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

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We provide novel evidence on the incidence of business taxes using comprehensive survey and experimental data from German firms. Managers respond asymmetrically to randomized hypothetical tax changes of opposite signs, consistent with asymmetric profit tax incidence. They indicate that tax cuts would be primarily passed on to workers and used for investment projects, while tax increases would be shifted to consumers via higher prices and absorbed by firm owners via lower profit distributions. We further show that the stated incidence on workers rises with the absolute size of the tax change, partially offsetting the burden borne by firm owners.

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, Sun and Yannelis, 2023) as well as hiring policy and wage negotiations (Arulampalam, Devereux and Maffini, 2012; Fuest, Peichl and Siegloch, 2018; Dwenger, Steiner and Rattenhuber, 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 par-

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ticular 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, Sanz-Maldonado and Slemrod, 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 large-scale survey of German firms to investigate how managers respond to hypothetical tax changes along many different margins and for different types of hypothetical 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 eliciting how firms would adjust to tax changes along a comprehensive set of adjustment margins within a unified framework while randomly varying the size and the sign of hypothetical tax changes. While we acknowledge that surveys have some limitations in comparison with well-identified observational studies (see further below in the Introduction and Section IV.B), 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 an analysis of how managers state, in a survey, that they

[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, Mei and Stantcheva (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 IV.B.

respond to hypothetical changes in tax rates along key managerial decision margins. We focus on managers' stated direct adjustment decisions in the short run, abstracting from general equilibrium implications.[2] We take a straightforward approach and ask firms how profit taxes would affect a set of decision margins in their companies. For this purpose, we randomly assigned survey respondents to hypothetical permanent tax increases and decreases of varying magnitudes, and inquired either how the additional funds available after a tax cut would be used or from which sources funds would be diverted to pay for the increased tax burden. Respondents were presented with an exhaustive list of categories to which they could attribute shares of the change in tax burden, e.g., wages, prices, shareholder distributions, investments, etc. Each of the shares was required to be an integer between zero and 100, and shares needed to sum to 100 across categories. In order to make sure that no relevant category was missing, we included an open field, where firms could indicate the missing category and the respective share. This design allows us to infer the (stated) full distribution of a, say, EUR 100 hypothetical change in tax burden and to determine the specific relative importance of each possible response margin.

Using this setup, we are able to examine how the stated adjustment decisions by the manager would impact employees, firm owners and customers (i.e., for a given level of pre-tax profits). At the same time, we also measure channels through which indirect effects would 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 hypothetical 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 hypothetical tax change allows us to tease out the sensitivity of the stated responses 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 stated incidence implications of differently signed hypothetical tax changes as well as differences in treatment intensity.

Our main findings can be summarized as follows. First, survey answers imply that managers would respond asymmetrically to tax-rate changes of different

[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, Peichl and Siegloch, 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.

signs. The survey responses suggest that for every EUR 100 of additional funds available due to a lower tax burden, EUR 32 would be received by workers in the form of higher wages or new jobs, EUR 29 would benefit firm owners through distributed and retained profits and only EUR 2 would be used to reduce output prices benefiting customers. Moreover, EUR 27 would be used to finance new investment projects and the remaining EUR 10 would be used for other purposes. This presents a stark contrast to the stated 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 would be financed less by workers (EUR 17), and more by owners (EUR 37) and consumers (EUR 18). Respondents state that they would finance the remaining EUR 28 through indirect channels: EUR 15 of the tax increase is offset by a reduction in planned investments, while the roles of increased tax-saving opportunities, new debt acquisition and other categories are comparatively minor.

Second, we observe heterogeneous responses with regard to the size of the tax change treatment. When exposed to a larger hypothetical tax change, respondents attribute a higher share to the employment margin. The results indicate that this increased worker incidence is mainly offset by lower 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 tax increase becomes larger. Conversely, for the tax decrease treatments, respondents assign a larger share to the employment margin as the tax reduction becomes larger.

Finally, exploiting the presence of a rich set of company characteristics in our survey data, we investigate treatment effect heterogeneity. Our results suggest that the positive investment responses to hypothetical tax cuts increase in company size, plausibly reflecting differences in investment opportunities and general growth prospects. We further document sector-specific differences. Respondents from the construction sector assign a substantially higher share of hypothetical tax hikes to consumers via price increases. This could be explained by relatively low profit margins and inelastic demand, which 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 state that additional funds would be used 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 stated incidence on owners depends on the legal form of the company. Managers in partnerships attribute a higher share of the incidence to owners compared to managers in corporations and sole proprietorships, potentially reflecting differing levels of profitability across legal forms.

Our survey design enables an examination of how an array of potential adjustment margins is affected by hypothetical 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 methodol-

ogy 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, Roth and Wohlfart, 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 (Moody's, 2024) over the same time horizon. Both tests indicate the predictive power of the stated actions for actual behavior.

Second, following the approach in Colarieti, Mei and Stantcheva (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 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. 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 consider the outcomes that are most relevant to the three

[3]In Colarieti, Mei and Stantcheva (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.

stakeholder groups: wages and employment, firm profits and distributions to owners, and consumption prices – though in the form of stated manager plans, as opposed to revealed actions. In addition, we consider 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 (2025) 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).[4]

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 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 respond more strongly to hypothetical 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 hypothetical 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

[4]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, Devereux and Maffini, 2012; Fuest, Peichl and Siegloch, 2018; Dwenger, Steiner and Rattenhuber, 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, Osbat and Reinelt (2022); Baker, Sun and Yannelis (2023); Jacob, Müller and Wulff (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, McClure and Stomberg, 2022; Bornemann, Jacob and Sailer, 2023; Ohrn, 2023).

examine tax changes of varying magnitudes.

I. Survey Design and Data

A. *Survey and Sampling*

Our tax incidence questions were fielded in the second wave of the German Business Panel (GBP) (2022). 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. (2025). 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.[5]

To address this issue, we construct survey weights to make our sample representative of the broader German firm population. Table 3 in Section II.B and Figure A14 in the Supplemental Appendix 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, Haider and Wooldridge, 2015). This approach is appropriate in our setting because the sampling design minimizes the need for weighting. Consistent with this, estimated coefficients are stable when controls are included, 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 using 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.[6]

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

[5]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.

[6]Supplemental 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.

survey via email. The GBP sent invitation emails for the online survey on 45 work days between November 16, 2020 and January 22, 2021.[7] Firms were randomly assigned to one of the 45 days. After 7, 14, and 28 days, reminder emails 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.[8]

B. 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 through A7 in the Supplemental Appendix provide examples of the tax incidence questions as appearing in the online interface of the survey in German. Respondent companies were randomly assigned to one of the six different treatment groups defined by the combination of i) direction of tax change, either increase or decrease; and ii) magnitude of the tax change, either 1%, 10% or 25%.[9] 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.[10] 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

[7] 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).

[8] Figure A1 in the Supplemental Appendix illustrates the distribution of the completion share in our data. Additionally, Supplemental 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.

[9] 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****?"*

[10] 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.

as the corporate income tax.[11] 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.[12] 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 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.[13]

[11]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.

[12]Of course, larger firms are more likely to operate internationally (Eaton, Kortum and Kramarz, 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 III for an analysis of response heterogeneity with respect to firm size and other firm characteristics.

[13]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, Steiner and Rattenhuber (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.

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).[14]

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, Lester and Verdi, 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, Devereux and Maffini, 2012).

According to classical tax incidence literature (Harberger, 1962; Fuest, Peichl and Siegloch, 2018), higher taxes can also lead to increased product prices for customers due to lower output as firms reduce investment. This often results in a shrinkage at both firm and industry levels (Djankov et al., 2010; Brekke et al., 2017; Giroud and Rauh, 2019), driven by marginally profitable firms exiting the market or downsizing at the firm level due to rising marginal costs of capital and labor (Jacob, Müller and Wulff, 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, Müller and Wulff, 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, Peichl and Siegloch, 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

[14]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.

(Ohrn, 2018).[15] 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 in the Supplemental Appendix 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.

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 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.

An important feature of our experimental design is that we can test asymmetric firm responses to tax increases and decreases. Conceptually, there are several

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

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, Peichl and Siegloch, 2018). In contrast, tax cuts may offer more flexibility for expansion. On the pricing side, firms may more readily 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 hypothetical tax changes of different magnitudes have different effects on stated incidence, 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 randomized. While this could theoretically introduce some ordering effects, we are confident that this is not a major concern in our setting for two reasons. First, as the entered shares had to sum to 100, respondents could not consider the options in isolation but in the context of the full picture. Moreover, respondents could only proceed to the next screen once the sum constraint was satisfied. Second, the descriptive survey results presented below do not reveal a pecking-order pattern, in the sense that the first few categories are chosen to a larger degree than the others. Furthermore, we acknowledge that in the final implementation of the online survey by the GBP, the order of the second and third categories was switched across the increase and decrease treatment groups. This is illustrated, for example, in Figure A2 in combination with Figure A5. 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.

C. 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.[16] 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.[17]

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 (Supplemental Appendix A.4) summarizes the results of our balancing tests. The figure shows the p-values for difference-in-means tests for each characteristic across every combination of treatments. The overall share of significant differences is 2.9%, 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.

II. Full Distribution of Tax Changes

In this section, we present our main results on the stated 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.

[16] 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 Supplemental 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.

[17] 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.

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
Number of Employees	6,749	68	2,221	1	5	37
Corporation	6,749	0.73	0.44	0	1	1
Sole Proprietor	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.

A. *Empirical Strategy*

For each of our incidence categories, we estimate the following equation using Ordinary Least Squares (OLS):[18]

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

where the dependent variable y_i is the share attributed to the respective category. The independent variables of interest are Increase_i, Medium Change_i, and Large Change_i and their interactions, which are indicator variables for the respective sign and magnitude (10% and 25%, respectively). The set of coefficient estimates, $\beta_i, i = 0, \ldots, 5$, allows us to empirically test for asymmetry between positive and negative tax treatments, as well as the incremental effects of the magnitude of the hypothetical tax change.[19]

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

[19] 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 Supplemental Appendix A.8.1 and suggest that

B. Results

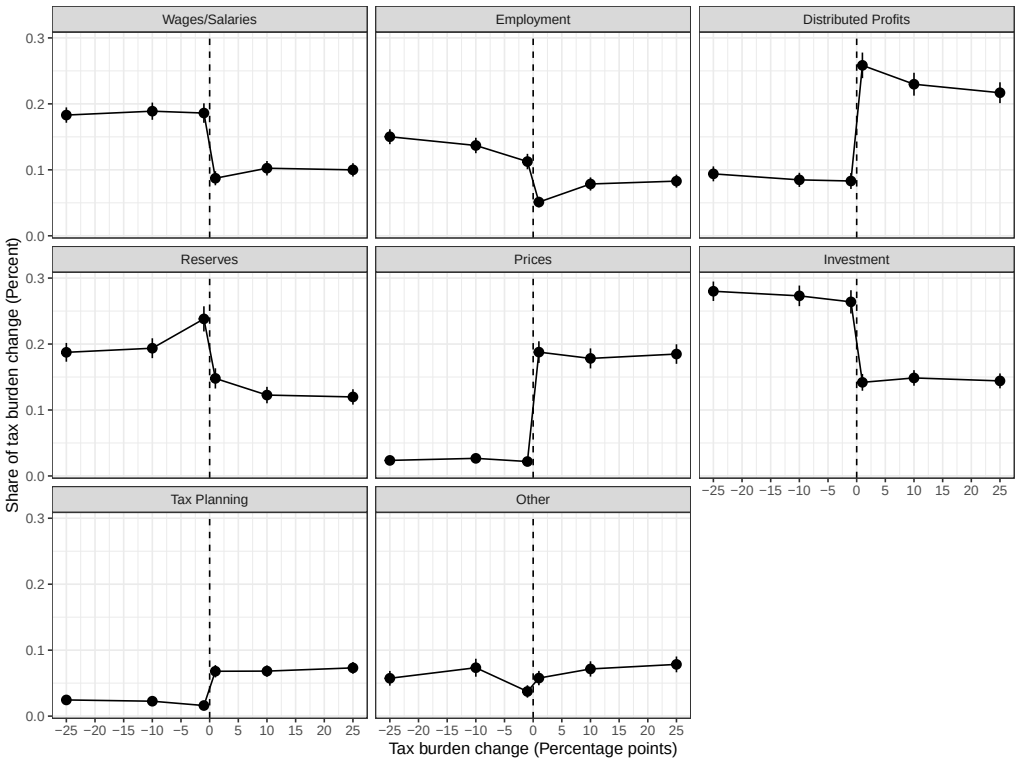


FIGURE 1. STATED RESPONSES TO PERCENTAGE CHANGE IN PROFIT TAX BURDEN BY OUTCOME MARGIN AND TREATMENT GROUP

Note: Figure 1 shows stated responses to tax rate changes by outcome margin and treatment group. Each panel shows the estimated share for the respective margin 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 I.B for more details on the categories and Section IV.A for how they relate to the incidence margins investigated in the previous literature. Robust confidence bounds are indicated by vertical lines.

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 coefficient plots that illustrate how the average usage of categories varies with the dose of our tax treatment. These plots reveal a substantial asymmetry in the response to our treatment. For almost all categories, they

the estimated effects are largely in line with those derived without the inclusion of controls.

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 and weighted incidence shares. None of these differences are substantial.[20] The sample percentages indicate that companies made extensive use of most categories. Only the category *Tax Planning* was 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, 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. We find that for our tax increase treatment, respondents state on average that workers would bear about 17% of the profit tax incidence, with 10% being 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 managers state that only 5% of a 1% hypothetical tax burden increase would be 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, respondents assign an incidence to 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

[20] We explore the sensitivity of our findings with respect to the applied weighting scheme in Supplemental Appendix Section A.8.1. Our treatment effects are robust to applying survey weights. See the discussion in Section I.A 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	Unweighted	Weighted	$y_i = 0$	$y_i = 1$	$y_i \in (0,1)$
Wages/Salaries	Decrease	3,348	0.186	0.179	0.393	0.020	0.587
	Increase	3,401	0.097	0.092	0.637	0.009	0.354
Employment	Decrease	3,348	0.133	0.121	0.567	0.007	0.426
	Increase	3,401	0.071	0.064	0.734	0.008	0.258
Distributed Profits	Decrease	3,348	0.087	0.093	0.701	0.020	0.279
	Increase	3,401	0.235	0.219	0.463	0.056	0.480
Reserves	Decrease	3,348	0.206	0.212	0.459	0.054	0.487
	Increase	3,401	0.130	0.133	0.605	0.026	0.368
Prices	Decrease	3,348	0.024	0.027	0.892	0.002	0.105
	Increase	3,401	0.184	0.199	0.457	0.047	0.495
Investment	Decrease	3,348	0.272	0.264	0.318	0.047	0.636
	Increase	3,401	0.145	0.149	0.495	0.012	0.492
Tax Planning	Decrease	3,348	0.021	0.021	0.908	0.004	0.088
	Increase	3,401	0.070	0.067	0.707	0.009	0.284
Other	Decrease	3,348	0.056	0.067	0.881	0.032	0.087
	Increase	3,401	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 Supplemental Appendix A.5).

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 stated 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, Peichl and Siegloch, 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. Including retained earnings and reserves, total owner incidence sums to 37%. When turning to profit distributions, the survey data suggest that firm owners would 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

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.

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 would 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. For reserves, we also see some differences, with a sizable share of 13% being attributed 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. Reserve adjustments decline with the magnitude of both tax increases and tax decreases. We find that 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. Thus, it becomes apparent that for higher tax increases, respondents shift the incidence 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 stated incidence on consumer prices features by far the highest asymmetry we detect. On average, respondents would pass the additional tax burden 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.

Investment effects. Furthermore, we detect asymmetric responses for the shares attributed to the investment category. The averages suggest that investment levels would be less affected by tax increases as by tax decreases. With 27%, the investment category is 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 A9 and A10 (Supplemental Appendix A.2.3 and A.2.4) present examples of the questions as appearing in the online interface of the survey. Par-

ticipants 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 Supplemental 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 of 2004 (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 (2025) 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.

III. 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 response to the tax change treatment 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, Peichl and Siegloch, 2018). Likewise, economic sector differences may influence treatment effects due to variation in competition, profit margins, and factor input intensity (Fuest, Peichl and Siegloch, 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, 2025). 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.

A. *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 category shares for different subgroups of our data, allowing these characteristics to differentially affect the responses to tax increases and decreases.[21]

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

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

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.[22] 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.

B. 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 most of the categories, the differences by company size are negligible in size and insignificant. The point estimates suggest that the impact of a hypothetical tax cut on investment varies with company size. Respondents from medium-sized companies attribute a 10 percentage point larger share to funding new investment compared to respondents from micro-enterprises, and respondents from large companies attribute even a 13 percentage point larger share. 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 would 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, Kortum and Kramarz, 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

[22] 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.

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 among respondents from the construction sector in the tax increase group, who attribute a substantial 12 percentage point larger share of the tax burden to consumers via price increases compared to the other sectors. This higher pass-through to prices seems to offset a lower stated incidence on the owners of construction companies, who state that they would be less affected by a tax increase compared to the other industries. Interestingly, this result does not materialize for tax decreases, where construction firms do not differ significantly from firms in other industries. One possible explanation for this could be the generally high level of competitiveness in the construction sector and thereby lower profit margins. Hence, firms in this sector have less wiggle room to absorb increased costs caused by tax hikes, which only leaves the option to pass them down to consumers. Additionally, the construction industry is characterized by inelastic demand compared to other industries, which further increases the pass-through of tax increases to consumers (Hillebrandt, 2000).

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

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

indicate that this would 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, respondents from partnerships and sole proprietorships indicate that worker incidence via wage adjustments would be less prevalent for them compared to corporations, which holds true for both the tax increase and decrease groups. 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 assign substantially larger shares to distributed profits for both decreases and increases compared to corporations with a 5 percentage point difference. 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-19 pandemic has an impact on its stated distribution. Figure 5 again shows level and partial effects estimates for each category for increases and decreases separately. For the tax decrease groups, we observe that respondents state that they would use a larger share of additional funds for profit distributions if their company was not severely affected by the pandemic as indicated by its impact on the company's net income. Companies with a substantial drop in net income due to lock-down or supply chain disruptions likely experienced a severe tightening of liquidity constraints. Hence, a decrease in taxes would then be used to pay off debt or short-term liabilities. This notion is supported by the opposite-sign partial effect on the category *Other* in combination with Figure A10 in the Supplemental Appendix, which suggests that a substantial share of the free text answers alluded to repayment of debt.

For the tax increase groups, the responses suggest a pronounced difference in financing the additional costs from reduced profit distributions depending on the net income impact of the pandemic. A similar logic can be applied here as for the tax decrease treatments. 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.

Furthermore, firms that experienced a substantial net-income impact from COVID-19 state that their prices would respond less to increases in taxes compared to firms without a substantial impact. This finding may reflect differences

in demand elasticities across these subgroups, as the impact on net income arguably stems from a decrease in revenue. Consistent with distributed profits and price adjustments being less available to firms severely impacted by the pandemic, we observe that these firms would instead push the burden onto workers.

IV. 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 (IV.A). 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 (IV.B).

A. Cross-Validation with Observational Studies

We examine how our baseline results (presented in Section II.B) 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, Mei and Stantcheva (2024) and apply a *cross-validation method*. This approach evaluates how well our incidence estimates from hypothetical tax changes align with those from previous research based on observational data.

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

We begin by outlining the general methodology used to derive estimates for the stated corporate tax incidence shares based on 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.[23] Considering a given level of pre-tax profit, the initial incidence indicates how a change in the profit tax burden

[23]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

is shared across these stakeholders at the margin through changes in wages and employment, distributed and retained profits, and prices. 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*, *Employment*, *Distributed Profits*, *Reserves*, and *Prices*.[24] 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.[25] While we abstract from such a general equilibrium 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.

Using our definition of initial incidence, 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 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, Peichl and Siegloch (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, Peichl and Siegloch (2018), we define the initial incidence on workers as the ratio

within the setting considered and omit certain other groups. For example, Fuest, Peichl and Siegloch (2018) consider a small open economy setting, where output prices are fixed and consumers therefore cannot share in the burden of the tax. Jacob, Müller and Wulff (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, Peichl and Siegloch, 2018; Jacob, Müller and Wulff, 2023).

[24] One could argue that changes in retained earnings or reserves should not be attributed to the owners of the company. For cash-constrained firms, reserves serve a precautionary motive and may be used for future investment opportunities or provide insurance for employees in the event of separation. In the short run, however, reserves increase the value of the firm and thus first and foremost benefit owners (Duan and Moon, 2025).

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

of the sum of *Wages/Salaries* and *Employment* to the sum of *Wages/Salaries*, *Employment*, *Distributed Profits* and *Reserves* for tax increases, i.e., we compute the incidence as $(0.097 + 0.071)/(0.097 + 0.071 + 0.13 + 0.235) = 0.32$ based on the values in Table 3. The incidence on owners is defined analogously.

This definition is related but not identical to the concept of initial incidence used in earlier literature. For instance, Fuest, Peichl and Siegloch (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, 2025)[26], the vast majority of empirical work on business tax incidence focuses on a single adjustment margin and stakeholder group. Broader incidence effects are typically inferred indirectly, either through model-based calculations or general equilibrium modeling (Suárez Serrato and Zidar (2016); Fuest, Peichl and Siegloch (2018); Baker, Sun and Yannelis (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 the stated responsiveness of wages and employment, profits, 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 hypothetical 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, Peichl and Siegloch (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 firm in Fuest, Peichl and Siegloch (2018) has 265 employees, whereas the

[26]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 (2025) also examine both firm- and worker-level responses. However, neither study includes data on consumer prices and thus cannot assess incidence on consumers directly.

firms analyzed by Risch (2024) are significantly smaller, with an average of 20 employees. Our estimate of worker incidence (32%) falls between these two studies, aligning with our sample's firm size, which averages 68 employees, also positioned between the samples in Fuest, Peichl and Siegloch (2018) and Risch (2024).

Regarding tax incidence on consumers and firm owners, Jacob, Müller and Wulff (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 reversed distribution of the tax burden: firm owners bear 66% and consumers 34%. 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. This could explain the lower stated incidence on consumers in our sample.

Tax Decreases. Analyzing previous studies on corporate tax cuts, estimates of the incidence on workers range from 40% to 80%.[27] Using U.S. worker-level filings linked to corporate tax returns, Dobridge, Landefeld and Mortenson (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%. Duan and Moon (2025) leverage variation in corporate tax rates resulting from

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

a small business tax cut in Quebec (Canada) to examine its effect on worker earnings. Their findings suggest a tax incidence of 36% on workers.[28]

Compared to these previous estimates of tax incidence from corporate tax reductions, our findings indicate a worker tax incidence of 52%, with the remaining 48% benefiting firm owners. This aligns very closely with the results of Carbonnier et al. (2022) and Kennedy et al. (2024).

Tax Increases and Decreases - Pooled. Finally, we compare our estimates with studies that pool tax increases and tax decreases to calculate corporate tax incidence.[29] To better align with the relevant incidence margins used in the referenced studies, we pool the incidence shares from Table 3 for tax increases and decreases.

Using state-level variation in corporate taxes over time in the United States, Baker, Sun and Yannelis (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 (36%), but we find a lower incidence on consumers (15%) and a higher incidence on firm owners (49%). Unlike Baker, Sun and Yannelis (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.

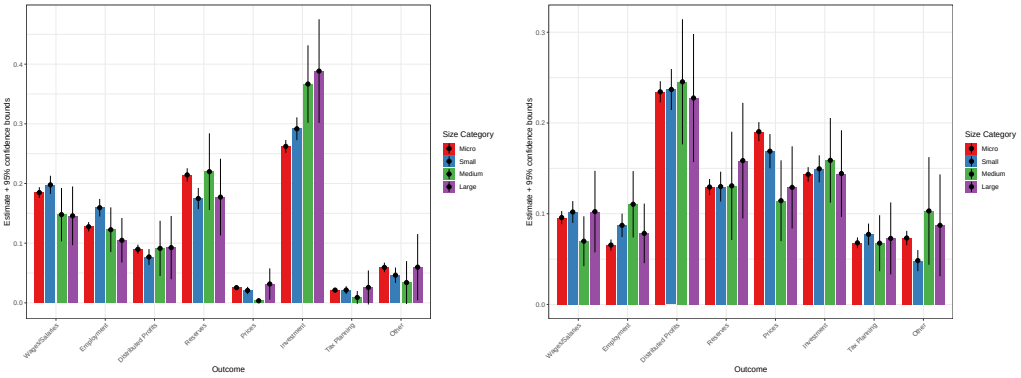
Liu and Altshuler (2013) estimate a worker tax incidence of approximately 60%, with a lower bound of 42% in their most conservative specification, using variation in effective U.S. marginal tax rates. Using the worker and owner shares from Table 3 for both tax increases and decreases, we estimate an incidence of approximately 43%, 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)

[28] Duan and Moon (2025) report a total labor incidence of 73%, of which 39 percentage points accrue to owner-workers. In Table B.1 in the Supplemental Appendix, we reproduce this headline labor incidence figure. In Table 5, we reclassify the owner-worker component as owner incidence so that worker and owner categories are mutually exclusive, yielding a worker incidence of 36%.

[29] Arulampalam, Devereux and Maffini (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.

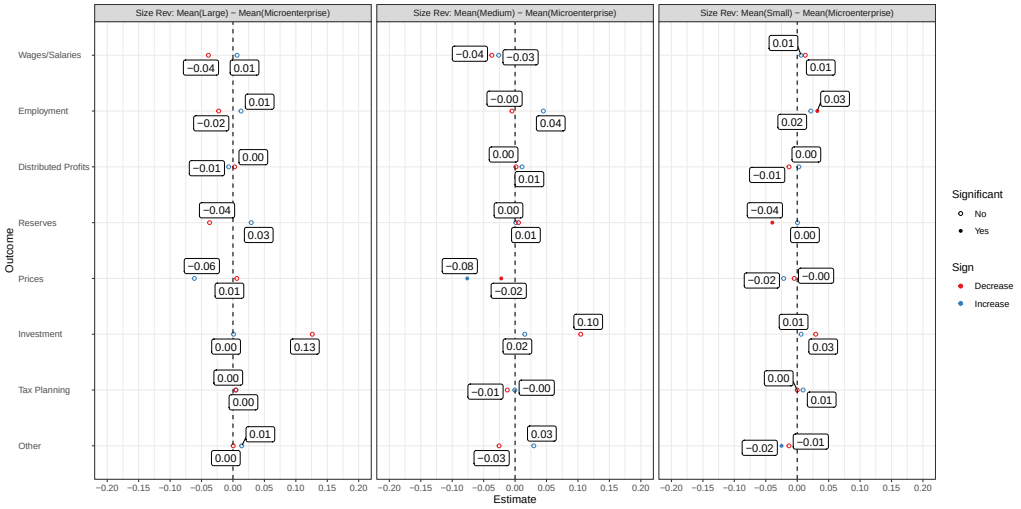
and Suárez Serrato and Zidar (2024) – estimate that the incidence of the U.S. state corporate income tax falls between 38.1% and 50% on capital, 25% to 40% on workers, and 10% to 30% on landowners. Although we are unable to measure the tax incidence on landowners, making a direct comparison with Suárez Serrato and Zidar (2016) challenging, our estimates align closely with their findings for workers and capital. Specifically, our estimated incidence shares of 43% on workers and 57% 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 be able to compare our estimates to the estimates of Suárez Serrato and Zidar (2024) and the previous versions, we need to transform their estimate of worker and owner incidence. Analogously to the case where we exclude consumers and rescale worker and owner shares to sum to 1, we need to exclude landowners from the comparative incidence measure. For example, in the original study, Suárez Serrato and Zidar (2016) derive a worker incidence of 30-35% and an owner incidence of 40%, with the rest benefiting land owners. To compare these numbers to our categories, we focus on the share of the tax burden that is borne by workers and owners only, and calculate for example the resulting worker incidence share as $30/(30+40) = 43$.

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, Mei and Stantcheva (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 IV.B.



(a) Tax Decrease

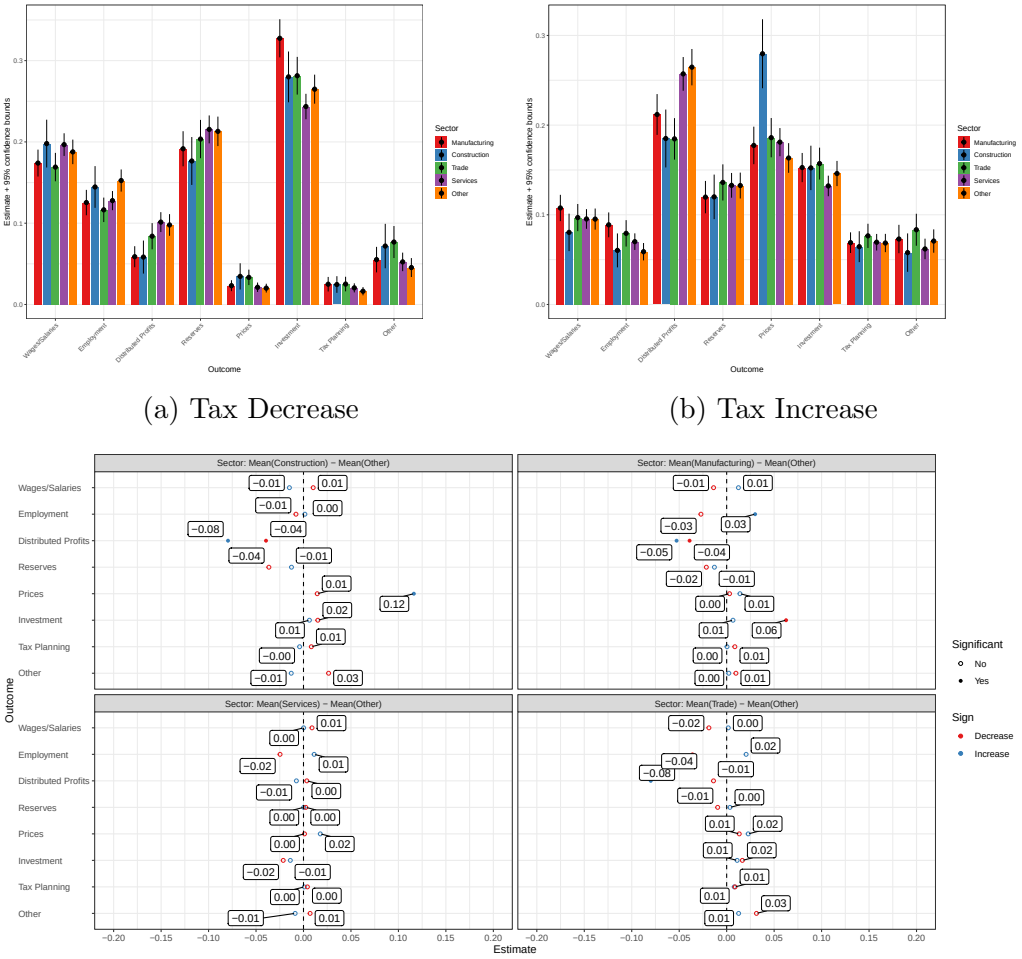
(b) Tax Increase



(c) Average Partial Effects

FIGURE 2. HETEROGENEITY IN MANAGERIAL ADJUSTMENT MARGINS BY FIRM SIZE

Note: Figure 2 shows heterogeneity in managerial adjustment margins 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.



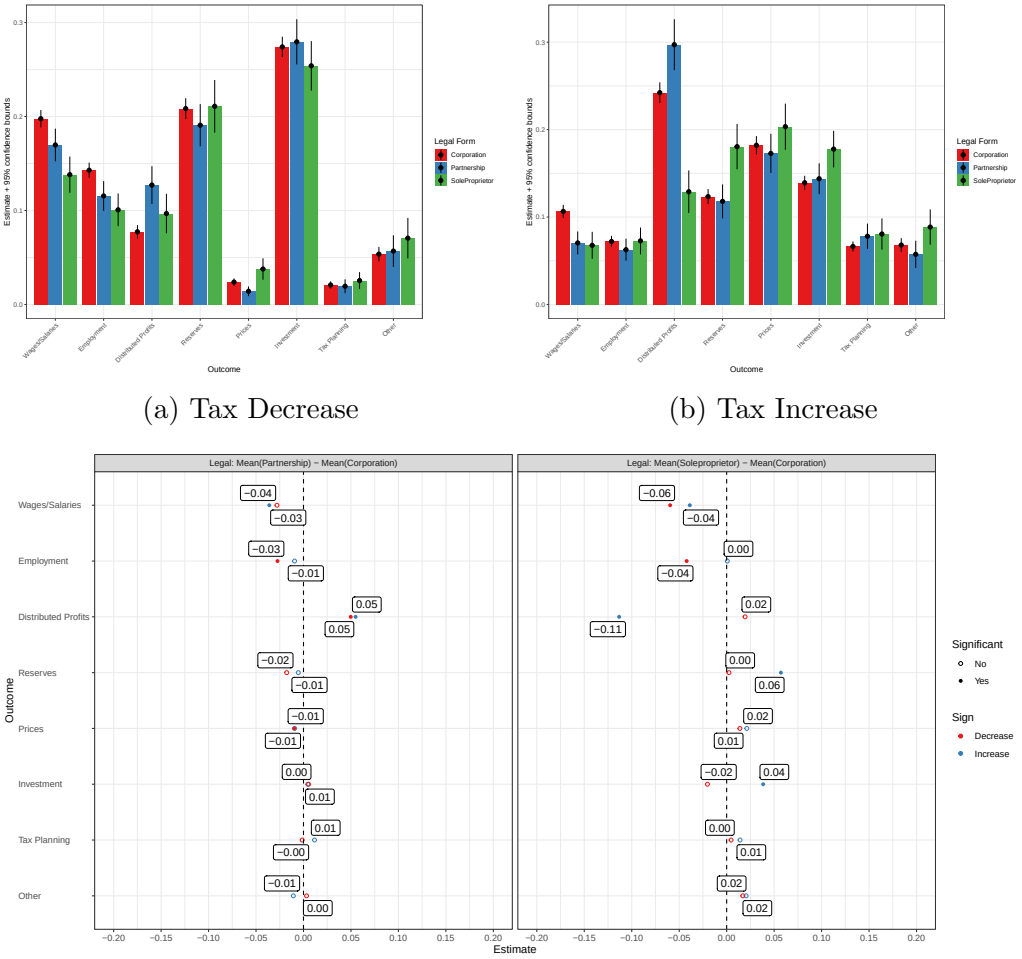
(a) Tax Decrease

(b) Tax Increase

(c) Average Partial Effects

FIGURE 3. HETEROGENEITY IN MANAGERIAL ADJUSTMENT MARGINS BY ECONOMIC SECTOR

Note: Figure 3 shows heterogeneity in managerial adjustment margins 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.



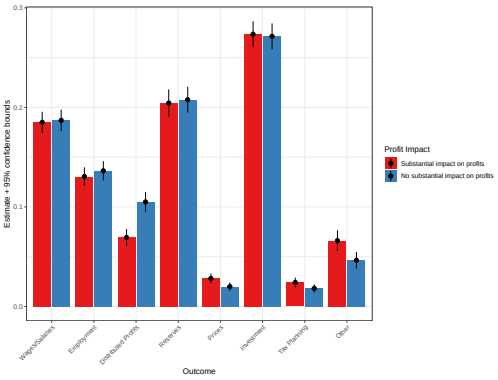
(a) Tax Decrease

(b) Tax Increase

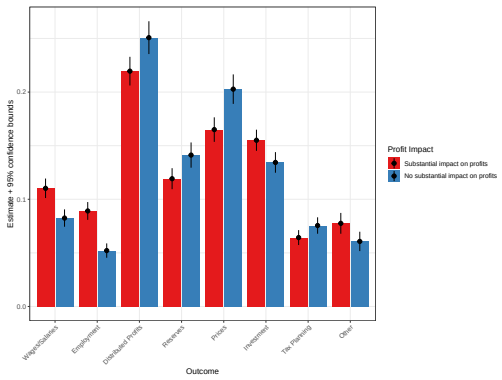
(c) Average Partial Effects

FIGURE 4. HETEROGENEITY IN MANAGERIAL ADJUSTMENT MARGINS BY ORGANIZATIONAL FORM

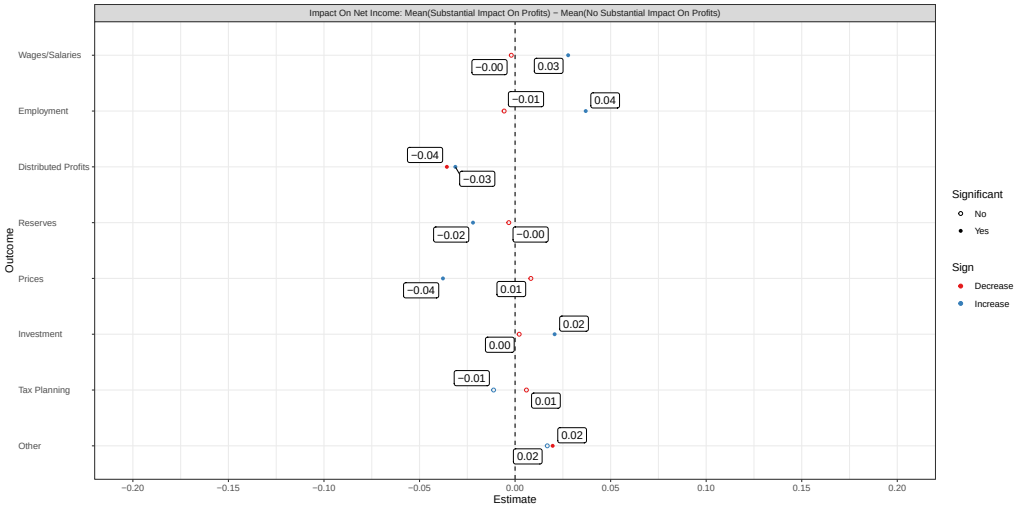
Note: Figure 4 shows heterogeneity in managerial adjustment margins by company legal form. Panel 4a and 4b illustrate different levels and associated robust standard errors of category usage by treatment sign calculated from the estimated coefficients from Equation (2), whereas Panel 4c shows average 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.



(a) Tax Decrease



(b) Tax Increase



(c) Average Partial Effects

FIGURE 5. HETEROGENEITY IN MANAGERIAL ADJUSTMENT MARGINS BY COVID-19 PROFIT IMPACT

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

Table 5—Cross-Validation - Selected Studies

Paper	Tax Variation	Tax Change	Country	Episode	Incidence on		
					Workers	Owners	Consumers
Baker, Sun and Yannelis (2023)	Variation in state corporate tax rates	Increases and Decreases (pooled)	USA	2006-2017	Range: 28%-36% Own: 36%	Range: 20%-21% Own: 49%	Range: 43%-51% Own: 15%
Carbonnier et al. (2022)	Large French corporate income tax credit	Decrease	France	2009-2015	50%, Range: 40%-60% Own: 52%	50% Own: 48%	-
Dobridge, Landefeld and Mortenson (2021)	Variation in the Domestic Production Activities Deduction	Decrease	USA	1999-2015	80% Own: 52%	20% Own: 48%	-
Duan and Moon (2025)	Corporate tax cuts	Decrease	Canada	2001-2017	36% Own: 52%	64% Own: 48%	-
Fuest, Peichl and Siegloch (2018)	Variation in local business tax changes	Increase (93% increases)	Germany	1993-2012	51% Own: 32%	49% Own: 68%	-
Jacob, Müller and Wulff (2023)	Variation in local business tax rate	Increase (98% increases)	Germany	2014–2017	-	36%, Range: 28%-39% Own: 66%	64%, Range: 61%-72% Own: 34%
Kennedy et al. (2024)	US corporate tax change (TCJA)	Decrease	USA	2013-2019	48% Own: 52%	51% Own: 48%	-
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% Own: 43%	43%, Upper bound: 58% Own: 57%	-
Risch (2024)	Variation in top marginal personal tax rate in the United States	Increase	USA	2008-2016	11-18% Own: 32%	80% Own: 68%	-
Suárez Serrato and Zidar (2016)	Variation in US state taxes and apportionment rules	Increase and Decrease (pooled)	USA	1980–2012	43-47%* Own: 43%	53-57%* Own: 57%	-
Suárez Serrato and Zidar (2023)	Variation in US state taxes and apportionment rules	Increase and Decrease (pooled)	USA	1980–2012	48%* Own: 43%	52%* Own: 57%	-
Suárez Serrato and Zidar (2024)	Variation in US state taxes and apportionment rules	Increase and Decrease (pooled)	USA	1980–2012	33-44%* Own: 43%	56-67%* Own: 57%	-

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. We refer to the section on *Incidence Calculation* in the text for details. 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 Supplemental 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. *To be able to compare our estimates to the estimates of Suárez Serrato and Zidar (2024) and the previous versions, we need to transform their estimate of worker and owner incidence. Analogously to the case where we exclude consumers and rescale worker and owner shares to sum to 1, we need to exclude landowners from the comparative incidence measure. For example, in the original study, Suárez Serrato and Zidar (2016) derive a worker incidence of 30-35% and an owner incidence of 40%, with the rest benefiting land owners. To compare these numbers to our categories, we focus on the share of the tax burden that is borne by workers and owners only, and calculate for example the resulting worker incidence share as $30/(30+40) = 43$.

B. Reliability of Survey Responses

Even though our results compare well to the findings of prior literature, as shown in the previous Section IV.A, 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, Haushofer and Roth, 2018; De Quidt, Vesterlund and Wilson, 2019; Haaland, Roth and Wohlfart, 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 IV.B and the accuracy of manager surveys in general in Section IV.B, we conduct several empirical validation exercises to mitigate remaining concerns and bolster the confidence in our results. First, in Section IV.B, 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 IV.B).

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, Roth and Wohlfart, 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, Haushofer and Roth, 2018; Haaland, Roth and Wohlfart, 2023; Stantcheva, 2023). Second, the neutral framing of our survey regarding taxation further reduces the likelihood of experimenter demand effects (Haaland, Roth and Wohlfart, 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, Vesterlund and Wilson, 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), which is inconsistent with either social desirability as well as strategic motives to influence the outcome of the survey. 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, Roth and Wohlfart, 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 IV.A). 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 IV.B). 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 17). 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 IV.B, 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 IV.B) and that their stated intentions are meaningful predictors of actual firm behavior (Section IV.B).

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, 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, Gorodnichenko and Kumar, 2018). Similarly, Coibion, Gorodnichenko and Ropele (2020) find that reported employment levels in surveys closely correspond to employment figures in administrative data. Additionally, according to Kumar, Gorodnichenko and Coibion (2023), firms' responses to hypothetical survey treatments closely match results from randomized control trials using non-hypothetical information, such as GDP forecasts from professional forecasters. Moreover, 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.

Although these examples are not in the context of large tax changes, they reinforce our confidence that firm managers' stated plans serve as reliable predictors of their actual behavior. In the following sections, we show that this result also holds in our survey.

Correspondence of Firm Characteristics. — We begin by establishing the degree of correspondence of firm characteristics as stated by the respondents 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. (2025) for the first wave of the GBP. We form four categories for revenues and the number of employees respectively and calculate the proportion of observations that are in the same size category between the survey and the Orbis data. Limited by the availability of revenue and employee count in Orbis, we can do this comparison for 606 observations for the revenue test and for 1,516

observations for the employee test. We additionally compute Cohen's Kappa and provide its 95% confidence interval.

The results for revenues are depicted in Table 6. We find a share of corresponding revenue categories amounting to 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. (2025) 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., 2025). 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.

Table 6—Correspondence Revenue.

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

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

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].

hypothetical tax change on firm-level employment.[30] The second exercise uses two questions of the same survey wave in order to test for the predictive power of stated employment decisions in response to the COVID-19 pandemic.

Responses to changes in LBT. Firms operating in Germany are in principle subject to three types of taxes on their income depending on the legal form: 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, Peichl and Siegloch, 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 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

[30] For this exercise, we use data on municipal scaling factors provided by the Federal and State Statistical Offices (SÄBL, 2022), municipal and zip codes provided by the German Federal Statistical Office (Destatis, 2022), and geo-locate the firm addresses in Orbis using (OSM, 2025) to assign the correct municipal scaling factor to each firm-year. Municipalities are assigned to firm locations using their geolocation provided by the Federal Agency for Cartography and Geodesy (BKG, 2022).

[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.

Table 8—Sample Selection LBT Validation.

Restriction	Firms	Observations
Firms with Linking Agreement	2,435	17,576
More than one financial year	2,077	14,187
Any tax change	732	1,202
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.

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 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

[32]The results are robust to alternative thresholds for defining a substantial employment response. When lowering the cutoff from 10% to at least 5%, the association remains significant for tax increases for the five-year window specification but becomes statistically insignificant for tax decreases.

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.

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

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.

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 is 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 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

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: Less Employment	-0.056	-0.061	0.205
	(0.023)	(0.029)	(0.032)
Survey: More 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.

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

V. 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 hypothetical business tax changes on a large set of possible adjustment margins and affected groups in a unified setting. Moreover, our experimental approach enables us to test for asymmetries in reported tax incidence along the sign of the tax treatment, as well as the

influence of the magnitude.

Our findings indicate that managers would respond asymmetrically to business tax changes of opposite signs. The survey experiment suggests that consumers would bear a substantial portion of tax hikes, as respondent firms report that they would pass on a significant share of higher costs through price increases, yet consumers would benefit only marginally from tax reductions. Similarly, capital owners state that they would bear a greater burden from tax increases (through reduced distributed profits) than they would gain from tax cuts. Conversely, employees would experience an asymmetric effect in the opposite direction: while tax hikes would have a limited impact on wages and employment, tax reductions could result in more substantial wage and employment gains. Our analysis of treatment intensity further reveals that larger tax change treatments have a stronger stated impact on employment adjustment 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 would exert different responses 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 capital 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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