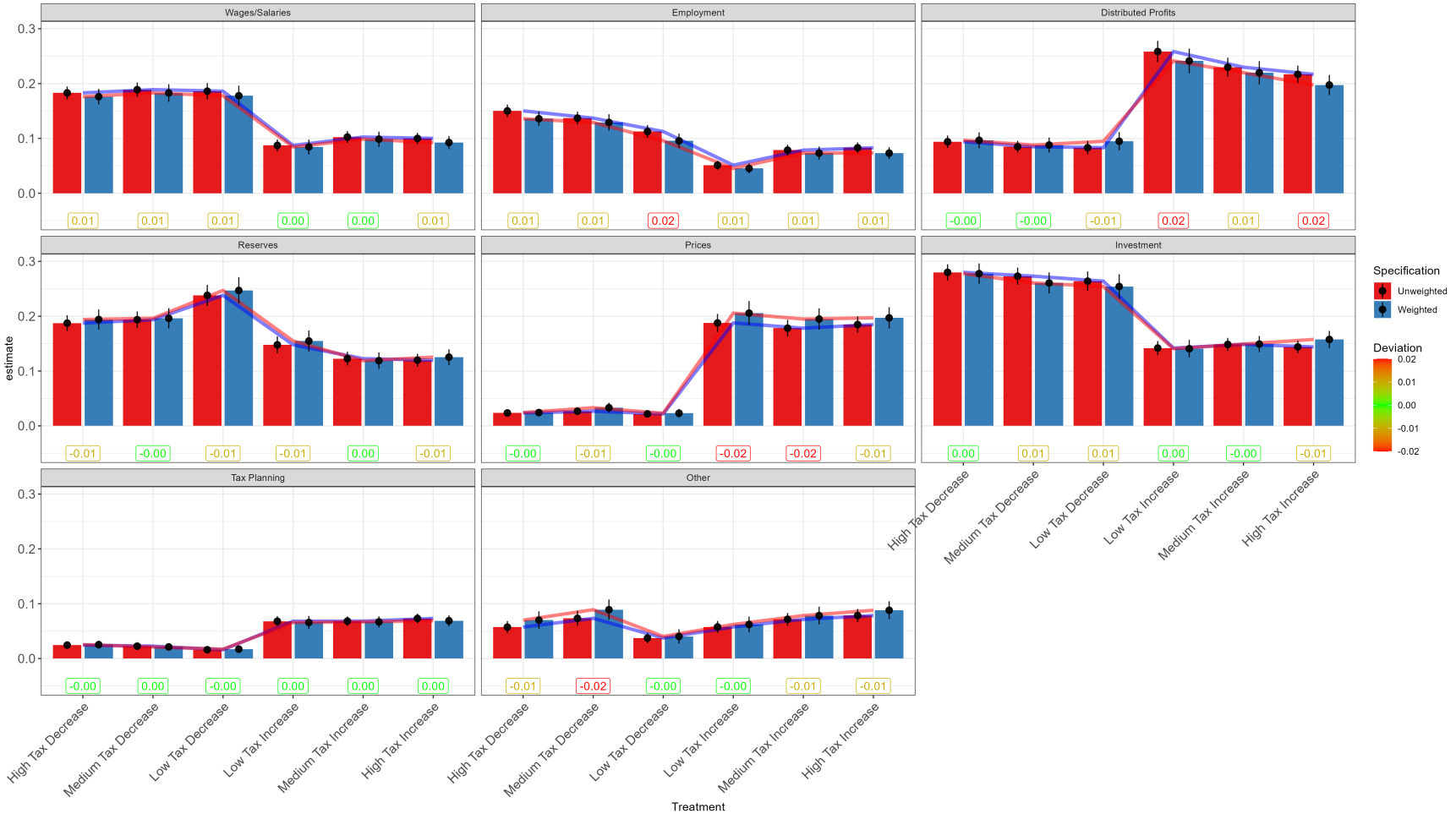
where the dependent variable, y_i, represents the proportion assigned to each category. The key explanatory variables include Increase_i, Medium Change_i, and Large Change_i along with their interaction terms, which serve as binary indicators for directionality and magnitude (10% and 25%, respectively). The estimated coefficients, $\beta_i, i = 0, \ldots, 5$, enable us to assess asymmetries between tax increases and decreases, as well as the differential impact of tax change magnitudes.

We begin by exploring the sensitivity of our main results with respect to the usage of survey weights. Figure A.14 compares our main estimates from Equation (1) with weighted regressions that apply the survey weights described in Section A.4. The comparison suggests that there are only minor differences between weighted and unweighted point estimates, with none of them exceeding a two percentage point difference in estimated incidence. However, the unweighted coefficients are estimated with greater precision. Therefore, we opt for unweighted regressions throughout our paper.

As part of our robustness analysis, we incorporate additional control variables in Table A.5 to enhance the accuracy of our estimates. These controls include industry-specific dummies for key economic sectors (Manufacturing, Construction, Trade, and Services), dummy variables for a firm's legal structure, and classifications for firm size—small, medium, and large—based on annual revenue. Additionally, we introduce two indicators identifying firms that experienced a substantial financial impact from the COVID-19 pandemic. A firm is classified as significantly affected by COVID-19 if its reported percentage change in revenues or net income falls within the lowest quartile of the distribution.

The results of Equation (1) estimated using OLS with controls, as shown in Table A.5, indicate that the estimated effects remain largely consistent with those obtained without controls. This results strengthens our confidence in the main findings presented in Section 2.3.2.

Figure A.14: Weighted versus Unweighted Regressions



Note: Figure A.14 shows the estimated incidence share for the respective category across the six different treatments based on Specification 1, once for the weighted and once for the unweighted regression. The boxed numbers below each pair of coefficient estimates indicate their difference. Robust confidence bounds are indicated by vertical lines.

Table A.4: Asymmetry and Magnitude Effects - Without Controls

	Wages/Salaries	Employment	Distributed Profits	Reserves	Prices	Investment	Tax Planning	Other
Constant	0.186***	0.113***	0.083***	0.238***	0.022***	0.264***	0.016***	0.037***
	(0.008)	(0.006)	(0.006)	(0.010)	(0.003)	(0.009)	(0.003)	(0.005)
Increase	-0.099***	-0.061***	0.175***	-0.090***	0.166***	-0.122***	0.052***	0.020***
	(0.009)	(0.007)	(0.012)	(0.012)	(0.009)	(0.011)	(0.005)	(0.007)
Medium Change	0.003	0.024***	0.002	-0.044***	0.005	0.009	0.007*	0.036***
	(0.010)	(0.008)	(0.008)	(0.012)	(0.004)	(0.012)	(0.004)	(0.008)
Large Change	-0.003	0.038***	0.011	-0.051***	0.002	0.016	0.008**	0.020***
	(0.010)	(0.008)	(0.008)	(0.012)	(0.004)	(0.012)	(0.004)	(0.007)
Increase x Medium Change	0.012	0.003	-0.030*	0.019	-0.014	-0.002	-0.006	-0.022*
	(0.013)	(0.011)	(0.016)	(0.016)	(0.012)	(0.015)	(0.008)	(0.012)
Increase x Large Change	0.016	-0.006	-0.052***	0.023	-0.005	-0.014	-0.003	0.001
	(0.012)	(0.011)	(0.015)	(0.016)	(0.012)	(0.015)	(0.008)	(0.011)
Controls	No	No	No	No	No	No	No	No
Num.Obs.	6749	6749	6749	6749	6749	6749	6749	6749
R2 Adj.	0.046	0.035	0.079	0.026	0.136	0.065	0.035	0.004

Note: Table A.4 presents the coefficient estimates and robust standard errors for our main specification (Equation (1)) **without controls**, estimated using OLS. The dependent variable represents the share allocated to each category. The key explanatory variables include Increase_i, which indicates whether a firm was subject to a tax increase treatment, as well as Medium Change_i and Large Change_i, which capture the magnitude of the tax change (10% and 25%, respectively). Robust standard errors are given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Asymmetry and Magnitude Effects - With Controls

	Wages/Salaries	Employment	Distributed Profits	Reserves	Prices	Investment	Tax Planning	Other
Constant	0.185***	0.103***	0.131***	0.257***	0.018***	0.243***	0.012***	0.023***
	(0.009)	(0.008)	(0.009)	(0.012)	(0.006)	(0.011)	(0.004)	(0.007)
Increase	-0.099***	-0.062***	0.175***	-0.090***	0.166***	-0.122***	0.052***	0.020***
	(0.009)	(0.007)	(0.011)	(0.012)	(0.009)	(0.011)	(0.005)	(0.007)
Medium Change	0.003	0.025***	0.000	-0.045***	0.005	0.010	0.007*	0.036***
	(0.010)	(0.008)	(0.008)	(0.012)	(0.004)	(0.012)	(0.004)	(0.008)
Large Change	-0.003	0.037***	0.011	-0.051***	0.002	0.016	0.009**	0.020***
	(0.010)	(0.008)	(0.008)	(0.012)	(0.004)	(0.012)	(0.004)	(0.007)
Increase x Medium Change	0.013	0.003	-0.026*	0.019	-0.015	-0.004	-0.007	-0.022*
	(0.013)	(0.011)	(0.015)	(0.016)	(0.012)	(0.015)	(0.008)	(0.012)
Increase x Large Change	0.015	-0.005	-0.049***	0.022	-0.007	-0.014	-0.004	0.000
	(0.012)	(0.010)	(0.015)	(0.016)	(0.012)	(0.015)	(0.008)	(0.011)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Num.Obs.	6749	6749	6749	6749	6749	6749	6749	6749
R2 Adj.	0.054	0.043	0.104	0.030	0.144	0.073	0.035	0.009

Note: Table A.5 presents the coefficient estimates and robust standard errors for our main specification (Equation (1)) **with controls**, estimated using OLS. The dependent variable represents the share allocated to each category. The key explanatory variables include Increase_i, which indicates whether a firm was subject to a tax increase treatment, as well as Medium Change_i and Large Change_i, which capture the magnitude of the tax change (10% and 25%, respectively). Controls include: economic sector (Manufacturing, Construction, Trade, and Services), legal form, firm size and percentage change in revenues or net income due to COVID-19. Robust standard errors are given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.7.2 Multivariate Fractional Logit Estimation

In addition to estimating Equation (1) using OLS, we employ an alternative estimation method to assess the robustness of our results. This second approach accounts for the fractional nature of our response variables, whereas OLS estimation of Equation (1) disregards both the bounded nature of the outcome variables and the unit-sum constraint.

To enhance clarity, we start by describing the system of equations relevant in our experimental design. Following Mullahy (2015), let $\boldsymbol{y} \equiv (\boldsymbol{y}_1, \ldots, \boldsymbol{y}_M)$ denote the $N \times M$ matrices of outcomes, where $y_{im} \in [0, 1]$ denotes the share in percent attributed to category m in company i, N the number of firms in the sample, and M the number of categories. Letting $\boldsymbol{X}$ denote the $N \times K$ matrix of additional covariates, we can characterize the system of share equations as

$$E[y_{im}|\boldsymbol{X}] = G_m(\boldsymbol{X}; \boldsymbol{\beta}) \in (0, 1), \ m = 1, \ldots, M \tag{11}$$

$$\sum_{m=1}^{M} y_{im} = 1, \ i = 1, \ldots, N, \tag{12}$$

$$\Pr(y_{im} = 0|X) > 0 \ \forall m = 1, \ldots, M, \tag{13}$$

$$\Pr(y_{im} = 1|X) > 0 \ \forall m = 1, \ldots, M, \tag{14}$$

where $\boldsymbol{\beta} = (\boldsymbol{\beta}_1, \ldots, \boldsymbol{\beta}_2)$ is a $K \times M$ vector of parameters and $G_m(\boldsymbol{X}; \boldsymbol{\beta})$ a parametric conditional mean function. Equation (11) signifies the bounded nature of our outcome variables. Note that the condition as stated precludes the case in which one share obtains a boundary value $\mu \in \{0, 1\}$ for some combination of covariates $\boldsymbol{X}$.[100] Equation (12) is the unit-sum constraint, stemming from the fact that, by construction, the shares of different categories need to sum to one for each firm in the sample. Equations (13) and (14) illustrate that individual shares might attain boundary values with non-trivial probabilities, which

[100] The fringe case where a share would obtain a boundary value for all combinations of covariates is not particularly interesting for further analysis and not a concern in our setting.

requires special care when choosing the correct econometric specification. Taken together, the four equations characterize our data structure as so-called compositional or **multivariate fractional response data**. Our main interest lies in estimating the parameters $\boldsymbol{\beta}$ of the conditional mean functions $G_m(\boldsymbol{X};\boldsymbol{\beta})$.

Like mentioned above, in our baseline specification (i.e., OLS estimation), we ignore the bounded nature of our outcome variables (Equation (11)) as well as the unit-sum constraint (Equation (12)) and assume a linear conditional mean function for each category m. Ignoring the underlying restrictions of our data set has two main potential drawbacks, as pointed out, e.g., by Mullahy (2015) or Murteira and Ramalho (2016). First, similar to a linear probability model, predicted shares are not guaranteed to fall in the interval $[0, 1]$ for all combinations of covariates, and do not necessarily sum to one. Second, the model might misrepresent the partial effects of covariates.

Because of the aforementioned shortcomings of the linear model (OLS) and to check for the robustness of our results when accounting for these shortcomings, we also consider an alternative specification for the conditional mean functions $G_m(\boldsymbol{X};\boldsymbol{\beta}), m = 1, \ldots, M$. Following Mullahy (2015), we specify the M conditional means to have a multivariate logit functional form given as

$$E[y_{im}|\boldsymbol{X}] = G_m(\boldsymbol{X};\boldsymbol{\beta}) = \frac{\exp(\boldsymbol{x}_i'\boldsymbol{\beta}_m)}{\sum_{l=1}^{M}\exp(\boldsymbol{x}_i'\boldsymbol{\beta}_l)},\ m = 1, \ldots, M. \tag{15}$$

The linear specification for the index, $\boldsymbol{x}_i'\boldsymbol{\beta_m}$, is defined analogous to our main OLS specification in Equation (1). As for the conventional multinomial logit model, the parameters of the conditional mean functions $\boldsymbol{\beta}$ are not identified without imposing a normalization restriction. We choose investment as the reference category. Suppose without loss of generality that category M is the *investment* category. That way, we can rewrite the conditional

means as

$$E[y_{im}|\boldsymbol{X}] = G_m(\boldsymbol{X};\boldsymbol{\beta}) = \frac{\exp(\boldsymbol{x}_i'\boldsymbol{\delta}_m)}{1+\sum_{l=1}^{M-1}\exp(\boldsymbol{x}_i'\boldsymbol{\delta}_l)},\ m = 1,\ldots,M, \tag{16}$$

where $\delta_m \equiv \beta_m - \beta_M$. Interpretation of signs and magnitudes of the estimated δ coefficients is in general not straightforward. Far more useful in our context, where we want to compare the results of the multivariate fractional logit model with the OLS estimates, are the average partial effects resulting from the model, which are invariant to the selected normalization procedure. The average partial effects for the multivariate fractional logit model, when considering a dummy variable, are given by

$$\begin{aligned}
\hat{APE}_{mk} &= \frac{1}{N}\sum_{i=1}^{N}\hat{PE}_{mki} \\
&= \frac{1}{N}\sum_{i=1}^{N}\frac{\Delta E[y_{im}|\boldsymbol{x}_i]}{\Delta x_{ik}} \\
&= \frac{1}{N}\sum_{i=1}^{N}\frac{\exp(\boldsymbol{x}'_{-k,i}\boldsymbol{\beta}_{m,-k}+\beta_{mk})}{1+\sum_{l=1}^{M-1}\exp(\boldsymbol{x}'_{-k,l}\boldsymbol{\beta}_{l,-k}+\beta_{lk})} - \frac{\exp(\boldsymbol{x}'_{-k,i}\boldsymbol{\beta}_{m,-k})}{1+\sum_{l=1}^{M-1}\exp(\boldsymbol{x}'_{-k,l}\boldsymbol{\beta}_{l,-k}+\beta_{lk})},
\end{aligned} \tag{17}$$

where $\Delta x_{ik} = 1$ and $\boldsymbol{x}_{-k,i}$ denotes the vector of explanatory variables for observation i excluding variable k.

Table A.6 compares the average partial effects estimated using OLS and the Multivariate Fractional Logit (MFL) model across different treatment conditions. The comparison focuses on three key contrasts: (i) increases versus decreases, (ii) medium changes (10%) versus small changes (1%), and (iii) large changes (25%) versus small changes (1%). The results show that the estimated effects are largely consistent across both models, reinforcing the robustness of our main findings.

Table A.6: Comparison of Average Partial Effects.

	Increase vs. Decrease		Medium vs. Small Change		Large vs. Small Change	
	OLS	FMLOGIT	OLS	FMLOGIT	OLS	FMLOGIT
Wages/Salaries	-0.099(0.009)***	-0.092(0.005)***	0.009(0.006)	0.006(0.006)	0.005(0.006)	0.002(0.006)
Employment	-0.061(0.007)***	-0.064(0.004)***	0.026(0.005)***	0.026(0.005)***	0.035(0.005)***	0.036(0.005)***
Distributed Profits	0.175(0.012)***	0.146(0.006)***	-0.014(0.008)	-0.016(0.008)	-0.016(0.008)	-0.016(0.008)
Reserves	-0.090(0.012)***	-0.078(0.006)***	-0.035(0.008)***	-0.040(0.008)***	-0.039(0.008)***	-0.044(0.008)***
Prices	0.166(0.009)***	0.158(0.005)***	-0.002(0.006)	-0.005(0.006)	-0.001(0.006)	-0.002(0.006)
Investment	-0.122(0.011)***	-0.128(0.006)***	0.008(0.007)	0.002(0.007)	0.009(0.007)	0.001(0.007)
Tax Planning	0.052(0.005)***	0.049(0.003)***	0.003(0.004)	0.004(0.004)	0.007(0.004)	0.007(0.004)
Other	0.020(0.007)**	0.010(0.005)*	0.025(0.006)***	0.023(0.006)***	0.020(0.006)***	0.016(0.006)**

Note: Table A.6 compares the average partial effects from our preferred OLS specification (Equation (1)) with the FMLOGIT specification, which takes into account the fractional nature of our response variables as well as their interdependency. The comparison examines three main contrasts: (i) increases versus decreases, (ii) medium changes (10%) against small changes (1%), and (iii) large changes (25%) against small changes (1%). Robust standard errors are given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.8 Robustness Tests - Heterogeneity

In Section 2.4 of the main text, we examine treatment effect heterogeneity across firm size, economic sector, legal structure, and net income impact from COVID-19. In this section, we extend the analysis from Section 2.4 by incorporating all relevant firm heterogeneity characteristics into a single estimation model, thereby controlling for other firm characteristics when testing effect heterogeneity along a specific margin. We assess variation in incidence using the following OLS regression

$$y_i = \beta_0 + \beta_1 \text{ Increase}_i + \boldsymbol{\gamma}_1' \, \boldsymbol{x}_i^* + \boldsymbol{\gamma}_2' \text{ Increase}_i \times \boldsymbol{x}_i^* + \varepsilon_i, \tag{18}$$

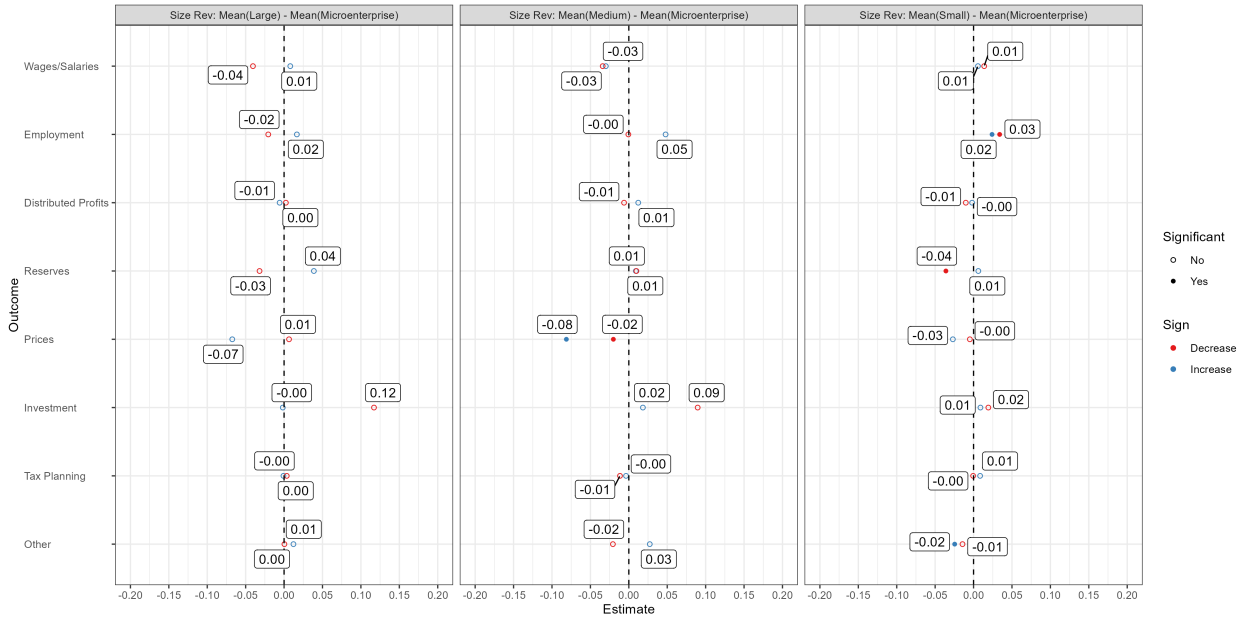
where $\boldsymbol{x}_i^*$ represents a vector of firm characteristics: dummies for firm size, economic sector, legal structure, and net income impact from COVID-19.[101] As in Specification 2, we aggregate the treatment intensity groups and classify treatment solely based on whether an individual firm was part of a tax increase treatment or not, as indicated by Increase_i, a dummy variable. Since treatment intensity was randomly assigned and is, therefore, uncorrelated with firm characteristics, this approach should not introduce bias.

Figure A.15 (firm size), Figure A.16 (economic sector), Figure A.17 (legal form), and Figure A.18 (net income impact from COVID-19) display the average partial effects for each comparison with the baseline, based on the estimated coefficients from Equation (18). Significant average partial effects, determined using the Benjamini and Yekutieli (2001) correction, are represented by filled dots, while insignificant effects are shown as hollow circles.

[101] We measure firm size based on reported 2019 revenue, following the European Commission's classification: micro-enterprises (<EUR 2 million), small (<EUR 10 million), medium (<EUR 50 million), and large (≥ EUR 50 million). For economic sector classification, firms self-reported their industry in the survey, and we assign them to manufacturing, construction, trade, or services, with all others categorized as *other*. Legal forms are grouped into corporations, partnerships, and sole proprietors. Finally, we classify firms based on their self-reported impact from COVID-19. Respondents rated the effect on net income from *-100* to *+100*, and we define a dummy variable equal to one for firms below the median value.

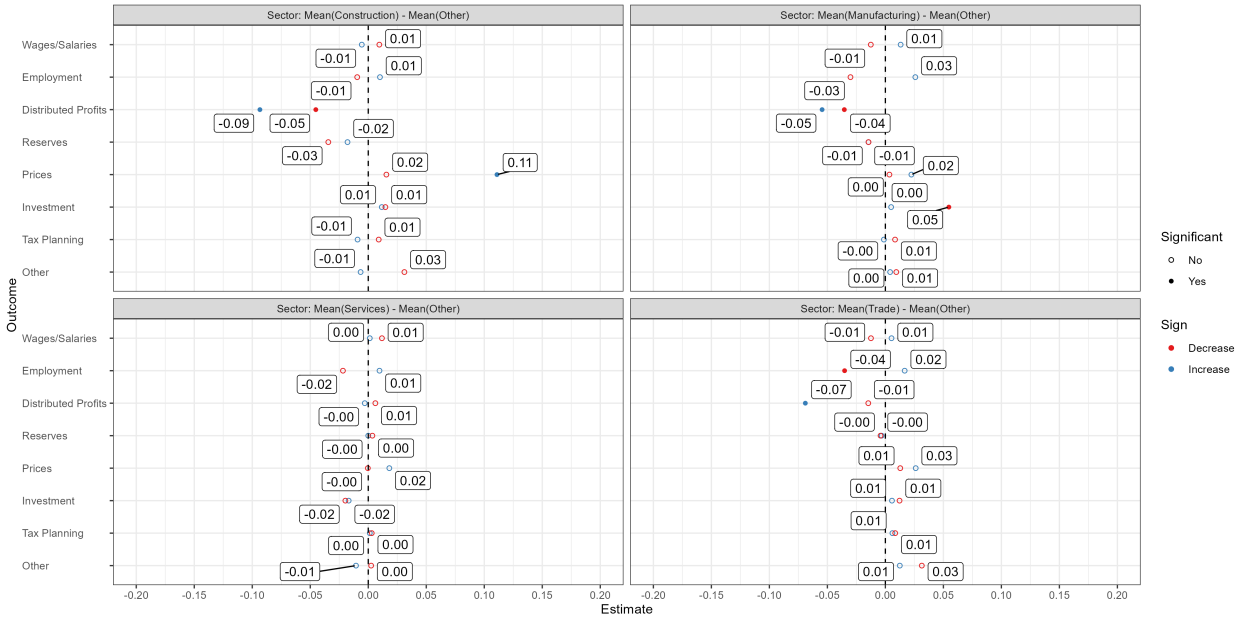
The results closely align with those in Section 2.4 for almost all heterogeneities, where we estimate separate OLS regressions for each of the four firm characteristics. The only exception is the net income impact of the COVID-19 pandemic, where the point estimates turn insignificant when including controls for other firm characteristics. This is likely due to correlation of the economic impact of the pandemic with industry and size.

Figure A.15: Average partial effects of size category with control variables.



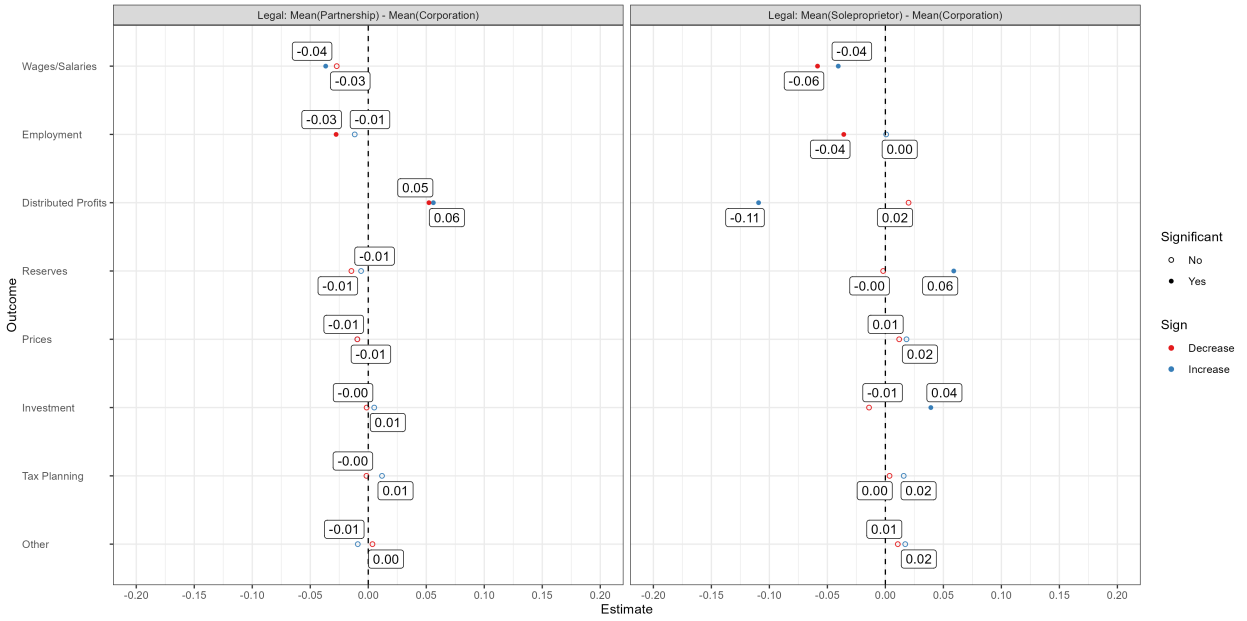
Note: Figure A.15 shows heterogeneity in incidence by company size measured by revenues. The figure shows average partial effects for each comparison with the baseline based on the estimated coefficients from Equation (18). Average partial effects with a significant p-value after applying the Benjamini and Yekutieli (2001) correction are denoted by filled dots, whereas insignificant effects are illustrated by hollow circles.

Figure A.16: Average partial effects of economic sector with control variables.



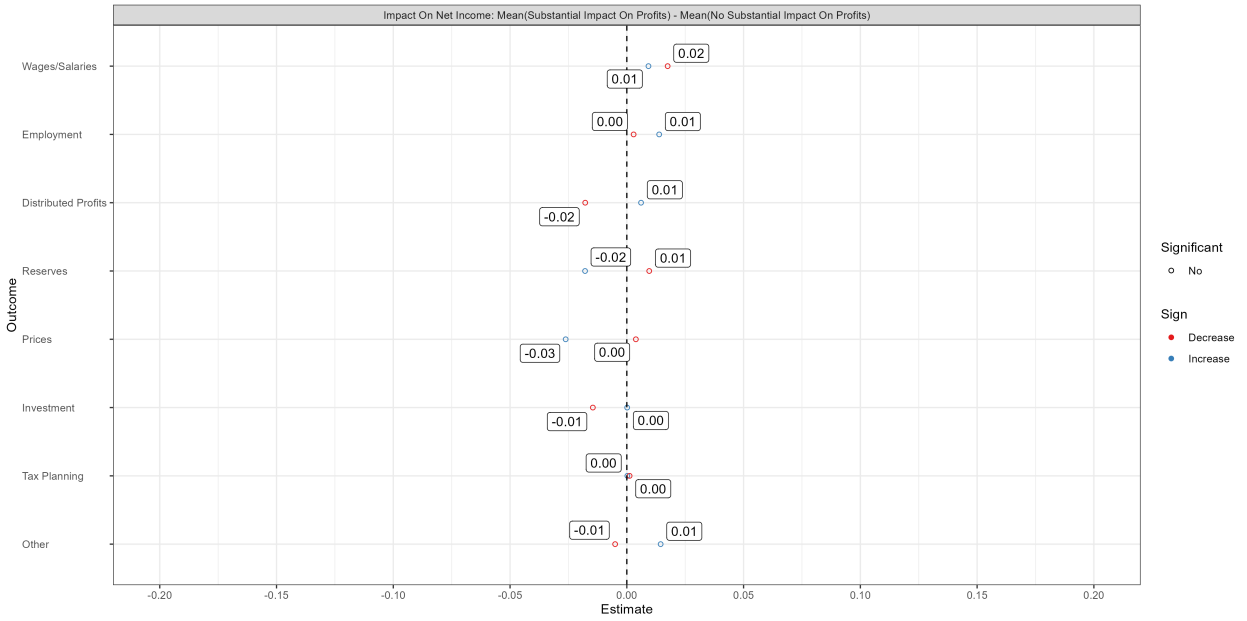
Note: Figure A.16 shows heterogeneity in incidence by economic sector. The figure shows average partial effects for each comparison with the baseline based on the estimated coefficients from Equation (18). Average partial effects with a significant p-value after applying the Benjamini and Yekutieli (2001) correction are denoted by filled dots, whereas insignificant effects are illustrated by hollow circles.

Figure A.17: Average partial effects of legal form with control variables.



Note: Figure A.17 shows heterogeneity in incidence by company legal form. The figure shows average partial effects for each comparison with the baseline based on the estimated coefficients from Equation (18). Average partial effects with a significant p-value after applying the Benjamini and Yekutieli (2001) correction are denoted by filled dots, whereas insignificant effects are illustrated by hollow circles.

Figure A.18: Average partial effects of COVID-19 impact on net income with control variables.



Note: Figure A.18 shows heterogeneity in incidence depending on whether the company was substantially impacted in its net income by COVID-19. The figure shows average partial effects for each comparison with the baseline based on the estimated coefficients from Equation (18). Average partial effects with a significant p-value after applying the Benjamini and Yekutieli (2001) correction are denoted by filled dots, whereas insignificant effects are illustrated by hollow circles.

A.9 Incidence Estimates in Previous Literature

Table A.7 provides a comprehensive summary of previous studies on tax incidence. In contrast to Table 5, which highlights selected recent studies, Table A.7 also incorporates corporate tax incidence estimates from earlier published research and working papers.

Table A.7: Tax Incidence Estimates

					Incidence on			
Paper	**Tax Variation**	**Tax Change**	**Country**	**Episode**	**Workers**	**Firm Owners**	**Consumers**	**Land Owners**
Arulampalam et al. (2012)	Cross-company differences in tax liability	Increases and Decreases (pooled)	Belgium, Finland, France, Germany, Italy, Netherlands, Spain, Sweden, UK	1996-2003	49% (long run) 64% (short run)	?	?	?
Azémar and Hubbard (2015)	Cross-country variation in the statutory corporate tax	Increases and Decreases (pooled)	13 OECD countries	1980–2004	60%	40%	?	?
Baker et al. (2023)	Variation in state corporate tax rates	Increases and Decreases (pooled)	USA	2006-2017	Primary spec.: 28% Alternative: 36%	Primary spec.: 20% Alternative: 21%	Primary spec.: 51% Alternative: 43%	0%
Carbonnier et al. (2022)	Large French corporate income tax credit	Decrease	France	2009-2015	50% Range: 40%-60%	50%	?	?
Carroll (2009)	Variation in states' corporate taxes	Increases and Decreases (pooled)	USA	1970-2007	>100% (i.e. 250%)	?	?	?
Desai et al. (2007)	Cross-country differences in corporate taxes	Increases and Decreases (pooled)	52 countries	1989-2004	Baseline: 57%, Range: 45%-75%	Baseline: 43%, Range: 25%-55%	0%	0%
Dobridge et al. (2021)	Variation in the Domestic Production Activities Deduction	Decrease	USA	1999-2015	80%	20%	0%	0%
Duan and Moon (2024)	Corporate tax cuts	Decrease	Canada	2001-2017	73%, owner-workers: 39%	27%	?	?
Dwenger et al. (2019)	Federal tax cut/Variation in effective corporate tax burden	Decrease	Germany	1998–2006	19% (long-run) - 28% (short-run)	?	?	?
Felix and Hines (2022)	State tax changes	Variation between unionized and non-unionized workers	USA	2000	31% (fully unionized firm)	?	?	?
Felix (2007)	Variation in Corporate tax rate	Increases and Decreases (pooled)	30 countries	1979–2002	>100% Range: 235%-620%	?	?	?
Felix (2009)	Variation in states' corporate taxes	Increases and Decreases (pooled)	USA	1977-2005	>100%, Gravelle (2011): 141%-360%	?	?	?

(Table continues on the next page)

Paper	Tax Variation	Tax Change	Country	Episode	Incidence on			
					Workers	Firm Owners	Consumers	Land Owners
Fuest et al. (2018)	Variation in local business tax changes	Increases (93% Increases)	Germany	1993-2012	51%	49%	0%	0%
Hassett and Mathur (2006)	Cross-country variation in corporate tax rate	Increases and Decreases (pooled)	72 countries	1981-2003	>100%, Gravelle (2011): 630%	?	?	?
Hassett and Mathur (2015)	Cross-country variation in the statutory corporate tax	Increases and Decreases (mostly decreases)	66 countries	1981–2005	50%	?	?	?
Jacob et al. (2023)	Variation in local business tax rate	Increase	Germany	2014–2017	0%	36% Range: 28%-39%	64% Range: 61%-72%	0%
Kennedy et al. (2024)	US corporate tax change (TCJA)	Decrease	USA	2013-2019	48%	51%	0%	0%
Liu and Altshuler (2013)	Variation in Corporate Income Tax across industry and time	Increases and Decreases (pooled)	USA	1982, 1992, 1997	60%, Lower bound: 42%	40%, Upper bound: 58%	?	?
Ohrn (2023)	Federal corporate tax break	Decrease	USA	1998–2012	17%-25% (Top-5 highest paid executives)	?	?	?
Risch (2024)	Change in top marginal personal tax rate in the United States	Increase	USA	2008-2016	11-18%	approx. 80%	0%	0%
Suárez Serrato and Zidar (2016)	Variation in US state taxes and apportionment rules	Increases and Decreases (pooled)	USA	1980–2012	30-35%	40%	0%	25-30%
Suárez Serrato and Zidar (2023)	Variation in US state taxes and apportionment rules	Increases and Decreases (pooled)	USA	1980–2012	35%	38.1%	0%	26.8%
Suárez Serrato and Zidar (2024)	Variation in US state taxes and apportionment rules	Increases and Decreases (pooled)	USA	1980–2012	25-40%	50%	0%	10-25%

Note: Table A.7 summarizes previous estimates of tax incidence found in the literature on workers, capital/firm owners, consumers, and land owners. ?: Indicates that no information on the incidence for this group was given.

Chapter 2 Appendix References

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B Appendix to Chapter 3

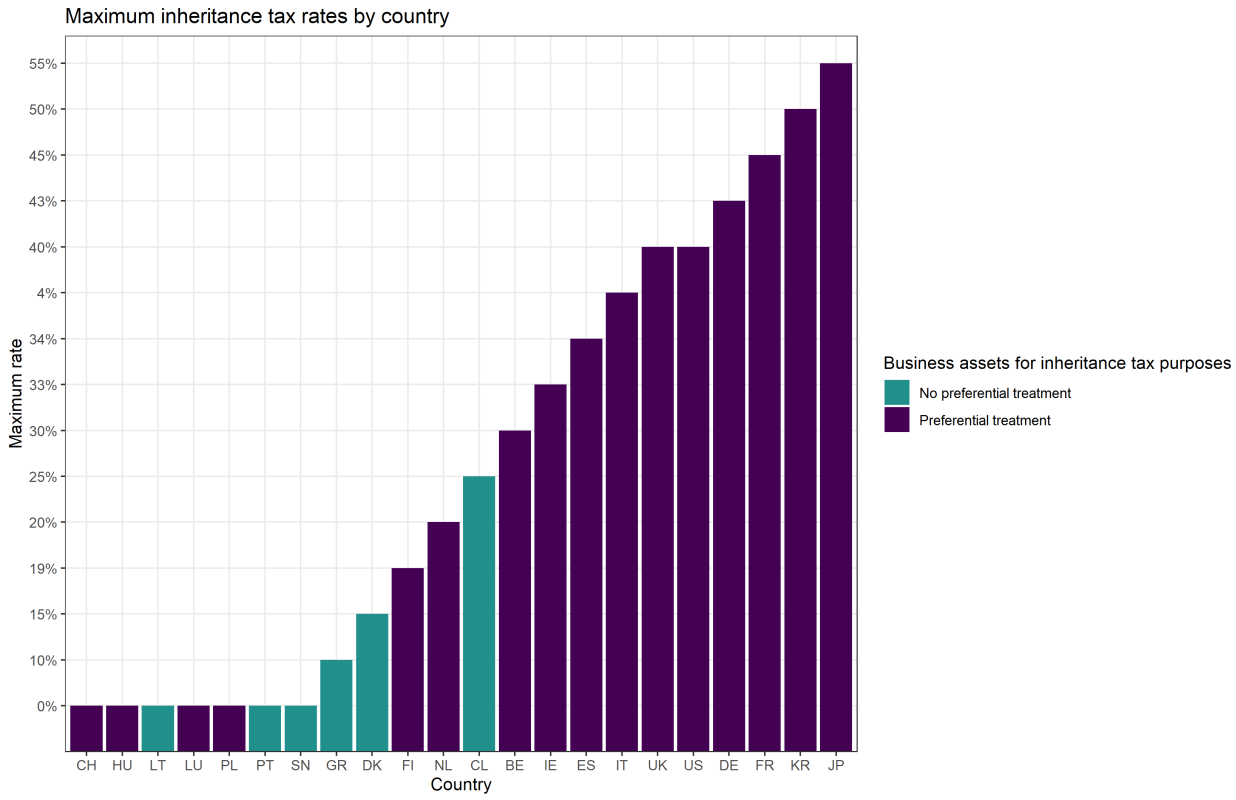
B.1 Additional Tables and Figures

Table B.1: Inheritance and Gift Tax Schedules under the Different Regimes

Taxable bequests (EUR 1,000)	before 2009			Taxable bequests (EUR 1,000)	in 2009			since 2010		
	Tax Class				Tax Class					
	I	II	III		I	II	III	I	II	III
52	7	12	17	75	7	30	30	7	15	30
256	11	17	23	300	11	30	30	11	20	30
512	15	22	29	600	15	30	30	15	25	30
5,113	19	27	35	6,000	19	30	30	19	30	30
12,783	23	32	41	13,000	23	50	50	23	35	50
25,565	27	37	47	26,000	27	50	50	27	40	50
$\geq$ 25,565	30	40	50	$\geq$ 26,000	30	50	50	30	43	50

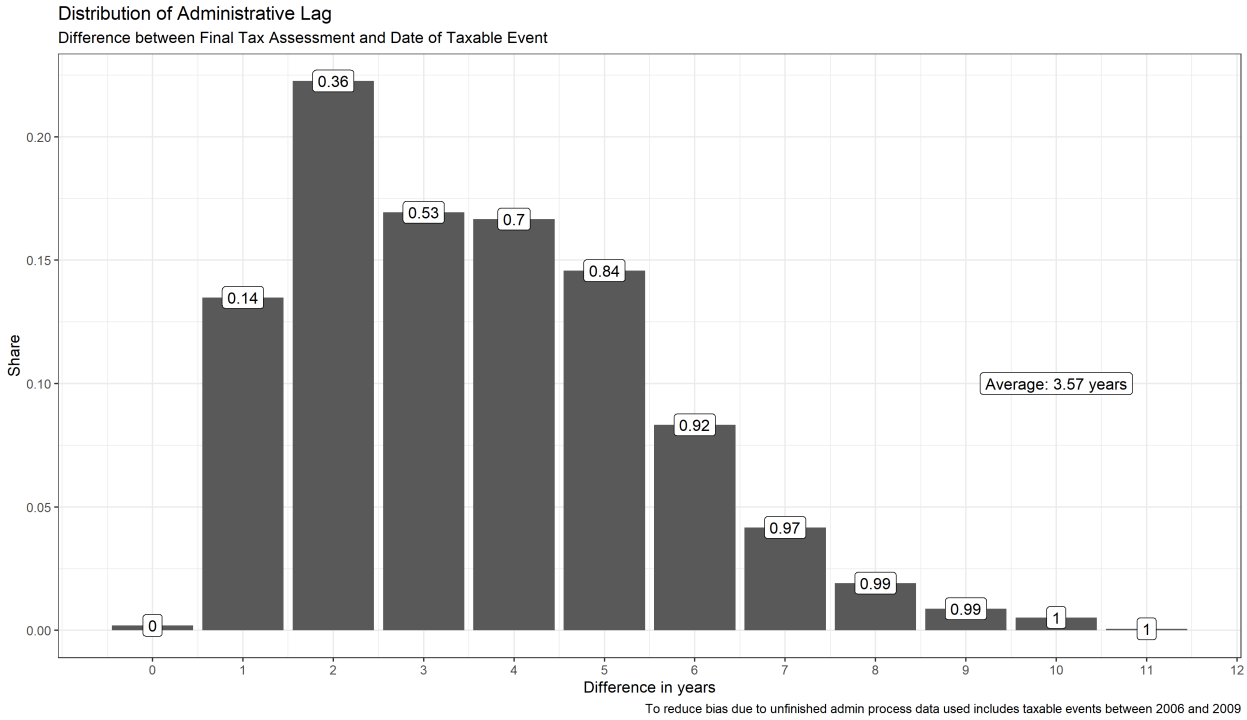
Note: Table B.1 displays the (progressive) tax rate schedule for gifts and inheritances during three different periods of German Tax Law, out of which the tax rates after the onset of 2010 are most relevant to our empirical setting. Tax classes generally relate to the degree of kinship (with I denoting close family and III non-related recipients), albeit a receipt of assets that are treated preferentially (i.e. business assets, (closely held) company shares and agricultural assets) is by law tantamount to being a recipient within tax class I. Troll/Gebel/Jülicher/Gottschalk: ErbStG, "X. Reform der ErbSt 2009" and "XI. Entwicklung der ErbSt von 2009 bis 2016", 2021.

Figure B.1: International Wealth Transfer Tax Regimes



Note: Figure B.1 displays the depicted maximum inheritance tax rates when recipients are close family members, with purple (green) colour fill as an indication that business assets are (not) treated preferentially. Tax rates to third parties can be higher.OECD (2021).

Figure B.2: Structure of Coverage between First and Subsequent Tax Determination Dates



Note: Figure B.2 displays the distribution of (yearly) tax assessment dates of gift and tax returns relative to the year of the taxable event. Authors' calculations based on data from the RDC of the Federal Statistical Office and the Statistical Offices of the Federal States.

Table B.2: Descriptive Statistics of Bequest Sample

	Obs.	Mean	Std. Dev.	P01	P50	P99
Agricultural property						
Overall value of receipt	10,389	186.06	653.35	3	78	1,389
Age of Recipient at Transfer	10,380	58.05	15.81	16	58	89
Above Allowance	10,389	0.78	0.41	0	1	1
Minor Recipient	10,380	0.01	0.12	0	0	1
Son	10,389	0.07	0.26	0	0	1
Daughter	10,389	0.05	0.22	0	0	1
Female Giver	10,389	0.27	0.45	0	0	1
Effective tax rate (p.p.)	4,681	6.62	7.61	0	4	27
Share of fav. assets (p.p.)	4,543	41.29	39.04	0	33	100
Count of recipients per transfer	4,681	2.22	2.40	1	1	13
Business assets						
Overall value of receipt	14,212	2,726.53	22,511.47	14	689	35,385
Age of Recipient at Transfer	14,197	51.29	17.26	9	52	87
Above Allowance	14,212	0.82	0.38	0	1	1
Minor Recipient	14,197	0.04	0.20	0	0	1
Son	14,212	0.29	0.45	0	0	1
Daughter	14,212	0.22	0.42	0	0	1
Female Giver	14,212	0.28	0.45	0	0	1
Effective tax rate (p.p.)	7,019	3.27	5.79	0	0	27
Share of fav. assets (p.p.)	6,538	59.84	44.00	0	85	100
Count of recipients per transfer	7,019	2.02	1.62	1	2	8
Cash and financial assets						
Overall value of receipt	650,438	252.10	2,090.02	7	70	2,417
Age of Recipient at Transfer	649,181	60.46	15.96	16	61	91
Above Allowance	650,438	0.90	0.30	0	1	1
Minor Recipient	649,181	0.01	0.11	0	0	1
Son	650,438	0.06	0.23	0	0	1
Daughter	650,438	0.05	0.22	0	0	1
Female Giver	650,438	0.59	0.49	0	1	1
Effective tax rate (p.p.)	322,558	12.05	8.80	0	11	29
Share of fav. assets (p.p.)	43,241	31.40	44.15	0	0	100
Count of recipients per transfer	322,568	2.02	1.89	1	1	10
Company shares						
Overall value of receipt	3,688	4,919.16	49,672.84	10	891	43,916
Age of Recipient at Transfer	3,680	48.79	17.96	8	50	85
Above Allowance	3,688	0.84	0.37	0	1	1
Minor Recipient	3,680	0.05	0.22	0	0	1

Table B.2: Descriptive Statistics of Bequest Sample Continued

Son	3,688	0.28	0.45	0	0	1
Daughter	3,688	0.24	0.42	0	0	1
Female Giver	3,688	0.25	0.43	0	0	1
Effective tax rate (p.p.)	1,692	4.42	6.83	0	1	29
Share of fav. assets (p.p.)	1,576	52.65	44.35	0	74	100
Count of recipients per transfer	1,692	2.18	1.66	1	2	8
Real estate						
Overall value of receipt	333,020	254.78	1,252.19	6	94	2,052
Age of Recipient at Transfer	332,643	57.58	16.12	15	58	89
Above Allowance	333,020	0.83	0.37	0	1	1
Minor Recipient	332,643	0.02	0.12	0	0	1
Son	333,020	0.09	0.28	0	0	1
Daughter	333,020	0.08	0.27	0	0	1
Female Giver	333,020	0.53	0.50	0	1	1
Effective tax rate (p.p.)	177,924	11.21	9.04	0	10	29
Share of fav. assets (p.p.)	37,474	34.14	45.59	0	0	100
Count of recipients per transfer	177,927	1.87	1.62	1	1	8

Note: Table B.2 shows descriptive statistics for the sample of bequests after our selection process detailed in Table 13. Overall receipts are expressed in Thousand Euros. Above Allowance is an indicator for a transfer above the personal allowance of the recipient, Minor Recipient is a dummy variable equal to one if the recipient is below 18 years old at the time of transfer. Son and Daughter are indicator variables indicating the recipient gender and relation of the recipient to the donor. Female Giver is an indicator equal to one if the bequestor is female. Summary statistics are given for each asset type separately. For comparison purposes we also show asset classes that are not the main focus of our analysis, namely, cash and financial assets as well as real estate.

Authors' calculations based on data from the RDC of the Federal Statistical Office and the Statistical Offices of the Federal States.

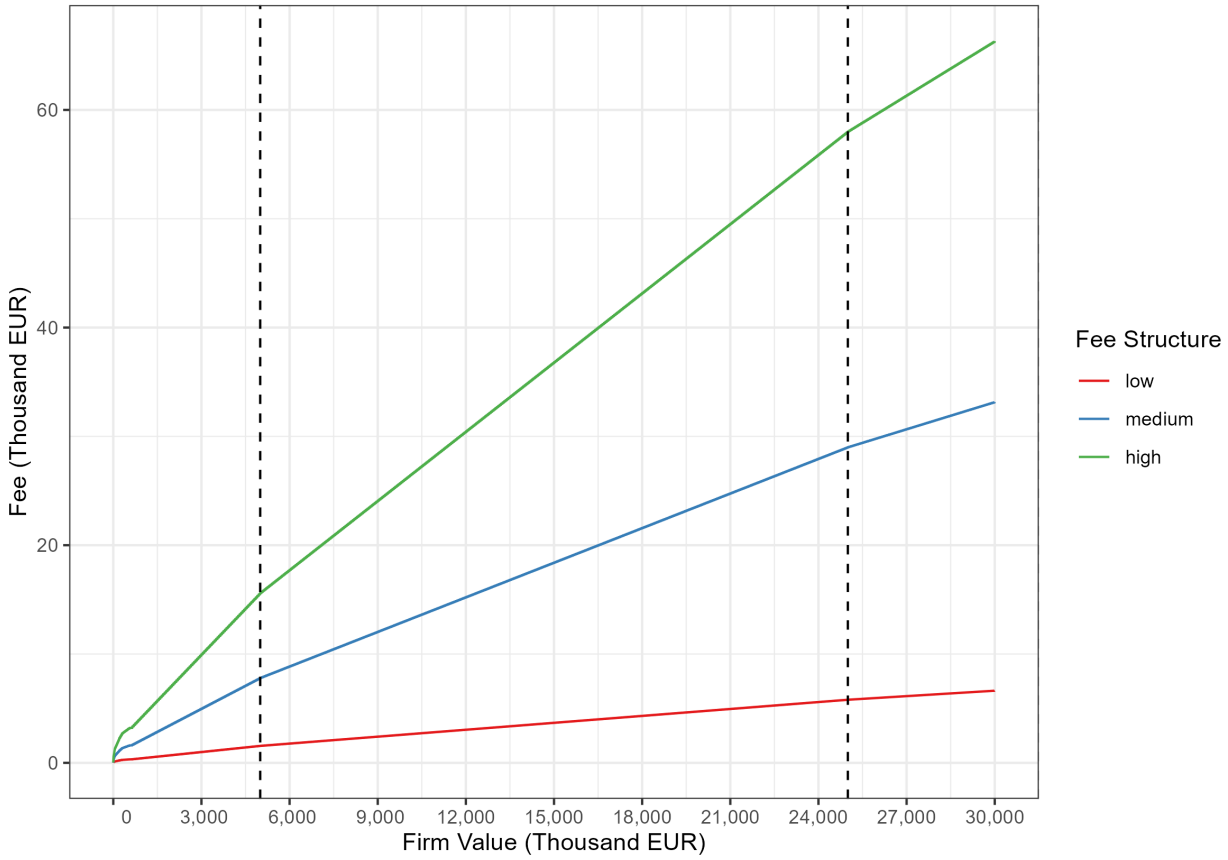
Table B.3: Calculation of Foregone Tax Revenue

Scenario	Actual tax	Counterfactual Scenario	Δ Revenue	Excess Mass	Share Avoiders	Foregone Revenue
Panel A: Event Window I (2012)						
75% exempted	93,031.99	3,322,189.30	3,229,089.10	9.02	0.89	2,873,889.30
50% exempted	93,031.99	6,723,778.00	6,630,579.60	9.02	0.89	5,901,215.84
25% exempted	93,031.99	10,145,452.00	10,052,155.00	9.02	0.89	8,946,417.95
0% exempted	93,031.99	13,609,604.00	13,516,209.00	9.02	0.89	12,029,426.01
10% flat tax	93,031.99	4,330,831.50	4,237,697.00	9.02	0.89	3,771,550.33
12.5% flat tax	93,031.99	5,564,546.10	5,471,339.20	9.02	0.89	4,869,491.89
15% flat tax	93,031.99	6,798,260.70	6,704,981.50	9.02	0.89	5,967,433.54
Panel B: Event Window II (2014)						
75% exempted	79,900.50	610,556.56	530,656.06	5.62	0.82	435,137.97
50% exempted	79,900.50	1,223,944.00	1,144,043.50	5.62	0.82	938,115.67
25% exempted	79,900.50	1,880,818.50	1,800,918.00	5.62	0.82	1,476,752.76
0% exempted	79,900.50	2,542,909.30	2,463,008.80	5.62	0.82	2,019,667.22
10% flat tax	79,900.50	945,278.86	865,378.37	5.62	0.82	709,610.26
12.5% flat tax	79,900.50	1,211,968.80	1,132,068.30	5.62	0.82	928,296.01
15% flat tax	79,900.50	1,478,658.70	1,398,758.20	5.62	0.82	1,146,981.72

Note: Table B.3 illustrates our calculations of foregone tax revenue for the different scenarios. Panel A displays the calculation steps for Event Window I, whereas Panel B displays the same for Event Window II. Each panel shows the overall taxes paid for the transfers within the respective bunching windows, which tax revenue would have been collected in the respective hypothetical scenario as well as the difference between counterfactual and actual revenue. Multiplying the difference in tax revenue with the share of avoiders, calculated as $(\hat{b}-1)/\hat{b}$, where $\hat{b}$ is the estimated excess mass of transfers in the bunching window, yields an estimate for the foregone tax revenue. To put the numbers into perspective, we relate the foregone revenue to the overall volume of collected inheritance and gift tax in 2011, which amounts to EUR 4,221,122 Thousand.

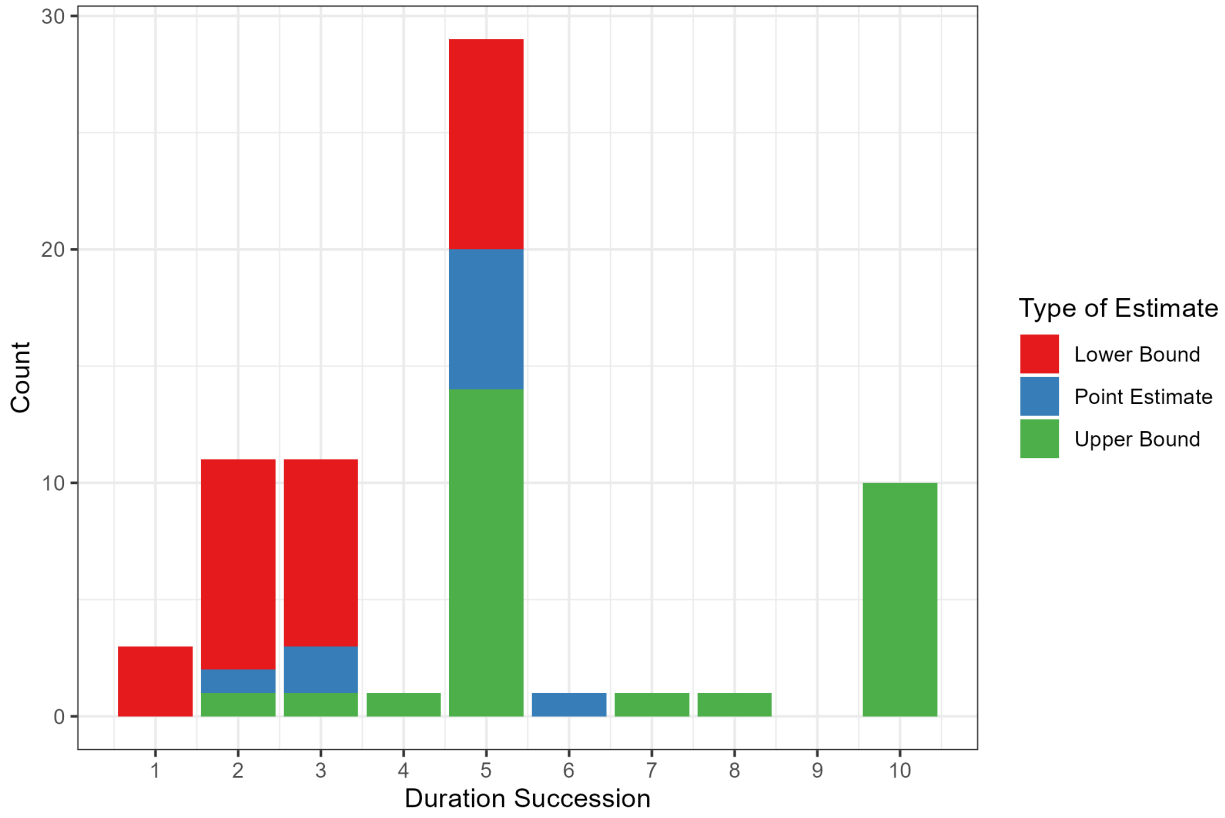
Authors' calculations based on data from the RDC of the Federal Statistical Office and the Statistical Offices of the Federal States.

Figure B.3: Tax Advisory Fee Schedule based on Transfer Values



Note: Figure B.3 depicts the absolute amounts of tax advisory fee in EUR based on the cost of preparing a tax declaration for gifts or inheritances. The depicted amounts can be reduced by a factor of up to 90% on the discretion of the tax advisor. German tax advisor fee regulation (*Steuerberatervergütungsverordnung*), Annex 1, Table A.

Figure B.4: Recommended duration of business successions

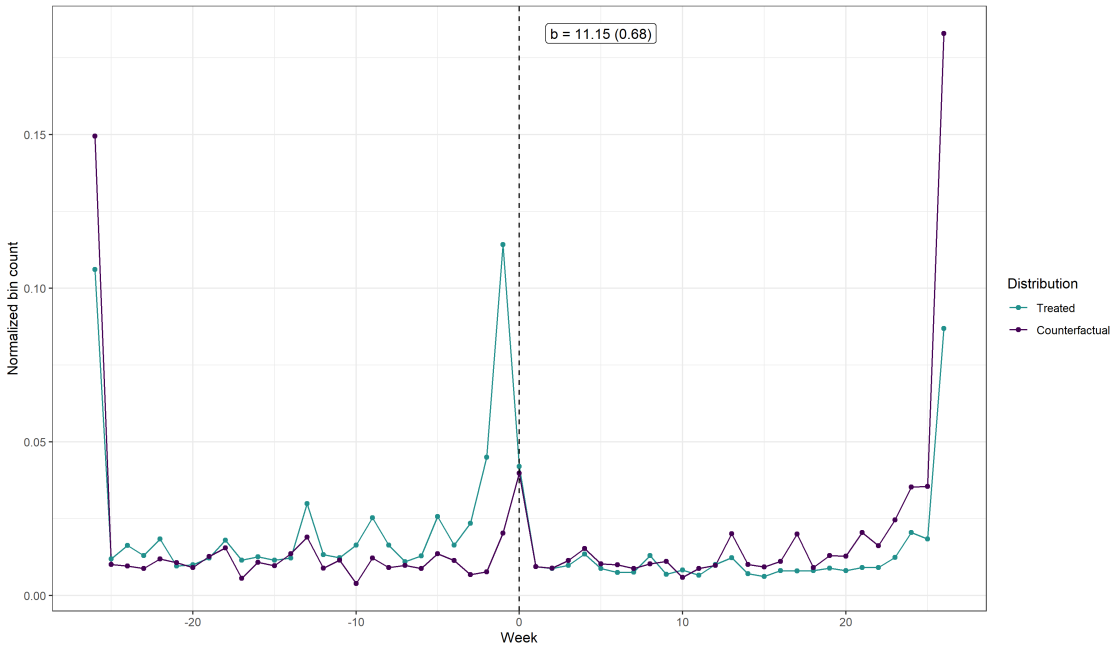


Note: Figure B.4 shows the recommendation count for a specific duration period of intergenerational business succession by all German chambers of industry and commerce. Out of overall 79 chambers, 36 provided specific recommendations about the ideal succession duration period on their websites. Often, these recommendations relate to a time span, which mean that multiple years are optimal from the viewpoint of the guidelines. This results in the sum of counts displayed in the figure being larger than 36. Websites of all (regional) German chambers of industry and commerce (*Industrie- und Handelskammern*), accessed in early September 2024.

B.2 Additional Event Analysis

Event Window III: The 2016 reform. As a final exercise we estimate the excess mass for the retroactive reform implementation date on 1 July 2016 which is illustrated in Figure B.5. Also the third event window features sizeable bunching in the distribution immediately before the reform date. Interesting in that case is that there seems to be excess mass in the entire region on the left of the event window endpoint with missing mass in the distribution for the entire region to the right. As the distribution of transfers appears to be increased almost for the entire left-hand side, determination of the bunching window is rather difficult. We provide a lower-bound estimate by considering the excess mass within three weeks before the reform implementation. That way, we probably underestimate the true reaction as increasing the bunching window only increases the estimated excess mass. Nevertheless, we find strong and significant bunching even in the direct vicinity of the event date.

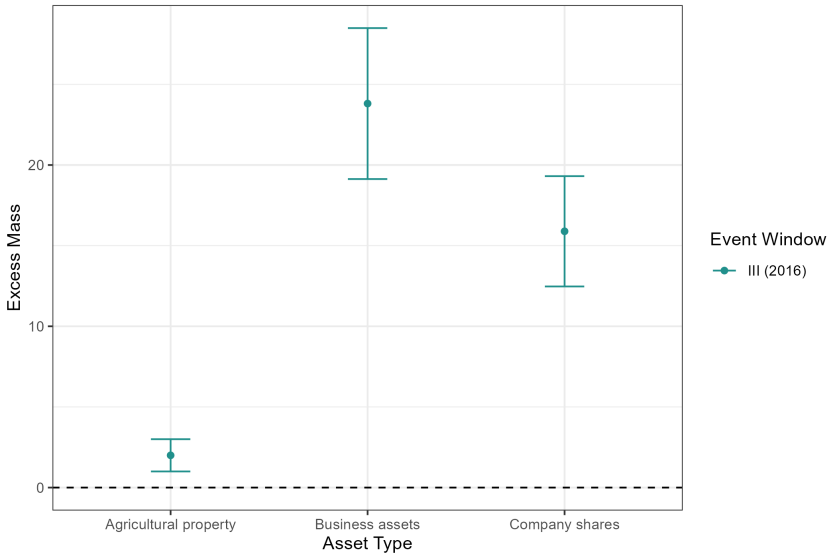
Figure B.5: Difference-in-Bunching around 2016 reform.



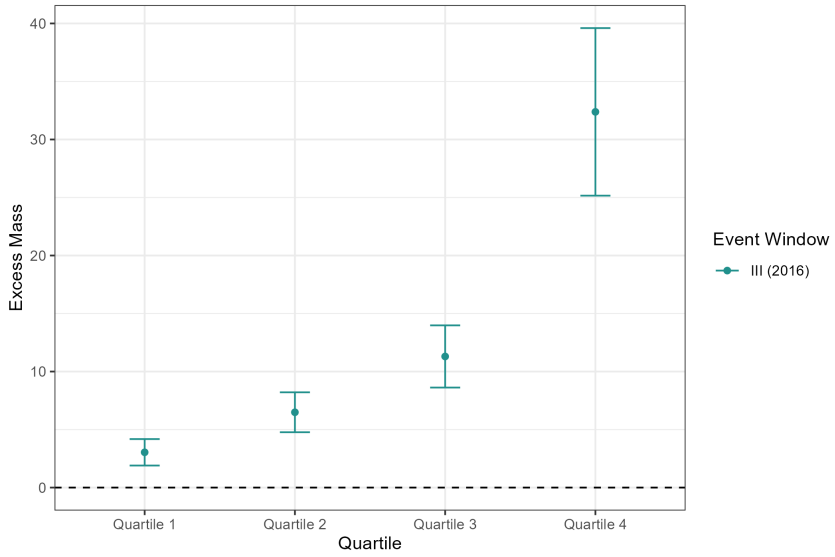
Note: Figure B.5 displays normalized weekly transfer counts for the year surrounding 30 June 2016, the retroactive implementation date of the last major reform of the German Inheritance and Gift Tax Law. The bins for each distribution is expressed as a share of overall transfers occurring around a one-year window around the event window endpoint. Weeks are centred around the end point, where week zero starts with the event date and includes the six days thereafter. All details are described in Section 3.4. The treated distribution for Figure B.5 includes transfers in a one-year window around 1 July 2016 whereas the counterfactual distribution comprises transfers in the same window around 1 January 2010 and 2011. The boxed number indicated the excess mass estimate in the three weeks before the event window deadline with bootstrapped standard errors in parentheses. Authors' calculations based on data from the RDC of the Federal Statistical Office and the Statistical Offices of the Federal States.

Figure B.6: Heterogeneity Excess Mass 2016 Reform

(a) Asset Class



(b) Wealth Quartile



Note: Figure B.6 shows excess mass estimates and two standard error confidence bounds for different sample splits for the mid-2016 reform. Figure B.6a shows excess mass estimates for the different preferentially treated asset classes (agricultural property, business assets and company shares) whereas Figure B.6b shows excess mass estimates for the different wealth quartiles. Authors' calculations based on data from the RDC of the Federal Statistical Office and the Statistical Offices of the Federal States.

C Appendix to Chapter 4

C.1 Additional Figures and Tables

Table C.1: Current Inheritance and Gift Taxation Overview

(a) Tax Rate Schedule

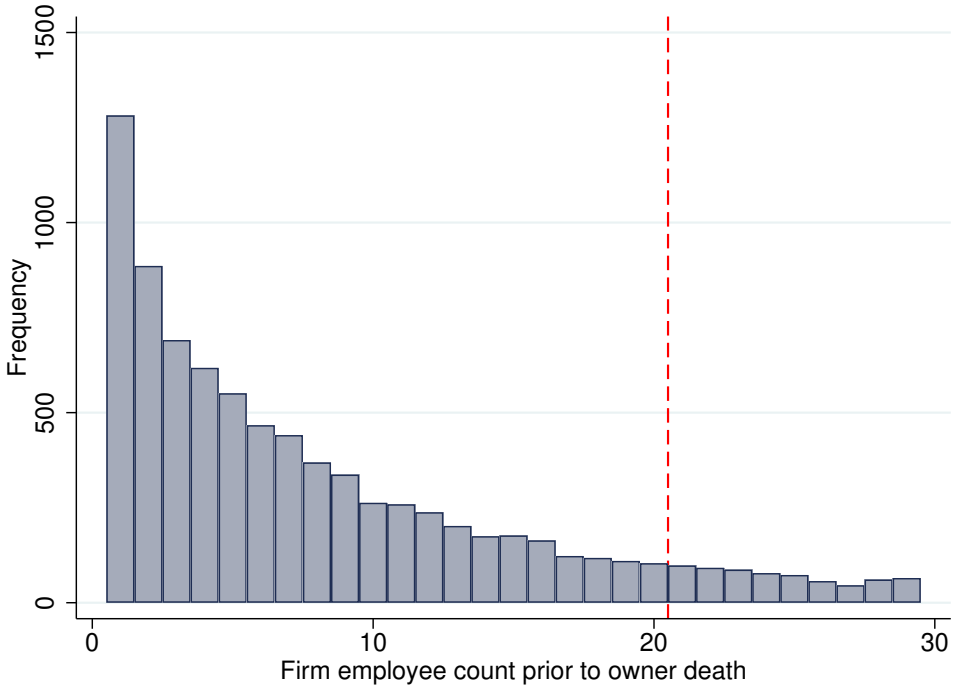
Taxable bequests (in EUR 1,000)	Tax Class I	Tax Class II	Tax Class III
75	7	15	30
300	11	20	30
600	15	25	30
6,000	19	30	30
13,000	23	35	50
26,000	27	40	50
$\geq$ 26,000	30	43	50

(b) Tax Allowances

Relational Degree	Tax Class	Value (in EUR 1,000)
Spouses	I	500
Children	I	400
Grandchildren	I	200
Parents & Grandparents	I	100
Siblings, Divorcées	II	20
Unrelated	III	20

Note: Table C.1 shows the applicable inheritance and gift tax rates (Table C.1a) and the different allowances based on the type of relation between donor and recipient (Table C.1b) during our period of interest.

Figure C.1: Visual Verification of Threshold Manipulation



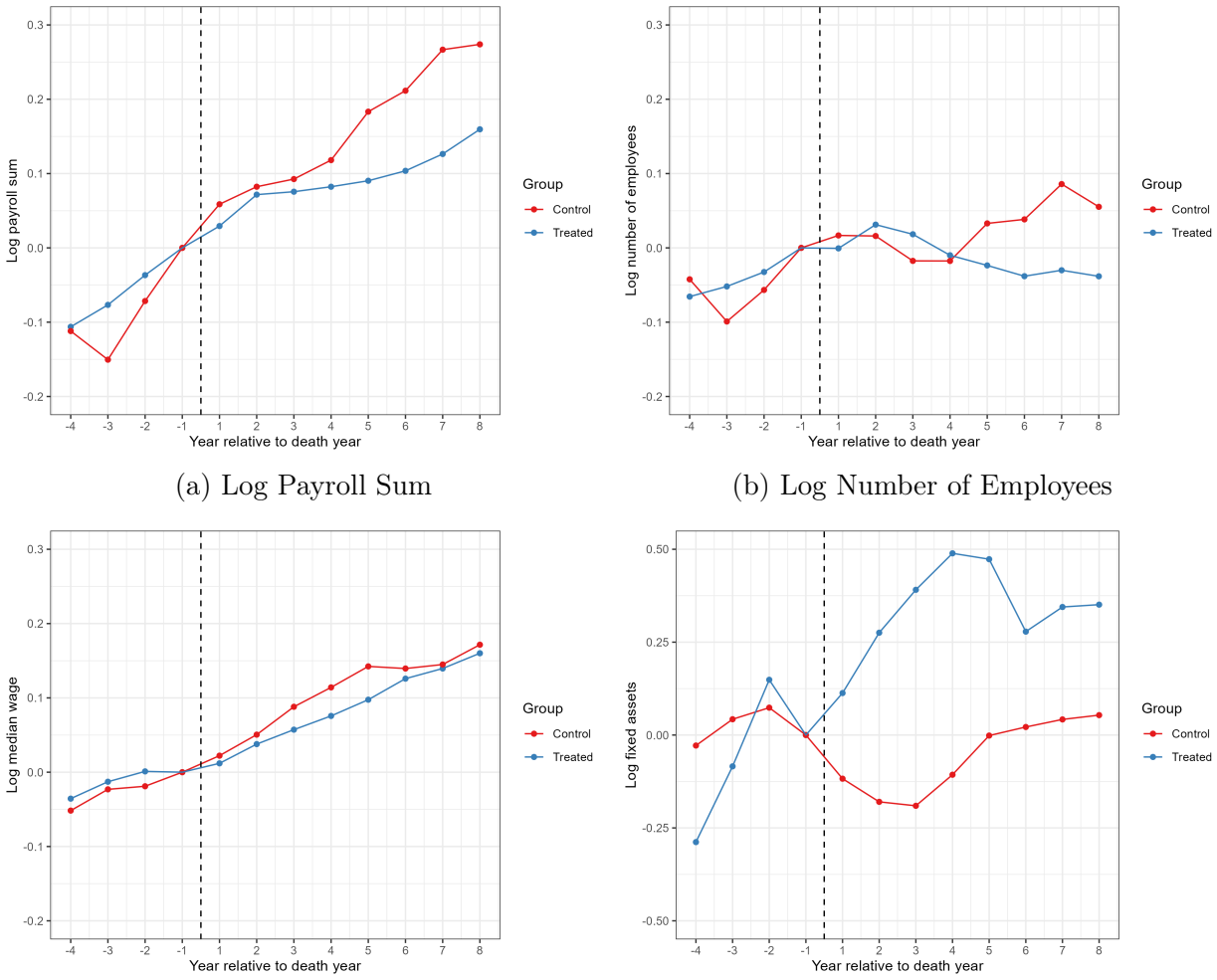
Note: Figure C.1 shows the distribution of firm counts in the year prior to death before restricting on non-exiting single-owner-firms with a shared name between successor and predecessor. The red dotted line marks the employee threshold above which the payroll sum regulation became binding.

Table C.2: Summary of inheritance or estate tax preferential treatment of business assets

Country	**Preferential Treatment Type**	**Minimum Time of Ownership**	**Conditions (Labor or Capital Restrictions)**
Belgium	Reduced tax rates	3 years (heir)	Local management; maintain capital; rate depends on beneficiary type
Finland	Preferential valuation (40% of tax value); 10-year interest-free deferral	5 years (heir)	Management; minimum ownership 10%
France	75% exemption	4 years (heir)	Management; signed commitment to conserve shares
Germany	85% or 100% exemption; abatement assets over EUR 26 million	5 to 7 years (heir)	Local management; maintain wage bill; minimum ownership 25%
Ireland	90% exemption	2 years (donor) or 6 years (heir)	Minimum ownership 25%; management or employment required
Italy	100% exemption	5 years (heir)	Specified heirs; local management; maintain employees
Japan	Payment deferral	5 years (heir)	Local management; maintain employees and wage bill; SMEs only
Korea	100% exemption, capped; taxable value capped at KRW 20 to 50 billion	5 to 7 years (heir); 10 years (donor)	Maintain 80% ownership; maintain employees and wage bill; SMEs only
Spain	95% exemption	10 years (heir)	Carry out economic activity; exempt from wealth tax; minimum ownership share 5% (individual) or 20% (family)
Switzerland	Preferential valuation; 80% reduction of tax liability	10 years (heir)	Local management; minimum ownership 51%
UK	50% or 100% exemption; payment in interest-free installments over 10 years	2 years (donor)	Exemption for privately held / unlisted companies (100%); management required
United States	Preferential valuation, capped at USD 1.18 million	5 years (donor); 10 years (heir)	Business is minimum share of donor's estate; specified heirs

Source: OECD (2021)

Figure C.2: Raw trends of main outcomes of interest



(a) Log Payroll Sum

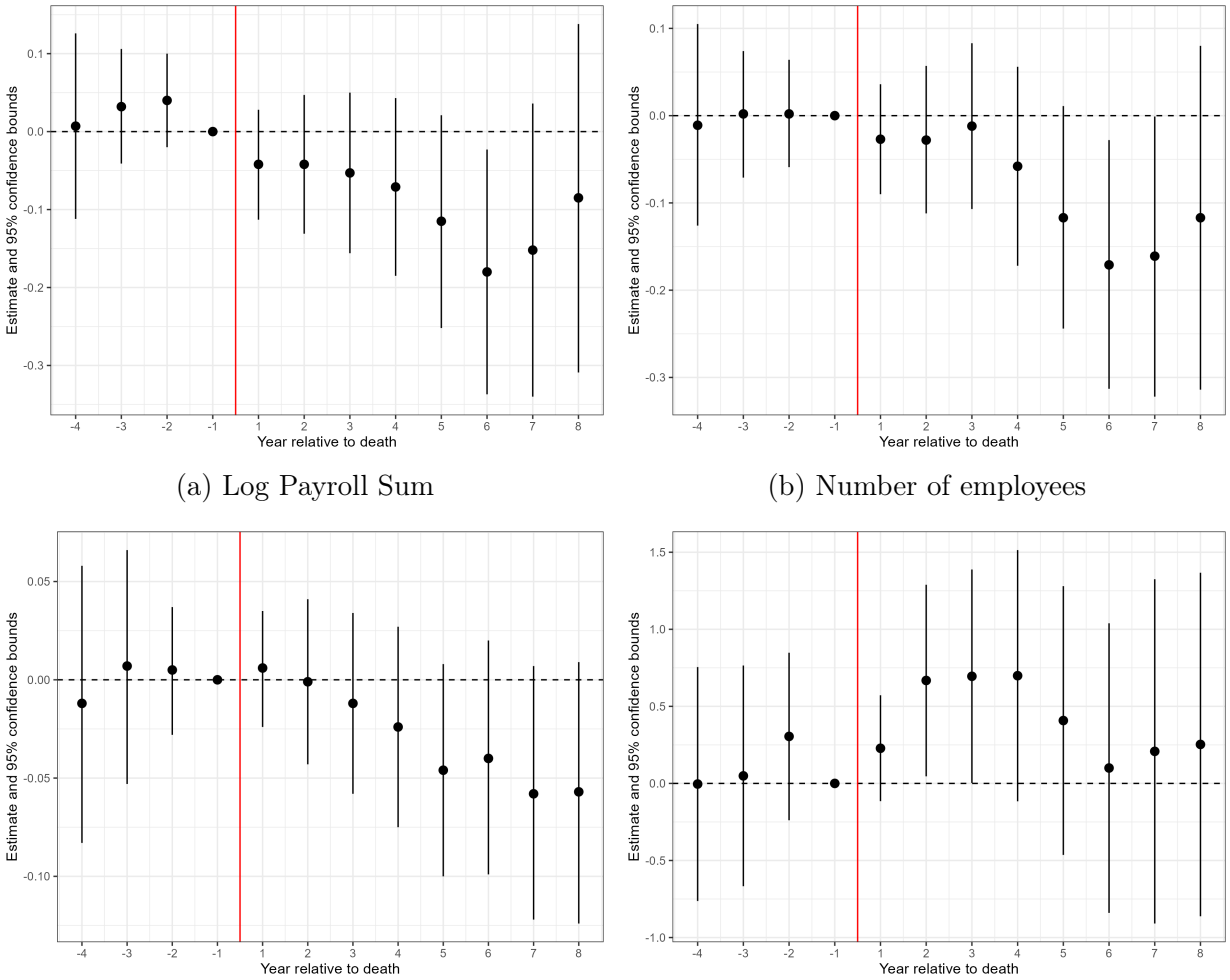
(b) Log Number of Employees

(c) Log Median Wage

(d) Log Fixed Assets

Figure C.2 shows raw averages of our main outcomes of interest for the treated and control group separately. The control group consists of firms with a total number of employees between 15 and 20 in the year before the inheritance. Averages are taken over relative time to treatment across all cohorts with no adjustment for fixed effects.

Figure C.3: Robustness Test: Dynamic Effects of the Payroll Sum Requirement for Larger Control Firms



(a) Log Payroll Sum

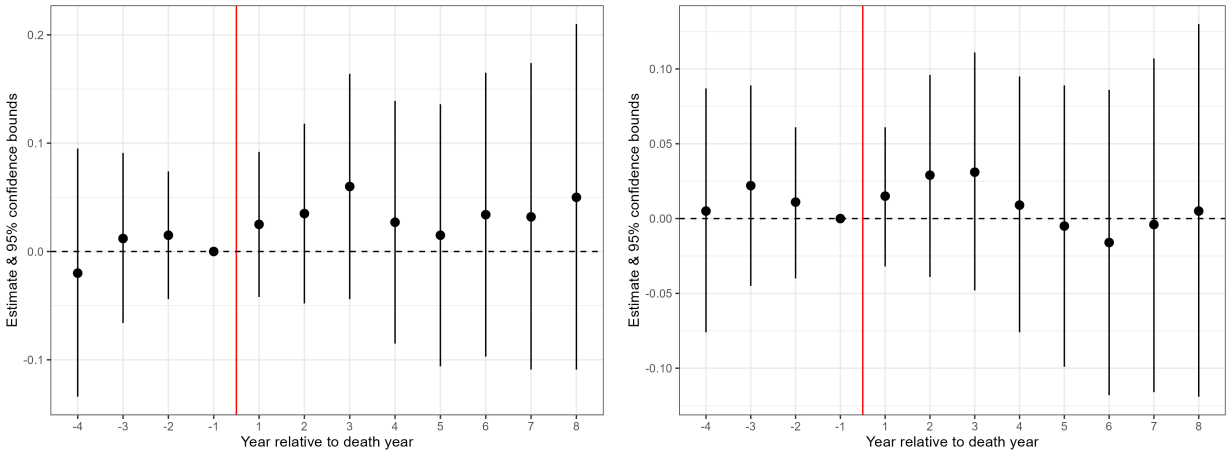
(b) Number of employees

(c) Log median wage

(d) Log Fixed Assets

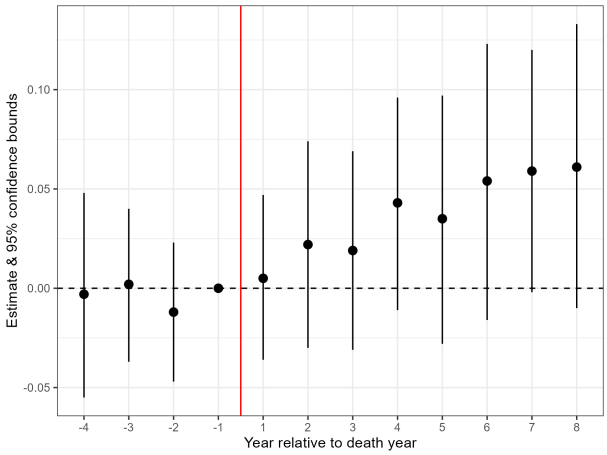
Figure C.3 shows the results of our event study specification Equation 9 for our four main outcomes, where we restricted the control group to consist of establishments with at least 15 employees in the year before the inheritance. In each figure, we plot the coefficient estimates on the relative-time treatment dummy as well as the 95% confidence interval around the estimate. Standard errors are clustered on the individual firm level. The coefficients for the payroll sum and the number of employees are based on the PPML specification in Equation 10, while the estimates for median wage and fixed assets are from the OLS specification in Equation 9.

Figure C.4: Robustness Test: Placebo Treatment at 10 Employees in the Year of Death.

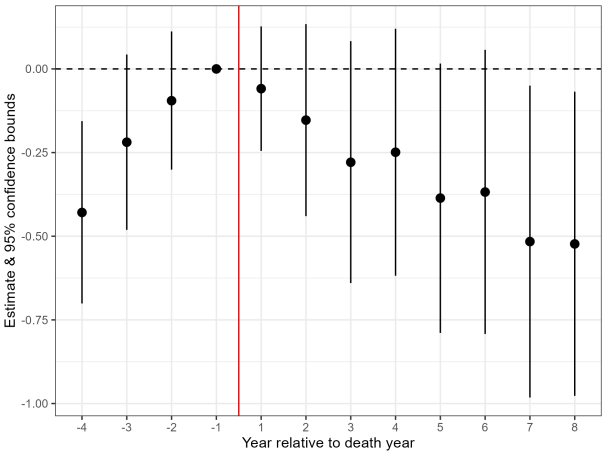


(a) Log Payroll Sum

(b) Number of employees



(c) Log median wage



(d) Log Fixed Assets

Figure C.4 shows the results of our event study specification Equation 9 for our four main outcomes, where we falsely assume that the payroll sum requirement was to be applicable for firms with more than 10 employees. Our placebo treatment group hence comprises of firms with more than 10 and less than 20 employees in the year before the inheritance, and our control group is formed by firms with less than 10 employees in the year before the succession. In each figure, we plot the coefficient estimates on the relative-time treatment dummy as well as the 95% confidence interval around the estimate. Standard errors are clustered on the individual firm level. The coefficients for the payroll sum and the number of employees are based on the PPML specification in Equation 10, while the estimates for median wage and fixed assets are from the OLS specification in Equation 9.

Table C.3: Difference-in-Means Test for Full Sample

	Control Group		Treated Group		Mean Difference	
	Mean	Count	Mean	Count	Difference	t-Statistic
Age at death	75.48	447	76.36	148	-0.879	-0.80
Firm age	33.83	444	31.36	149	2.466	1.21
Total assets	1101.90	370	1649.72	120	-547.821***	-3.37
Leverage	0.36	327	0.43	109	-0.045	-1.65
Investment	0.07	364	0.07	116	-0.002	-0.06
Cash Share	0.18	367	0.17	120	0.005	0.24
Employees	14.07	459	24.70	152	-10.639***	-39.16
Payroll sum	856.26	459	1557.49	152	-701.229***	-13.13
Median wage	80.58	455	78.97	151	1.613	0.76
Mean wage	83.05	455	82.32	151	0.724	0.33

Note: Table C.3 shows descriptive statistics of our final dataset of firms whose owner for all firms irrespective of owner age at death. The depicted averages and observation counts are split up by employee-size class (10-20 employees for the control group and 21-30 for the treatment group) and measured in the year before the succession. The two rightmost columns show differences in means between the two groups for the respective variables as well as t-statistics for the differences. Imbalances for the number of employees and payroll are by construction, as the assignment to the treatment and control group depends on these variables. Leverage is defined as current liabilities over total assets. Investment is equal to the percentage increase in the share of fixed assets over total assets per period. Median wage refers to the median daily wage earned at firms.

C.2 Data collection

C.2.1 Orbis

We source financial and ownership information on German firms from Bureau van Dijk's Orbis data base. Orbis offers company financial statement data as well as descriptive information, such as legal form, status of operations, and date of incorporation. Furthermore, the Orbis database includes time varying information on company ownership since 2007, including direct and total control as well as information on the global ultimate owner (GUO) of an entity. The ownership data contains the name of a specific shareholder of a company as a text string. Unfortunately, these strings are not in a consistent format. Hence, in order to identify the last name and first name of a shareholder, we rely on a dictionary approach.[102]

From the financial data we collect information on balance sheet items such as total assets and leverage, defined as current and non-current liabilities and long-term debt over total assets. Unfortunately, coverage of financial information is rather poor for our companies of interest, especially in regards to the number of employees and cost of employment (payroll sum), which is why we rely on administrative linked employee-establishment data for these variables instead.

C.2.2 Death Notice Data

The CompGen family announcements project records family announcements, such as death, birth and marriage notices from regional and supra-regional newspapers. For a recorded death, the available information may include the lastname, firstname, birthname, occupation, date of birth and death, place of birth and death, place and date of burial and service, place of last residence as well as information on the newspaper or newspapers where the obituary

[102] Specifically, we use the comprehensive first name database provided by Matthias Winkelmann available at `https://github.com/MatthiasWinkelmann/firstname-database`. After identifying all unique name parts of every name string of natural person owners of German firms in Orbis and removing all titles and abbreviations, we identify first names using the dictionary. The remaining name parts constitute last names, which we collect in a second dictionary. Based on the first name and last name dictionaries we are able to identify the last name and first first name for every name string regardless of the format.

was published. In total, the database provides death information for 2,178,776 individuals, with recorded death reaching back until before the two world wars.

C.2.3 Scraped Newspapers

We selected the set of newspapers by visiting each website and assessing how extensive the provided obituary archive is and hence how many obituaries could be recovered. Most newspapers provide the last name, first name, date of birth and date of death directly in the HTML code of their website and in the majority of cases a picture of the published obituary as well. Using the pictures of the obituaries, we are able to recover the obituary text using optical character recognition trained on German texts. Naturally, the coverage of the online archives is better for more recent periods. However, many newspapers have extensive archives that reach back into our period of interest.

C.2.4 Tombstone Data

The CompGen tombstone project has been initialized in 2007 by a small group of genealogists. In Germany, gravesides have a usual lay time of about 25 years, after which tombstones are either given to interested relatives or destroyed and used for road construction. In order to preserve the information on the deceased, voluntary contributors take pictures of all tombstones on a given graveyard and digitalize the inscriptions in a database. The included information generally comprises the full name as well as the date of birth and death. The death years of deceased individuals range from 1408 until 2021. Overall, this reflects 5,728,734 inscriptions of deceased individuals. Furthermore, the postal code of the graveyard as well as its name is provided. In total, the project includes data from 7,927 distinct graveyards covering 2,278 zip codes.

Kurzlebenslauf

05/2019 - 06/2025	**Universität Mannheim** Wissenschaftlicher Mitarbeiter und Doktorand am Lehrstuhl für Allgemeine Betriebswirtschaftslehre und Betriebswirtschaftliche Steuerlehre Prof. Dr. Philipp Dörrenberg Promotionsstudium an der Graduate School of Economics and Social Sciences (GESS), Centre for Doctoral Studies in Business
10/2016 - 03/2019	**Universität Konstanz** M.Sc. Wirtschaftswissenschaften
10/2012 – 09/2016	**Universität Konstanz** B.Sc. Mathematische Finanzökonomie