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ABSTRACT

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

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.

JEL Classification: D22, H00, H22, H25, J23, J30

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

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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 in cost structure. 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 policy.

Existing empirical literature using observational data typically 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. 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 they provide 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 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, due to data availability, the literature offers only limited evidence on the effects on firm returns and consumer prices. Moreover, due to limited availability of different types of tax reforms, it does not address whether business tax increases and decreases have symmetric effects – a key question, given that downward wage and price rigidity or partial irreversibility of decisions may lead to asymmetric responses.

For a complete understanding of the effects of profit taxes and their incidence implications, it is important to consider in a comparable setting all dimensions along which taxes can exert effects 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 (see further below in the Introduction and Section 5.2), our survey approach complements the existing literature by allowing us to study important aspects of business tax incidence that are difficult to consider using non-survey approaches.[1]

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.[2] We take a straightforward approach and ask firms how profit 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

[1] 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, in the context of estimating the effect of an economic policy, survey-reported behavior is comparable to revealed preference results using observational data (Parker and Souleles, 2019). Colarieti et al. (2024) study survey responses to hypothetical income shocks and show that their survey findings closely match realized behaviors observed in prior research. We extensively discuss potential caveats of our specific survey design in Section 5.2.

[2] 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 under plausible parameter values capital owners bear the entire incidence of the tax. However, this central result no longer holds 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.

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.

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 profit distributions and reserves. For tax increases, this implies that firm owners are 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 of 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.

Our survey design enables an examination of how an 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 limitations inherent to our survey-based approach. Rather than relying on observed behavior in response to actual policy changes, our methodology is based on self-reported responses to hypothetical tax changes. A potential drawback of using such an approach is that the hypothetical tax changes might lead to reduced effort from respondents or give rise to experimenter demand effects (Haaland et al., 2023; Bursztyn et al., 2025). For example, when facing a hypothetical tax increase, managers may hesitate to report lowering wages or laying off employees, particularly if they aim to be perceived as more socially responsible by the experimenter. We offer two main reasons that strengthen our confidence in the ability of our survey-based approach to yield meaningful insights.

First, we empirically investigate the predictive power of the respondents' hypothetical answers by comparing their reported 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 rates. We then correlate the survey-reported impact of a tax change on employment with actual employment changes after a change in the local business tax rate. In the second test, we exploit two survey questions about planned employment adjustments in the year after the survey by correlating them with employment changes observed in Orbis over the same time horizon. Both tests indicate the predictive power of the stated actions for actual behavior.

Second, following the approach in Colarieti et al. (2024), 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.[3] By carefully taking into account differences in the tax variation used to identify incidence parameters, we find similar

[3] In Colarieti et al. (2024), respondents allocate randomized, positive or negative hypothetical income shocks across spending, saving, and debt repayment over four quarters – a methodological approach which is very similar to our approach. Based on a cross-validation exercise, they show that their survey findings closely match realized behaviors observed in prior research, suggesting that surveys with hypothetical treatments can be a valuable tool for predicting actual behavior.

results for those margins for which empirical evidence exists. The cross-validation serves two specific purposes. First, it acts as a validation exercise. If our survey-based incidence estimates align with those from previous literature, it strengthens confidence in our findings on asymmetries and the effects of tax change magnitude. Second, it helps contextualize the variation observed in prior incidence studies, which often stems from differences in the direction of tax changes, sample composition, or adjustment margins.[4] Thanks to our experimental design – which distinguishes between tax increases and decreases and captures various adjustment margins within a single framework – our results provide a unified perspective that helps interpret the heterogeneity in existing research.

We identify three main contributions of our paper. First, we consider multiple possible different adjustment margins and study the distributional effect on the most relevant stakeholder groups – i.e., workers, firm owners, consumers – in one unified setting. Specifically, in contrast to existing studies in the literature, we simultaneously observe the outcomes that are most relevant to the three stakeholder groups: wages and employment, firm profits and distributions to owners, and consumption prices. In addition, we observe further outcomes that affect the three stakeholder groups indirectly (such as investments or tax planning). Kennedy et al. (2024) study the effects of TCJA-induced tax cuts on several firm outcomes – sales, profits, investment, shareholder distributions – as well as worker-level outcomes such as employment and annual earnings. However, they do not observe consumption prices. Duan and Moon (2024) also study firm- and worker-level outcomes simultaneously, but likewise do not observe consumption prices. The few other studies that examine multiple adjustment margins differ from ours in that they either focus on different margins relevant to only one single stakeholder group – like workers (Giroud and Rauh, 2019; Risch, 2024) – or use general equilibrium models and structural estimations to assess the impact of taxes on various groups (Suárez Serrato and Zidar, 2016).[5]

Our cross-validation exercise (see above) highlights two relevant points in the context of the existing literature. First, many prior studies attempt to infer the incidence on stakeholder groups for which they do not empirically observe the relevant outcome variables. While this underscores the importance of estimating incidence across multiple groups simultaneously, such calculations rely on several assumptions. Second, our results

[4] For example, our incidence results for tax decreases closely align with the findings of Duan and Moon (2024), likely due to the similarity in sample composition.

[5] Of course, our paper relates to many studies on business tax incidence that consider a single adjustment margin, for example in the context of worker incidence (Arulampalam et al., 2012; Fuest et al., 2018; Dwenger et al., 2019) or consumer incidence (for which we generally have limited evidence due to the availability of consumer price data; see recent exceptions such as Dedola et al., 2022; Baker et al., 2023; Jacob et al., 2023). We also relate to a large set of papers that examine the effect of business taxes on single indirect margins, including papers on investment effects (Zwick and Mahon, 2017; Ohrn, 2018; Chen et al., 2023; Jacob and Zerwer, 2023), tax avoidance (Dyreng et al., 2022) or CEO compensation (De Simone et al., 2022; Bornemann et al., 2023; Ohrn, 2023).

are fairly comparable to those of relevant existing studies based on observational data.

Second, we investigate whether the sign of a business tax change matters for its effects and incidence. Our finding that prices react more strongly to business tax increases than to tax decreases complements existing evidence of asymmetric price responses in the context of value-added taxes (Benzarti et al., 2020), sales taxes (Yilmazkuday, 2017), and excise taxes (Bergman and Hansen, 2019). In addition, Benzarti (2025) highlights that the effect of payroll taxes on employment is likely to be asymmetric (although the evidence is very scarce). However, we are not aware of any studies evaluating asymmetric responses in the context of business profit tax incidence. Relevant related papers such as Kennedy et al. (2024) do not consider tax increases and decreases simultaneously.

Third, adjustment costs may imply that tax changes of different sizes have different effects. We provide a systematic evaluation of this question based on randomized variation in tax change magnitude, thereby complementing a small set of papers that compare small and large tax reforms/kinks in other contexts (e.g., Chetty et al., 2011). Relevant existing studies do not simultaneously examine tax changes of varying magnitudes.

2. Survey Design and Data

2.1. Survey and Sampling

Our tax incidence questions were fielded in the second wave of the German Business Panel (GBP). 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.[6]

To address this issue, we construct survey weights to make our sample representative of the broader German firm population. Table 3 in Section 3.2 and Figure A14 present both unweighted and weighted results, illustrating that they are largely comparable. Given the similarity between weighted and unweighted results, we rely on unweighted results in our main analysis for two reasons. First, when the goal is to estimate causal relationships rather than produce population-level descriptives, unweighted regressions often yield more efficient and interpretable estimates (Solon et al., 2015). This approach is appropriate in our setting because the sampling design appears ignorable: estimated coefficients remain

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

stable when including controls, and we find no significant differences in key observables between respondent and non-respondent firms. These findings suggest that any selection into the sample is unlikely to bias our estimates, supporting the validity of unweighted regressions. Using unweighted results therefore provides a consistent analytical framework, enhances transparency, and simplifies exposition. Second, our heterogeneity analysis – where we examine differential effects across various firm characteristics – would require constructing separate weights for each characteristic to ensure representativeness along those specific dimensions. This approach would introduce substantial complexity and potentially reduce comparability across subsamples.[7]

The contact information of firms was obtained from the Bureau van Dijk Orbis database. The subsample of firms that participated in the survey was drawn randomly from the overall address pool and invited to participate in the online survey via email. The GBP sent invitation e-mails for the online survey on 45 work days between November 16, 2020 and January 22, 2021.[8] Firms were randomly assigned to one of the 45 days. After 7, 14, and 28 days, reminder e-mails were sent. Responses were collected from November 16, 2020 through June 24, 2021. The response rate to the survey was 2.5%, and about 57% of respondents completed the survey with a completion rate of 90% or higher (N = 8,955). The final sample used in the main analysis consists of 6,749 responses, after excluding observations with missing values for control and weighting variables.[9]

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 A2 and A6 provide examples of the tax incidence questions as appearing in the online interface of the survey in German. Respondent companies were randomly assigned

[7] Online Appendix A.5 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.

[8] The survey period overlaps with the height of the second wave of the COVID-19 pandemic. We check whether the economic environment during our survey period had an effect on the responses by testing differences in answers across the distinct months the survey was in the field. None of our outcome variables show statistically significant differences on average (The highest F-statistic is 1.78, with a p-value of 0.1).

[9] Figure A1 in the Online Appendix illustrates the distribution of the completion share in our data. Additionally, Online Appendix A.6 compares firms that completed the survey with those in the Orbis database that did not participate or did not complete the survey, showing similarity in terms of total assets, number of employees, operating revenue, employee costs, and taxes on income. 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.

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 25%.[10] 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.[11] 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 local business tax, the personal income tax, as well as the corporate income tax.[12]

The way the question is phrased has to strike a balance between the need to be concise and the need to be sufficiently precise. To keep the flow of the survey as natural as possible, the hypothetical scenario in the question does not explicitly fix all assumptions one could make about the circumstances of the tax change. For instance, the phrase "as a result of a tax increase (decrease)" combined with "permanent" suggests to respondents that the question refers to a legislative tax policy change. Since such policy changes rarely target individual companies, respondents likely interpreted the liability change in the question as applying to their own firm as well as other firms in their jurisdiction.[13] In addition, this wording implies that the tax change will not be reverted in the foreseeable future. It is also not explicitly mentioned that the change in tax burden is thought to derive from a marginal change in the existing tax regime affecting businesses broadly and not from an additional lump-sum tax or lump-sum payment. Our presumption is that business owners are familiar with profit taxes as rate-based systems. When presented with a profit tax burden increase, they may default to their experience with these distortionary systems rather than picturing a lump-sum tax. Furthermore, although loss-making firms

[10] The wording displayed above corresponds to the tax increase treatment. 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****?"*

[11] In order to ease the computational burden for respondents and attain sufficient power to test for differences in the tax treatments, we discretized the tax shocks to six distinct values (-25%, -10%, -1%, 1%, 10%, 25%). This design choice strikes a balance between being able to detect non-linearities in incidence with cognitive demand.

[12] The German corporate tax is levied on the income of incorporated firms. The local business tax is payable by both pass-through firms and corporations, and is also applied as a tax on the profits of a business. The personal income tax 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 personal income tax rates. Partnerships and sole proprietorships can apply a credit for local business taxes paid toward their income taxes, up to a certain threshold. This reduces the impact of changes to local business taxes for non-corporate businesses.

[13] Of course, larger firms are more likely to operate internationally (Eaton et al., 2011) and compete with foreign companies not subject to the same tax policies. Consequently, they may perceive the tax change scenario as affecting only themselves and their domestic competitors, while their broader set of global rivals remains unaffected. The respondents' ability to account for their firm-specific context in their responses increases their reliability. See Section 4 for an analysis of response heterogeneity with respect to firm size and other firm characteristics.

would not expect an additional tax burden in a loss-making year, the relevant consideration is the permanent nature of the tax change – since firms that intend to remain in business generally anticipate positive tax liabilities in the long run, the policy may still influence their forward-looking plans.

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

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). Hence, 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

[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.

[15] 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.

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 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).[16] 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 A10 and A11 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. In the tax increase treatment, respondents most commonly cited company liquidation, relocation, and cost-cutting measures as potential responses to a tax hike.

An important feature of our experimental design is that we can test asymmetric firm responses to tax increases and decreases. Conceptually, there are several institutional and behavioral factors that may explain why firms react differently depending on the direction of the change. On the labor side, downward wage rigidity – driven by collective bargaining agreements, minimum wage laws, and employment protection – can limit firms' ability to adjust wages or employment in response to tax hikes (Fuest et al., 2018). In contrast, tax cuts may offer more flexibility for expansion. On the pricing side, firms may more readily

[16] 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/shareholders	Higher distributions to partners/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?* Conditional on the firm's legal form, respondents either saw "distributions to partners" or "distributions to shareholders". (*): Note that the category *Decrease in Debt Capital* was not available in the tax decrease treatment. We therefore integrated the *Increase in Debt Capital* category into the *Others* category for the tax increase treatment to facilitate comparisons between tax increases and decreases.

pass tax increases to consumers than pass on savings from tax cuts. Evidence from VAT changes supports this pattern, showing stronger price responses to tax hikes than to cuts (Benzarti et al., 2020). One possible explanation is that, in the case of tax increases, the benefits of raising prices outweigh the menu costs, whereas for tax decreases, this no longer holds (e.g., due to inflation expectations). While this has been explored in consumption taxes, similar dynamics may apply to profit taxation. Investment behavior may also reflect asymmetries: firms could act more aggressively in response to tax cuts than hikes, as lower taxes increase returns on capital. Differences in pass-through to consumers and workers can shape these effects (Jacob, 2022). Additionally, firms may be more motivated to engage in tax planning to mitigate tax increases than to restructure operations in response to tax relief. In sum, institutional frictions, pricing behavior, and strategic considerations can lead to directionally different responses to tax increases and decreases.

Our design also allows us to study if tax changes of different magnitudes have different incidence effects, for example due to adjustment costs (Chetty et al., 2011). Generally, as noted by Benzarti (2025) in the context of the canonical incidence model for consumption taxes, standard models are derived using small tax changes and they may therefore not be well-defined for large tax changes.

The order, in which the answer options were presented to the participants, was not ran-

domized. 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, for example, in Figure A2 in combination with Figure A6. 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.

2.3. 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.[17] Table 2 provides some insights about the distribution of company characteristics in our sample. 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, 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. Approximately 70% of survey respondents are the owner or CEO of the corresponding firm.[18]

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 A12 (Online Appendix A.4) summarizes the results of our balancing

[17] As the set of survey respondents is based on available contact information in Bureau van Dijk's Orbis database, we in principle have access to a much larger set of variables. However, except for the number of employees and total assets, coverage for variables such as turnover, cost of employees, and taxes paid is quite low (see also Table A2 in the Online Appendix). Moreover, we can only merge this information with the survey responses if the respective respondent has agreed to link their responses to external data sources. Since only about 36% of respondents consented to data linkage ($N = 2{,}435$), we refrain from using only linkable data in our main analysis.

[18] 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 the survey is redirected to the appropriate decision-maker within the firm, who is capable of providing relevant answers to our survey questions.

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 2.9%, 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	0.07	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 Sector	6,749	0.27	0.45	0	0	1
CEO	6,749	0.70	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. The number of observations for revenue is lower because revenue was also collected in categorical form, but only the continuous responses are reported here. When combining the continuous and categorical responses, the sample size increases to 6,749.

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.

3.1. Empirical Strategy

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

[19] In addition to estimating Equation (1) using OLS, Section A.8.2 in the Online 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.

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

where the dependent variable y_i 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]

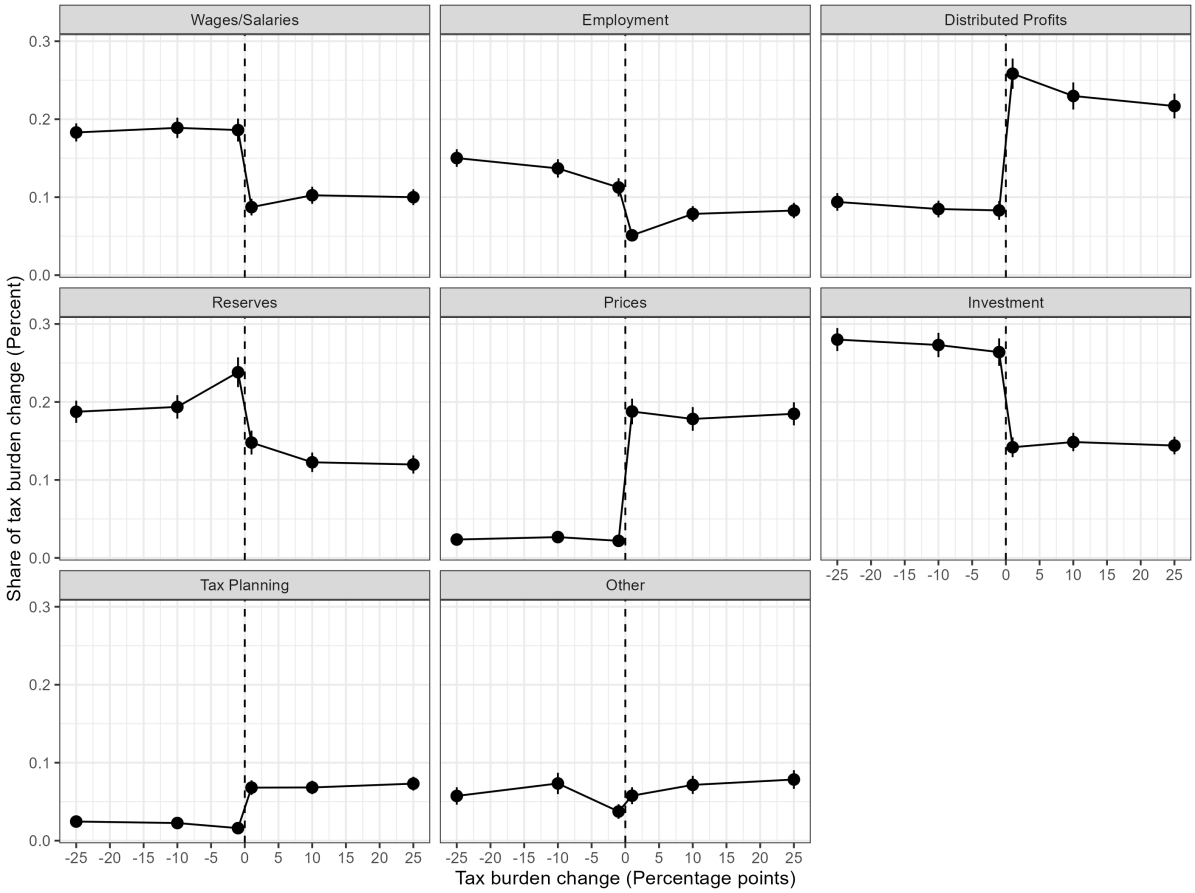
3.2. Results

Non-linearity of tax effects. 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 categories, they exhibit pronounced discontinuities at the dashed 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 interesting exceptions (see below). 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 exhibits less pronounced variation. Building on this insight, we now explore the results for the individual categories in greater depth.

Category usage. 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

[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 an indicator for firms that experienced a significant impact from the COVID-19 pandemic on their net income. We define a firm as significantly impacted by COVID-19 if the respondent was below the median with respect to the stated percentage change in net income due to COVID-19. The results are provided in Table A5 in Online Appendix A.8.1 and suggest that the estimated effects are largely in line with those derived without the inclusion of controls.

Figure 1: Tax Change Effects by Outcome Margin and Treatment Group



Note: Figure 1 shows the tax change effects by outcome margin and treatment group. Each panel shows the estimated incidence share for the respective category across the six different treatments based on Equation (1). Each category represents a managerial adjustment margin. The available margins include adjustments to wages and salaries (*Wages/Salaries*), the number of employees (*Employment*), profit distributions to partners and shareholders (*Distributed Profits*), retained earnings and reserves (*Reserves*), consumer prices (*Prices*), (capital) investment (*Investment*), the usage of tax saving opportunities (*Tax Planning*), and other choices indicated in the open field question (*Other*). See Section 2.2 for more details on the categories and Section 5.1 for how they relate to the incidence margins investigated in the previous literature. Robust confidence bounds are indicated by vertical lines.

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.

Next, we present exact figures and statistical tests for the patterns shown in Figure 1, summarized in Table 4. Table 4 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,

[21] We explore the sensitivity of our findings with respect to the applied weighting scheme in Appendix Section A.8. Our treatment effects are robust to applying survey weights. See the discussion in Section 2.1 for an explanation of why we use unweighted results for the rest of the paper.

Table 3: Descriptive Statistics Incidence Shares

			Mean		Sample Percentages		
Outcome	Treat Sign	Obs	Unwghtd.	Weighted	$y_i = 0$	$y_i = 1$	$y_i \in (0,1)$
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 presents descriptive statistics for the outcome variables of the experiment. Additionally, we report the shares of firms that did not select the category ($y_i = 0$), selected only this category ($y_i = 1$), or selected this category along with others ($y_i \in (0,1)$). To be included in the sample, firms must have a completion rate of at least 90% and non-missing values for all control variables. The survey weights are calibrated to ensure representativeness of the German firm population (see Online Appendix A.5).

we provide test statistics for the difference in coefficients, with significant differences denoted by stars. 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.

Worker incidence. 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).

Owner incidence. 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, category usage 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 the tax burden out of their own pockets, but they 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.

Consumer incidence. 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% 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 existing evidence of asymmetric price responses in the context of value-added taxes (Benzarti et al., 2020), sales taxes (Yilmazkuday, 2017), and excise taxes (Bergman and Hansen, 2019), which find that prices react more strongly to increases than to decreases, we are not aware of studies that evaluate this asymmetry in the context of business profit taxes.

Reserves. 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 COVID-19 pandemic 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.

Table 4: Incidence Shares by Treatment Combination

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). The difference between tax increases and decreases and its significance are given below the estimates for each intensity pair. F-statistics from testing for differences between the estimates of Low, Medium, and High intensity in the same row are given in the rightmost column, where a significant result indicates that at least one estimate is different. Robust standard errors for the composite coefficients are given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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.

Investment effects. 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 greater or equal to 5% about their reasoning for this choice. Figures A8 and A9 (Online Appendix A.2) present examples of the questions as appearing in the online interface of the survey. Participants rated their reasoning on a scale from 0 to 100. A rating of 0 indicated that investment adjustments were primarily driven by changes in available funds following a tax increase or decrease, whereas a rating of 100 suggested that the perceived profitability of investments was the dominant factor. Lower values therefore reflect capital constraints, while higher values indicate that tax changes primarily influence the profitability of investment opportunities.

Figure A13 in Online Appendix A.7 illustrates the results of these follow-up questions. We binned the possible responses into three categories. Answers below 25 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 changes in the profitability of investment projects after a tax shock. This finding aligns well with the investment behavior of U.S. firms following the American Jobs Creation Act (AJCA), which notably reduced the tax burden on U.S. 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. Moreover, Duan and Moon (2024) conclude that the higher investment response of small manufacturing firms in Canada is likely driven by firms being cash-constrained before the tax reduction. The responses also illustrate that respondents probably did not interpret

our tax treatment as a lump-sum cost shock. If this were the case, there would be no reason to answer that the investment is less worthwhile. However, for more than 50%, the tax treatment seems to have a substantial impact on the profitability of the investment.

Effects on tax planning. 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.

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, wage setting power or tax saving opportunities, while smaller firms may be more vulnerable to disruptions (Fuest et al., 2018). Likewise, economic sector differences may influence treatment effects due to variation in competition, profit margins, and factor input intensity (Fuest et al., 2018). Organizational form can also shape a firm's response to 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 (Faulkender and Petersen, 2012; Duan and Moon, 2024). 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.

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:

$$y_i = \beta_0 + \beta_1 \text{Increase}_i + \boldsymbol{\gamma}_1' \boldsymbol{x}_i + \boldsymbol{\gamma}_2' \text{Increase}_i \times \boldsymbol{x}_i + \varepsilon_i, \tag{2}$$

where $\boldsymbol{x}$ denotes the vector for the firm characteristic of interest (e.g., size dummies). 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.[22]

We measure firm size by reported revenue in 2019 and follow the definition by the European Commission by considering firms as micro-enterprises 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 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.[23] 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.

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, while Panel 2c shows average partial effects for the comparisons between each respective group and the baseline (micro-enterprises). 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

[22] We also explore effect heterogeneity in a specification that includes all control variables (and their interactions) in one estimation model. The findings, presented in Online Appendix A.9, closely align with our main results.

[23] In Germany, there exists a mixed company type called *GmbH & Co. KG*, which combines elements of a corporation and a pass-through entity. The structure 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.

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 micro-enterprises, 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.

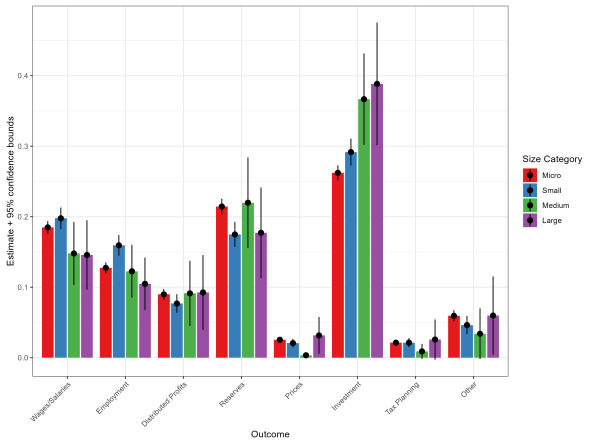
We also detect some evidence for varying pass-through to consumers for tax increases depending on the size of the firm, suggesting that larger firms finance 8 percentage points less via price adjustments compared to micro-enterprises. This may reflect that larger firms are more likely to operate internationally (Eaton et al., 2011) and compete with foreign companies not subject to the same tax policies. When deciding whether to pass tax increases on to customers, larger firms may therefore consider the competitive disadvantage of raising prices while international competitors do not face the same pressure.

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. 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.

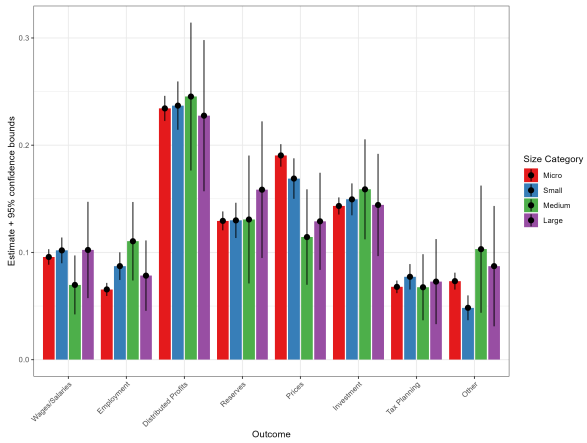
Economic Sector. Our results with respect to sector differences in incidence are summarized in Figure 3. Panels 3a and 3b again show the average category usage for each sector separated by the sign of the tax change, while Panel 3c illustrates the partial effects to test for differences between each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

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

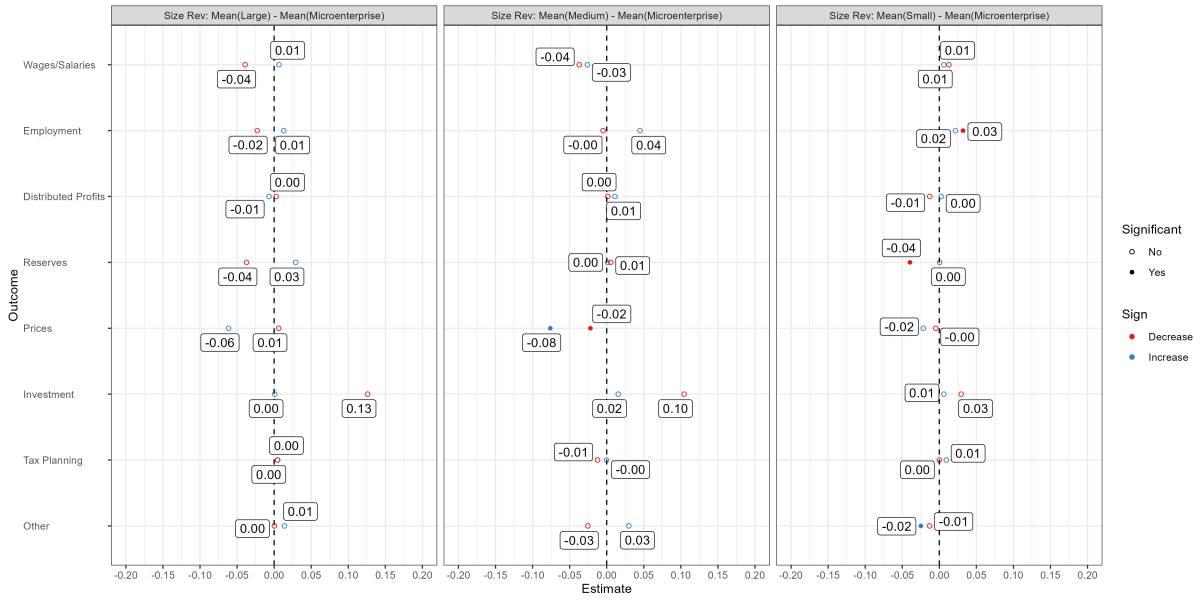
Figure 2: Incidence Heterogeneity by Firm Size.



(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.

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.

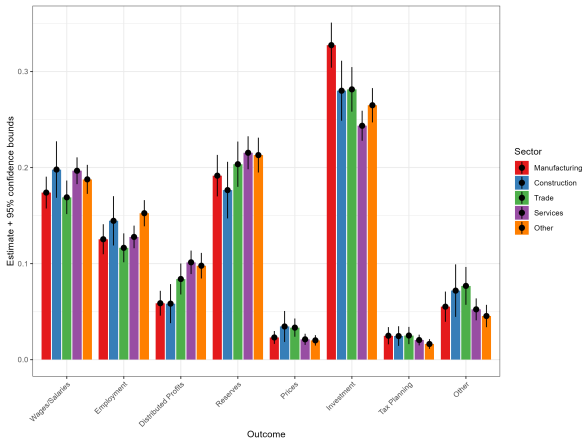
Our findings for the trade sector provide an additional indication that the level of competition is a significant driver of incidence. Similarly to construction, competition in the trade sector is relatively high as opposed to manufacturing and services, hence low profit margins cannot cushion the impact 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 margins.

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 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.

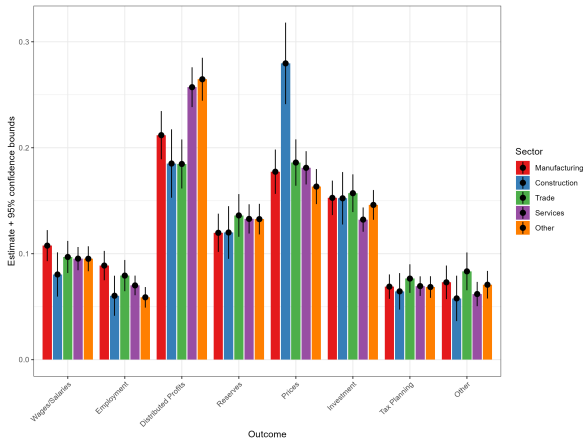
Second, the data suggest substantial differences in owner incidence depending on the organizational form of the companies. Partnerships state with 5 percentage points substantially larger effects on distributed profits for both decreases and increases compared to corporations. Sole proprietors, on the other hand, differ substantially only for tax increases, where the payout incidence is an 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 and less investment. 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-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 was not 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

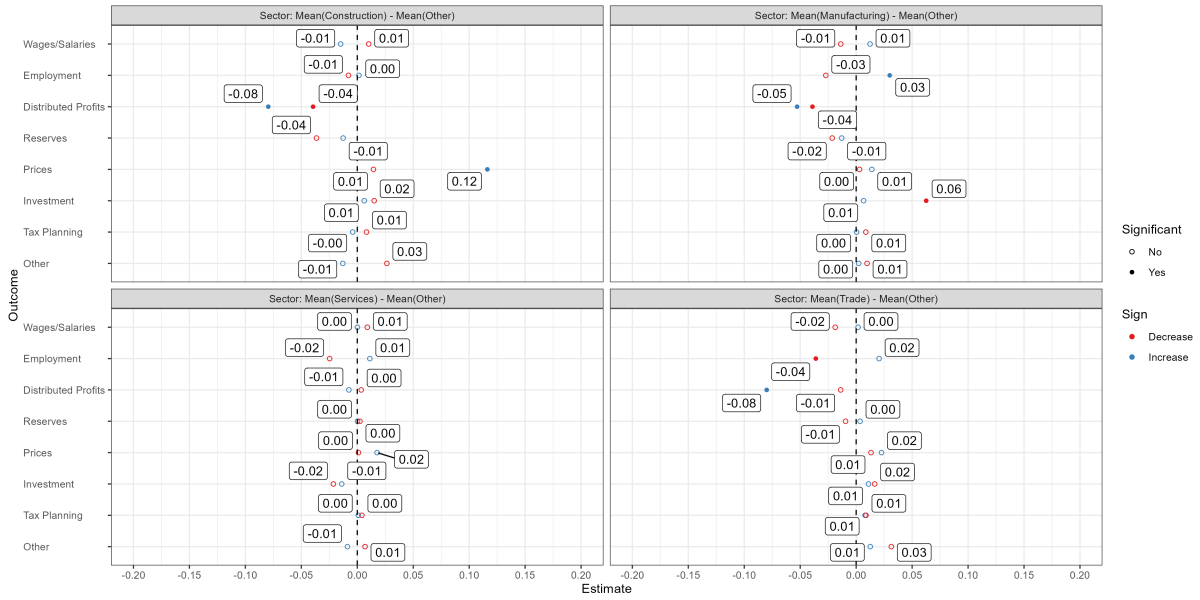
Figure 3: Incidence Heterogeneity by Economic Sector.



(a) Tax Decrease



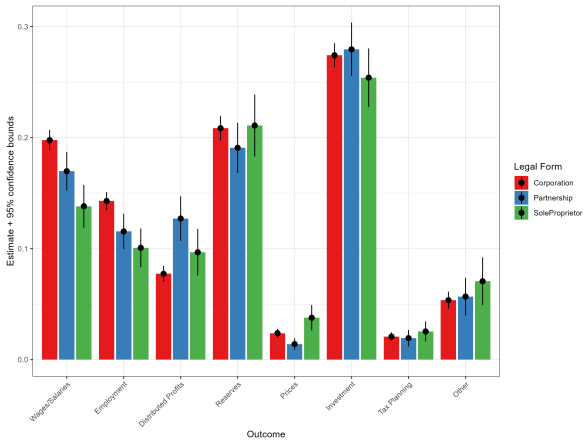
(b) Tax Increase



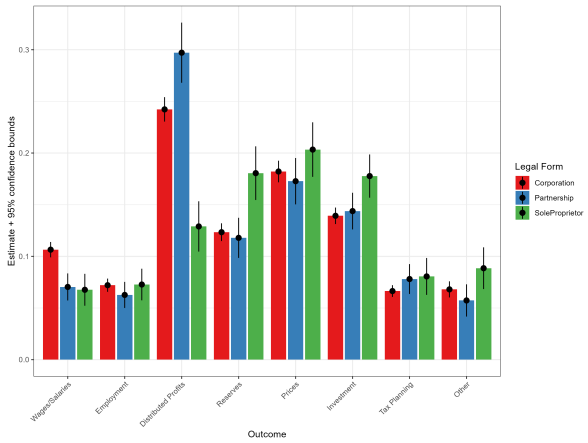
(c) Average Partial Effects

Note: Figure 3 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.

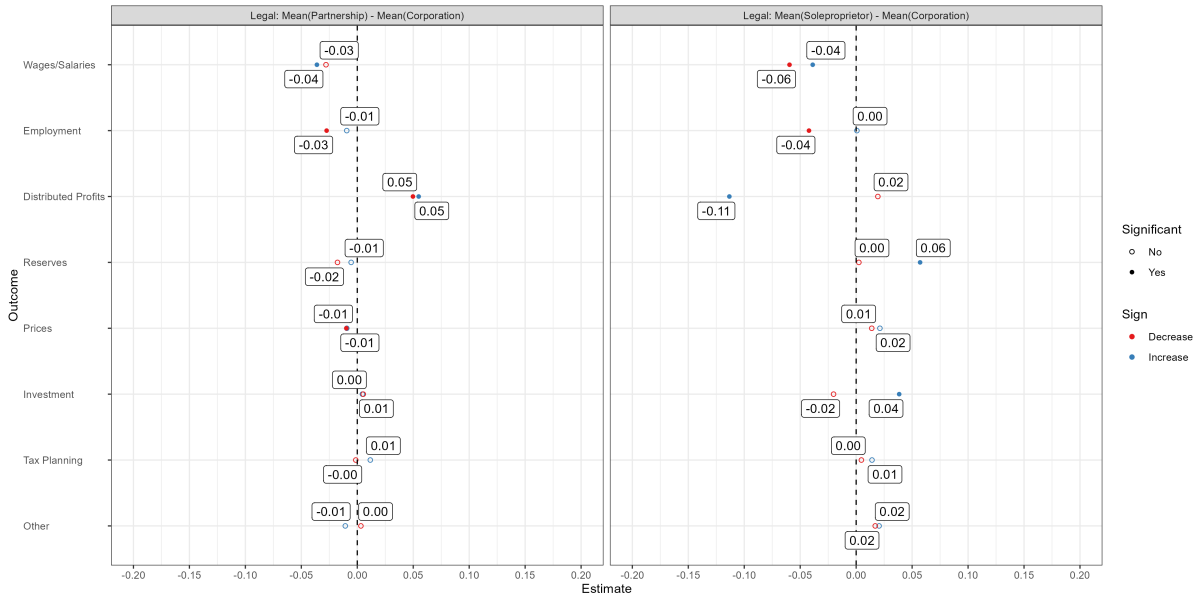
Figure 4: Incidence Heterogeneity by Organizational Form.



(a) Tax Decrease



(b) Tax Increase



(c) Average Partial Effects

Note: Figure 4 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.

short-term liabilities. This notion is supported by the opposite-sign partial effect on the category *Other* in combination with Figure A10, 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 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. 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.

5. Cross-Validation with Literature and Reliability of Survey Responses

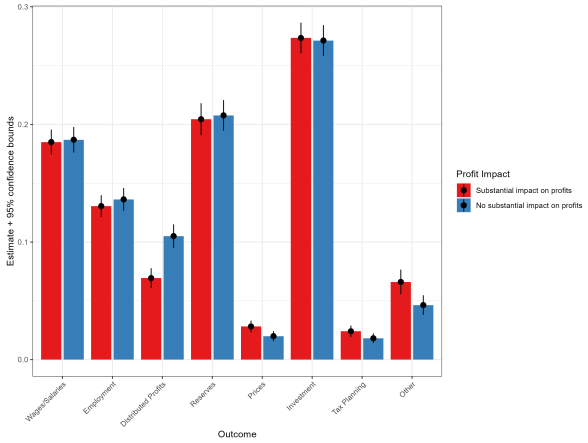
The validity of our findings critically depends on the reliability of our survey responses. In this section, we first cross-validate our results with existing literature using observational data (5.1). We then discuss several potential threats to the reliability of our survey-based findings and conduct empirical validation exercises, testing if respondents report firm characteristics accurately and if stated actions predict actual behavior (5.2).

5.1. Cross-Validation with Observational Studies

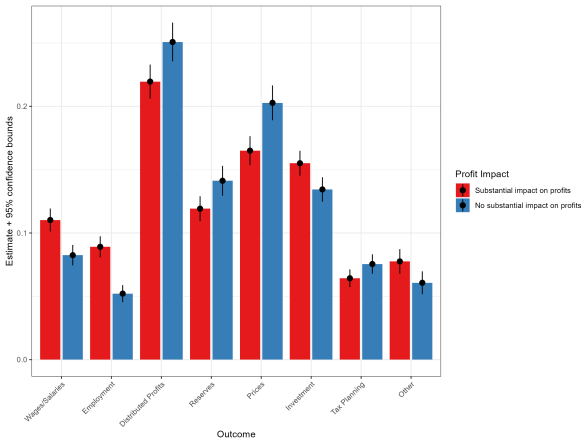
We examine how our baseline results (presented in Section 3.2) compare to prior studies on the incidence of corporate taxes using (non-survey) observational data. To assess the reliability of our survey estimates in predicting real economic behavior, we follow Colarieti et al. (2024) and apply a *cross-validation 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.

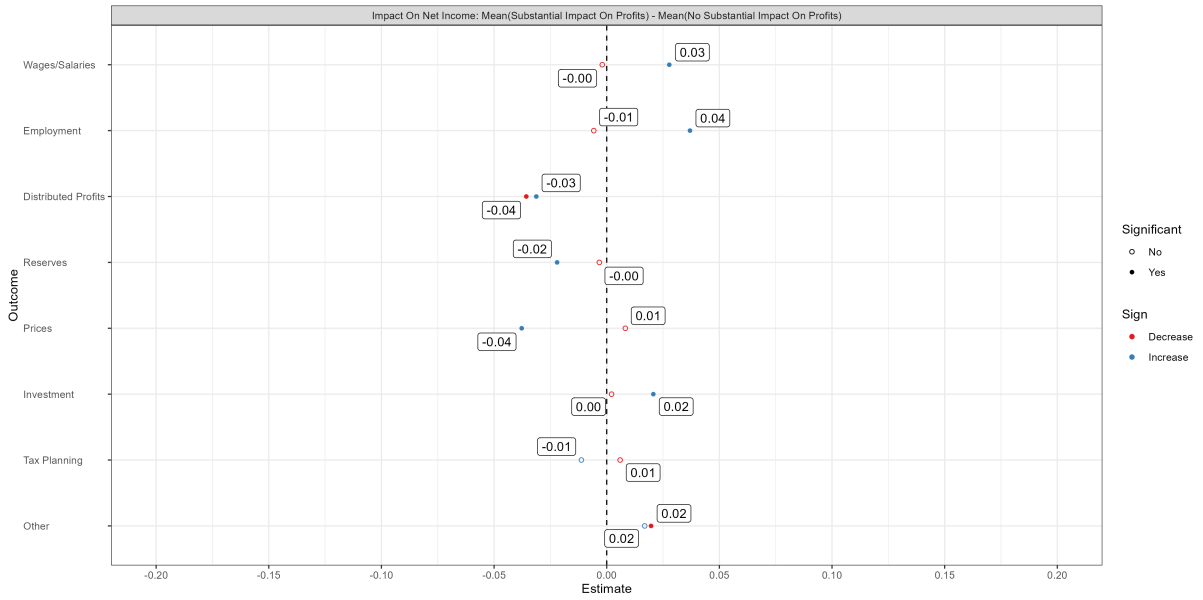
Figure 5: Incidence Heterogeneity by COVID Profit Impact.



(a) Tax Decrease



(b) Tax Increase



(c) Average Partial Effects

Note: Figure 5 shows heterogeneity in incidence by whether the company was substantially impacted by COVID. 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.

We begin by outlining the general methodology used to derive corporate tax incidence estimates from our survey experiment. Next, we illustrate the process with an example before 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.[24] Considering a given level of pre-tax profit, the initial incidence indicates how a change in the profit tax burden is shared across these stakeholders at the margin through changes in wages, distributed profits, and prices.[25] The second-round effect on the tax incidence, on the other hand, stems from effects caused by, for example, changes in firm investment behavior or production levels, 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 second-round effects and provide evidence on the initial incidence. In our setup, the initial incidence corresponds to the categories *Wages/Salaries*, *Distributed Profits*, and *Prices*, as these are the most commonly used categories in previous studies.[26] 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 a theoretical model that accounts for the feedback effects of second-round incidence on the initial incidence categories.[27] While we abstract from such a general equilibrium

[24] In principle, there are different possibilities how taxpayers can be divided into groups that share in the tax burden depending on context and the question of interest. For instance, one could look at how the burden is shared between producers and consumers, among different factors of production such as capital, labor and land, or among income groups or other measures of economic well-being (Fullerton and Metcalf, 2002). Most studies focus on a subset of groups that can in principle share in the burden within the setting considered and omit certain other groups. For example, Fuest et al. (2018) consider a small open economy setting, where output prices are fixed and consumers therefore cannot share in the burden of the tax. Jacob et al. (2023) only look at firm owners and consumers, as wage effects are unlikely to occur in their setting because of minimum wage regulations. In general, theory suggests that the incidence of the corporate income tax can fall both on the sources (capital, labor, and land) as well as the uses side (consumers and governments) of production (Fullerton and Metcalf, 2002; Auerbach, 2006; Fuest et al., 2018; Jacob et al., 2023).

[25] We do not include the employment adjustment margin in our cross-validation exercise, as all the referenced studies in our cross-validation 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.

[26] 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.

[27] We acknowledge that this also implies that we cannot speak directly to the question of how our results impact long-run production. Nevertheless, the observed effects on employment and prices offer suggestive evidence of potential production effects. For example, when firms respond to business tax increases by reducing their workforce, it is likely that production levels will also decline as a result. Moreover, when a product is taxed, consumer demand might drop, forcing firms to cut production

model, our results on second-round incidence effects nevertheless provide valuable insights for future theoretical research. They underscore the importance and magnitude of these second-round effects in shaping the overall tax incidence.

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/Salaries*, *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 local business tax on workers and firm owners. To compare their incidence estimates with ours, we use the incidence shares from Table 3. Specifically, for Fuest et al. (2018), we define the initial incidence on workers as the ratio of the *Wages/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 $0.097/(0.097 + 0.235)$ based on the values in Table 3. 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 local business tax 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 dead-weight losses and over-shifting – both of which can result in a tax burden exceeding the generated tax revenue (Fullerton and Metcalf, 2002).

Importantly, while some recent studies analyze multiple firm-level outcomes or combine firm and worker data (Kennedy et al., 2024; Duan and Moon, 2024)[28], the vast majority of empirical work on business tax incidence focuses on a single adjustment margin and stakeholder group – most commonly wages for workers or prices for consumers. Broader incidence effects are typically inferred indirectly, either through model-based calculations or general equilibrium modeling (Suárez Serrato and Zidar, 2016; Fuest et al., 2018; Baker

and reduce input purchases, directly altering the net market price of those inputs (Fullerton and Metcalf, 2002).

[28] Kennedy et al. (2024) analyze the effects of TCJA-induced tax cuts on various firm outcomes – such as sales, profits, investment, and shareholder distributions – as well as worker-level outcomes like employment and annual earnings. Duan and Moon (2024) also examine both firm- and worker-level responses. However, neither study includes data on consumer prices and thus cannot assess incidence on consumers directly.

et al., 2023).

In contrast, our experimental design allows us to directly observe outcomes that are central to all three major stakeholder groups – workers, firm owners, and consumers – within one unified framework. Specifically, we simultaneously measure effects on wages and employment, profit distributions, and consumption prices, as well as additional indirect margins such as investment and tax planning. This comprehensive empirical coverage enables a direct assessment of the distributional impact of tax changes, without relying on structural assumptions or imputing incidence from one group to another.

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 local business taxes 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 (median: 7). Our estimate of worker incidence (29%) falls between these two studies, aligning with our sample's firm size, which averages 68 employees (median: 5), 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%.[29] Using U.S. worker-level filings linked

[29] 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

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 factor explaining this similarity is the composition of our sample, which consists primarily of smaller firms (similar to Duan and Moon (2024)), with 67% employing fewer than 10 workers. In such firms, the owner-worker incidence – explicitly considered by Duan and Moon (2024) – 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.[30] To better align with the relevant incidence margins used in the referenced studies, we

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.

[30] 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 explicitly 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.

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 (using bar-code-level retail prices), 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 U.S. marginal tax rates. Using the wage and distributed profit shares from Table 3 for both tax 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 US 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 Suárez Serrato and Zidar (2016), Suárez Serrato and Zidar (2023), and Suárez Serrato and Zidar (2024).

To sum up, the takeaway from the cross-validation is that our survey responses reliably indicate firms' behavioral patterns in response to hypothetical tax changes. This reliability likely stems from the fact that these scenarios closely mirror real-world decision-making processes as in Colarieti et al. (2024). 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. We further corroborate the reliability of our survey responses in capturing real-world firm behavior in Section 5.2.

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%	-	-
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%	-	-
Jacob et al. (2023)	Variation in local business tax rate	Increase (98% increases)	Germany	2014–2017	-	-	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%	-	-
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%	-	-
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%	-	-
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%	-	-
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%	-	-

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 category 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 Online Appendix B. "-" : Indicates that no information on the incidence for this group was provided, or that it was explicitly assumed to be 0% in the respective study.

5.2. Reliability of Survey Responses

Even though our results compare well to the findings of prior literature, as shown in the previous Section 5.1, 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; Bursztyn et al., 2025): (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 in Section 5.2.1 and the accuracy of manager surveys in general in Section 5.2.2, we conduct several empirical validation exercises to mitigate remaining concerns and bolster the confidence in our results. First, in Section 5.2.3, 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 (Section 5.2.4).

5.2.1. 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; Bursztyn et al., 2025). 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. In the presence of a social desirability bias, one would also expect respondents to claim to a much greater extent that they pass on tax cuts to their customers by lowering prices. However, the responses indicate only a minimal pass-through of tax cuts to prices (two percent). 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 5.1). 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 5.2.4). In the first test, we merge our survey responses with Orbis financial data and information on changes in statutory local business tax rates. We then examine the correlation between managers' stated employment responses to a tax change and actual employment adjustments following local business tax 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 implies that a single decision-maker accurately reflects the firm's overall behavior (iii). A common concern with survey-based data – particularly when relying on a single respondent per firm – is the risk of bias, measurement error, and limited representativeness, as highlighted by Bertrand and Mullainathan (2001) in the context of individual-level surveys. This concern becomes more salient as firm size increases, since decision-making in larger firms is typically distributed across multiple departments and stakeholders. In particular, CEOs and top executives – especially in multinational corporations – operate within complex organizational structures that constrain their ability to unilaterally implement decisions. Unlike small business owners, who often exercise direct control over pricing and wage-setting, CEOs must navigate internal bureaucracy, shareholder interests, financial constraints, and competitive pressures – all of which influence how tax burdens are ultimately distributed. However, in our sample, the majority of firms are relatively small: 81% employ fewer than 20 people. Given this size distribution, the assumption that a single manager can serve as a reasonable proxy for firm-level behavior is more defensible in our context. We also see that for the larger firms in our survey, the share of CEOs as respondents is significantly lower compared to smaller companies (see Footnote 18). This suggests that the survey is redirected to the appropriate representative in the company who is most able to answer our question. Moreover, as we show in Section 5.2.2, prior research using business survey data indicates that firm managers generally provide reliable and accurate forecasts of their firms' behavior. We also show that respondents reliably report firm characteristics in our setting (Section 5.2.3) and that their stated intentions

are strong predictors of actual firm behavior (Section 5.2.4).

5.2.2. Accuracy of Managers' Behavioral Forecasts

Beyond potential biases in survey responses, another concern is whether firm managers can accurately forecast their own behavioral responses to tax changes. Prior research using business survey data suggests that managers generally provide accurate forecasts of firm outcomes such as sales growth, investment, employment, price-setting, and reported firm characteristics. For example, Bloom and Van Reenen (2007) and Bloom and Van Reenen (2010) demonstrate that management surveys can yield reliable and consistent measures that correlate meaningfully with objective outcomes such as profitability and sales growth. 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.

5.2.3. Correspondence of Firm Characteristics

We begin by establishing the degree of correspondence of firm characteristics as stated by the respondents in the survey 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

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 revenue 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].

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 89%, 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 0.80, 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.

5.2.4. 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 opposed 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 local business taxes 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.

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 employment 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].

Table 8: 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 8 illustrates the sample selection process for the LBT change validation exercise.

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: corporate income tax, personal income tax, and local business tax (LBT). Local business taxes apply to both corporate and transparent entities (sole proprietors and partnerships) and are levied on the firm's operating profits. 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 (Fuest et al., 2018). The decentralized authority of local governments to set LBT tax rates results in a substantial number of tax changes, which we can use to test the stated actions of our survey respondents.

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 consecutive 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

Table 9: 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 9 shows the distribution of LBT increases and decreases for the firms in our sample which we are able to link to external data sources.

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.[31] This requirement further reduces our sample to a total of 382 firms experiencing 588 changes in LBT. Finally, we require these firms to 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 8. The resulting subset constitutes approximately 3% of our initial sample of respondent firms. Table 9 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. In Column (1) of Table 10, we consider firms that were assigned to the tax decrease treatment and experienced at least one decrease in LBT during the sample

[31] 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.

period. For these firms, we regress an indicator for a positive change in employment in the year of the tax decrease from Orbis on an indicator for assigning at least 10 percentage points to the employment category in the survey.[32] The large 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 response to actual tax changes, compared to firms which did not indicate employment as a relevant margin.

Columns (2) and (3) show the results of a similar exercise for firms that were assigned and actually experienced a tax increase. Here, the dependent variable is an indicator for a negative change in employment after the tax change. The sample in Column (2) includes all tax increases dating back to 2007. 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. Therefore, in Column (3), we restrict the observed changes in LBT to a five-year window (2018-2022) around the survey date, where firms arguably were in similar economic circumstances compared to the survey. Here, the coefficient for the association is significant on the 10% level.

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-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.

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 related to a potential *increase in employee numbers* and was stated as follows: *"Are you currently planning to hire additional employees in the short term (0-12 months)?"* The second question addressed a potential *decrease in the number of employees* and had a more direct connection to an exogenous shock in the firm's economic environment, and read: *"What measures are you taking in the short-term (0-12 months) to cope with the*

[32] 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 increases, the association becomes marginally weaker but remains significant for the five-year window.

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

	Dependent Variable: Employment change indicator (Orbis)		
	$\mathbb{1}(\Delta\text{Emp.} > 0)$	$\mathbb{1}(\Delta\text{Emp.} < 0)$	
Intercept	0.136*	0.216***	0.222***
	(0.077)	(0.036)	(0.044)
Survey: Employment Change	0.864***	0.126	0.228*
	(0.077)	(0.088)	(0.121)
Sample	Full	Full	5-year window
Survey Treatment	Tax Decrease	Tax Increase	Tax Increase
Num.Obs.	23	169	110
R2 Adj.	0.179	0.008	0.031

Note: Table 10 shows results of testing the correspondence between survey responses and actual responses to changes in LBT. In Column (1), we consider firms that were assigned to the tax decrease treatment and experienced at least one decrease in LBT during the sample period. For these firms, we regress an indicator for a positive change in employment in the year of the tax decrease from Orbis on an indicator for assigning at least 10 percentage points to the employment category in the survey. Columns (2) and (3) show the results of a similar exercise for firms that were assigned and actually experienced a tax increase. Here, the dependent variable is an indicator for a negative change in employment after the tax change. In Column (3), we restrict the observed changes in LBT to a five-year window (2018-2022) around the survey date. Robust standard errors are given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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 these 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 from Orbis 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 em-

Table 11: Proxy Test COVID.

	Dependent Variable: Employment change (Orbis)		
	Perc. Change	$\mathbb{1}(\Delta\text{Emp.} > 0)$	$\mathbb{1}(\Delta\text{Emp.} < 0)$
Survey: Reduce Employment	-0.056**	-0.061**	0.205***
	(0.023)	(0.029)	(0.032)
Survey: Increase Employment	0.059**	0.170***	-0.049**
	(0.024)	(0.026)	(0.023)
Num.Obs.	1,506	1,506	1,506
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 from Orbis over the year after the survey was conducted on indicator variables for hiring and firing plans stated in the survey, 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. Robust standard errors are given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

ployment 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.

6. Conclusion

The question of who bears the economic incidence of taxes on company profits is a first-order question and remains an active area of research. We contribute to this literature strand by pursuing a novel empirical strategy based on reported 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 business 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, yet they benefit only marginally from tax reductions. Similarly, capital owners experience a greater

burden from tax increases (through reduced distributed profits) than they gain from tax cuts. Conversely, employees experience an asymmetric effect in the opposite direction: while tax hikes have a limited impact on wages and employment, tax reductions result in more substantial wage and employment gains. 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 compared to small tax changes. This non-linearity seems to stabilize for medium to large changes, indicating that a local perturbation in the tax rate exerts different effects compared to larger tax changes. Relying on the large set of observable company characteristics, we further investigate heterogeneity in profit tax incidence.

While our methodology enables the simultaneous analysis of multiple adjustment margins and allows us to explore heterogeneity in responses to the sign and magnitude of tax changes, it also faces limitations inherent in a survey-based approach. For instance, the use of hypothetical tax scenarios may lead to reduced respondent effort or experimenter demand effects. Although we cannot entirely rule out these concerns, we address them through detailed discussions, comparisons with established findings in the literature, and a series of empirical validation tests, all of which indicate that our survey results might yield meaningful and informative insights.

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.

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Online Appendix for

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

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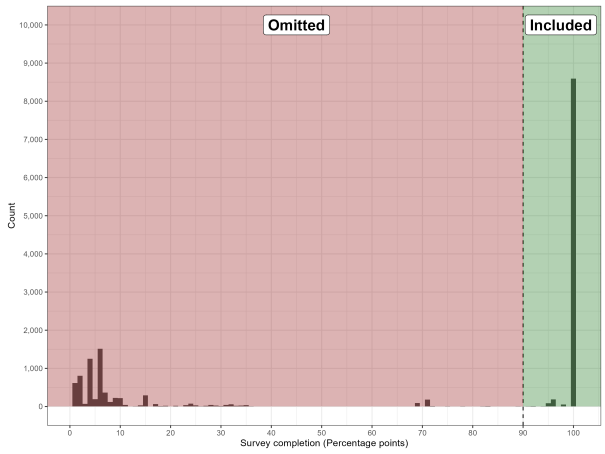
A. Additional Figures and Tables

A.1. Completion Rate

Figure A1 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 = 8,955) 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.6 in the Online 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 A1: Completion Rate



Note: Figure A1 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

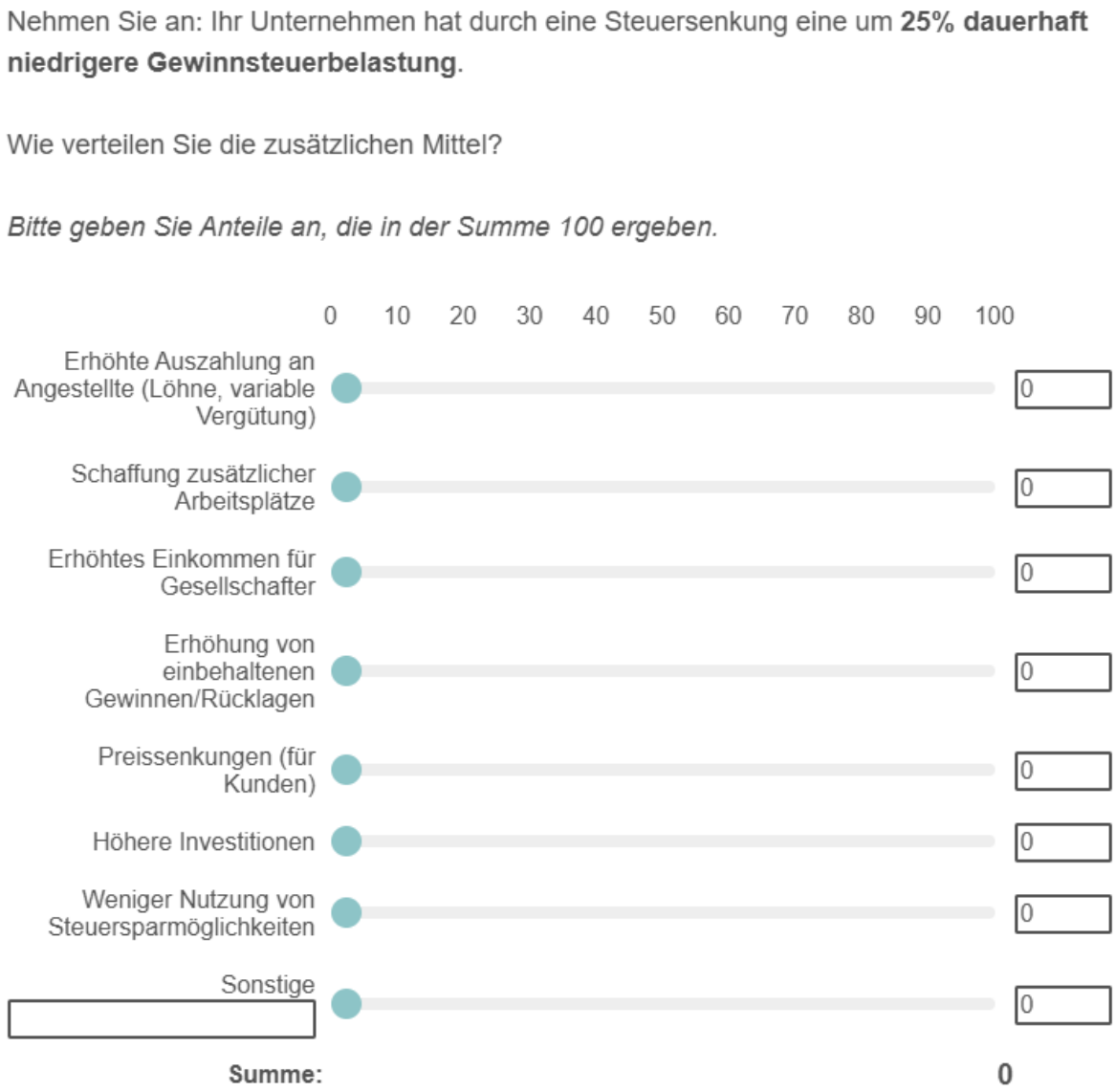
Figure A2: Example Survey Question Tax Decrease Treatment - 1%

Note: Figure A2 shows an example of the tax decrease (1%) 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 A3: Example Survey Question Tax Decrease Treatment - 10%

Note: Figure A3 shows an example of the tax decrease (10%) 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 A4: Example Survey Question Tax Decrease Treatment - 25%



Note: Figure A4 shows an example of the tax decrease (25%) 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 Treatments

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

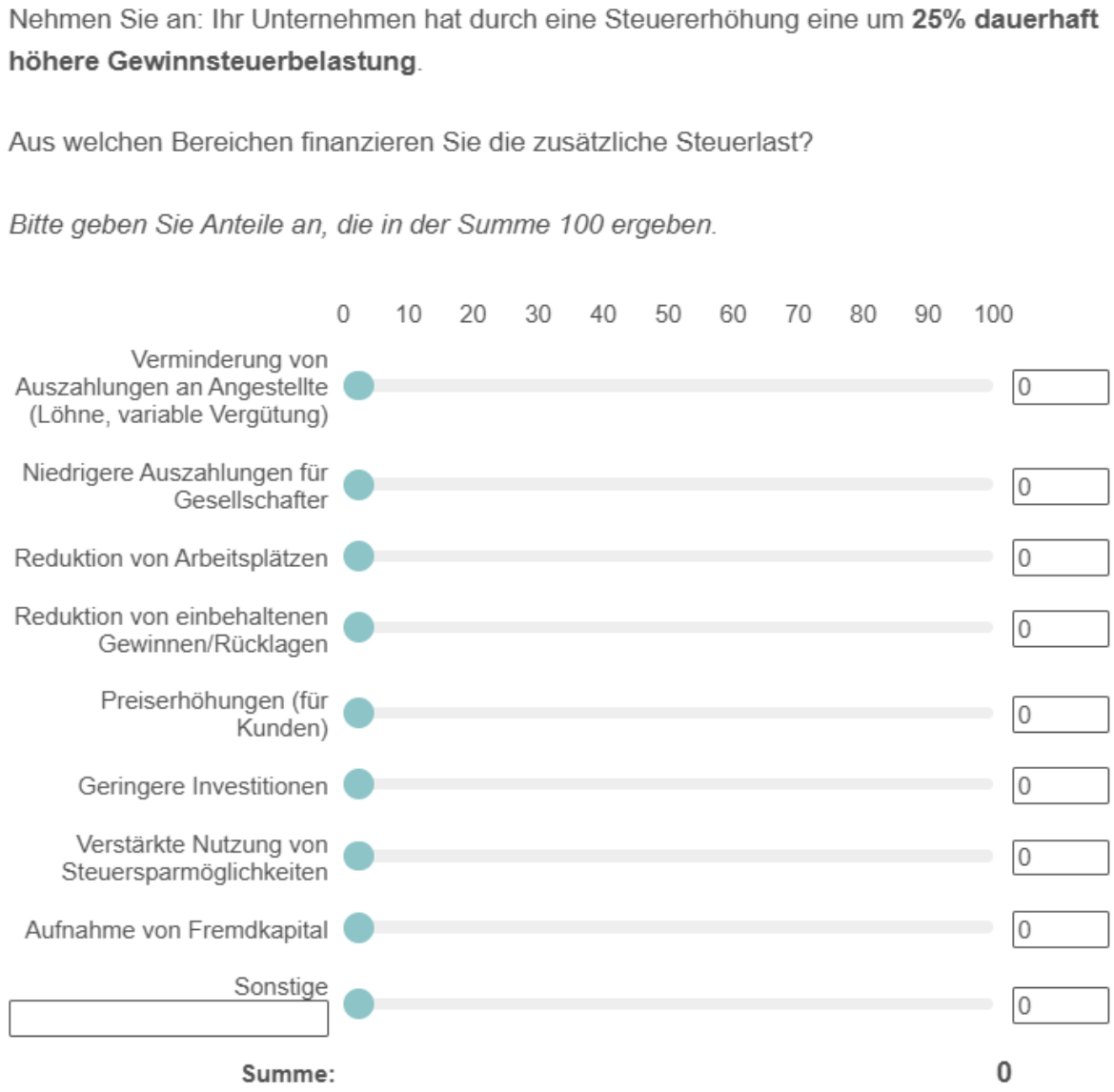
Figure A5: Example Survey Question Tax Increase Treatment - 1%

Note: Figure A5 shows an example of the tax increase (1%) 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 A6: Example Survey Question Tax Increase Treatment - 10%

Note: Figure A6 shows an example of the tax increase (10%) 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 A7: Example Survey Question Tax Increase Treatment - 25%



Note: Figure A7 shows an example of the tax increase (25%) 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.

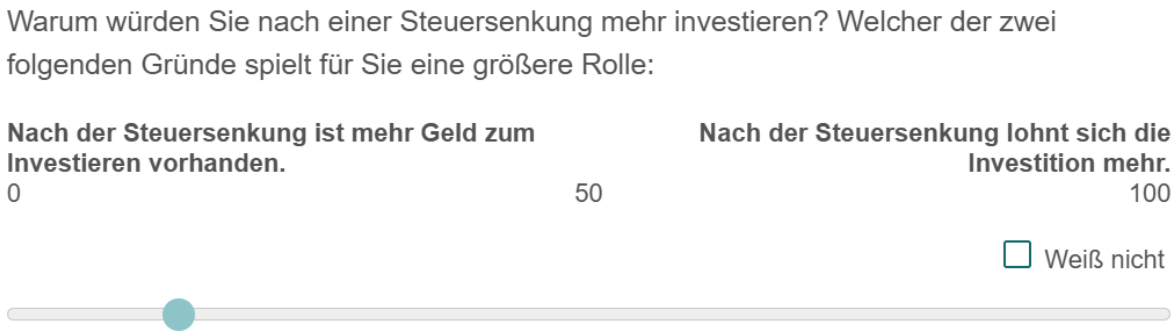
A.2.3. Reasons for Change in Investment - Tax Decrease Treatments

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 A8: Example Survey Question Reasons for Change in Investment - Tax Decrease



Note: Figure A8 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.

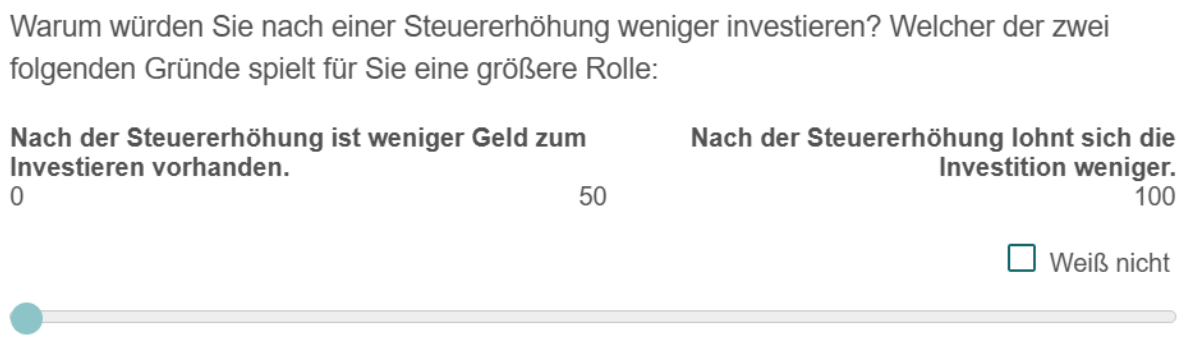
A.2.4. Reasons for Change in Investment - Tax Increase Treatments

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 A9: Example Survey Question Reasons for Change in Investment - Tax Increase



Note: Figure A9 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.

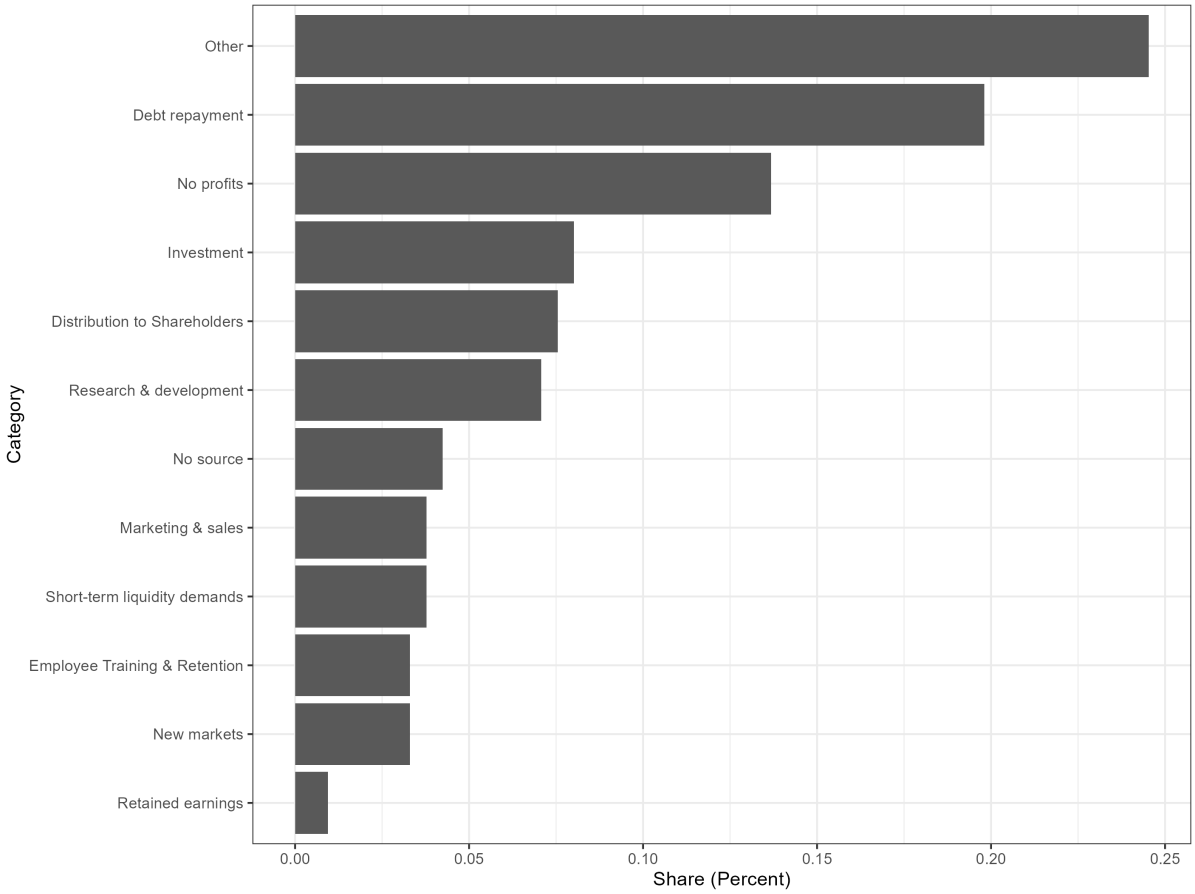
A.3. Text Entries - Other Category

After receiving the randomized tax decrease and increase treatments, participating firms selected from a comprehensive list of adjustment categories, detailed in Section 2.2 and Online 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 A10 and A11 illustrate the text responses given in the *Others* category for the tax decrease and tax increase treatments, respectively.

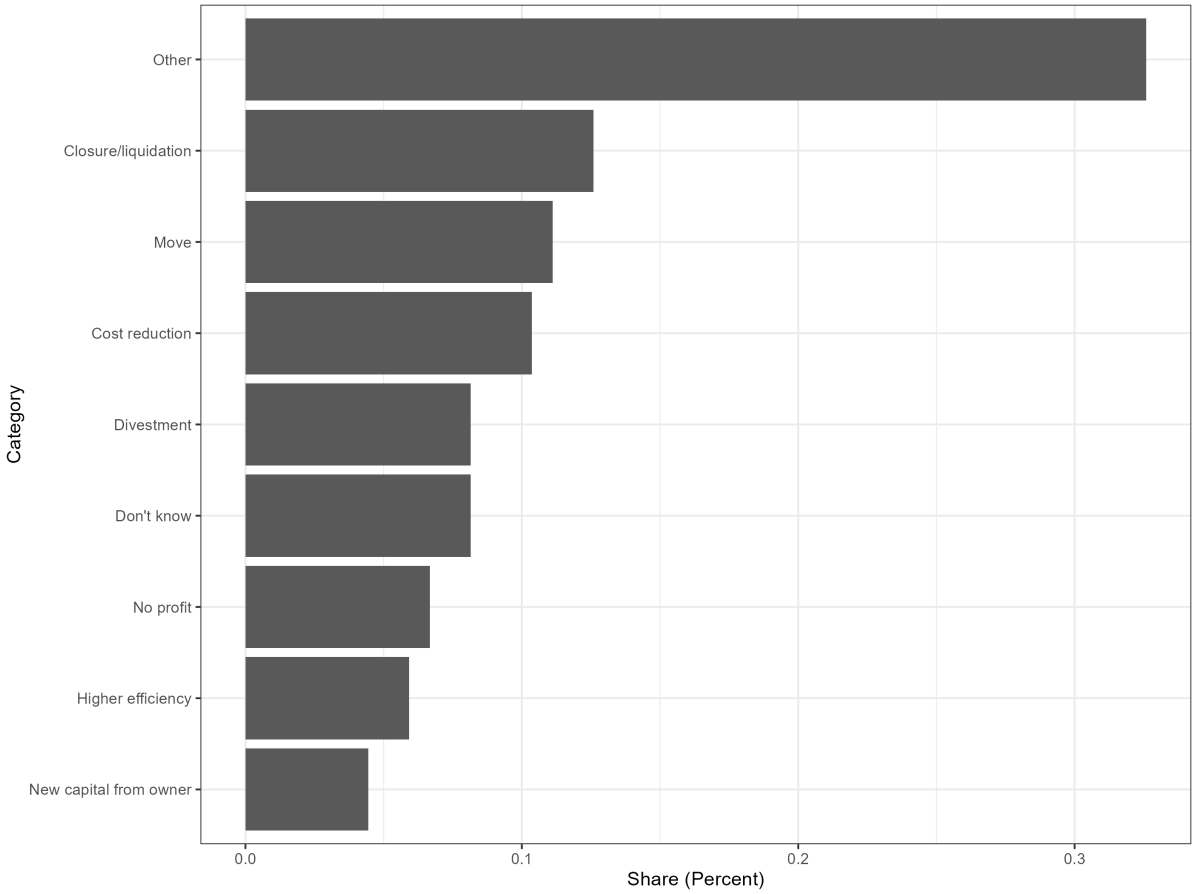
For the **tax decrease treatment**, the most frequently mentioned missing category appears to be the use of additional funds for debt repayment, as indicated by terms such as liabilities (Verbindlichkeiten), repayment (Rückzahlung), and loans (Kredite, Darlehen). Additionally, some firms noted that they 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 A10: Free Text Entries - Tax Decrease Treatment



Note: Figure A10 displays the most common categories selected by respondents in the **tax decrease treatment** after indicating a positive share in the *Others* category.

Figure A11: Free Text Entries - Tax Increase Treatment

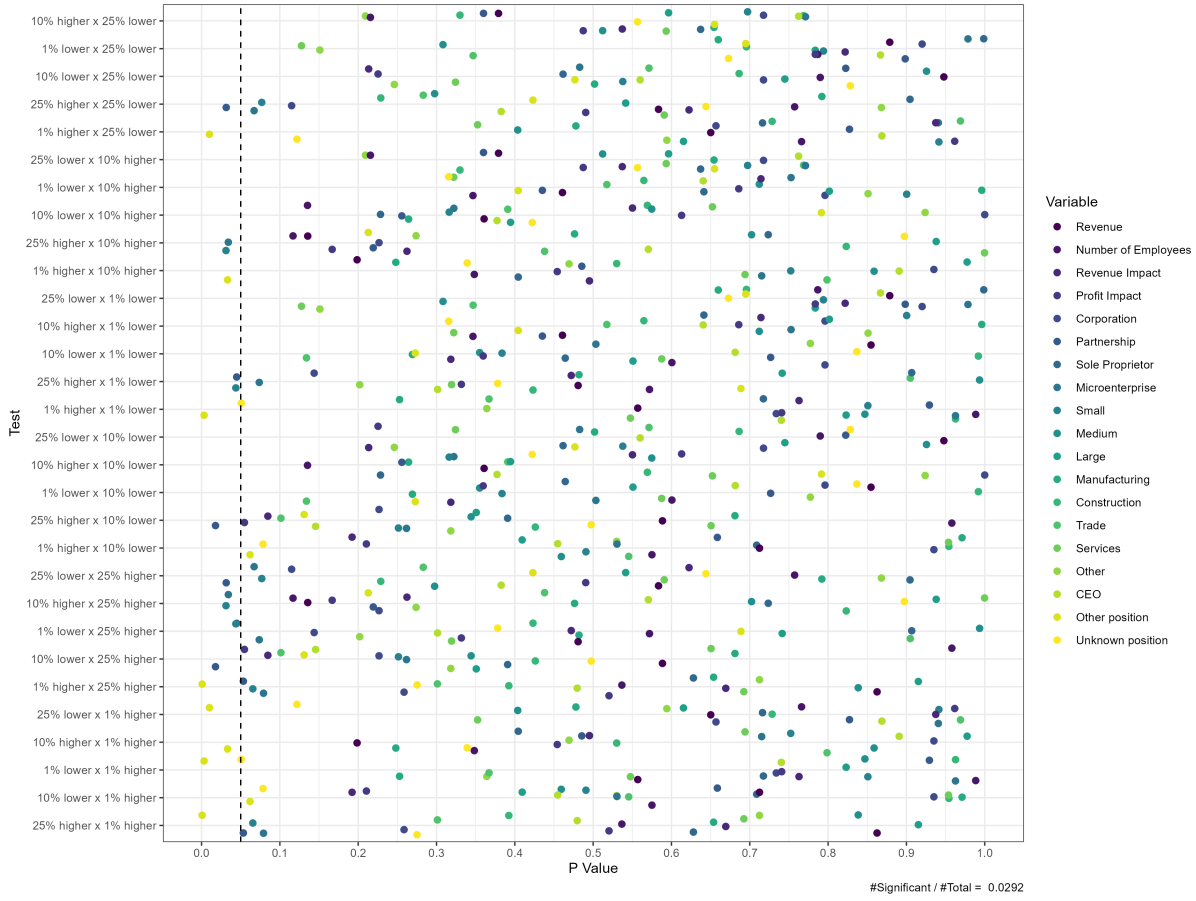


Note: Figure A11 displays the most common categories selected by respondents in the **tax increase treatment** after indicating a positive share in the *Others* category.

A.4. Balance Tests

To assess the effectiveness of our randomization procedure, we conducted multiple balance tests using the available characteristics of survey respondents. Figure A12 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 A12: Covariate Balance Tests Across Treatment Cells.



Note: Figure A12 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.5. 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 German Business Panel (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),[1]
- **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 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.[2] This

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

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

approach helps stabilize the variance of the survey weights and minimizes the influence of extreme values in the analysis.

Table A1 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.

Table A1: 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 A1 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.6. Orbis Comparison: Participants vs. Non-participants

Section A.6 presents a balance table (Table A2) 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, cost of employees, and taxes on income, 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 A2: Orbis Comparison: Participants vs. Non-participants

	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	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
Taxes on Income	58	658,508.95	230,522.50	39,746	965,969.31	142,365.00	0.82

Note: Table A2 compares the sample of firms participating in our survey with the reference group of German firms from Orbis that did not participate or did not complete our survey, using data from the reporting year 2019. The comparison includes total assets, number of employees, turnover, cost of employees, and taxes on income, with non-negative values required for all 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 A3 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 A3 and Table A2 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 A3 (linked firms) and 6 in Table A2 (survey participants). Additionally, firms in the linking group and with information on employees in Orbis reported a lower number of employees in the survey itself (Mean: 43, Median: 6), reinforcing the reliability of our survey responses.

Table A3: 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

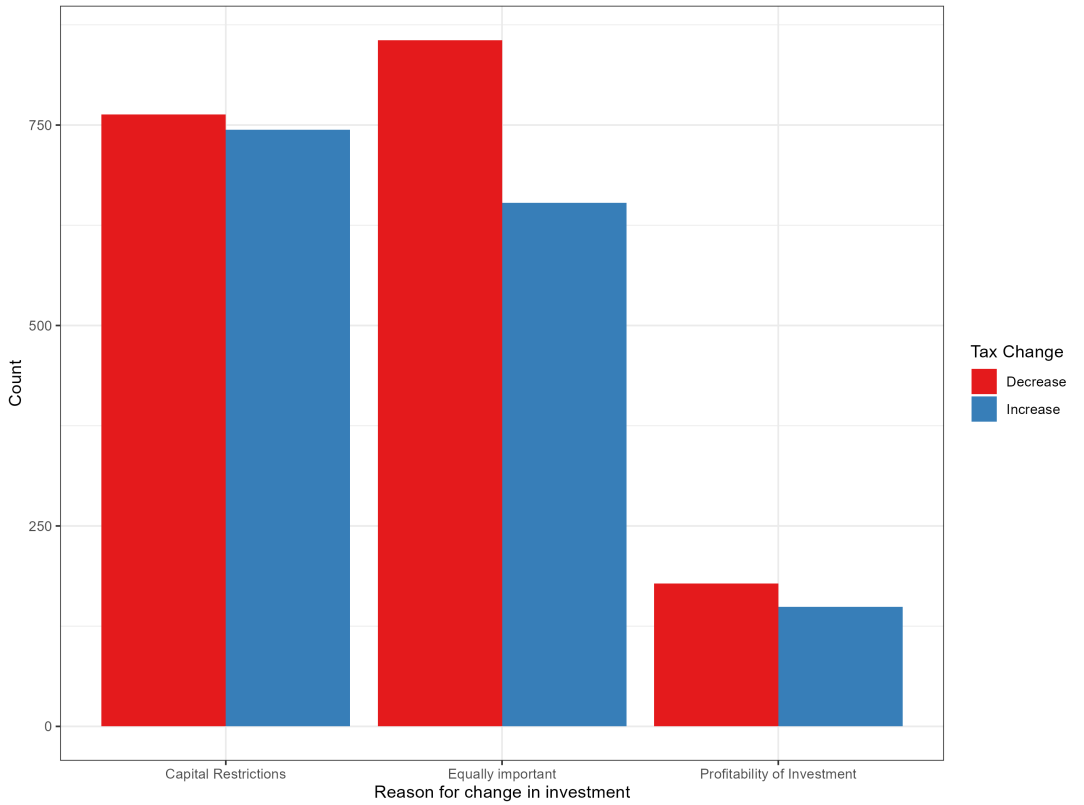
Note: Table A3 compares firms in our survey that consented to linking their survey data with external databases like Orbis to those that declined. The comparison is based on key firm characteristics, including revenue, number of employees, legal form, and sector. For each variable, we report the number 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. The smaller sample size for revenues is due to a lower response rate for the numerical revenue category in our survey.

A.7. 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 A8 in Online 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 A13: Reasons for Change in Investment



Note: Figure A13 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.

Figure A13 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.

A.8. Robustness Tests - Main Results

A.8.1. Ordinary Least Squares (OLS) Estimation

In this section, we present the results of the Ordinary Least Squares (OLS) estimation of Equation (1). These results, shown in Table A4, form the basis of the main findings discussed in Section 3.2.

For each category of incidence (i.e., the dependent variables listed in Table A4), we apply Ordinary Least Squares (OLS) estimation to Equation (1):

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

where the dependent variable, y_i, 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.

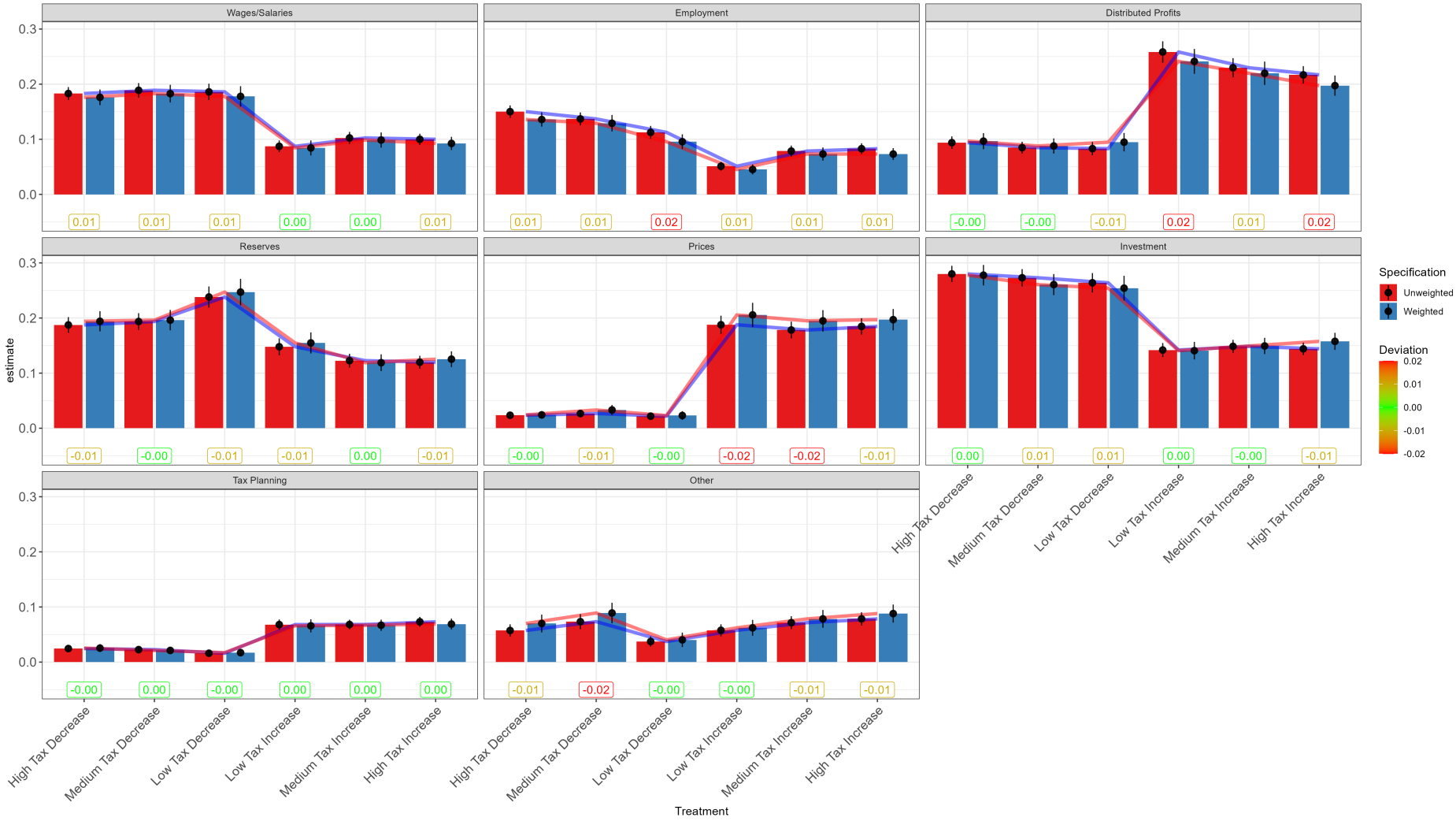
We begin by exploring the sensitivity of our main results with respect to the usage of survey weights. Figure A14 compares our main estimates from Equation (1) with weighted regressions that apply the survey weights described in Section A.5. The comparison suggests that there are only minor differences 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 A5 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 dummies for firm size – small, medium, and large – based on annual revenue. Additionally, we introduce an indicator 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 net income is lower than the median of the distribution.

The results of Equation (1) estimated using OLS with controls, as shown in Table A5, 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 3.2.

Figure A14: Weighted versus Unweighted Regressions



Note: Figure A14 shows the estimated incidence share for the respective category across the six different treatments based on Equation (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 A4: 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 A4 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), and interaction terms. The benchmark group represented by the constant coefficient is respondents assigned to a small tax decrease. Hence, the incidence share for respondents assigned to a small tax increase is given by the sum of the constant coefficient and the coefficient of Increase. Robust standard errors are given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: 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 A5 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.8.2. 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

$$E[y_{im}|\boldsymbol{X}] = G_m(\boldsymbol{X}; \boldsymbol{\beta}) \in (0, 1), \; m = 1, \ldots, M \tag{3}$$

$$\sum_{m=1}^{M} y_{im} = 1, \; i = 1, \ldots, N, \tag{4}$$

$$\Pr(y_{im} = 0|X) > 0 \; \forall m = 1, \ldots, M, \tag{5}$$

$$\Pr(y_{im} = 1|X) > 0 \; \forall m = 1, \ldots, M, \tag{6}$$

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 (3) 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 $\boldsymbol{X}$.[3] Equation (4) 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 (5) and (6) illustrate that individual shares might attain boundary values with non-trivial probabilities, which 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 (3)) as well as the unit-sum constraint (Equation (4)) 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

[3] 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.

$[0, 1]$ 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 multinomial logit functional form given as

$$E[y_{im}|\boldsymbol{X}] = G_m(\boldsymbol{X};\boldsymbol{\beta}) = \frac{\exp(\boldsymbol{x}_i'\boldsymbol{\beta}_m)}{\sum_{l=1}^{M}\exp(\boldsymbol{x}_i'\boldsymbol{\beta}_l)},\ m = 1, \ldots, M. \tag{7}$$

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

$$E[y_{im}|\boldsymbol{X}] = G_m(\boldsymbol{X};\boldsymbol{\beta}) = \frac{\exp(\boldsymbol{x}_i'\boldsymbol{\delta}_m)}{1+\sum_{l=1}^{M-1}\exp(\boldsymbol{x}_i'\boldsymbol{\delta}_l)},\ m = 1, \ldots, M, \tag{8}$$

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

$$\begin{aligned}
\hat{APE}_{mk} &= \frac{1}{N}\sum_{i=1}^{N}\hat{PE}_{mki} \\
&= \frac{1}{N}\sum_{i=1}^{N}\frac{\Delta E[y_{im}|\boldsymbol{x}_i]}{\Delta x_{ik}} \\
&= \frac{1}{N}\sum_{i=1}^{N}\frac{\exp(\boldsymbol{x}_{-k,i}'\boldsymbol{\beta}_{m,-k}+\beta_{mk})}{1+\sum_{l=1}^{M-1}\exp(\boldsymbol{x}_{-k,l}'\boldsymbol{\beta}_{l,-k}+\beta_{lk})} - \frac{\exp(\boldsymbol{x}_{-k,i}'\boldsymbol{\beta}_{m,-k})}{1+\sum_{l=1}^{M-1}\exp(\boldsymbol{x}_{-k,l}'\boldsymbol{\beta}_{l,-k}+\beta_{lk})},
\end{aligned} \tag{9}$$

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

Table A6 compares the average partial effects estimated using Ordinary Least Squares (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 A6: 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 A6 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.9. Robustness Tests - Heterogeneity

In Section 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 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

$$y_i = \beta_0 + \beta_1 \text{ Increase}_i + \boldsymbol{\gamma}_1' \, \boldsymbol{x}_i^* + \boldsymbol{\gamma}_2' \text{ Increase}_i \times \boldsymbol{x}_i^* + \varepsilon_i, \tag{10}$$

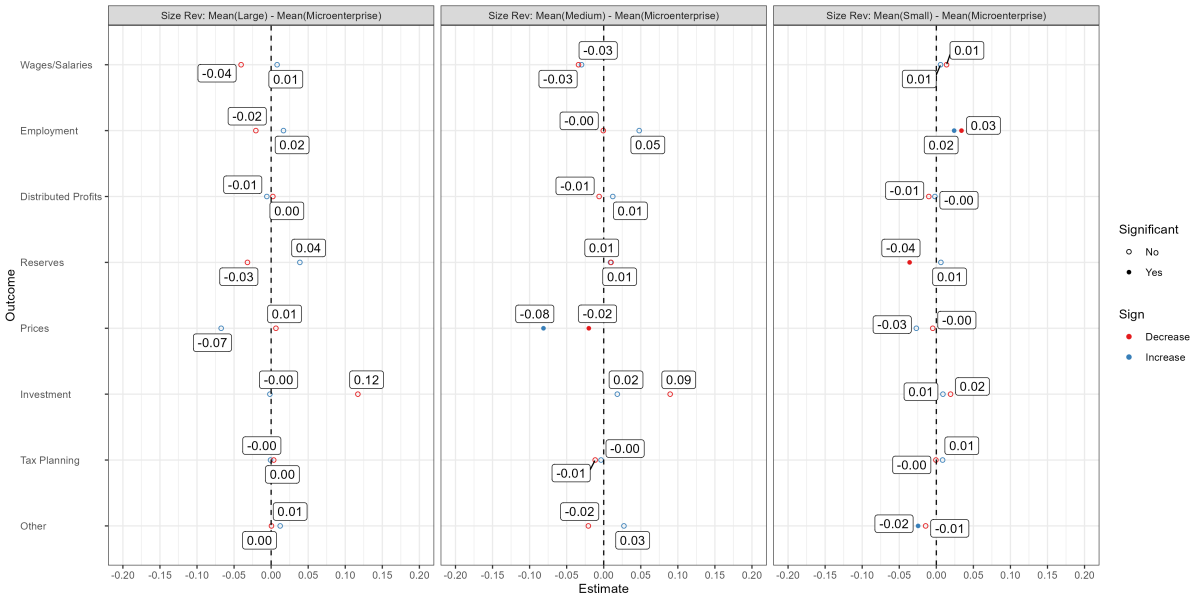
where $\boldsymbol{x}_i^*$ represents a vector of firm characteristics: dummies for firm size, economic sector, legal structure, and net income impact from COVID-19.[4] As in Specification (2), we aggregate the treatment intensity groups and classify treatment solely based on whether an individual firm 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 A15 (firm size), Figure A16 (economic sector), Figure A17 (legal form), and Figure A18 (net income impact from COVID-19) display the average partial effects for each comparison with the baseline, based on the estimated coefficients from Equation (10). 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.

The results closely align with those in Section 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.

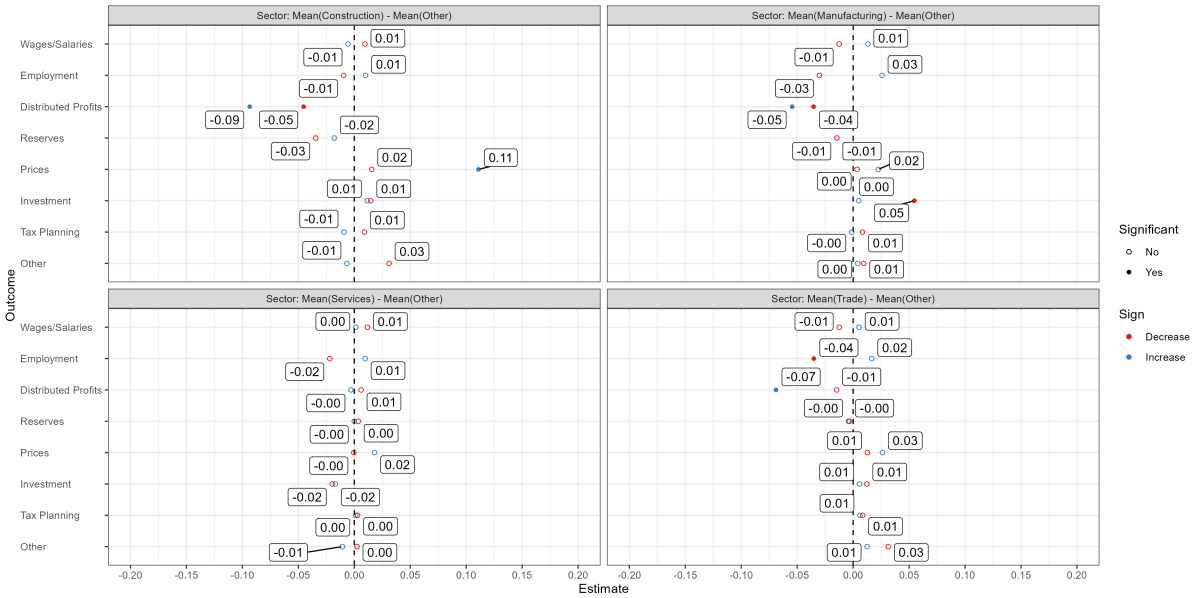
[4] We measure firm size based on reported 2019 revenue, following the European Commission's classification: micro-enterprises (<EUR 2 million), small (<EUR 10 million), medium (<EUR 50 million), and large (≥ EUR 50 million). For economic sector classification, firms self-reported their industry in the 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.

Figure A15: Average partial effects of revenue size category with control variables.



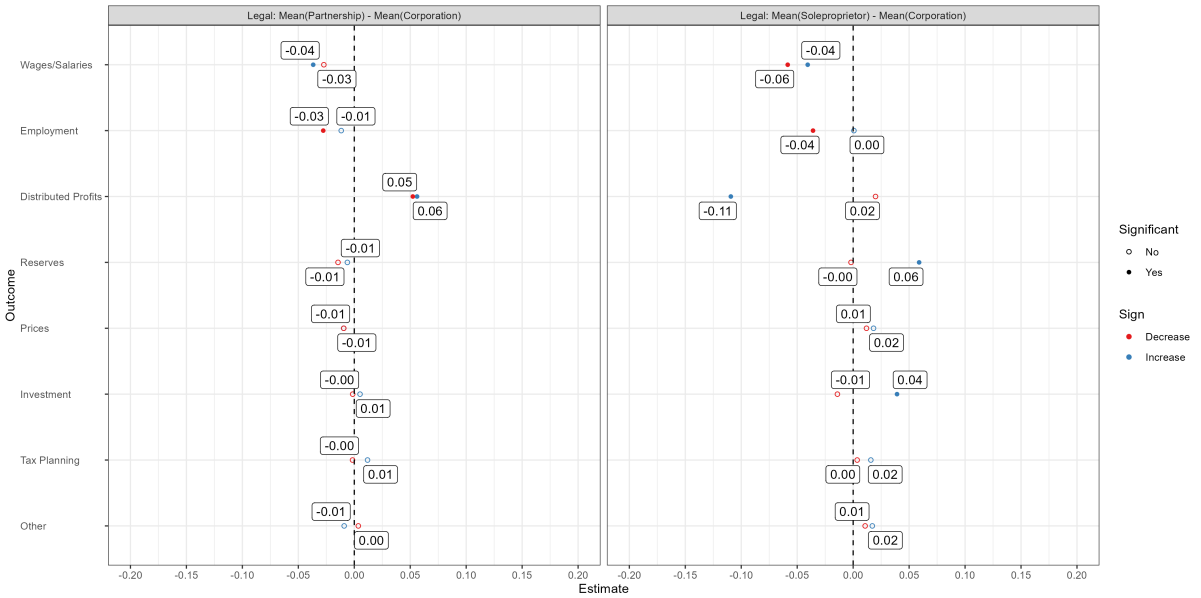
Note: Figure A15 shows heterogeneity in incidence by company size measured by revenues. The figure shows average partial effects for each comparison with the baseline based on the estimated coefficients from Equation (10). Average partial effects with a significant p-value after applying the Benjamini and Yekutieli (2001) correction are denoted by filled dots, whereas insignificant effects are illustrated by hollow circles.

Figure A16: Average partial effects of economic sector with control variables.



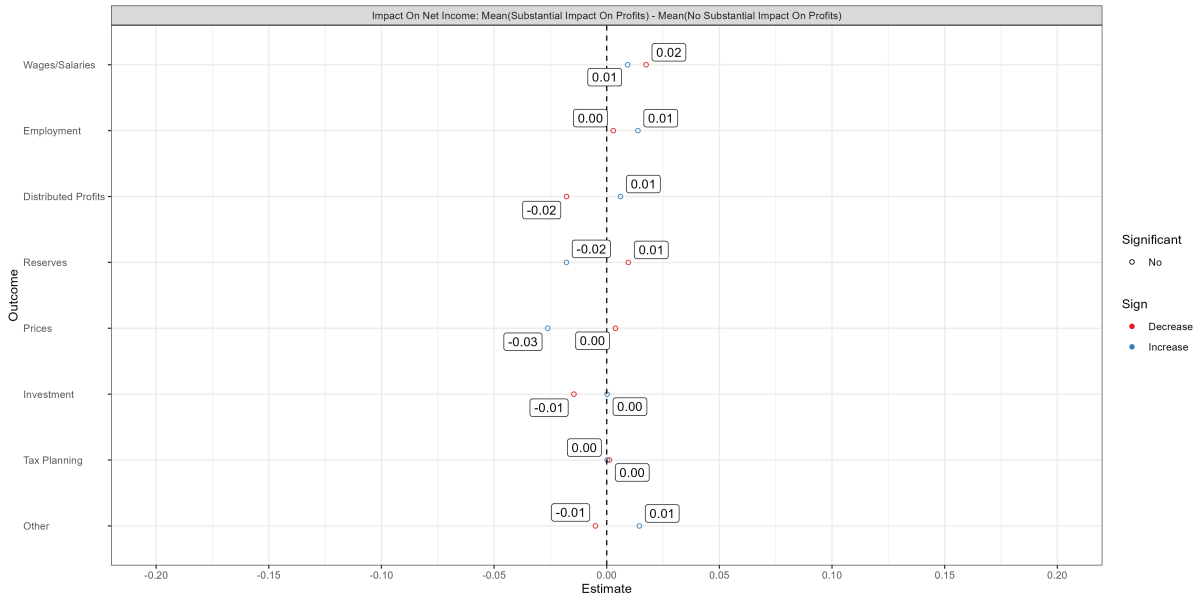
Note: Figure A16 shows heterogeneity in incidence by economic sector. The figure shows average partial effects for each comparison with the baseline based on the estimated coefficients from Equation (10). Average partial effects with a significant p-value after applying the Benjamini and Yekutieli (2001) correction are denoted by filled dots, whereas insignificant effects are illustrated by hollow circles.

Figure A17: Average partial effects of organizational form with control variables.



Note: Figure A17 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 (10). 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 A18: Average partial effects of COVID-19 impact on net income with control variables.



Note: Figure A18 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 (10). 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.

B. Incidence Estimates in Previous Literature

Table B1 provides a comprehensive summary of previous studies on tax incidence. In contrast to Table 5, which highlights selected recent studies, Table B1 also incorporates corporate tax incidence estimates from earlier published research and working papers.

Table B1: Tax Incidence Estimates

Paper	Tax Variation	Tax Change	Country	Episode	Incidence on			
					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)	51% (long run) 36% (short run)	-	-
Azémar and Hubbard (2015)	Cross-country variation in the statutory corporate tax	Increases and Decreases (pooled)	13 OECD countries	1980–2004	60%	-	-	-
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%	-
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%	-	-
Dobridge et al. (2021)	Variation in the Domestic Production Activities Deduction	Decrease	USA	1999-2015	80%	20%	-	-
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 (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%	-	-	-
Felix and Hines (2022)	State tax changes	Variation between unionized and non-unionized workers	USA	2000	31% (fully unionized firm)	-	-	-
Fuest et al. (2018)	Variation in local business tax changes	Increases (93% Increases)	Germany	1993-2012	51%	49%	-	-
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	-	36% Range: 28%-39%	64% Range: 61%-72%	-
Kennedy et al. (2024)	US corporate tax change (TCJA)	Decrease	USA	2013-2019	48%	51%	-	-

(Table continues on the next page)

Paper	Tax Variation	Tax Change	Country	Episode	Incidence on			
					Workers	Firm Owners	Consumers	Land Owners
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%	-	-
Suárez Serrato and Zidar (2016)	Variation in US state taxes and apportionment rules	Increases and Decreases (pooled)	USA	1980–2012	30-35%	40%	-	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%	-	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%	-	10-25%

Note: Table B1 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 provided, or that it was explicitly assumed to be 0% in the respective study.

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