Inside the Firm:

Evidence from Survey Experiments on the Incidence of Business Taxes, Price Setting, and Tax Preferences

Inaugural Dissertation to Obtain the Academic Degree of a Doctor in Business Administration at the University of Mannheim

submitted by

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Baden-Baden, July 2025 *Fabian Eble*

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List of Abbreviations

AJCA American Jobs Creation Act.

ATE Average Treatment Effect.

BvD Bureau van Dijk.

CATE Conditional Average Treatment Effect.

DPAD Domestic Production Activities Deduction.

ECB European Central Bank.

EU European Union.

GATE Group Average Treatment Effect.

GBP German Business Panel.

GDP Gross Domestic Product.

MFL Multivariate Fractional Logit.

OLS Ordinary Least Squares.

TCJA Tax Cuts and Jobs Act.

US United States.

VAT Value Added Tax.

1 Introduction

Despite the breadth of research on tax-related managerial decisions, empirical studies have largely relied on archival data from public financial and non-financial filings (Bloomfield et al., 2016; Bischof et al., 2024). While these databases offer valuable ex-post information on managers' decisions, they shed little light on the internal processes, organizational structures, and forward-looking considerations that drive firms' decision-making. Crucially, they cannot capture the expectations, perceptions, and objectives that shape managers' choices. In contrast, survey data open up a window into the otherwise unobservable: the beliefs, motivations, reasoning, and expectations that underlie managerial behavior (Graham et al., 2005; Bloomfield et al., 2016; Stantcheva, 2023). By shedding light on dimensions that are otherwise unobservable, surveys enable researchers to investigate questions that remain out of reach for traditional revealed-preference methods (Stantcheva, 2023).[1]

A major advantage of survey research is its capacity to embed randomized experiments, enabling researchers to introduce controlled variation into the data-generating process (Haaland et al., 2023; Stantcheva, 2023). Rather than passively observing behavior, survey experiments actively shape the environment by manipulating specific features, such as the information available to respondents. This creates exogenous variation, providing a clean test of theoretical mechanisms and causal relationships (Haaland et al., 2023; Stantcheva, 2023). By selectively altering information, researchers can isolate its effect on choices or attitudes without confounding influences, making this method especially valuable for evaluating economic models and informing policy. Moreover, survey experiments allow the study of multiple behavioral margins within a unified framework. Systematically varying elements

[1] Like any empirical method, survey research has limitations and potential sources of bias. A common concern is social desirability bias, where respondents adjust answers to align with perceived norms (Stantcheva, 2023; Bursztyn et al., 2025). Closely related are experimenter demand effects, which arise when participants infer the researcher's expectations and respond accordingly (De Quidt et al., 2018, 2019; Haaland et al., 2023; Stantcheva, 2023). Surveys also rely on self-reported beliefs, which may not reflect actual behavior (Dechezleprêtre et al., 2025). Question misinterpretation and self-selection can further undermine validity by introducing measurement error and sample bias (Stantcheva, 2023). Lastly, question framing and wording can influence response behavior (Bloomfield et al., 2016; Stantcheva, 2023).

– such as the size or direction of a policy change – enables direct comparisons often not possible with observational data.

One major challenge in conducting survey-based experiments with firms is the limited availability of firm-level survey data across countries (Coibion et al., 2020a). This dissertation addresses that gap by using data from the German Business Panel (GBP), a long-term survey project funded by the German Research Foundation (Bischof et al., 2024). The GBP collects high-frequency data from a broad and diverse sample of firms in Germany, targeting top decision-makers such as CEOs and owners. Its design enables the measurement of both the inputs to managerial decision-making – such as expectations, perceptions, and organizational structures – and the outcomes of those decisions, including, for example, investment, pricing, and employment. Crucially, the GBP also supports experimental variation, making it possible to identify causal effects and explore behavioral responses to policy changes. By covering firms of all sizes and legal forms, including privately held businesses, the GBP provides unique insights that complement traditional archival datasets.[2]

Using data from three survey experiments conducted as part of the GBP, this dissertation examines how firms respond to policy changes and information provision with regard to decisions such as investments, wages, and prices, as well as their preferences regarding taxation. Chapter 2 presents evidence on how managerial choices influence the distribution of profit tax burdens, highlighting how incidence varies with the direction and size of tax changes. Chapter 3 investigates whether central bank inflation forecasts influence firms' pricing plans in a high-inflation environment, showing that such information can moderate price-setting behavior. Chapter 4 examines how economic policy narratives shape firms' tax preferences, demonstrating that appeals to fiscal responsibility are more effective than fairness-based arguments in increasing support for taxes. By using survey experiments, this dissertation provides novel evidence on how firms respond to economic policies and information, shedding

[2] In particular, private firms – though the most common type of business in most economies – remain understudied due to limited data availability (Poschke, 2018; Bischof et al., 2024). The GBP helps fill this gap by offering insights into managerial decision-making across firms of varying sizes and legal forms, complementing traditional archival sources.

light on the behavioral mechanisms that shape corporate decision-making.

Chapter 2[3] investigates how decision-makers in German firms respond to changes in profit taxation, a key determinant of the economic incidence of taxes. Understanding how firms adjust to tax changes is essential for assessing their welfare and distributional consequences. While a substantial empirical literature examines tax incidence, most studies are limited to one specific margin – such as the effect of corporate taxes on wages (Fuest et al., 2018) or prices (Jacob et al., 2023). Observational data rarely allow for a unified analysis across multiple behavioral margins, and they are particularly ill-suited for comparing firm responses to tax changes that differ in sign or size.

To overcome these limitations, we conduct a randomized survey experiment in which firm owners are asked how they would react to hypothetical changes in their profit tax burden. The tax scenarios vary in direction (increase or decrease) and magnitude (1%, 10%, or 25%), and respondents report expected adjustments across a range of margins, including wages, employment, investment, dividends, reserves, consumer prices, and tax planning. The results reveal striking asymmetries: tax cuts tend to benefit workers more than tax hikes hurt them, while tax increases disproportionately burden consumers through higher prices – an effect that is less pronounced for tax reductions. These findings highlight that the distributional effects of profit tax changes are highly sensitive to both their direction and size.

Chapter 3[4] examines whether central bank inflation forecasts influence firms' pricing plans in a high-inflation environment. While communication has become an increasingly important monetary policy tool, evidence on its effectiveness in shaping firm behavior is limited, particularly in high-inflation environments and developed countries (Coibion et al., 2020a). Using a randomized survey experiment with German firms during the 2022 infla-

[3] This is joint work with Richard Winter, Philipp Doerrenberg, Davud Rostam-Afschar, and Johannes Voget. It is currently under revision (R&R) at the *American Economic Journal: Economic Policy* and is also available as a *TRR 266 Accounting for Transparency Working Paper* (Winter et al., 2025).

[4] This is joint work with Philipp Doerrenberg, Christopher Karlsson, Davud Rostam-Afschar, Benjamin Tödtmann, and Johannes Voget. It is currently under revision (R&R) at the *Journal of the European Economic Association*. An earlier version of Chapter 3, titled *Followers or ignorants? Inflation expectations and price setting behavior of firms*, is also available as a TRR 266 Accounting for Transparency Working Paper (Doerrenberg et al., 2023).

tion surge, we test how different types of inflation-related information affect planned price changes. Firms are assigned to different conditions: the control group is reminded only of their own inflation expectations, while treatment groups are shown their own expectations alongside central bank projections, either general inflation forecasts or those supplemented with expected developments in energy or labor costs.

We find that central bank forecasts significantly reduce planned price increases, with the strongest effects among firms with high initial inflation expectations or limited prior attention to inflation, pointing to belief updating as the key mechanism. While forecasts of specific input costs do not further reduce prices beyond general forecasts, they do influence how firms weigh prior expectations when planning prices. Finally, firms' inflation expectations translate nearly one-to-one into pricing plans in the control group, underscoring the importance of inflation expectations for price setting in high-inflation environments. Taken together, our findings indicate that information policies targeting firms can serve as a complement to traditional monetary tools, helping to manage firms' pricing behavior and support broader inflation control.

Chapter 4[5] investigates how economic policy narratives influence firm decision-makers' preferences for taxation. While business leaders have increasingly voiced opinions on public policy (Mkrtchyan et al., 2024) – often in ways that appear to go against their financial interests regarding taxes – little is known about what shapes these preferences. Using a randomized survey experiment, we test whether policy narratives influence firms' support for higher taxes. We frame a large fiscal stimulus package using two contrasting narratives: one stresses fiscal responsibility, presenting taxes as necessary to restore budgetary balance after crisis-related spending; the other emphasizes redistribution, portraying taxes as a response to firm-level hardship caused by uncontrollable shocks like the pandemic. Respondents are then asked how much they would increase different types of taxes to fund such programs.

[5] This is joint work with Laura Arnemann, Philipp Doerrenberg, Davud Rostam-Afschar, Johannes Voget, Florian Buhlmann, and Christopher Karlsson. An earlier version of Chapter 4, titled *Narratives about fiscal policy: Are firm decision-makers' tax preferences driven by redistribution or fiscal consolidation motives?*, is also available as a *TRR 266 Accounting for Transparency Working Paper* (Arnemann et al., 2025).

We find that narratives emphasizing fiscal consolidation significantly increase firms' willingness to support higher taxes. In contrast, appeals to misfortune have little impact, except in the case of capital gains taxes, where support increases – likely due to redistribution motives or the lower perceived economic cost of this tax. Additional analysis reveals that firms are more willing to raise taxes they do not personally pay. Overall, the chapter shows that targeted communication strategies can meaningfully influence firms' tax preferences, providing new evidence on the drivers of corporate support for taxation – insights that are especially valuable for governments facing fiscal stress and the need to raise additional revenue.

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

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

Co-authors: Richard Winter, Philipp Doerrenberg, Davud Rostam-Afschar, Johannes Voget

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

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

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2.1 Introduction

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

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

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

different types of tax reforms further limit the informative value of existing observational studies. For example, due to data availability, the literature offers only limited evidence on the effects on firm returns and consumer prices. Moreover, due to limited availability of different types of tax reforms, it does not address whether business tax increases and decreases have symmetric effects – a key question, given that downward wage and price rigidity or partial irreversibility of decisions may lead to asymmetric responses.

For a complete understanding of the effects of profit taxes and their incidence implications, it is important to consider in a comparable setting all dimensions along which taxes can exert effects and to examine if different types of tax reforms have different incidence effects. This is where our paper comes in: we use data from a novel large-scale survey of German firms to provide evidence on the tax responses of firms along many different margins and for different types of tax reforms. Our aim is to improve the understanding of the full picture of profit tax incidence within one unified setting. A survey approach is well suited for this purpose, as it allows measuring a comprehensive set of adjustment margins within a unified framework while randomly varying the size and the sign of the tax change. While we acknowledge that surveys have some limitations in comparison with well-identified observational studies (see further below in the Introduction and Section 2.5.2), our survey approach complements the existing literature by allowing us to study important aspects of business tax incidence that are difficult to consider using non-survey approaches.[6]

Our starting point is the effect taxes have on the managerial decision margins. We focus on the short-run direct effects of the managers' adjustment decisions, abstracting from general equilibrium effects.[7] We take a straightforward approach and ask firms how profit

[6] 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 & Souleles, 2019). Colarieti et al. (2024) study survey responses to hypothetical income shocks and show that their survey findings closely match realized behaviors observed in prior research. We extensively discuss potential caveats of our specific survey design in Section 2.5.2.

[7] Conceptually, there are several ways how incidence can be measured (Fullerton & 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

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

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

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 & Metcalf, 2002; Suárez Serrato & Zidar, 2016; Fuest et al., 2018). Generally, Harberger (1962), which constitutes the seminal paper in the incidence field, developed a simple two-sector closed economy model and finds that under plausible parameter values capital owners bear the entire incidence of the tax. However, this central result no longer holds once an open economy setting is considered, where capital mobility becomes a relevant factor. Gravelle (2013) provides an overview of several recent theoretical models and shows how their insights hinge on the underlying assumptions being made. The results critically depend on factor mobility, factor substitution, capital intensity, international product substitution elasticities, and country size.

that the survey-based approach allows us to distinguish the effects of differently signed tax changes as well as differences in treatment intensity.

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

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

Finally, by exploiting the presence of a rich set of company characteristics in our survey data, we investigate heterogeneity in profit tax incidence. Our results suggest that the positive investment effects of tax cuts increase in company size, plausibly reflecting differences in investment opportunities and general growth prospects. We further document sector-specific differences. Incidence on consumers via price increases is substantially higher in the construction sector, which could be explained by relatively low profit margins and inelastic

demand. These features have been shown to shift the incidence from firm owners to consumers (Fullerton & Metcalf, 2002). For tax decreases, we find that manufacturing firms are most likely to utilize additional funds for new investment projects relative to other industries, which we attribute to the generally higher degree of capital intensity in that sector. Our results further suggest that the legal form of the company has a substantial impact on the incidence of its owners. We find that a higher share of incidence attains to partnerships compared to corporations and sole proprietors. This finding may reflect differing levels of profitability across legal forms.

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

First, we empirically investigate the predictive power of the respondents' hypothetical answers by comparing their reported actions to realized actions in two distinct settings. In the first test, we merge our survey responses to Orbis financial data and information on changes in statutory local business tax rates. We then correlate the survey-reported impact of a tax change on employment with actual employment changes after a change in the local business tax rate. In the second test, we exploit two survey questions about planned employment adjustments in the year after the survey by correlating them with employment

changes observed in Orbis over the same time horizon. Both tests indicate the predictive power of the stated actions for actual behavior.

Second, following the approach in Colarieti et al. (2024), we cross-validate our estimates of initial incidence on workers, firm owners, and consumers by comparing them to prior literature relying on observational data sources.[8] 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.[9] Thanks to our experimental design – which distinguishes between tax increases and decreases and captures various adjustment margins within a single framework – our results provide a unified perspective that helps interpret the heterogeneity in existing research.

We identify three main contributions of our paper. First, we consider multiple possible different adjustment margins and study the distributional effect on the most relevant stakeholder groups – i.e., workers, firm owners, consumers – in one unified setting. Specifically, in contrast to existing studies in the literature, we simultaneously observe the outcomes that are most relevant to the three stakeholder groups: wages and employment, firm profits and distributions to owners, and consumption prices. In addition, we observe further outcomes that affect the three stakeholder groups indirectly (such as investments or tax planning). Kennedy et al. (2024) study the effects of Tax Cuts and Jobs Act (TCJA)-induced tax cuts

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

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

on several firm outcomes – sales, profits, investment, shareholder distributions – as well as worker-level outcomes such as employment and annual earnings. However, they do not observe consumption prices. Duan and Moon (2024) also study firm- and worker-level outcomes simultaneously, but likewise do not observe consumption prices. The few other studies that examine multiple adjustment margins differ from ours in that they either focus on different margins relevant to only one single stakeholder group – like workers (Giroud & Rauh, 2019; Risch, 2024) – or use general equilibrium models and structural estimations to assess the impact of taxes on various groups (Suárez Serrato & Zidar, 2016).[10]

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 react more strongly to business tax increases than to tax decreases complements existing evidence of asymmetric price responses in the context of value added taxes (VATs) (Benzarti et al., 2020), sales taxes (Yilmazkuday, 2017), and excise taxes (Bergman & 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.

[10] Of course, our paper relates to many studies on business tax incidence that consider a single adjustment margin, for example in the context of worker incidence (Arulampalam et al., 2012; Fuest et al., 2018; Dwenger et al., 2019) or consumer incidence (for which we generally have limited evidence due to the availability of consumer price data; see recent exceptions such as Dedola et al. (2022), Baker et al. (2023), and Jacob et al. (2023)). We also relate to a large set of papers that examine the effect of business taxes on single indirect margins, including papers on investment effects (Zwick & Mahon, 2017; Ohrn, 2018; Chen et al., 2023; Jacob & Zerwer, 2024), tax avoidance (Dyreng et al., 2022) or CEO compensation (De Simone et al., 2022; Bornemann et al., 2023; Ohrn, 2023).

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

2.2 Survey Design and Data

2.2.1 Survey and Sampling

Our tax incidence questions were fielded in the second wave of the GBP. The GBP constitutes a large-scale survey of executives and high-level decision makers of companies operating in Germany, which periodically assesses their views and expectations regarding topics in accounting and tax policy. A detailed overview of the survey methodology and content is provided by Bischof et al. (2024). Firms participating in the GBP closely align with the target population in terms of industry affiliation. However, there is a slight under-representation of small firms and sole proprietors, and a corresponding over-representation of larger firms when contrasted to the universe of German firms.[11]

To address this issue, we construct survey weights to make our sample representative of the broader German firm population. Table 2.3 in Section 2.3.2 and Figure A.14 present both unweighted and weighted results, illustrating that they are largely comparable. Given the similarity between weighted and unweighted results, we rely on unweighted results in our main analysis for two reasons. First, when the goal is to estimate causal relationships rather than produce population-level descriptives, unweighted regressions often yield more efficient and interpretable estimates (Solon et al., 2015). This approach is appropriate in our setting because the sampling design appears ignorable: estimated coefficients remain stable when including controls, and we find no significant differences in key observables

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

between respondent and non-respondent firms. These findings suggest that any selection into the sample is unlikely to bias our estimates, supporting the validity of unweighted regressions. Using unweighted results therefore provides a consistent analytical framework, enhances transparency, and simplifies exposition. Second, our heterogeneity analysis – where we examine differential effects across various firm characteristics – would require constructing separate weights for each characteristic to ensure representativeness along those specific dimensions. This approach would introduce substantial complexity and potentially reduce comparability across subsamples.[12]

The contact information of firms was obtained from the Bureau van Dijk (BvD) Orbis database. The subsample of firms that participated in the survey was drawn randomly from the overall address pool and invited to participate in the online survey via email. The GBP sent invitation e-mails for the online survey on 45 work days between November 16, 2020 and January 22, 2021.[13] Firms were randomly assigned to one of the 45 days. After 7, 14, and 28 days, reminder e-mails were sent. Responses were collected from November 16, 2020 through June 24, 2021. The response rate to the survey was 2.5%, and about 57% of respondents completed the survey with a completion rate of 90% or higher (N = 8,955). The final sample used in the main analysis consists of 6,749 responses, after excluding observations with missing values for control and weighting variables.[14]

2.2.2 Tax Incidence Survey Questions

The survey experiment started with the following question:

[12] 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.

[13] 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).

[14] Figure A.1 in the Appendix illustrates the distribution of the completion share in our data. Additionally, 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.

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

Figures A.2 and A.6 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%.[15] 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.[16] These differences in applicable tax rates also motivated us to choose the term profit tax for our question over something more specific such as the corporate income tax, as respondent firms might be subject to different taxes. The term profit tax is inclusive of the German local business tax, the personal income tax, as well as the corporate income tax.[17]

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

[15] 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****?"*

[16] 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.

[17] 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.

their own firm as well as other firms in their jurisdiction.[18] 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.[19]

Table 2.1 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

[18] Of course, larger firms are more likely to operate internationally (Eaton et al., 2011) and compete with foreign companies not subject to the same tax policies. Consequently, they may perceive the tax change scenario as affecting only themselves and their domestic competitors, while their broader set of global rivals remains unaffected. The respondents' ability to account for their firm-specific context in their responses increases their reliability. See Section 2.4 for an analysis of response heterogeneity with respect to firm size and other firm characteristics.

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

margins: wages and salaries, employment, distributed profits, retained earnings or reserves, consumer prices, investments, use of tax saving opportunities, and other categories (in the form of an open field question).[20]

In the following, we motivate the choice of our set of adjustment margins and how they affect the three stakeholder groups we consider. We distinguish between adjustment margins that have a direct effect on stakeholder groups and those with more indirect implications. For the direct impact on wages and employment, profit distributions and prices, the affected stakeholders are straightforward (workers, owners and consumers, respectively). However, managers may also choose adjustment margins that influence future pre-tax profits, thereby indirectly affecting stakeholder incidence. Numerous studies have documented the influence of tax changes on investment decisions (Hanlon et al., 2015; Zwick & Mahon, 2017; Ohrn, 2018; Giroud & Rauh, 2019; Chen et al., 2023). Hence, an increase in profit taxes might prompt managers to curtail capital investments. This reduction could lead to diminished labor productivity and lower *future* wages (Arulampalam et al., 2012).

According to classical tax incidence literature (Harberger, 1962; Fuest et al., 2018), higher taxes can also lead to increased product prices for customers due to lower output 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 & Rauh, 2019), driven by marginally profitable firms exiting the market or downsizing at the firm level due to rising marginal costs of capital and labor (Jacob et al., 2023). Thus, tax-induced investment changes can significantly impact the incidence on firm owners, employees, and customers in the long run.

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

[20] 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 enough resources in the company to finance the additional burden.

on factors like the labor supply elasticity, tax deductibility options, or the degree to which higher tax incidence affects shareholders versus employees (Fuest et al., 2018; Dyreng et al., 2022). Additionally, higher taxes could lead to an increase in debt financing or a decrease in retained earnings (Djankov et al., 2010). Such shifts may complicate financing of investment or exacerbate principal-agent problems, particularly when a larger proportion of investment is externally financed (Ohrn, 2018).[21] Changes in financing structures, coupled with negative investment effects, can therefore result in greater tax incidence on either workers or firm owners, contingent upon factors like the labor supply elasticity and capital mobility.

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

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

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

Table 2.1: 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 2.1 shows the different incidence categories available to respondents for the tax increase and decrease treatment arms, respectively. Based on participants being randomly assigned to either the tax increase or tax decrease group, they are presented with the following question: *Assume that your company has a (1%/10%/25%) permanently higher/lower profit tax burden as a result of a tax increase/cut. How do you finance the additional burden/distribute the additional funds?* Conditional on the firm's legal form, respondents either saw "distributions to partners" or "distributions to shareholders". (*): Note that the category *Decrease in Debt Capital* was not available in the tax decrease treatment. We therefore integrated the *Increase in Debt Capital* category into the *Others* category for the tax increase treatment to facilitate comparisons between tax increases and decreases.

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 the context of consumption taxes, similar dynamics may apply to profit taxation. Investment behavior may also reflect asymmetries: firms could act more aggressively in response to tax cuts than hikes, as lower taxes increase returns on capital. Differences in pass-through to consumers and workers can shape these effects (Jacob, 2022). Additionally, firms may be more motivated to engage in tax planning to mitigate tax increases than to restructure operations in response to tax relief. In sum, institutional frictions, pricing behavior, and strategic considerations can lead to directionally different responses to tax increases and decreases.

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

The order, in which the answer options were presented to the participants, was not 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 A.2 in combination with Figure A.6. This implementation issue does not affect the within-sign experimental design, i.e., the different tax increase treatments are consistent with each other. For the comparison of effects between tax increases and decreases, on the other hand, we cannot rule out that the differential ordering has an effect. However, it is unlikely that this inconsistency drives our results, for the same reasons mentioned above.

2.2.3 Summary Statistics and Covariate Balance

The survey collects data on fundamental company characteristics such as legal form, industry affiliation, as well as revenue and number of employees in the previous year.[22] Table 2.2

[22] As the set of survey respondents is based on available contact information in BvD'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 A.2 in the 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

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.[23]

In order to investigate how well the randomization procedure worked, we conducted multiple balance tests utilizing the available characteristics of the survey respondents in our data. Figure A.12 (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.

2.3 Full Distribution of Tax Changes

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

main analysis.

[23] 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 2.2: Descriptive Statistics

	N	Mean	SD	P10	Median	P90
Revenue	5,259	19,831,465	486,011,410	90,000	720,000	6,500,000
Num. Emp.	6,749	68	2,221	1	5	37
Corporation	6,749	0.73	0.44	0	1	1
Sole Prop.	6,749	0.13	0.33	0	0	1
Partnership	6,749	0.14	0.35	0	0	1
Manufacturing	6,749	0.17	0.38	0	0	1
Construction	6,749	0.07	0.26	0	0	0
Trade	6,749	0.16	0.36	0	0	1
Services	6,749	0.33	0.47	0	0	1
Other Sector	6,749	0.27	0.45	0	0	1
CEO	6,749	0.70	0.46	0	1	1

Note: Table 2.2 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.

2.3.1 Empirical Strategy

For each of our incidence categories, we estimate the following equation using Ordinary Least Squares (OLS):[24]

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

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

[24] In addition to estimating equation (1) using OLS, Section A.8.2 in the Appendix presents results from a Multivariate Fractional Logit (MFL) model to evaluate the robustness of our findings. Unlike OLS, which ignores the bounded nature of the outcome variables and the unit-sum constraint, the MFL model explicitly accounts for the fractional structure of the response variables. The comparison of average partial effects between the two methods shows a high degree of consistency, further strengthening confidence in our main results.

their interactions, which are indicator variables for the respective sign and magnitude (10% and 25%, respectively). The set of coefficient estimates, $\beta_i, i = 0, \ldots, 5$, allows us to empirically test for asymmetry between tax increases and decreases, as well as the incremental effects of the magnitude of the tax change.[25]

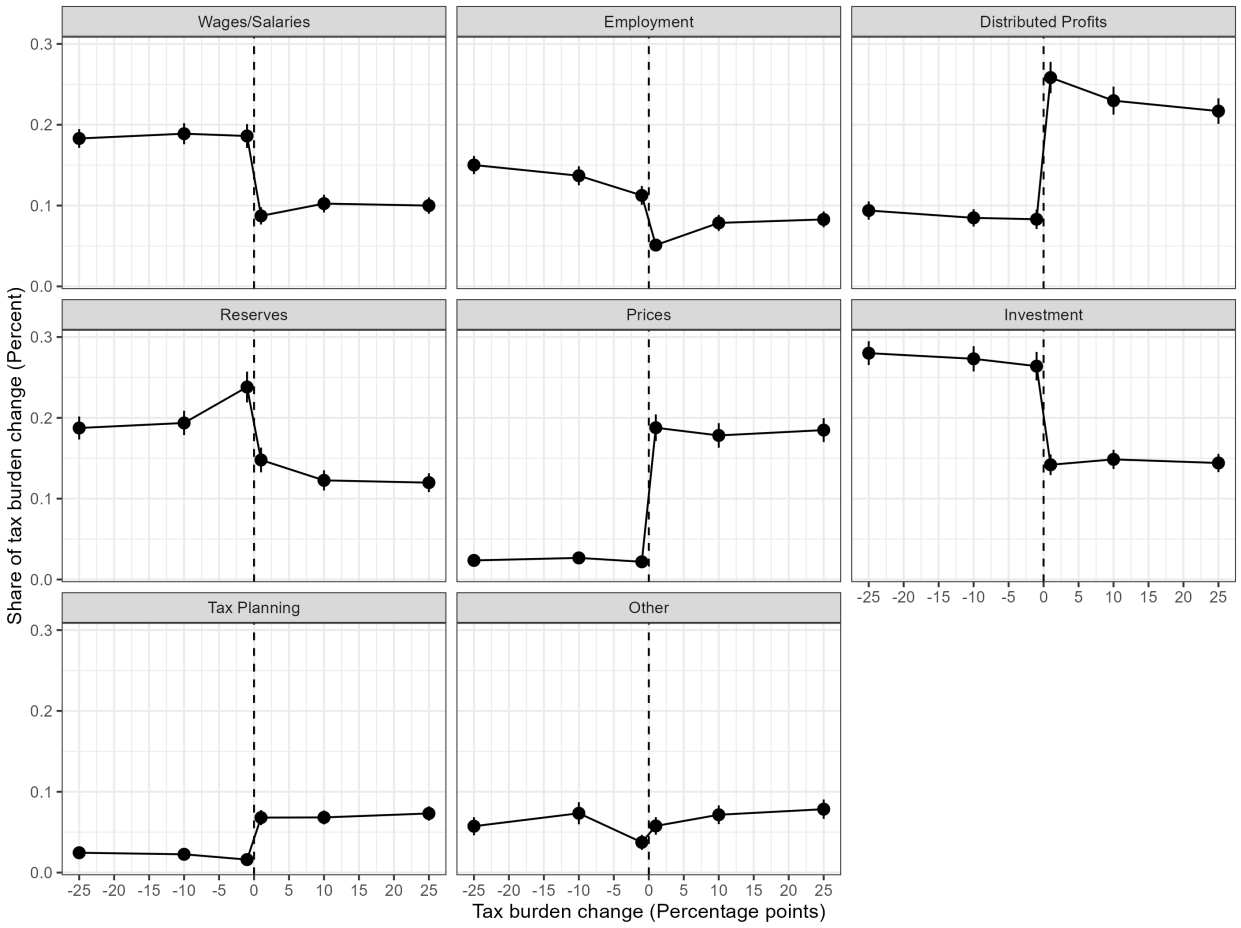
2.3.2 Results

Non-linearity of tax effects. We begin our analysis by plotting the aggregated coefficients for different treatment groups across the intensity of our treatment. Figure 2.1 presents incidence curves that illustrate how the average usage of categories varies with the dose of our tax treatment. These incidence curves reveal a substantial asymmetry in the effects of our treatment. For almost all categories, they exhibit pronounced discontinuities at the dashed line, where the treatment shifts from a tax decrease to a tax increase. Interestingly, aside from this discontinuity at zero, the incidence curves remain relatively flat across treatment doses, with some interesting exceptions (see below). As a first key takeaway, we conclude that, in our setting, the asymmetry between tax increases and decreases seems to play a major role, whereas the treatment dose exhibits less pronounced variation. Building on this insight, we now explore the results for the individual categories in greater depth.

Category usage. Next, we examine the frequency with which categories are chosen. Table 2.3 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

[25] 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 A.5 in Appendix A.8.1 and suggest that the estimated effects are largely in line with those derived without the inclusion of controls.

Figure 2.1: Tax Change Effects by Outcome Margin and Treatment Group



Note: Figure 2.1 shows the tax change effects by outcome margin and treatment group. Each panel shows the estimated incidence share for the respective category across the six different treatments based on equation (1). Each category represents a managerial adjustment margin. The available margins include adjustments to wages and salaries (*Wages/Salaries*), the number of employees (*Employment*), profit distributions to partners and shareholders (*Distributed Profits*), retained earnings and reserves (*Reserves*), consumer prices (*Prices*), (capital) investment (*Investment*), the usage of tax saving opportunities (*Tax Planning*), and other choices indicated in the open field question (*Other*). See Section 2.2.2 for more details on the categories and Section 2.5.1 for how they relate to the incidence margins investigated in the previous literature. Robust confidence bounds are indicated by vertical lines.

$(0, 1)$. We observe only small differences in means between the unweighted and weighted incidence shares. None of these differences are substantial.[26] The sample percentages indicate that companies made extensive use of most categories. Only the categories *Prices* and *Tax Planning* were used by less than 10% of respondents in the tax decrease group.

[26] We explore the sensitivity of our findings with respect to the applied weighting scheme in Appendix Section A.8. Our treatment effects are robust to applying survey weights. See the discussion in Section 2.2.1 for an explanation of why we use unweighted results for the rest of the paper.

Table 2.3: Descriptive Statistics Incidence Shares

			Mean		Sample Percentages		
Outcome	Treat Sign	Obs	Unwghtd.	Weighted	$y_i = 0$	$y_i = 1$	$y_i \in (0,1)$
Wages/Salaries	Decrease	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 2.3 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 Appendix A.5).

Next, we present exact figures and statistical tests for the patterns shown in Figure 2.1, summarized in Table 2.4. Table 2.4 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. Regarding the effects on workers, we find that for tax increases, workers bear about 17% of the profit tax incidence, with 10% channeled through reduced wages and 7% through reductions in employment. For wages, there is little difference between treatment doses, ranging from 8.7% to 10.2%, with an insignificant F-statistic for the joint

test of coefficient differences. For employment, on the other hand, we find that the magnitude of the tax increase affects the share attributed to this category. While only 5% of a 1% tax burden increase is financed through reductions in employment, this share rises to 8% when the tax burden increase amounts to 25%.

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

Owner incidence. When turning to profit distributions, the survey data suggest that firm owners bear about 24% of the additional tax burden, compared to merely 9% of additional funds received in the case of a tax cut. Similar to the employment effects, category usage seems to be affected by the magnitude of the tax change as well, however, this only holds true for tax increases. While firm owners bear about 26% of a small tax change directly through reduced profit distributions, this share decreases by about 4 percentage points for large profit tax burden increases. Thus, it becomes apparent that for higher tax increases, the incidence shifts from firm owners to workers. This pattern is consistent with firm own-

Table 2.4: Level Estimates for Treatment Combinations

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

Note: Table 2.4 shows the incidence level estimates for the different treatment cells estimated from equation (1). Test statistics for differences between tax increases and decreases are given below the coefficient estimates for each intensity pair. F-statistics for the joint test of equality of coefficients are given in the rightmost column, where a significant result indicates that at least one pair of coefficients is different. Robust standard errors for the composite coefficients are given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

ers covering modest increases in the tax burden out of their own pockets, but they are less willing or able to cope with the additional tax burden as the magnitude of the tax increase grows. For larger tax changes, more drastic measures become necessary, such as job cuts, to keep the company profitable.

Consumer incidence. The incidence on consumer prices features by far the highest asymmetry we detect. On average, the additional tax burden is passed on to consumers by 18%, whereas only 2% of the additional funds available after a tax decrease would be used to lower prices. This pass-through rate seems to be unaffected by the size of the tax change, as the F-statistic for differences in coefficients is insignificant for both treatment signs. While this result complements existing evidence of asymmetric price responses in the context of VATs (Benzarti et al., 2020), sales taxes (Yilmazkuday, 2017), and excise taxes (Bergman & Hansen, 2019), which find that prices react more strongly to increases than to decreases, we are not aware of studies that evaluate this asymmetry in the context of business profit taxes.

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

Investment effects. Furthermore, we detect asymmetries for the responses of investment to tax changes. The averages suggest that investment levels are less affected by tax

increases as they are by tax decreases. With 27%, investments are almost twice as responsive to tax decreases than increases (15%). This asymmetric response is implied by the heterogeneous impact of the tax change on workers and consumers, as different pass-through possibilities across the sign of the tax change directly affect the investment sensitivity (Jacob, 2022).

To better understand the underlying mechanisms that drive companies to adjust their investment behavior in response to a tax change, we asked respondents selecting shares for investment greater or equal to 5% about their reasoning for this choice. Figures A.8 and A.9 (Appendix A.2) present examples of the questions as appearing in the online interface of the survey. Participants rated their reasoning on a scale from 0 to 100. A rating of 0 indicated that investment adjustments were primarily driven by changes in available funds following a tax increase or decrease, whereas a rating of 100 suggested that the perceived profitability of investments was the dominant factor. Lower values therefore reflect capital constraints, while higher values indicate that tax changes primarily influence the profitability of investment opportunities.

Figure A.13 in 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 United States (US) firms following the American Jobs Creation Act (AJCA), which notably reduced the tax burden on US companies. Faulkender and Petersen (2012) observe that capital-constrained firms, in particular, significantly increased their investments after experiencing a positive cash flow shock due to the AJCA. Similarly, Zwick and Mahon (2017) find that tax incentives related to bonus depreciation lead to an

increase in investment and that profitable firms respond more strongly to incentives when they receive immediate cash flows from the reform, compared to tax-loss firms, which must wait to benefit from these deductions in the future. Moreover, Duan and Moon (2024) conclude that the higher investment response of small manufacturing firms in Canada is likely driven by firms being cash-constrained before the tax reduction. The responses also illustrate that respondents probably did not interpret our tax treatment as a lump-sum cost shock. If this were the case, there would be no reason to answer that the investment is less worthwhile. However, for more than 50%, the tax treatment seems to have a substantial impact on the profitability of the investment.

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

2.4 Treatment Effect Heterogeneity

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

Firm size plays a central role in determining how businesses respond to external shocks,

as larger firms often have more resources, wage setting power or tax saving opportunities, while smaller firms may be more vulnerable to disruptions (Fuest et al., 2018). Likewise, economic sector differences may influence treatment effects due to variation in competition, profit margins, and factor input intensity (Fuest et al., 2018). Organizational form can also shape a firm's response to tax changes, particularly in terms of governance structures and risk sharing. Finally, we consider financial distress, as firms with pre-existing financial vulnerabilities may experience heightened sensitivity to policy interventions, credit constraints, or market conditions (Faulkender & Petersen, 2012; Duan & Moon, 2024). By analyzing these dimensions of heterogeneity, we aim to provide a more nuanced understanding of the treatments' impact and shed light on which types of firms feature the highest sensitivity in different margins and under what conditions.

2.4.1 Empirical Strategy

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

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

where $\boldsymbol{x}_i$ denotes the vector for the firm characteristic of interest (e.g., size dummies). This specification allows us to easily calculate and test incidence shares for different subgroups of our data, allowing these characteristics to differentially affect incidence for tax increases and decreases.[27]

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

We measure firm size by reported revenue in 2019 and follow the definition by the European Commission by considering firms as micro-enterprises if they have annual revenues of less than EUR 2 million, small if their revenues are below 10 million, medium for revenue below 50 million and large for revenues exceeding 50 million. For economic sector, we utilize the provided self-classification of the company in our survey and assign them to either manufacturing, construction, trade, or services, with any firm not falling into those categories as belonging to the group *other*. We sort our firms into groups of legal forms, distinguishing between corporations, partnerships and sole proprietors.[28] Finally, we perform a split on whether the firm indicated that it was severely impacted by the COVID-19 pandemic. In the survey, respondents were asked about the impact of the pandemic on their net income, and could report changes in net income on a scale from *-100* to *+100*. We construct a dummy based on this variable equal to one for firms below the median value.

2.4.2 Results

Firm size. Figure 2.2 shows results for heterogeneous effects by company size as measured by the firm's stated revenue in 2019. Panels 2.2a and 2.2b show level estimates for average category usage across the four size categories for tax decreases and increases respectively, while Panel 2.2c 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 tax cut on investment varies with company size, suggesting that a 10 percentage point larger share is attributed to funding new investment by medium

[28] 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.

companies compared to micro-enterprises, and even a 13 percentage point larger share by large companies. However, due to the small number of large companies in our survey, this result is insignificant after controlling for the false discovery rate. Investments of larger firms often benefit from larger economies of scale, which would be consistent with an increasing share attributed to investment when the tax burden decreases.

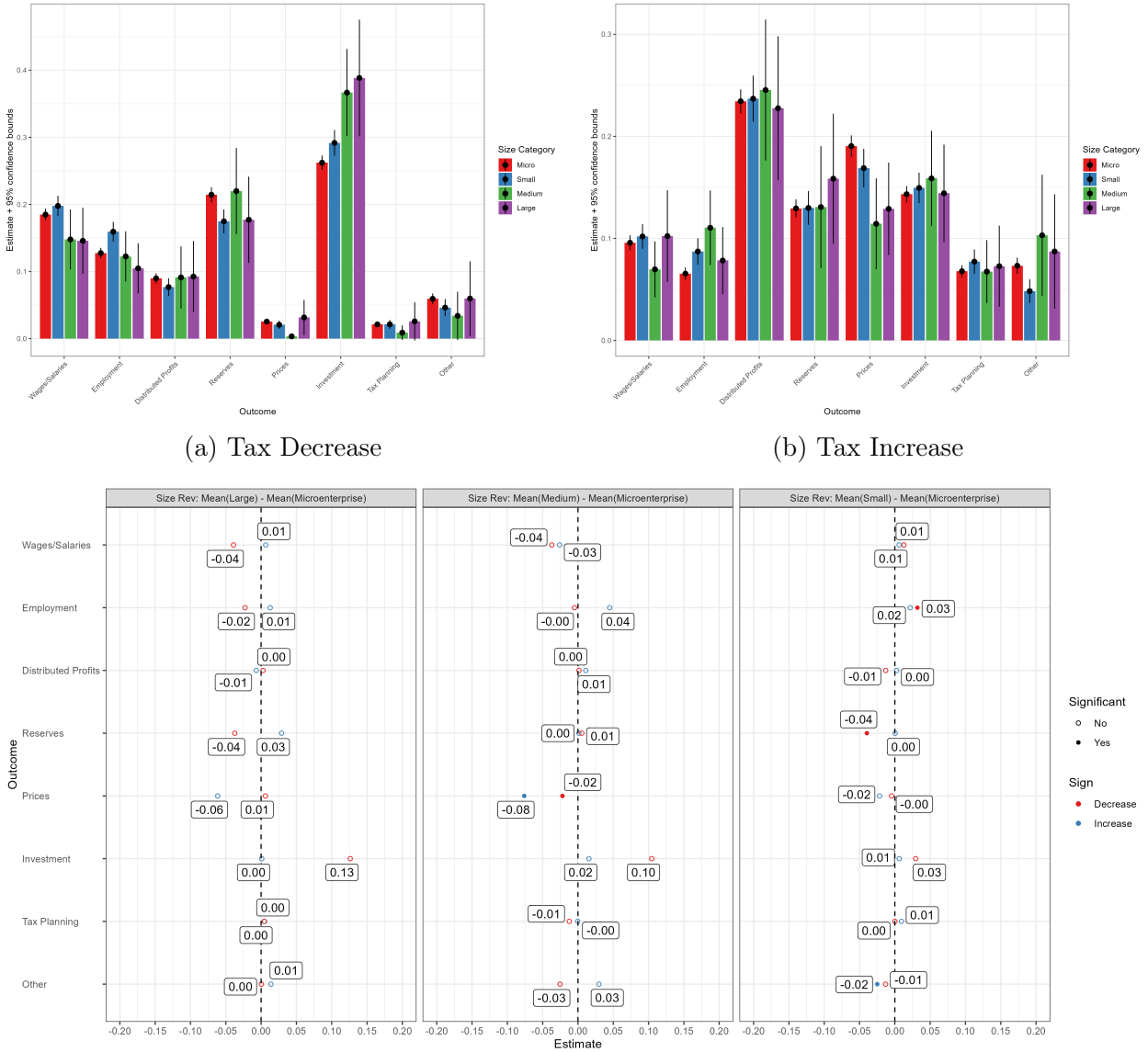
We also detect some evidence for varying pass-through to consumers for tax increases depending on the size of the firm, suggesting that larger firms finance 8 percentage points less via price adjustments compared to micro-enterprises. This may reflect that larger firms are more likely to operate internationally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

Though the adjusted p-values exceed the 5% threshold, our point estimates suggest that larger firms are more prone to adjusting employment at the extensive margin when faced with a tax hike. A possible driver of this effect might be that for smaller firms, employment is a rather discrete choice compared to larger companies. In a firm with 4 employees, any employment adjustment is relatively large – changing the workforce by 25% with the addition or removal of a single worker. By contrast, a firm with 30 employees can make smaller, more incremental changes, as adjusting by one employee represents only a 3% shift in labor input. 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 2.3. Panels 2.3a and 2.3b again show the average category usage for each sector separated by the sign of the tax change, while Panel 2.3c illustrates the partial effects to test for differences between each sector and the baseline.

We find the most striking heterogeneity in the construction sector, which features a substantial 12 percentage point larger incidence on consumers in the case of a tax increase

Figure 2.2: Incidence Heterogeneity by Firm Size



(a) Tax Decrease

(b) Tax Increase

(c) Average Partial Effects

Note: Figure 2.2 shows heterogeneity in incidence by company size measured by revenues. Panel 2.2a and 2.2b illustrate different levels and associated robust standard errors of category usage by treatment sign calculated from the estimated coefficients from equation (2), whereas Panel 2.2c 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.

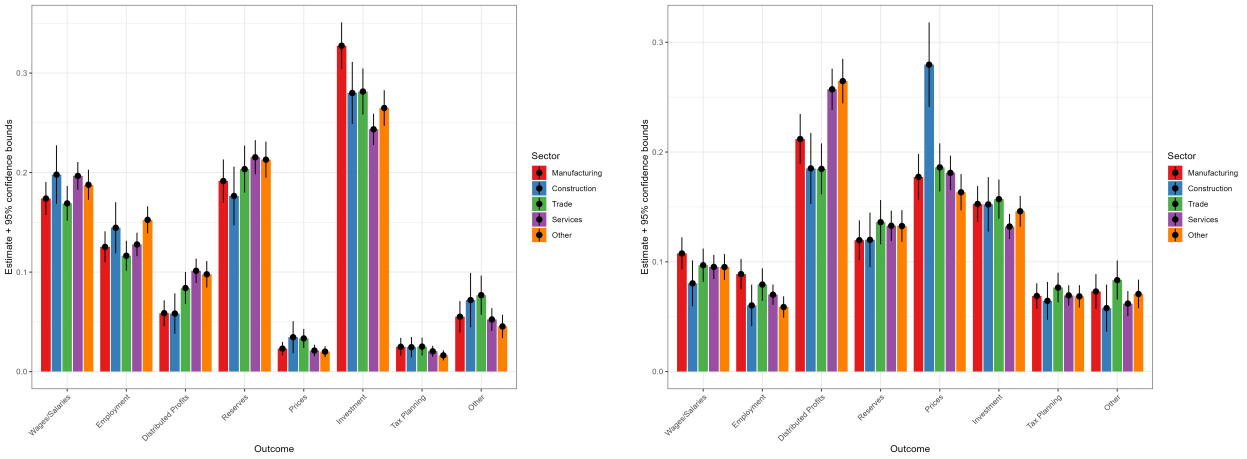
compared to the other sectors. The higher pass-through to prices seems to offset a lower incidence on the owners of construction companies, who are less affected by a tax increase compared to the other industries. Interestingly, this result does not materialize for tax decreases, where construction firms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

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

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

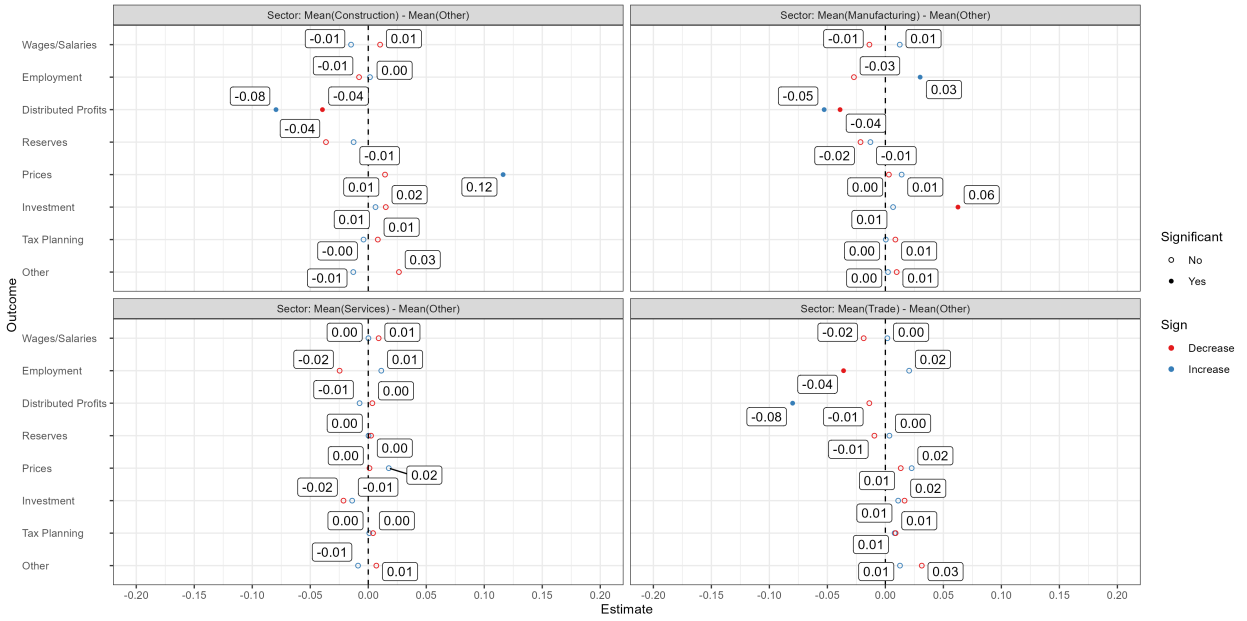
Organizational form. We detect interesting heterogeneities by legal form of the respondent firm. Figure 2.4 shows level estimates of category usage (Panel 2.4a and 2.4b) as well as partial effects estimates (Panel 2.4c). First, the results indicate that worker incidence via wage adjustments is less prevalent in partnerships and sole proprietors compared to cor-

Figure 2.3: Incidence Heterogeneity by Economic Sector



(a) Tax Decrease

(b) Tax Increase



(c) Average Partial Effects

Note: Figure 2.3 shows heterogeneity in incidence by economic sector. Panel 2.3a and 2.3b illustrate different levels and associated robust standard errors of category usage by treatment sign calculated from the estimated coefficients from equation (2), whereas Panel 2.3c 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.

porations, which can be observed for both tax increases and decreases. These effects remain significant even after controlling for differences in size and industry.

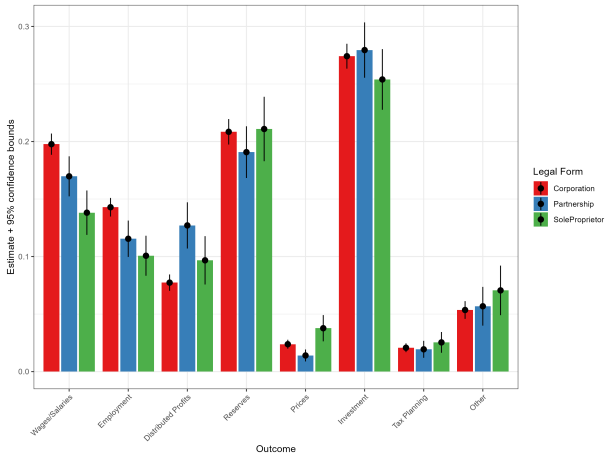
Second, the data suggest substantial differences in owner incidence depending on the organizational form of the companies. Partnerships state with 5 percentage points substantially larger effects on distributed profits for both decreases and increases compared to corporations. Sole proprietors, on the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

Financial distress. Finally, we explore whether the economic condition a respondent company was in during the COVID-Pandemic has an impact on its stated category selection. Figure 2.5 shows level and partial effects estimates for each category for increases and decreases separately. For tax decreases, we observe that owners benefit more from the additional funds if their company was not severely affected by the pandemic as indicated by its impact on the companies net income. Companies with a substantial drop in net income due to lock-down or supply chain disruptions likely experienced a severe tightening of liquidity constraints. Hence, a decrease in taxes would then be used to pay off debt or short-term liabilities. This notion is supported by the opposite-sign partial effect on the category *Other* in combination with Figure A.10, which suggests that a substantial share of the free text answers alluded to repayment of debt.

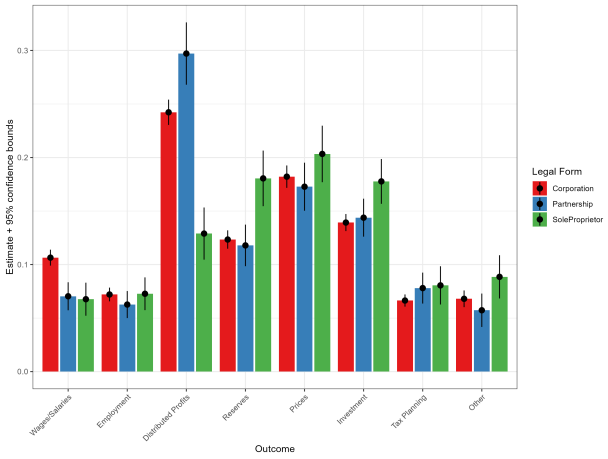
For tax increases, we also see a pronounced difference in payout incidence depending on the net income impact of the pandemic. A similar logic can be applied here as for the tax decrease treatment. Firms with a substantial negative impact on their net income might be in a precarious situation where no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

We further detect a lower pass-through of tax hikes to consumers from firms that ex-

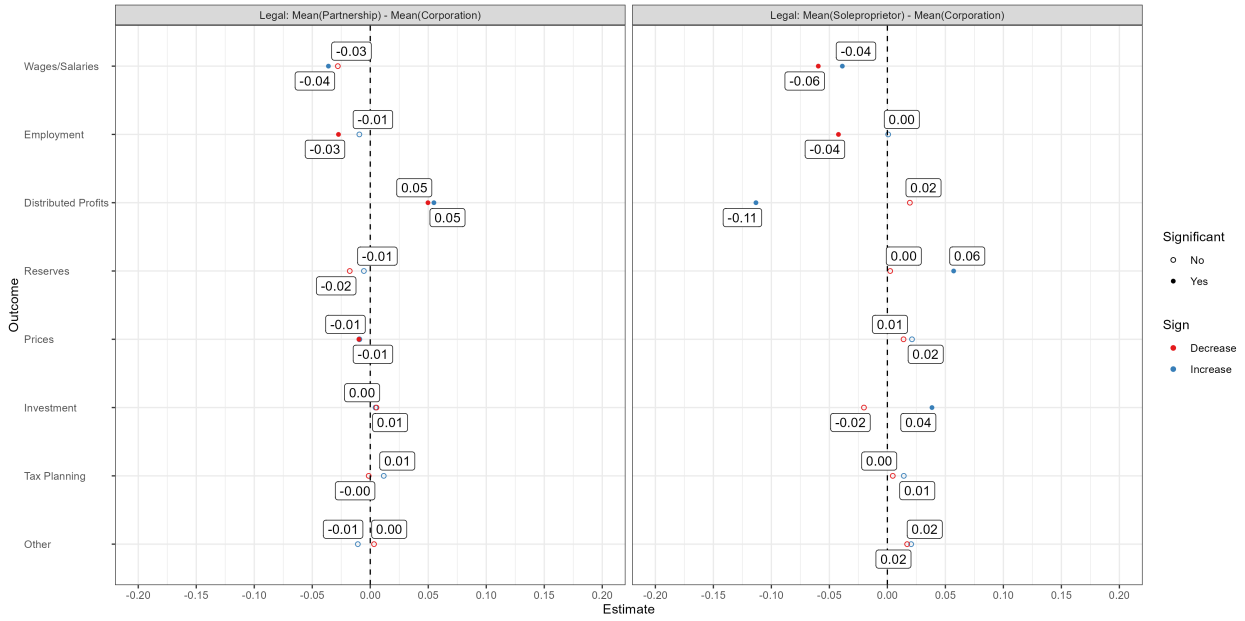
Figure 2.4: Incidence Heterogeneity by Organizational Form



(a) Tax Decrease



(b) Tax Increase



(c) Average Partial Effects

Note: Figure 2.4 shows heterogeneity in incidence by company legal form. Panel 2.4a and 2.4b illustrate different levels and associated robust standard errors of category usage by treatment sign calculated from the estimated coefficients from equation (2), whereas Panel 2.4c 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.

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

2.5 Cross-Validation with Literature and Reliability of Survey Responses

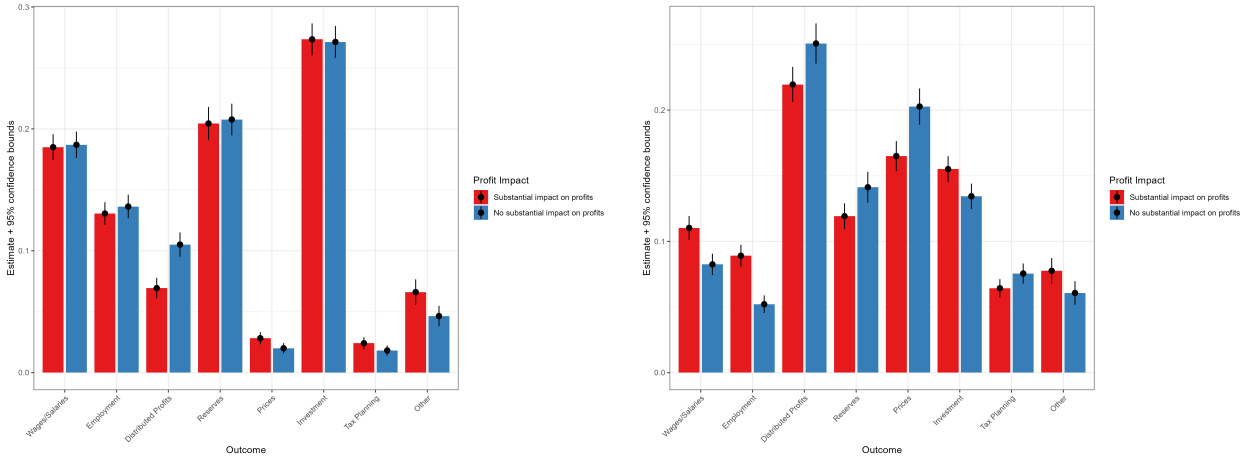
The validity of our findings critically depends on the reliability of our survey responses. In this section, we first cross-validate our results with existing literature using observational data (2.5.1). We then discuss several potential threats to the reliability of our survey-based findings and conduct empirical validation exercises, testing if respondents report firm characteristics accurately and if stated actions predict actual behavior (2.5.2).

2.5.1 Cross-Validation with Observational Studies

We examine how our baseline results (presented in Section 2.3.2) compare to prior studies on the incidence of corporate taxes using (non-survey) observational data. To assess the reliability of our survey estimates in predicting real economic behavior, we follow Colarieti et al. (2024) and apply a *cross-validation method*. This approach evaluates how well our incidence estimates from hypothetical tax changes align with those from previous research based on observational data.

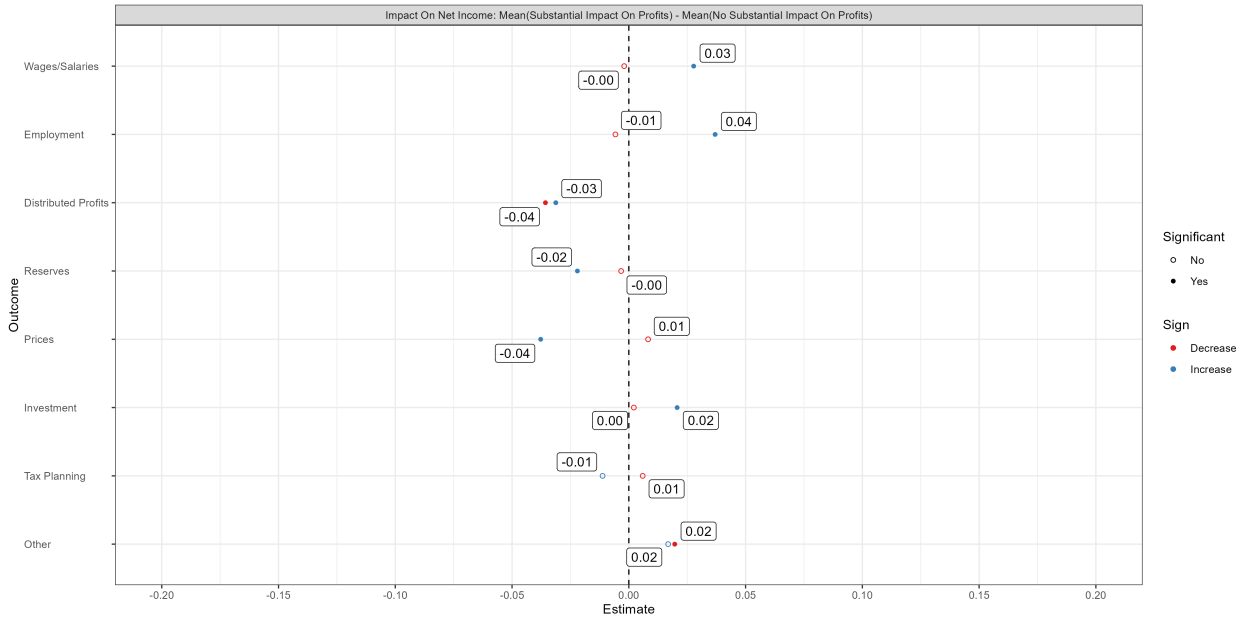
For the cross-validation, we present incidence estimates from prior studies, distinguishing between tax increases, tax decreases, and studies that pool both types of tax changes. Table 2.5 summarizes key details for each cross-validation, including the reference study, the tax variation analyzed, the direction of the tax change, the country sample, the specific episode examined, the incidence estimate reported in the study, and our corresponding survey-based

Figure 2.5: Incidence Heterogeneity by COVID Profit Impact



(a) Tax Decrease

(b) Tax Increase



(c) Average Partial Effects

Note: Figure 2.5 shows heterogeneity in incidence by whether the company was substantially impacted by COVID. Panel 2.5a and 2.5b illustrate different levels and associated robust standard errors of category usage by treatment sign calculated from the estimated coefficients from equation (2), whereas Panel 2.5c 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.

estimate.

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

Incidence calculation. The basis for the calculation of our tax incidence estimates is the results for the incidence shares presented in Table 2.3. We define the *initial* incidence of the profit tax as the short-term impact of the tax change on workers, firm owners, and consumers.[29] Considering a given level of pre-tax profit, the initial incidence indicates how a change in the profit tax burden is shared across these stakeholders at the margin through changes in wages, distributed profits, and prices.[30] 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 2.5) abstract from second-round effects and provide evidence on the initial incidence. In our setup, the initial incidence corresponds to the categories *Wages/Salaries*, *Distributed Profits*, and *Prices*, as these are the most commonly used categories in previous studies.[31] Although we observe some aspects

[29] 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 & Metcalf, 2002). Most studies focus on a subset of groups that can in principle share in the burden within the setting considered and omit certain other groups. For example, Fuest et al. (2018) consider a small open economy setting, where output prices are fixed and consumers therefore cannot share in the burden of the tax. Jacob et al. (2023) only look at firm owners and consumers, as wage effects are unlikely to occur in their setting because of minimum wage regulations. In general, theory suggests that the incidence of the corporate income tax can fall both on the sources (capital, labor, and land) as well as the user side (consumers and governments) of production (Fullerton & Metcalf, 2002; Auerbach, 2006; Fuest et al., 2018; Jacob et al., 2023).

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

[31] One could argue that changes in retained earnings or reserves could also be attributed to the owners of

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.[32] 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.

Given our interpretation of the *Wages/Salaries*, *Distributed Profits*, and *Prices* categories as components of the initial incidence of the profit tax on workers, firm owners, and customers, we can compare our results with existing findings based on observational data. Since the impact on these categories is expressed as a percentage of the change in tax burden, they are measured in the same units and can be directly compared. For each cross-validation in Table 2.5, we determine the initial incidence for the relevant category based on the factors (*Wages/Salaries*, *Distributed Profits*, *Prices*) considered in the previous study, as well as the direction of the tax change (*increase*, *decrease*, or *increase and decrease (pooled)*). For example, the incidence estimates in Fuest et al. (2018) are derived from a sample of tax increases in Germany, where the authors analyze the incidence of the German local business tax on workers and firm owners. To compare their incidence estimates with our estimates, we use the incidence shares from Table 2.3. Specifically, for Fuest et al. (2018), we define the initial incidence on workers as the ratio of the *Wages/Salaries* incidence share to the sum of the *Wages/Salaries* and *Distributed Profits* incidence shares for tax increases, i.e., we compute the incidence as $0.097/(0.097 + 0.235)$ based on the values in Table 2.3. The incidence on firm owners is defined analogously.

the company, in addition to distributed profits. However, this holds true only if the reserves are eventually distributed to the firm's owners rather than used for future investments.

[32] 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 & Metcalf, 2002).

This definition is related but not identical to the concept of initial incidence used in earlier literature. For instance, Fuest et al. (2018) estimate the incidence of the German local business tax on workers and firm owners by calculating their respective welfare changes within a simple partial equilibrium model. In their framework, the economic incidence of the tax is defined as the welfare change for each group relative to the total welfare change across all groups. In contrast, our measure is based on the change in tax revenue, abstracting from dead-weight losses and over-shifting – both of which can result in a tax burden exceeding the generated tax revenue (Fullerton & Metcalf, 2002).

Importantly, while some recent studies analyze multiple firm-level outcomes or combine firm and worker data (Kennedy et al., 2024; Duan and Moon, 2024)[33], the vast majority of empirical work on business tax incidence focuses on a single adjustment margin and stakeholder group – most commonly wages for workers or prices for consumers. Broader incidence effects are typically inferred indirectly, either through model-based calculations or general equilibrium modeling (Suárez Serrato and Zidar, 2016; Fuest et al., 2018; Baker et al., 2023).

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

Tax increases. Regarding tax increases, we compare our incidence estimates with three observational studies, as presented in Table 2.5, which assess tax incidence in the context

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

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

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

Tax decreases. Analyzing previous studies on corporate tax cuts, estimates of the incidence on workers range from 40% to 80%.[34] Using US worker-level filings linked to corporate

[34] Two additional studies on corporate tax decreases, Dwenger et al. (2019) and Ohrn (2023), also analyze worker tax incidence but are not directly comparable to our estimates. Dwenger et al. (2019) exploit exogenous variation in effective corporate tax burdens resulting from two tax reforms in Germany and estimate a worker tax incidence between 19% and 28%. However, their estimate is based on a combination of positive wage effects and negative employment effects. Since our experimental design does not allow for negative factor adjustments for tax decreases, a direct comparison with their preferred estimate is challenging. Ohrn (2023) analyzes the effect of two corporate tax breaks in the US on the compensation

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

Compared to these previous estimates of tax incidence from corporate tax reductions, our findings indicate a worker tax incidence of 68% when considering the categories wages and distributed profits. This aligns most closely with the results of Duan and Moon (2024). A key factor explaining this similarity is the composition of our sample, which consists primarily of smaller firms (similar to Duan and Moon (2024)), with 67% employing fewer than 10 workers. In such firms, the owner-worker incidence – explicitly considered by Duan and Moon (2024) – plays an important role in determining worker incidence, whereas in larger firms, owner-workers may be less prevalent.

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.

Tax increases and decreases - pooled. Finally, we compare our estimates with studies that pool tax increases and tax decreases to calculate corporate tax incidence.[35] To better align with the relevant incidence margins used in the referenced studies, we pool the incidence shares from Table 2.3 for tax increases and decreases.

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

Liu and Altshuler (2013) estimate a worker tax incidence of approximately 60%, with a lower bound of 42% in their most conservative specification, using variation in effective US marginal tax rates. Using the wage and distributed profit shares from Table 2.3 for both tax increases and decreases, we estimate an incidence of approximately 47%, which falls well within the range identified by Liu and Altshuler (2013). Finally, using a general equilibrium model and a structural estimation approach to assess the impact of taxes on various groups, Suárez Serrato and Zidar (2016) – along with further refinements in Suárez Serrato and

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

Zidar (2023) and Suárez Serrato and Zidar (2024) – estimate that the incidence of the US state corporate income tax falls between 38.1% and 50% on capital, 25% to 40% on workers, and 10% to 30% on landowners. Although we are unable to measure the tax incidence on landowners, making a direct comparison with Suárez Serrato and Zidar (2016) challenging, our estimates align closely with their findings for workers and capital. Specifically, our estimated incidence shares for wages (47% incidence on workers) and distributed profits (53% incidence on capital) are close to the ranges identified by Suárez Serrato and Zidar (2016), Suárez Serrato and Zidar (2023), and Suárez Serrato and Zidar (2024).

To sum up, the takeaway from the cross-validation is that our survey responses reliably indicate firms' behavioral patterns in response to hypothetical tax changes. This reliability likely stems from the fact that these scenarios closely mirror real-world decision-making processes as in Colarieti et al. (2024). Rather than being abstract or unfamiliar, hypothetical tax changes reflect the strategic financial and operational considerations that firms regularly evaluate. As a result, firms' responses to potential tax policy shifts tend to align with the actual actions observed in observational data. We further corroborate the reliability of our survey responses in capturing real-world firm behavior in Section 2.5.2.

Table 2.5: Cross-Validation - Selected Studies

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

Note: Table 2.5 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 2.3. The table highlights selected recent studies that are most suitable for comparison with our incidence estimates. A more comprehensive overview, including incidence estimates from further research, can be found in Appendix A.10. "-" : 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.

2.5.2 Reliability of Survey Responses

Even though our results compare well to the findings of prior literature, as shown in the previous Section 2.5.1, one might be concerned that they may be subject to behavioral or cognitive biases. More specifically, the effects we document could be driven by one or more of the following sources of bias, which have been documented by a large literature in the fields of experimental and behavioral economics (De Quidt et al., 2018, 2019; Haaland et al., 2023; Stantcheva, 2023; Bursztyn et al., 2025): (i) social desirability, (ii) hypothetical nature of the treatment, and (iii) representative agent assumption. If these biases are present in our setting, our survey results may have little predictive power of actual behavior of firms, which we are ultimately interested in.

After discussing each source of bias and the accuracy of manager surveys in general, we conduct several empirical validation exercises to mitigate remaining concerns and bolster the confidence in our results. First, we show that respondents offer a high degree of reliability in stating characteristics of their firms. Second, we show that participants' stated actions are predictive of actually implemented actions.

Biases in survey responses. Regarding desirability bias and experimenter demand (i), managers may, for instance, hesitate to report lower wages or layoffs, particularly if they seek to be perceived as socially responsible by the experimenter (Haaland et al., 2023; Bursztyn et al., 2025). However, we argue that these concerns are of limited relevance in our setting for three reasons. First, experimenter demand effects are likely less pronounced in online surveys compared to face-to-face interviews due to the increased anonymity afforded to participants (De Quidt et al., 2018; Haaland et al., 2023; Stantcheva, 2023). Second, the neutral framing of our survey regarding taxation further reduces the likelihood of experimenter demand effects (Haaland et al., 2023; Stantcheva, 2023). Third, since we employ a between-subject design, experimenter demand effects are likely less problematic than in within-subject designs (De Quidt et al., 2019). Moreover, if firms were systematically providing socially desirable

answers or attempting to influence the survey outcome with exaggerated or untruthful statements, we would not expect to observe significant differences based on the magnitude of the tax change. However, such differences are indeed present in our results. In the presence of a social desirability bias, one would also expect respondents to claim to a much greater extent that they pass on tax cuts to their customers by lowering prices. However, the responses indicate only a minimal pass-through of tax cuts to prices (two percent). Finally, if a CEO faces unexpected tax increases and has to distribute the costs, also the actual decision making involves social aspects. Hence, the eventual decisions may also reflect social desirability concerns.

Another potential concern is the use of hypothetical tax changes in our treatments (ii). Their hypothetical nature may reduce respondent effort, as such scenarios can be difficult to translate into real-world decision-making (Haaland et al., 2023). To assess whether such bias is present, we implement several validation checks. First, we cross-validate our estimates of initial incidence on workers, firm owners, and consumers by comparing them to prior literature based on observational data sources (Section 2.5.1). Our findings align closely with existing empirical evidence, reinforcing our confidence in the validity of our survey results. Second, in this section, we evaluate the predictive power of respondents' hypothetical answers by comparing their stated actions to their actual behavior in two distinct settings discussed below. 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 household 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 23). 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 discuss below, prior research using business survey data indicates that firm managers generally provide reliable and accurate forecasts of their firms' behavior. We also show that, in our setting, respondents reliably report firm characteristics and that their stated intentions are strong predictors of actual firm behavior.

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

tive outcomes such as profitability and sales growth. Link et al. (2024) show that firms' planned investment volumes serve as strong predictors of realized investment levels in the subsequent year (based on survey responses). Regarding pricing behavior, survey evidence indicates that planned price changes align well with actual price changes or subsequent price revisions, based on survey questions comparing expected and past price changes, as well as price data from a selected subgroup of firms with online price records (Coibion et al., 2018). Similarly, Coibion et al. (2020b) find that reported employment levels in surveys closely correspond to employment figures in administrative data. Additionally, Kumar et al. (2023) demonstrate that firms' responses to hypothetical survey treatments closely match results from randomized control trials using non-hypothetical information, such as Gross Domestic Product (GDP) forecasts from professional forecasters. Furthermore, firm and manager characteristics – such as firm age and managerial position – largely conform to administrative or official records (Coibion et al., 2018, 2020b; Kumar et al., 2023).

Overall, these findings reinforce our confidence that firm managers' stated plans serve as reliable predictors of their actual behavior. In what follows, 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. (2024) for the first wave of the GBP. We form four categories for revenues and the number of employees respectively and calculate the proportion of observations that are in the same size category between the survey and the Orbis data. Limited by the availability of revenue and employee count in Orbis, we can do this comparison for 606 observations for the revenue test and for 1,516 observations for

Table 2.6: Correspondence Revenue

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

Note: Table 2.6 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].

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

The results for revenues are depicted in Table 2.6. 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 2.7. The sum of the diagonal elements is 0.80, with a Cohen's Kappa of 0.62. These results mirror closely the findings of Bischof et al. (2024) for the first survey wave.

It should be noted that deviations between the survey and Orbis categorization is not necessarily indicative of incorrect survey responses. In the survey, it was specifically asked how many full-time employees subject to social security the firm employs, whereas the number of employees variable in Orbis is defined as the total number of employees included in the company's payroll. As these definitions are not necessarily congruent (e.g., due to apprenticeships, part-time employment or parental leave), slight deviations can be expected (Bischof et al., 2024). Overall, the comparison shows that firms state easily verifiable company characteristics with a high degree of reliability, which provides a general level of confidence in the survey responses.

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

Table 2.7: 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 2.7 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 0.80. Cohen's Kappa is 0.62, with 95% confidence interval [0.58, 0.65].

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 employment adjustments of a hypothetical tax change on firm-level employment in the survey. 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 local business taxes. Firms operating in Germany are in principle subject to three types of taxes on their income depending on the legal form: the corporate income tax, the personal income tax, and the local business tax. 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 local business tax can be set by the local governments on the municipality level, however, the tax base and criteria for liability are set at the federal level (Fuest et al., 2018). The decentralized authority of local governments to set local business tax rates results in a substantial number of tax changes, which we can use to test the stated actions of our survey respondents.

Table 2.8: Sample Selection Local Business Tax 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 2.8 illustrates the sample selection process for the local business tax change validation exercise.

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 local business tax, whereas respondent firms in the tax decrease group are required to having experienced a decrease in the local business tax.[36] This requirement further reduces our sample to a total of 382 firms experiencing 588 changes in the local business tax. 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 2.8. The resulting subset constitutes approximately 3% of our initial sample of respondent firms. Table 2.9 summarizes the number of realized local business tax 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.

[36] 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 2.9: Overview Local Business Tax 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 2.9 shows the distribution of local business tax increases and decreases for the firms in our sample which we are able to link to external data sources.

For the final set of firm-years, we run cross-sectional regressions for the two treatment signs separately. In Column (1) of Table 2.10, we consider firms that were assigned to the tax decrease treatment and experienced at least one decrease in the local business tax 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.[37] 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.

[37] The results are robust to variations in this threshold. For tax decreases, we are unable to detect an association if firms that indicated that less than 5% of the tax burden decrease would be used to create new employment are classified as a substantial employment impact. For tax increases, the association becomes marginally weaker but remains significant for the five-year window.

Columns (2) and (3) show the results of a similar exercise for firms that were assigned and actually experienced a tax increase. Here, the dependent variable is an indicator for a negative change in employment after the tax change. The sample in Column (2) includes all tax increases dating back to 2007. The coefficient estimate is smaller compared to the tax decrease group and lacks significance. This is to be expected, as some of the tax changes occurred in vastly different economic environments. Therefore, in Column (3), we restrict the observed changes in the local business tax 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.

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

Responses to COVID-19 pandemic. As data availability restricts the sample usable for our validation test on a potentially selected subset of firms, we provide an alternative test, which can be performed for a larger subgroup. For this second test, we exploit two questions that were asked in the same wave of the survey. Both questions are of a similar nature compared to our tax incidence questions. The first question related to a potential *increase in employee numbers* and was stated as follows: *"Are you currently planning to hire additional employees in the short term (0-12 months)?"* The second question addressed a potential

Table 2.10: 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
R^2 Adj.	0.179	0.008	0.031

Note: Table 2.10 shows results of testing the correspondence between survey responses and actual responses to changes in the local business tax. In Column (1), we consider firms that were assigned to the tax decrease treatment and experienced at least one decrease in the local business tax 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 the local business tax to a five-year window (2018-2022) around the survey date. Robust standard errors are given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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

Table 2.11: Proxy Test COVID

	Dependent Variable: Employment change (Orbis)		
	Perc. Change	$\mathbb{1}(\Delta\text{Emp.} > 0)$	$\mathbb{1}(\Delta\text{Emp.} < 0)$
Survey: Reduce Employment	-0.056**	-0.061**	0.205***
	(0.023)	(0.029)	(0.032)
Survey: Increase Employment	0.059**	0.170***	-0.049**
	(0.024)	(0.026)	(0.023)
Num.Obs.	1,506	1,506	1,506
R2 Adj.	0.007	0.051	0.050
Sample Means	0.060	0.220	0.180

Note: Table 2.11 shows estimates from regressing changes in employment from Orbis over the year after the survey was conducted on indicator variables for hiring and firing plans stated in the survey, respectively. In Column (1), the dependent variable is the percentage change in employment over one year, whereas in Columns (2) and (3) the dependent variables are indicators for a positive or a negative change in employment, respectively. Robust standard errors are given in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

as proxies. We thus view the following results as complementary to the previous exercise.

We regress changes in employment from Orbis in the year after the survey was conducted on indicators for whether the firm stated that it would increase or decrease employment over the next 12 months. For this exercise, we only require two years of employment data for our respondent firms, which is a much weaker restriction than corresponding signs of tax changes. Therefore, the regressions presented in Table 2.11 are based on 1,506 firms. The first column shows the result for a specification where one-year percentage changes in employment are regressed on dummies for firms indicating increases or decreases in employment respectively, whereas Columns (2) and (3) show results for indicator variables for positive and negative changes in employment, respectively. We find highly significant coefficients on both dummy variables, indicating that survey responses are indeed predictive of actual behavior. When interpreting the magnitude of the coefficient, one should keep in mind that firms were operating in a high-uncertainty environment, where even short term developments were difficult to predict.

2.6 Conclusion

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

Our findings highlight a pronounced asymmetry in how business tax increases and decreases affect economic agents. Consumers bear a substantial portion of tax hikes, as firms pass on a significant share of higher costs through price increases, yet they benefit only marginally from tax reductions. Similarly, capital owners experience a greater burden from tax increases (through reduced distributed profits) than they gain from tax cuts. Conversely, employees experience an asymmetric effect in the opposite direction: while tax hikes have a limited impact on wages and employment, tax reductions result in more substantial wage and employment gains. Our analysis of treatment intensity further reveals that larger tax changes have a stronger impact on employment than on firm owner payouts and retained earnings compared to small tax changes. This non-linearity seems to stabilize for medium to large changes, indicating that a local perturbation in the tax rate exerts different effects compared to larger tax changes. Relying on the large set of observable company characteristics, we further investigate heterogeneity in profit tax incidence.

While our methodology enables the simultaneous analysis of multiple adjustment margins and allows us to explore heterogeneity in responses to the sign and magnitude of tax changes, it also faces limitations inherent in a survey-based approach. For instance, the use of hypothetical tax scenarios may lead to reduced respondent effort or experimenter demand effects. Although we cannot entirely rule out these concerns, we address them through detailed dis-

cussions, 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.

3 Central Bank Inflation Forecasts and Firms' Price Setting in Times of High Inflation

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Abstract: Using a randomized survey among firms, we study how information about inflation, energy costs, and wage dynamics affects firms' pricing strategies in a high-inflation environment. Firms exposed to information about central bank inflation forecasts intend to raise prices less than uninformed firms. The effect is more pronounced for firms whose inflation expectations are less aligned with central bank forecasts, those that are less attentive to past inflation dynamics, and those that are more satisfied with overall economic policy. The study highlights the important role of central bank communication in managing inflation, which is particularly crucial during periods of high inflation.

Keywords: Price setting, firms, inflation expectations, firm survey.

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After all, it is the everyday economic decisions of people and companies that we seek to influence with our policy and communication.

(Lagarde, 2020)

3.1 Introduction

Firms' expectations of future inflation are believed to be a key determinant of actual inflation (Coibion et al., 2018, 2020b; Weber et al., 2022; Werning, 2022). This relationship suggests that central banks might benefit from monitoring and influencing these inflation expectations through enhanced communication strategies. A pivotal question emerges from this premise: Can an effective information policy, such as sharing current and projected inflation figures, directly impact the way firms set their prices?

While this question is vital for assessing the role of central bank communication in managing inflation dynamics, empirical evidence on the causal effect of inflation information on firms' price setting is scarce, which is, among other factors, mainly due to the limited availability of firm surveys (compared to household surveys) (Coibion et al., 2020a). We aim to address this gap by providing causal evidence of how information on current and expected future inflation rates influences firms' pricing plans in a high-inflation environment. To this end, we survey 2,000 firms in Germany during the high-inflation year 2022 and conduct an information provision experiment.[38] In the survey, we start by eliciting firms' expectations of inflation over various time horizons. Following this, firms in an active control group are merely reminded of their inflation forecasts, whereas firms in the treatment group receive the central bank's official inflation projections (in addition to what control group firms see). The survey then proceeds to collect data on firms' planned price changes, which are known to closely align with realized price changes of firms (Coibion et al., 2018, 2020b; Kumar et al., 2023).[39]

[38] We simplify terminology by talking about "firms" when we mean "firm decision-makers".

[39] Evidence suggesting that planned price changes align with actual price changes is derived from survey questions asking about expected and past price changes, or from analyzing current prices of only a selected

We obtain two main results. First, providing information on the central bank's inflation forecasts for 2023 – which are around 7 percentage points lower than average firm expectations for this year – reduces planned prices of firms by 22%. Thus, information-induced updates about future inflation rates induce firms to adjust their pricing plans, suggesting that providing information about inflation dynamics can be an effective way for central banks to break the transmission of elevated and distorted inflation expectations into price setting and dampen upward inflation dynamics.

Second, firms in two additional treatment groups receive the central bank's energy and labor cost projections, in addition to the central bank inflation forecasts. This allows us to investigate whether the extent and type of inflation-related information matters for firms' price-setting behavior. In particular, we can test if additional information on single components of the central bank's inflation forecasts, which should be relevant for firms' input cost perceptions, has an additional effect on firms' price-setting behavior compared to information on inflation dynamics alone. We find that this is not the case: treatment effects on planned price changes in these additional treatments are similar to the simple information treatment on general inflation forecasts alone, with reductions of 19% (energy cost projections) and 22% (labor cost projections). This suggests that the additional treatments contain no incremental information for firms, as firms are potentially aware that the additional projections are already incorporated in the overall central bank inflation forecast.

In additional tests, we investigate the mechanisms and explanatory factors behind our treatment effects by conducting a series of sample splits. First, we consider the role of pre-treatment inflation expectations and find that firms with high pre-treatment inflation expectations, i.e., those further away from the central bank prediction, demonstrate a more pronounced price-plan response to the treatments. That is, respondents that experienced a greater information shock are more significantly affected in their pricing strategies. This indi-

subgroup of firms with available price data (Coibion et al., 2018, 2020b; Kumar et al., 2023). Moreover, it has been shown that survey-reported behavior is often close to revealed preference results in archival data (Parker & Souleles, 2019; D'Acunto et al., 2022; Coibion et al., 2023).

cates that treatment effects stem from changes in beliefs rather than mere priming (Haaland et al., 2023). Second, we find that firms previously inattentive to past inflation dynamics adjust their prices more significantly upon receiving central bank inflation forecasts than their more informed counterparts. This is consistent with the previous finding that the extent of the information shock matters. Among equally inattentive firms, we observe that additional information on input cost developments is generally more effective in moderating pricing plans.

Third, we assess the role of labor and energy as input costs. Firms considering these factors crucial for their pricing are less responsive to central bank forecasts about these costs. This suggests that highlighting energy or labor costs might inadvertently draw attention to increasing cost burdens, thereby neutralizing the intended effect of reducing inflation expectations through information dissemination. Conversely, firms placing less importance on these factors in their pricing decisions respond more markedly to the combined information about labor and energy costs, suggesting enhanced credibility of the information provided. Fourth, our findings indicate that firms dissatisfied with economic policy do not significantly alter their prices upon receiving central bank forecasts, emphasizing the role of institutional credibility. This finding complements results from household studies, which show that low trust in the central bank is typically linked with a diminished desire to be informed (Hayo & Neuenkirch, 2014). Therefore, the credibility and trust in the central bank seem to significantly affect the effectiveness of monetary policy communication (Christelis et al., 2020; Ehrmann et al., 2023). Finally, we explore the frequency of firms' price setting. Our data show that although firms plan to adjust prices more frequently in times of high inflation, their planned frequency of price setting remains largely unchanged when exposed to our information treatments. The observed increase in the frequency of price setting by firms is consistent with recent research that suggests state-dependent pricing behavior among firms (Cavallo et al., 2023).

In a final step of our empirical assessment, we incorporate inflation expectations directly

into our regression analyses to shed light on the pass-through of inflation expectations to prices.[40] To this end, we investigate how (untreated) inflation expectations translate to pricing plans for firms in the control group. In addition, we estimate separate slope coefficients and intercepts for each treatment group. While the slope coefficient captures how untreated and treated inflation expectations translate into post-treatment price plans, the group-specific intercept reflects the average effect of our information treatments on pricing plans.

We observe a one-to-one relationship between expectations and firms' pricing plans for firms in the control group. Even though this one-to-one relationship is not a causal estimate, it is striking that firms' inflation expectations fully translate to prices. This finding indicates that in *high-inflation environments*, inflation expectations appear to be highly relevant for firms' price setting behavior. In contrast, in low-inflation environments, Coibion et al. (2018, 2020b) as well as Rosolia (2024) document only insignificant relationships between inflation expectations and firms' pricing plans.

Furthermore, our results suggest that firms incorporate less of their pre-treatment inflation expectations into their pricing plans when they receive forecasts about energy and labor costs from the central bank in addition to general inflation forecasts (lower slope coefficients). This implies that adding energy and labor cost projections to the overall inflation forecasts leads firms to give more weight to the information provided when deciding on their planned price setting. At the same time, the expectation of increased energy and labor costs results in higher average planned price levels (higher intercepts) in the energy and labor cost treatments, as input costs relevant for firms' price setting are explicitly stressed to them. Our observation that there are no notable differences in average treatment effects among our three treatments can be attributed to these two offsetting effects. That is, stressing input

[40] The primary purpose of our experiment is to examine the planned price response to inflation information, rather than the effect of treatment-induced updates in expectations on prices. Therefore, we do not survey post-treatment expectations, but elicit planned prices immediately after the treatment to ensure that participants take the provided information into account when reporting their price plans (see Section 3.3 for more discussion of this design choice).

cost factors reduces the weight firms place on pre-treatment inflation expectations but increases inflation-independent planned prices, potentially due to increased awareness of cost inflation dynamics.

Related literature. We contribute to existing work along several dimensions. First, we contribute to the literature assessing the role of information as a suitable policy tool for central banks striving for price stability. The importance of communication strategies to dampen overall uncertainty with regard to economic and monetary policy has risen since the 1990s (Blinder et al., 2008). Nevertheless, empirical evidence on the success of communication strategies related to inflation rates to affect firm decisions is still scarce (Coibion et al., 2020a). Testing this channel, we find that central bank communication can be a successful tool for dampening the transmission of biased high-inflation expectations to higher prices. Thereby, central banks can control and curb inflation by breaking expectations-price spirals of price-setters. This is particularly relevant when traditional instruments such as interest rate changes are costly and take time to materialize in the economy.

Second, we are the first to explicitly test how providing inflation forecast information affects firms' pricing strategies in a *high-inflation environment*. Prior studies in the literature are conducted in *low-inflation environments*, which may be the reason that they document only relatively small (Coibion et al., 2018, 2020b) or zero (Rosolia, 2024) effects. This difference in findings highlights the importance of the inflationary context in which firms operate. In low-inflation environments, the benefits of price changes due to rather small revisions in expected inflation might not outweigh price adjustment costs, which may explain the observation of zero effects (Rosolia, 2024). In contrast, in our high-inflation setting, where information on price dynamics is highly relevant, provision of inflation information translates into higher price adjustments, because the benefits of these adjustments outweigh the costs. The significance of the inflationary context regarding the magnitude of treatment effects is supported by literature on households and firms (Weber et al., 2025).

Third, whereas previous firm surveys test the impact of *central bank inflation targets* or most recent *annual realized inflation* on inflation expectations (Coibion et al., 2018, 2020b; Savignac et al., 2024; Hunziker et al., 2022; Huber et al., 2023), we focus on *central bank inflation forecasts*, which previous research has shown to be useful in affecting household expectations (Coibion et al., 2022; Dräger et al., 2024) and, perhaps more importantly, provide forward-looking information that may be particularly relevant for firms' decisions, as these often rely on expectations about future economic conditions rather than on past outcomes. We test the relevance of central bank inflation forecasts for firms' price setting in times of high uncertainty about future price developments, an environment in which inflation forecasts could become an even more important factor in firms' decision-making processes. Moreover, our results for an advanced economy (Germany) add to the findings for developing countries with persistently high inflation. Using an information provision experiment with firms in Uruguay, Caruso-Bloeck et al. (2023) find that firms adjust their inflation and GDP growth expectations but observe no effect on price changes when treating firms with expected disinflation projections due to a new monetary policy regime.

Fourth, we add another layer of information to our experiment, incorporating components of the overall central bank inflation forecasts that are relevant to firms' input cost developments – i.e., energy and wage costs. This allows us to make inferences about how information about input cost developments affects firms' planned price setting, thereby addressing a gap in the existing literature (Weber et al., 2022).

Fifth, an advantage of our setting is that our sample is not restricted to certain industries or larger firms, but includes firms from a wide range of industries and of different sizes. Furthermore, we show that the firms we survey are, on average, relatively well-informed about past inflation dynamics ex ante, which works against us finding an effect of our information treatment. The fact that we still find a treatment effect suggests that the effects of similar information policies could be even larger in settings where firms are less well informed (Weber

et al., 2025).[41]

Finally, on a broader level, we add to the literature studying the effects of aggregate-level variables on firm-level decisions and to the literature on managerial inattention. The fast-growing literature that studies the effects of aggregate-level variables on firm-level decisions shows that macroeconomic conditions explain variation in managers' decisions (Ball et al., 2009; Bonsall IV et al., 2013; Binz, 2022). In addition, firms' profitability and investments are influenced by monetary policy and macroeconomic announcements (Binz et al., 2022a, 2022b). We contribute to this stream of literature by providing causal evidence that (inflation) forecasts by monetary authorities directly influence managers' (pricing) plans. Moreover, literature on managerial inattention posits that managers, as all economic agents, have limited capacities (Ocasio, 1997; Sims, 2003; Dessein et al., 2016; Dessein & Santos, 2021). Ample empirical evidence shows that managerial capabilities explain the quality of managerial decisions and, eventually, firms' performance (Helfat & Martin, 2015). We show that a substantial portion of managers are inattentive to inflation dynamics, and that more attentive firms are influenced in their pricing plans to a lower extent when receiving central bank inflation forecasts.

Chapter 3 proceeds as follows. Section 3.2 describes the data. Section 3.3 describes the experimental setup. Section 3.4 provides descriptive information on firms' beliefs on past and future inflation, and their pricing plans. Section 3.5 presents the main results of our analyses, while Section 3.6 provides evidence on heterogeneous treatment effects. Section 3.7 dives deeper into the mechanism underlying our main results. Finally, Section 3.8 discusses the policy implications that emerge from our results.

[41] Previous survey studies include Coibion et al. (2020a), Candia et al. (2024), Weber et al. (2022), Savignac et al. (2024), and Link et al. (2023). Coibion et al. (2020a) provide an overview. The overall results indicate that the inflation environment affects how well households and firms are informed about recent inflation developments.

3.2 Data

Our analysis rests on survey data collected by the GBP between July 26, 2022, and November 2, 2022. Bischof et al. (2024) provide a detailed description of the GBP. Contact information of firms was obtained from the BvD Orbis database and using web scraping techniques. The sample of firms that participated in our survey was drawn randomly from the overall address pool and invited to participate in our online survey via email. The response rate of the survey was 2.1%[42], and a total of 1,944 respondents completed the questionnaire during the field phase.

The survey collects data on firm characteristics, including firm revenues, the number of employees, their industrial sector, and their legal form. Moreover, respondent characteristics like gender, education and position in the company are collected. Our set of surveyed firms is largely representative of the underlying population of German firms in terms of industry sector, and slightly larger with regard to the number of employees and revenues (see Table B.5 in Appendix B.2). Approximately 87% of survey respondents are the owner or CEO of the corresponding firm. The majority of firms in our sample have fewer than 50 employees (94%) and less than 10 million € in revenues (93%). With regard to industry composition, firms mainly come from the manufacturing and trade sector (28%). In the Appendix, we offer comprehensive information on the variable definitions and survey questions (Appendix B.1), along with detailed summary statistics on both firm and manager characteristics of the participating firms (Appendix B.2).

3.3 Experimental Setup

For the survey experiment, we assign respondents randomly to three treatment groups that receive information on the German central bank's inflation assessment and a control group which does not receive central bank information. The information underlying the three

[42] Defined as the number of firms that started the survey (N = 4,710) divided by the number of firms that opened the invitation email (N = 225,477).

treatments was retrieved from the June 2022 report of the German central bank (Deutsche Bundesbank, 2022). The German central bank did not update these forecasts during our period of data collection.[43]

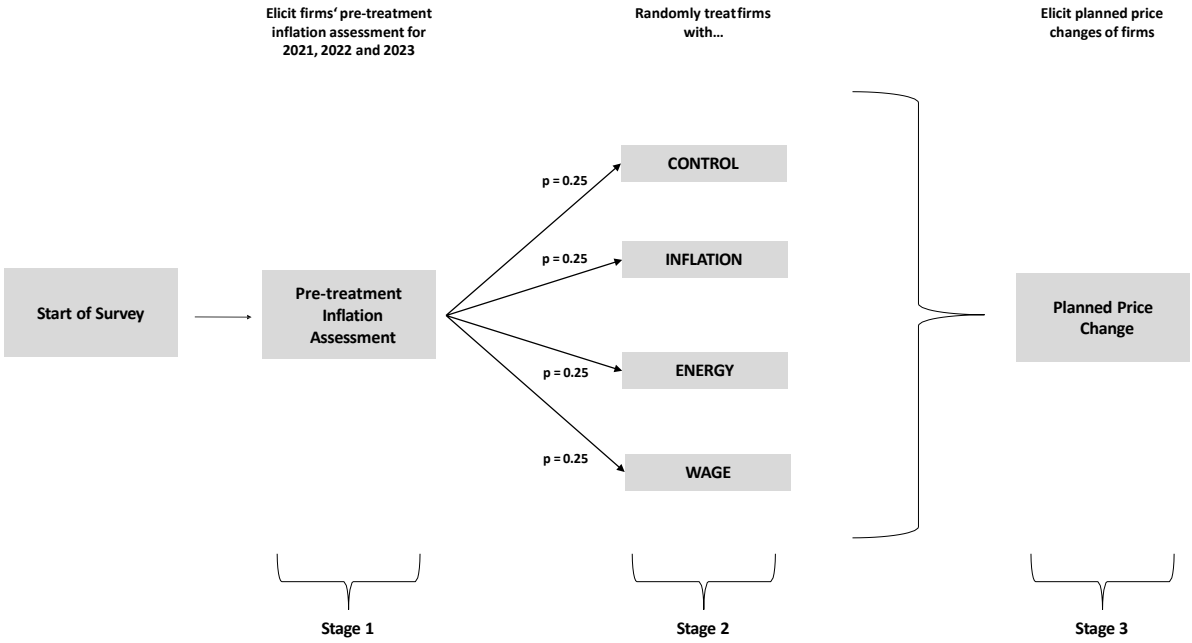
Figure 3.1 presents an overview of the survey flow. At the start of the survey, all participants are asked to inform us about their inflation assessment for the year 2021 (realized at the time of the survey), and their inflation expectations for the years 2022 (current) and 2023 (future). This allows us to measure beliefs prior to providing participants with additional information. This practice is in line with suggestions on the design of information provision experiments by Haaland et al. (2023). We then apply our information treatments.[44]

First, around one-quarter of survey participants receive our baseline **INFLATION treatment**. Firms in this group see their own inflation assessment from the previous question vis-à-vis the German central bank's inflation estimates for the three years (2021, 2022, 2023). The reported central bank estimates are 3.2% (2021), 7.1% (2022), and 4.5% (2023). Second, another quarter of participants receive the **ENERGY treatment**. In addition to the information set provided in the INFLATION treatment, firms receive information on the central bank forecasts of energy prices for 2021 to 2023. These central bank estimates for energy price changes are 10.1% (2021), 27.2% (2022), and 8.5% (2023). A third group receives the **WAGE treatment**. This information treatment is very similar in structure to the previous ENERGY treatment. However, instead of energy prices, firms receive central bank estimates on the development of wages (in addition to the information provided in group INFLATION). These estimates are 3.5% (2021), 4.3% (2022), and 4.5% (2023). Finally, a **CONTROL group** is provided with an overview of their own inflation estimates originat-

[43] We use inflation forecasts from the German Bundesbank because inflation data for Germany is likely more relevant for our sample of German firm decision-makers. Additionally, the German Bundesbank is regarded as a trusted institution by the German public due to its well-known focus on price stability (Ehrmann & Tzamourani, 2012; Hayo & Neuenkirch, 2014). However, we acknowledge that the European Central Bank (ECB), not the Bundesbank, is the institution responsible for setting the monetary policy strategy for the Eurozone. Nevertheless, the Bundesbank holds a seat on the ECB's governing council, which allows it to influence European Union (EU) monetary policy and assess its implications for future inflation in Germany.

[44] Translated experimental treatments can be found in Figure B.1 in Appendix B.1.

Figure 3.1: Experimental Design



Note: Figure 3.1 presents the design of our survey experiment.

ing from the survey question in Stage 1 (see Figure 3.1). Balancing tests (Appendix B.2) show that randomization worked well: inflation expectations, as well as firm and respondent characteristics, are balanced across groups.

Our experimental design has several features worth emphasizing. First, we ensure that CONTROL group firms are as exposed to the topic of inflation as treatment firms by presenting them with the same number of survey steps on inflation (rather than skipping the treatment screen for the CONTROL group). We accomplish this by explicitly treating firms in the CONTROL group with their pre-treatment assessment of inflation. Therefore, any effect observed on planned prices in the CONTROL group can be interpreted as the result of reminding firms about their inflation forecast.

Second, between-subject designs like ours typically have no natural anchor and, therefore, results inherently contain substantial noise. This is particularly the case with forecasts. We reduce this noise by asking for the 2021 inflation rate, which was realized at the time of the

survey. This provides a natural anchor and allows within-subject comparison of realized and expected inflation.

Third, note that our survey was designed to analyze the planned price response to our experimental treatments. Therefore, to ensure participants incorporate the provided information into their pricing plans, we collect planned prices immediately after the treatments. Although we assess inflation expectations before the treatment, we do not reassess them post-treatment, preventing us from linking updated expectations to prices for our treatment groups. We chose this strategy because eliciting pre- and post-treatment inflation expectations requires asking the same question twice and thus entails problems related to consistency bias, ordering, over-sensitivity to context, and experimenter demand (Haaland et al., 2023). Moreover, the alternative of using a different question design to elicit post-treatment inflation expectations can lead to different answers solely due to the difference in question-wording or design (Weber et al., 2025; Pavlova, 2025). Additionally, in settings where outcomes of interest are firm-level employment or investment, it is easier to elicit both the outcome of interest and inflation expectations before and after an information treatment using slightly similar question wordings. However, the problem of asking a similar question multiple times becomes more severe in our setting, where the outcome of interest is firm-level prices, since inflation and price-levels are closely related concepts. Thus, eliciting price plans and inflation expectations before and after the information treatment would mean asking a similar question *four times*, which we try to avoid in our survey design. However, to explore pass-through effects from inflation expectations to prices, we analyze the relationship between inflation expectations and prices of firms in the CONTROL group (only treated with their own forecasts) and investigate how their inflation expectations influence pricing plans.

Finally, our setup combines three levels of information additions: participants' own estimates (CONTROL), overall inflation forecasts (INFLATION), and key components of the inflation forecast that are relevant to firms' input costs (ENERGY, WAGE). This design allows us to estimate the incremental effect of each piece of information. We expect that

firms revise their expectations and plans to a stronger degree when receiving more information. A second dimension is the kind of information. ENERGY and WAGE treatments have distinctly different properties. Energy prices are highly volatile key drivers of the current inflation rates and expectations (Wehrhöfer, 2023). They may decrease in the future as quickly as they have increased before, which is why they rather affect firms' short-term planning. Labor costs, in contrast, are predicted to increase at a much lower rate, but are relatively stable and relevant for firms' long-term decisions. In sum, both the ENERGY and WAGE treatments contain information on input cost expectations that relate more directly to firms' price setting decisions compared to the INFLATION treatment.

3.4 Pre-treatment Beliefs on Inflation Expectations

As a first step, we study how well-informed firms are about realized inflation in 2021. We find that they are surprisingly well-informed. Figure 3.2a shows that 75% of respondents indicate inflation rates (measured before treatment) for 2021 that are within a 2-percentage-point range of the central bank's reported 3.2%. Firms in our high-inflation environment seem to be better informed about inflation dynamics compared to previous studies in low-inflation environments, presumably because higher inflation makes the topic more salient and increases the benefit of being informed.[45] Still, on average, firms slightly overestimate inflation by around 1.5 percentage points (mean: 4.7%), consistent with previous findings that firms tend to overestimate inflation (Weber et al., 2022).

When assessing the current (2022) and future (2023) inflation rates, the distribution becomes wider and deviates more from the German central bank's forecasts. For 2022, firms are around 3 percentage points above the central bank's forecast of 7.1% (mean: 10.5%) with only 50% of firms indicating a value within the 2-percentage-point distance (see Figure 3.2b).

[45] Coibion et al. (2018) report a share of only 49% when inflation rates were relatively low. For Germany, Link et al. (2023) find that firms are better informed about macroeconomic indicators (e.g. inflation) than households. Cavallo et al. (2017) show that the environment matters, as households in high-inflation environments (e.g., Argentina) are better informed about inflation than households in low-inflation environments (e.g., US).

Moreover, 81% of the firms in our sample have higher inflation expectations for 2022 than the central bank. For 2023, Figure 3.2c reveals that only 23% of respondents are somewhat close to the central bank's forecast of 4.5%. The mean firm expects inflation to be almost 7 percentage points higher (mean: 11.3%). Overall, 94% of our participants report inflation expectations that are higher than the central bank's forecast. Thus, our results indicate that firms' inflation expectations appear to be well above the central bank's inflation target of 2% in our high-inflation environment.[46] This is in line with results for households and firms in Germany (Coleman & Nautz, 2023; Wehrhöfer, 2023).

Finally, Figure 3.2d shows the distribution of planned price changes for firms in the CONTROL group. Firms in the CONTROL group are not influenced by additional information on the inflation assessment from the central bank, as we only remind them of their own inflation assessment. On average, these firms plan to increase prices by 15.4% in the next 12 months. Approximately 90% of firms plan to increase prices, and less than one percent plan price reductions.

3.5 Main Experimental Effects on Planned Price Changes

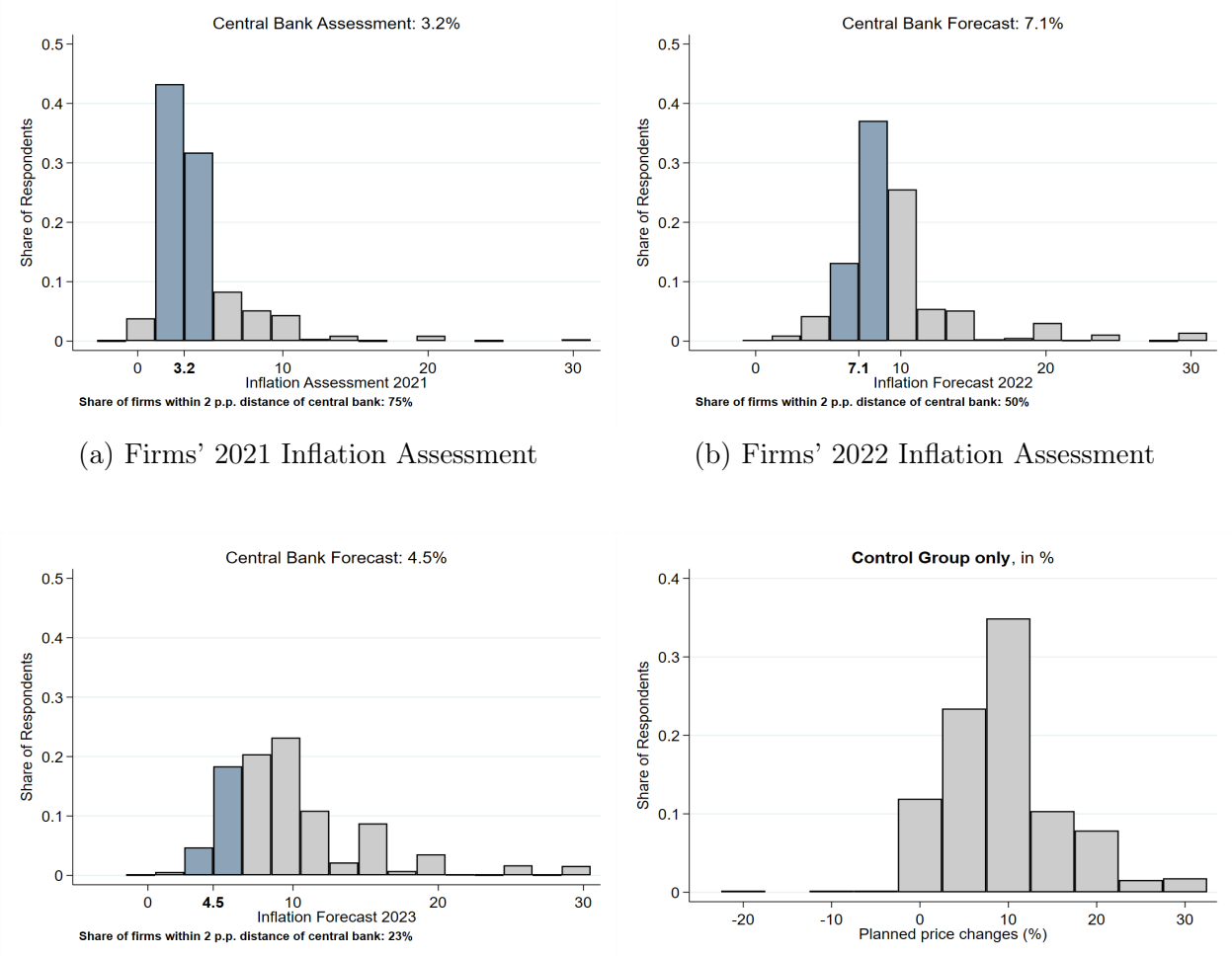
Next, we investigate how the information treatments affect firms' price-setting plans. The scope for change in beliefs is large, as the majority of firms (94%) have higher inflation forecasts for 2023 compared to the central bank's prediction. We hypothesize that in response to the information provision, firms will adjust their pricing plans, on average, downward. To test this hypothesis, we estimate the following regression model using OLS:

$$\Delta Price_{i+12m} = \alpha_0 + \alpha_1 \times INFLATION_i + \alpha_2 \times ENERGY_i + \alpha_3 \times WAGE_i + X_i'\gamma + \varepsilon_i. \quad (3)$$

The dependent variable $\Delta Price_{i+12m}$ represents the planned change of firm i's main

[46] The inflation expectations reported by firms in our survey broadly align with those elicited by the German central bank through its *Unternehmensstudie/Bundesbank-Online-Panel-Firmen (BOP-F)* during the same field phase period (Deutsche Bundesbank, 2025).

Figure 3.2: Firms' Inflation Assessment and Price-Setting Plans



(a) Firms' 2021 Inflation Assessment

(b) Firms' 2022 Inflation Assessment

(c) Firms' 2023 Inflation Assessment

(d) Planned Price Changes in Next 12 Months

Note: Figure 3.2a, Figure 3.2b and Figure 3.2c present histograms of firms' inflation assessments for 2021 (N = 1,872), 2022 (N = 1,898) and 2023 (N = 1,883). Horizontal axis: indicated inflation rate (question: *"How high do you estimate the inflation rate for 2021/2022/2023?"*). Vertical axis: share of survey respondents. Blue bars: answers in range of 2 percentage points distance to German central bank's inflation assessment (2021: 3.2%; 2022: 7.1%; 2023: 4.5%). Figure 3.2d shows surveyed firms' indicated price changes for the next 12 months. Horizontal axis: indicated price change (question: *"Compared to today, how do you plan to adjust the selling price of your main product or service in the next 12 months (in %)?"*) Vertical axis: share of survey respondents. Control group only (N = 444).

product's or service's price in the next 12 months. The binary variables $INFLATION_i$, $ENERGY_i$ and $WAGE_i$ take the value of one, if firm i was allocated to the INFLATION, ENERGY or WAGE treatment, respectively, and zero otherwise. α_0 represents the expected price change in the CONTROL group. α_1, α_2 and α_3 measure the incremental effect of the

INFLATION, ENERGY and WAGE treatments, respectively, relative to the CONTROL group.

X_i is a vector of control variables which we include in some specifications to enhance precision. It includes manager controls, firm controls, and time controls. Manager controls are the respondent's gender, education (no training, apprenticeship & other, master (crafts, technicians), University Degree or PhD), and the respondent's position in the company (owner/CEO, department head, other). Firm controls include the size group of the firm (micro-enterprise, small company, medium-sized company, large company)[47], the legal form of the firm (sole proprietor, partnerships, corporation, other), and the industry (NACE Revision 2 industry sections). As the survey is conducted on an ongoing basis, we also include the survey week into the vector of control variables. Descriptive statistics for the control variables can be found in Appendix B.2. The regression analysis employs OLS, and standard errors are clustered at the industry and survey-week level.

Results are summarized in Table 3.1. Column (1) presents the baseline experimental effects without conditioning on any control variable. Firms in the CONTROL group plan to increase prices of their main product or service by 15.4% in the 12 months ahead. Compared to the CONTROL group, firms that receive central bank forecasts in the INFLATION treatment plan to increase prices by 3.4 percentage points less, leading to a price increase of just 12%. This difference of 22% implies a strong economic effect of our treatment. Furthermore, we find reduced price changes of similar magnitude when providing firms additionally with energy price and labor cost developments as predicted by the central bank. Firms receiving the ENERGY (WAGE) treatment plan to increase prices by 19% (22%) less than firms in the CONTROL group.[48] All results are robust to the inclusion of control variables, as shown in column (2).[49]

[47] Classification is in line with the European Commission's definition for small and medium-sized enterprises (SMEs).

[48] We do not find a significant difference between the experimental groups INFLATION, ENERGY, and WAGE. P-values from the respective t-tests for specification (1) in Table 3.1 are: 0.775 (INFLATION vs. ENERGY), 0.955 (INFLATION vs. WAGE), and 0.665 (ENERGY vs. WAGE).

[49] Our findings remain robust when limiting the sample to firms with non-negative inflation expectations, and

Table 3.1: Experimental Groups and Planned Price Changes

Sample:	**All**		**Low Prior**		**High Prior**	
Dependent Variable: $\Delta Price_{i+12m}$	(1)	(2)	(3)	(4)	(5)	(6)
INFLATION	-3.380**	-3.257*	-1.205	-1.748	-3.760**	-3.605**
	(1.421)	(1.701)	(2.762)	(3.593)	(1.710)	(1.677)
ENERGY	-2.973***	-2.693***	-2.392	-1.677	-3.265**	-3.203**
	(0.644)	(0.652)	(2.002)	(2.481)	(1.365)	(1.384)
WAGE	-3.313**	-3.326**	-1.909	-1.580	-3.894*	-3.848*
	(1.351)	(1.464)	(3.018)	(3.044)	(2.019)	(2.096)
Constant (Baseline CONTROL)	15.368***	15.268***	9.986***	9.910***	17.098***	17.031***
	(1.388)	(0.689)	(2.763)	(1.599)	(1.824)	(0.891)
Controls	No	Yes	No	Yes	No	Yes
N	1912	1912	449	447	1411	1411
R^2	0.004	0.051	0.002	0.160	0.004	0.058

Note: OLS estimates from the regression of firms' planned price change in the next 12 months on experimental group dummies: $\Delta Price_{i+12m} = \alpha + \beta_1 \times INFLATION_i + \beta_2 \times ENERGY_i + \beta_3 \times WAGE_i + X_i'\gamma + \varepsilon_i$. Columns (1) and (2) include all observations. Columns (3) and (4) include only firms with forecasts of inflation for 2023 $\leq$ 6.5% (i.e., 2 p.p. above central bank forecast and lower). Columns (5) and (6) include only firms with forecasts of inflation for 2023 $>$ 6.5%. Controls as indicated in each column. Controls include firm controls (size groups, legal forms and 1-digit industries (WZ08 classification)), manager controls (education, position in the firm and the gender of the decision-maker) and week fixed effects. Standard errors clustered on industry and survey-week level. Due to two singleton observations in the missing category for firm size and legal form, column (4) reports fewer observations than column (3), as these cases were dropped in the estimation with controls. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Overall, two main insights emerge. First, information about current and forecasted future inflation rates matters for firms' price setting plans. Providing firms with the inflation assessment of the central bank reduces planned prices of firms in a statistically and economically meaningful way, suggesting that firms update their price-setting plans towards the inflation forecasts of the central bank. Thus, adequate communication policies towards firms can be an effective additional instrument for monetary policy-makers to better control firms' price setting, and thereby inflation in the economy as a whole. Second, providing firms with additional information on energy price and wage developments does not lead to substantial differences in planned price-setting behavior as compared to providing information on

excluding firms with exceptionally high inflation expectations (>30%). Results can be found in Table B.6 in Appendix B.3.

inflation alone.

3.6 Heterogeneity in Treatment Effects

3.6.1 Divergence between Treatment Information and Expectations

A major challenge in information experiments is distinguishing the effects of priming from actual belief updating (Haaland et al., 2023). The observation of stronger treatment effects among respondents whose priors are less aligned with the information treatment is frequently interpreted as evidence of an actual change in beliefs (Armantier et al., 2016; Haaland et al., 2023). Therefore, we first investigate heterogeneity with respect to treatment intensity, which depends on the divergence between firms' pre-treatment expectations and the central bank's inflation forecast for 2023 (4.5%). Following Coibion et al. (2018), we define firms to be close to the central bank's forecast if they deviate at most 2 percentage points upwards (low prior, n=449). Otherwise, firms are categorized as having a high prior (n=1,411). We estimate equation (3) separately for both groups. Results are displayed in columns (3) through (6) of Table 3.1.

We find that firms with a high prior show a larger reaction to the treatments compared to firms with a low prior, which exhibit no significant treatment effect. Note that firms in the CONTROL group with lower prior inflation expectations also plan lower price increases of about 10%, as compared to firms with high prior expectations, who plan to increase prices by about 17%. Given this difference, the relative effect sizes for the treatment groups compared to the CONTROL group are rather similar (e.g., 17% for low-prior firms vs. 22% for high-prior firms in the INFLATION treatment), even though the absolute effect sizes for firms with a high prior are larger than those for firms with low prior inflation expectations.

This finding suggests that firms with larger deviations from the information given in the treatment also adjust their planned prices downwards to a larger extent, on average, when compared to control group firms. We interpret this as an indication that the observed treatment effects are not due to priming but rather to actual updating of participants' beliefs.

3.6.2 Inattention to Inflation Dynamics

Next, we explore how inattention to inflation dynamics affects firms' price planning strategies and the effectiveness of our experimental interventions. A feature of our survey is that, pre-treatment, we ask firms not only about their expectations for future inflation, but also about their perception of recently *realized* inflation rates in 2021. Therefore, we can control for firms' general (in)attention to inflation dynamics and compare the differential impact of our information treatments on price-setting plans of attentive and inattentive firms.[50]

The idea and our implementation closely follow Coibion et al. (2018). They show that firms in New Zealand, which were initially uninformed about the inflation target of the Reserve Bank of New Zealand, revised their employment and investment decisions significantly when provided with information on the inflation target, compared to firms that did not receive any information, but they could not find a revision in firms' price setting.[51] Similar to Coibion et al. (2018), we find in Figure 3.2a that there is a portion of firms that is not well-informed about past inflation dynamics. We conjecture that well-informed firms will exhibit less adjustment in their pricing plans upon receiving our information treatment, compared to those with relatively limited knowledge. To explore this, we estimate the following equation:

[50] In our analysis, we concentrate on *central bank inflation forecasts*. We decide to focus on *central bank inflation forecasts* instead of *realized inflation* for two reasons. First, previous research, such as Coibion et al. (2022), indicates that using the *central bank's inflation forecasts* is particularly effective in influencing household expectations. However, this approach has not been extensively explored in firm surveys. Second, in periods of heightened uncertainty about future price developments, central bank inflation forecasts should become more relevant for firms' decision-making processes than past inflation dynamics. Nevertheless, given that we also assess firms' attentiveness to realized inflation in 2021, our results in Section 3.6.2 can be compared with findings from previous firm surveys. These surveys examine the effect of information on *central bank inflation targets* or the most *recent annual realized inflation* on inflation expectations and firm decision-making (Coibion et al., 2018, 2020b; Savignac et al., 2024; Hunziker et al., 2022).

[51] Similar to an *inflation target*, the *inflation rate in 2021* is a realized number at the time the survey was conducted. Although we agree that perceptions of current and future inflation can depend on the specific environment firms are in, testing knowledge about a specific value already realized at the time of the survey should reasonably capture the concept of attentiveness to inflation dynamics.

$$\Delta Price_{i+12m} = \alpha + \beta \times Inattention2021_i + \sum_{k=1}^{3} \delta_k TREATMENT_{ik} + \sum_{k=1}^{3} \theta_k Inattention2021_i \times TREATMENT_{ik} + X_i'\gamma + \varepsilon_i. \quad (4)$$

We define $Inattention2021_i$ as the absolute difference between firms' perceived inflation for 2021 and the realized inflation rate published by the central bank (3.2%). To explore the impact of inattention on future pricing decisions, we include $Inattention2021_i$ both as a standalone variable and in interaction with our three treatment groups. The coefficient estimate, denoted as β, allows us to quantify the effect of higher levels of inattention on pricing decisions in the CONTROL group, when firms are only reminded about their own assessment.

In Table 3.2, column (1), we observe that firms in the CONTROL group who possess perfect knowledge of realized inflation ($Inattention2021_i = 0$) have an average planned price adjustment of 11.1%. For each one percentage point deviation in inflation assessment from the realized inflation rate in 2021, firms, on average, increase their prices by an additional 2.1 percentage points. Next, the coefficients θ_k can be interpreted as the incremental effect of a one percentage point larger inattention on the effectiveness of our treatments, while the coefficients δ_k show the treatment effects on perfectly informed firms in the respective experimental group. Results presented in column (1) of Table 3.2 indicate that the information treatments do not have a statistically significant effect on firms that are well-informed. However, for firms that are more inattentive to inflation rates in 2021, the treatments are more effective. For firms receiving the INFLATION treatment and deviating by one percentage point in their past inflation assessment from the realized inflation rate, their planned price increases are 0.9 percentage points lower compared to their equally uninformed peers in the CONTROL group. The corresponding values for the ENERGY and WAGE treatments are 1.4 percentage points and 1.9 percentage points, respectively.

Table 3.2: Inflation Inattention (Absolute Difference) and Planned Price Changes

Dependent Variable: $\Delta Price_{i+12m}$	(1)	(2)
INFLATION	-1.963	-2.266
	(1.814)	(1.947)
ENERGY	-0.226	-0.427
	(1.677)	(1.557)
WAGE	0.486	0.142
	(2.035)	(1.875)
Inattention 2021	2.104***	1.966***
	(0.515)	(0.520)
INFLATION × Inattention 2021	-0.891*	-0.717
	(0.468)	(0.467)
ENERGY × Inattention 2021	-1.403**	-1.192*
	(0.649)	(0.641)
WAGE × Inattention 2021	-1.902***	-1.786***
	(0.545)	(0.544)
Constant	11.13***	11.37***
	(1.398)	(1.237)
Controls	No	Yes
N	1848	1848
R^2	0.056	0.103

Note: OLS estimates from equation (4): $\Delta Price_{i+12m} = \alpha + \beta \times Inattention2022_i + \sum_{k=1}^{3} \delta_k TREATMENT_{ik} + \sum_{k=1}^{3} \theta_k Inattention2022_i \times TREATMENT_{ik} + X_i'\gamma + \varepsilon_i$. Dependent variable: planned price change in the next 12 months. Independent variables: experimental group dummies, the absolute difference between firms' perceived inflation for 2021 and actual inflation in 2021 (i.e., inattention), a constant, and controls. Controls include firm controls (size groups, legal forms and 1-digit industries (WZ08 classification)), manager controls (education, position in the firm and the gender of the decision-maker) and week fixed effects. Standard errors clustered on industry and survey-week level. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

In sum, this indicates that our information treatments are more effective for less informed firms. Additionally, we find that providing more detailed information exerts a greater impact than less detailed information when considering uninformed firms. Our findings in a high-inflation environment complement previous results from Coibion et al. (2018) in a

low-inflation environment. They report a revision effect for inattentive firms regarding employment and investment decisions, but not for prices, when provided with information on the inflation target of the central bank. Our findings on firms' price plans underscore the relevance of information about inflation dynamics for price-setting behavior in high-inflation environments, where the expected benefits of price adjustments appear to exceed their associated costs.

3.6.3 Importance of Input Costs

Little research exists on how firms' inflation expectations interplay with the relationship between their input costs and their price setting (Weber et al., 2022). A feature of our survey is that we have different information treatments, two of which include the central bank's projected developments of energy costs (ENERGY) and labor costs (WAGE) on top of the central bank's inflation forecasts. In addition, we ask firms about the most important factors for their price setting. The two most frequently selected answers to this question are *energy/material costs* and *labor costs*. Nearly half of our participants (46%) indicate both of these factors to be important.[52]

Using this information, we investigate whether firms, for which labor and energy costs are important, react differently to our information treatments. Thereby, we are particularly interested in the difference between the INFLATION treatment on the one side and the ENERGY and WAGE treatments on the other side. We re-estimate equation (3) for the two different groups. Results are displayed in Table 3.3.

Columns (1) and (2) replicate the information from Table 3.1 for comparability. Recall that all three information treatments have a similar average effect on firms' planned price setting. This changes when we observe only firms which indicate energy and labor costs to be important pricing factors in Columns (3) and (4). For these firms, we find significant treat-

[52] In Appendix B.2, we show that the relevance of these cost factors holds true across all experimental groups, indicating that energy/material costs and labor costs are consistently regarded as among the most crucial factors in the price-setting decision of firms.

Table 3.3: Importance of Input Factors and Treatment Effects

			Labor & Energy important for pricing			
Sample:	**All**		**Yes**		**No**	
Dependent Variable: $\Delta Price_{i+12m}$	(1)	(2)	(3)	(4)	(5)	(6)
INFLATION	-3.380**	-3.257*	-3.084**	-2.929**	-3.573	-3.450
	(1.421)	(1.701)	(1.128)	(1.166)	(2.713)	(3.127)
ENERGY	-2.973***	-2.693***	0.069	-0.583	-5.236***	-5.077**
	(0.644)	(0.652)	(0.848)	(1.410)	(1.659)	(1.796)
WAGE	-3.313**	-3.326**	-2.175	-2.809	-4.331*	-4.349*
	(1.351)	(1.464)	(2.292)	(1.884)	(2.382)	(2.455)
Constant (Baseline CONTROL)	15.368***	15.268***	16.407***	16.687***	14.445***	14.374***
	(1.388)	(0.689)	(1.531)	(0.560)	(1.879)	(1.532)
Controls	No	Yes	No	Yes	No	Yes
N	1912	1912	877	877	1035	1035
R^2	0.004	0.051	0.003	0.071	0.008	0.073

Note: OLS estimates from the regression of firms' planned price change in the next 12 months on experimental group dummies: $\Delta Price_{i+12m} = \alpha + \beta_1 \times INFLATION_i + \beta_2 \times ENERGY_i + \beta_3 \times WAGE_i + X_i'\gamma + \varepsilon_i$. Columns (1) and (2) include all observations. Sample split in columns (3) through (6) is based on the question *"Which factors have the greatest influence on pricing in your company?"*. Columns (3) and (4) include only firms that indicated *labor costs* and *energy/material costs*. Columns (5) and (6) include only firms that did not indicate *labor costs* or *energy/material costs*. Controls as indicated in each column. Controls include firm controls (size groups, legal forms and 1-digit industries (WZ08 classification)), manager controls (education, position in the firm and the gender of the decision-maker) and week fixed effects. Standard errors clustered on industry and survey-week level. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

ment effects of comparable size for the INFLATION treatment, but not for the ENERGY and WAGE treatment. In other words, informing these firms about central bank inflation forecasts alone (INFLATION) makes them adjust their price plans, on average, downwards. However, when they are additionally informed about expected increases in input costs that are particularly relevant to them (ENERGY, WAGE), they no longer adjust their prices significantly. That is, by stressing energy or labor costs, we potentially draw attention to increases in their cost burden, thereby mitigating the downward shift of providing information on inflation forecasts.

Turning to columns (5) and (6) of Table 3.3, we observe an inverted pattern, when considering only firms for which energy and labor costs are *not* similarly important. Here, the

additional information provided in the ENERGY and WAGE treatments seem to amplify the treatment effect of central bank inflation forecasts on pricing plans. While firms in the INFLATION group reduce their price plans on average (insignificantly) by around 3.5 percentage points compared to CONTROL group firms, the effect is significant and on average around 5 percentage points (4 percentage points) for firms receiving the ENERGY (WAGE) treatment. That is, for firms for whom energy and labor costs are not a primary consideration in their pricing, the additional information about the developments of these costs tends to reinforce, rather than mitigate, the effect of central bank inflation forecasts. One could interpret this as follows: providing additional forecasts on inflation components lends credibility to the inflation forecast itself, as long as the components are not of first order importance to the firms receiving the information.

In sum, we interpret these findings as input costs not affecting the relationship between firms' inflation expectations and pricing plans as long as they are not explicitly mentioned (INFLATION). Confronting firms, however, directly with input cost projections in addition to central bank inflation forecasts (ENERGY, WAGE), can have differential effects on firms' price setting. Depending on whether firms deem these input costs important, the additional information can either mitigate or strengthen the impact of inflation forecasts on firms' pricing plans.

3.6.4 Satisfaction with Economic Policy

The effectiveness of central bank communication critically hinges on the credibility of the central bank regarding the public. The central bank's credibility, in turn, is enhanced with trust into the central bank and the general institutional environment (Blinder et al., 2024). In our survey, we ask firms the question *"How satisfied are you with economic policy?"* on a scale from zero (dissatisfied) to ten (satisfied). In this section, we use the answer to this question as a proxy for trust in the general institutional environment.

Descriptively, satisfaction with economic policy in our sample is quite low (mean: 2.9/10,

median: 3/10). Therefore, we define firms to have a low satisfaction when they indicate a value between zero and two as answer to the aforementioned question. To validate our proxy, we refer to prior literature. Among households, a low trust in the central bank is usually associated with less factual knowledge about central bank policies and a lower desire to be informed (Hayo & Neuenkirch, 2014; Dräger & Nghiem, 2025). Furthermore, household studies indicate that the credibility and trust in the central bank significantly influence the effectiveness of monetary policy communication (Christelis et al., 2020; Ehrmann et al., 2023). As we also measure firms' inattention with respect to past inflation dynamics (see section 3.6.2), we can assess whether firms that are rather unsatisfied with economic policy are less attentive to past inflation dynamics, which would increase the validity of our proxy. Indeed, firms with lower (higher) satisfaction estimate inflation in 2021 to be on average 5.3% (4.2%), while realized inflation in 2021 was at 3.2%. The difference between the groups is statistically significant at all conventional levels ($p<0.001$).

As in the previous section, we separately re-estimate equation (3) for firms with a higher and lower satisfaction with economic policy. We conjecture that firms that are generally more dissatisfied lend less credibility to the central bank information provided in our treatments, and therefore show weaker price plan adjustments as a reaction to our treatments. The estimation results are displayed in Table 3.4. We indeed find that firms in the low satisfaction group do, on average, not significantly adjust their pricing plans as a reaction to any of the information treatments. The remaining firms with a higher satisfaction, however, show significant average downward price plan adjustments as reactions to all information treatments. This is particularly noteworthy as firms that are more satisfied generally expect inflation to be *lower* (mean: 9.1%) and therefore have less room for updating their inflation expectations, as compared to their dissatisfied peers (mean: 13.6%).

In sum, even though we can only broadly approximate firms' institutional trust in the central bank by considering their satisfaction with economic policy, our results indicate that central bank communication is more effective when firms' institutional trust is higher,

Table 3.4: Satisfaction with Economic Policy and Treatment Effects

			Satisfaction with Economic Policy			
Sample:	**All**		**Low**		**Medium/High**	
Dependent Variable: $\Delta Price_{i+12m}$	(1)	(2)	(3)	(4)	(5)	(6)
INFLATION	-3.380**	-3.257*	-2.911	-3.415	-3.908**	-4.331*
	(1.421)	(1.701)	(1.768)	(1.987)	(1.414)	(2.327)
ENERGY	-2.973***	-2.693***	-1.450	-2.439	-4.458***	-4.370***
	(0.644)	(0.652)	(1.746)	(1.934)	(0.405)	(1.347)
WAGE	-3.313**	-3.326**	-1.697	-2.370	-4.870***	-5.037***
	(1.351)	(1.464)	(2.146)	(2.311)	(0.833)	(1.130)
Constant (Baseline CONTROL)	15.368***	15.268***	16.930***	17.481***	13.841***	13.971***
	(1.388)	(0.689)	(1.981)	(1.181)	(0.982)	(0.596)
Controls	No	Yes	No	Yes	No	Yes
N	1912	1912	951	951	958	958
R^2	0.004	0.051	0.002	0.084	0.008	0.073

Note: OLS estimates from the regression of firms' planned price change in the next 12 months on experimental group dummies: $\Delta Price_{i+12m} = \alpha + \beta_1 \times INFLATION_i + \beta_2 \times ENERGY_i + \beta_3 \times WAGE_i + X_i'\gamma + \varepsilon_i$. Columns (1) and (2) include all observations. Sample split in columns (3) through (6) is based on the question *"How satisfied are you with economic policy in Germany?"*. Columns (3) and (4) include firms indicating a low satisfaction (0-2). Columns (5) and (6) include firms that indicated an intermediate to high satisfaction (3-10). Controls as indicated in each column. Controls include firm controls (size groups, legal forms and 1-digit industries (WZ08 classification)), manager controls (education, position in the firm and the gender of the decision-maker) and week fixed effects. Standard errors clustered on industry and survey-week level. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

which should also be associated with greater satisfaction with the current economic policy environment.

3.6.5 Price-Setting Frequency

An increase in the average level of inflation should lead to an increase in the share of firms changing prices more frequently. As the price level rises, the benefits of a price change exceed the expected costs of not changing prices (Ball et al., 1988). Empirically, Cavallo et al. (2023) confirm that depending on the inflation environment, firms adjust their price-setting frequency, with more frequent price changes in higher inflation regimes. This idea contrasts with traditional price-setting models like Calvo (1983), that do not allow for state-dependent price setting and assume a constant probability for changing prices. Following

Cavallo et al. (2023), we would expect firms absent any treatment not only to increase their price level, as demonstrated in Panel (d) of Figure 3.2, but also the frequency at which they set their prices.

To explore this, we pose the following question in our survey: *Compared to past years: Do you think you will adjust the price of your main product or service more or less frequently in the next 12 months?* We offer the following options to choose from: *Much less frequently* (e.g. every 12 months in future, previously every 3 months), *Rather less frequently* (e.g. every 12 months in future, previously every 6 months), *Unchanged* (e.g. in future every 12 months, previously every 12 months), *Rather more frequently* (e.g. every 6 months in future, previously every 12 months), *Much more frequently* (e.g. every 3 months in future, previously every 12 months).

We provide some descriptive details on the answers to this question in Table 3.5. Panel A confirms that 64% of the firms in the CONTROL group indicate to increase prices more frequently in the near future. This confirms that firms do not only plan to increase price levels but also the frequency of their price setting, as conjectured. To quantify this, we also ask a subset of firms at which interval they adjusted their prices for their main product or service in the past *low-inflation environment* and in the current *high-inflation environment*. Firms in the CONTROL group indicate on average 16 months (9 months) as past (current) price adjustment frequency, so these firms somewhat less than halved the period between two price changes on average.

Interestingly, the distribution of answers is very similar for firms in the treatment groups (INFLATION, ENERGY, WAGE) when compared to the CONTROL group. Independent of the randomly assigned group, around 60% of firms indicate to increase prices more frequently. Panel B of Table 3.5 confirms the general impression that the distributions of answers between groups are not statistically different. This means that upon receiving our information treatments, firms do not adjust the price change frequency, but only the levels of planned prices (see Table 3.1).

Table 3.5: Price-Setting Frequency - Descriptives

Panel A: Price-Setting Frequency (*N*)

	CONTROL	INFLATION	ENERGY	WAGE	**Total**
Much less frequently	4	8	8	10	**30**
Rather less frequently	15	17	21	19	**72**
Unchanged	141	168	168	167	**644**
Rather more frequently	171	199	180	174	**724**
Much more frequently	111	109	109	103	**432**
Total	**442**	**501**	**486**	**473**	**1,902**

Panel B: Two-sample Kolmogorov-Smirnov test (*p*-values)

	CONTROL	INFLATION	ENERGY	WAGE
CONTROL		0.954	0.777	0.557
INFLATION			1.000	0.986
ENERGY				1.000

Note: **Panel A** of Table 3.5 presents the absolute frequency of price adjustment revisions over our different experimental groups (CONTROL, INFLATION, ENERGY, WAGE). Statistics are based on the following survey question: *"Compared to past years: Do you think you will adjust the price of your main product or service more or less frequently in the next 12 months?"* We differentiate firms with regard to the following price adjustment frequencies: *Much less frequently* (e.g. every 12 months in future, previously every 3 months), *Rather less frequently* (e.g. every 12 months in future, previously every 6 months), *Unchanged* (e.g. in future every 12 months, previously every 12 months), *Rather more frequently* (e.g. every 6 months in future, previously every 12 months), *Much more frequently* (e.g. every 3 months in future, previously every 12 months). The sample is restricted to firms which indicated a planned price adjustment. **Panel B** of Table 3.5 presents p-values of a two-sample Kolmogorov-Smirnov tests of the equality of distributions. We test if the respective distributions of Panel A are equal between the different experimental groups.

Vice versa, we note that firms who plan to increase their price-setting frequency have higher pre-treatment inflation expectations compared to their peers that do not plan to adjust price-setting frequency (12% vs. 10%, p-value of difference < 0.001). Therefore, in line with Table 3.1, these firms with a higher planned price-setting frequency (and higher prior inflation expectations) also reduce their pricing plans more strongly, which we confirm in Table 3.6. Thus, the information on current and future inflation dynamics in our treatments is especially relevant for firms which plan to increase prices more often in the future.

In sum, the evidence presented in this last subsection, even though mostly descriptive, supports the notion that price-setting frequency varies in different inflation environments

Table 3.6: Price-Setting Frequency and Treatment Effects

			Price-Setting Frequency Change			
Sample:	**All**		**Lower/Unchanged**		**Higher**	
Dependent Variable: $\Delta Price_{i+12m}$	(1)	(2)	(3)	(4)	(5)	(6)
INFLATION	-3.380**	-3.257*	-2.382***	-2.098	-3.517	-3.189
	(1.421)	(1.701)	(0.762)	(1.230)	(2.891)	(3.409)
ENERGY	-2.973***	-2.693***	0.222	0.609	-4.285**	-4.415*
	(0.644)	(0.652)	(2.257)	(2.782)	(1.792)	(2.061)
WAGE	-3.313**	-3.326**	0.141	0.346	-4.772*	-4.993*
	(1.351)	(1.464)	(1.694)	(1.988)	(2.285)	(2.477)
Constant (Baseline CONTROL)	15.368***	15.268***	9.447***	9.223***	18.624***	18.622***
	(1.388)	(0.689)	(1.043)	(1.530)	(2.376)	(1.582)
Controls	No	Yes	No	Yes	No	Yes
N	1912	1912	746	744	1156	1156
R^2	0.004	0.051	0.004	0.061	0.006	0.076

Note: OLS estimates from the regression of firms' planned price change in the next 12 months on experimental group dummies: $\Delta Price_{i+12m} = \alpha + \beta_1 \times INFLATION_i + \beta_2 \times ENERGY_i + \beta_3 \times WAGE_i + X_i'\gamma + \varepsilon_i$. Columns (1) and (2) include all observations. Sample split in columns (3) through (6) is based on the question *"Compared to past years: Do you think you will adjust the price of your main product or service more or less frequently in the next 12 months?"*. Columns (3) and (4) include firms indicating the options *Much less frequently* (e.g. every 12 months in future, previously every 3 months), *Rather less frequently* (e.g. every 12 months in future, previously every 6 months), or *Unchanged* (e.g. in future every 12 months, previously every 12 months). Columns (5) and (6) include firms indicating the options *Rather more frequently* (e.g. every 6 months in future, previously every 12 months) or *Much more frequently* (e.g. every 3 months in future, previously every 12 months). Controls as indicated in each column. Controls include firm controls (size groups, legal forms and 1-digit industries (WZ08 classification)), manager controls (education, position in the firm and the gender of the decision-maker) and week fixed effects. Standard errors clustered on industry and survey-week level. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

(*state-dependent price setting*), in line with recent empirical evidence provided by Cavallo et al. (2023).

3.7 Price-Setting Mechanism

In Section 3.5, we demonstrate that our experimental treatments effectively influenced firms' pricing strategies, leading to a general downward shift. Moreover, there were no significant effect differences, on average, between the INFLATION, ENERGY, and WAGE treatments. This section provides a detailed exploration of the underlying mechanisms behind this find-

ing. We refine equation (3) to directly include firms' pre-treatment inflation expectations for 2023 in our regression analysis. This approach allows us to examine variations in both the intercept and slope coefficients across the different treatment groups, as different signals may have different value and perceived precision, conditional on pre-treatment inflation expectations. Consequently, this can influence the relative effectiveness of each treatment.

3.7.1 CONTROL vs. INFLATION Treatment

We start by comparing the impact of supplementing firms' own forecasts solely with central bank predictions, as is done in the experimental group INFLATION. This involves comparing firms in the CONTROL group with firms in the INFLATION group. To this end, we estimate the following equation using OLS, solely based on firms from these two groups:

$$\begin{aligned}\Delta Price_{i+12m} = \alpha + \beta \times E_{2022}Inflation_{i2023} + \delta \times INFLATION_i \\ + \theta \times (INFLATION_i \times E_{2022}Inflation_{i2023}) + X_i'\gamma + \varepsilon_i. \end{aligned} \tag{5}$$

Like in equation (3), $\Delta Price_{i+12m}$ indicates the planned change of firm i's main product's or service's price in the next 12 months. Further, $E_{2022}Inflation_{i2023}$ indicates firm i's expectation for the inflation rate for the year 2023, as assessed in the year 2022. As 2023 was entirely in the future at the time of our survey, this variable reflects firms' inflation expectations. The binary variable $INFLATION_i$ takes the value of one, if firm i is allocated to the INFLATION treatment, and is zero otherwise. The vector X_i includes the same set of variables as described in Section 3.5. Standard errors are clustered at the industry and week level.

The coefficients can be interpreted as follows. β represents the pass-through of pre-treatment inflation expectations to the price-setting plans of firms in the CONTROL group. As we use a control group in which we remind firms about their own inflation expectations before asking about their planned price setting, firms should be strongly anchored on their

Table 3.7: Experimental Results - CONTROL vs. INFLATION

Groups included:	CONTROL + INFLATION	
Dependent Variable: $\Delta Price_{i+12m}$	(1)	(2)
Baseline: **CONTROL group**		
Infl. 2023	1.002***	1.038***
	(0.219)	(0.248)
INFLATION	-1.236	-0.919
	(2.816)	(3.086)
INFLATION × Infl. 2023	-0.243	-0.282
	(0.246)	(0.262)
Constant	4.543*	4.211
	(2.226)	(2.631)
Controls	No	Yes
N	922	922
R^2	0.125	0.179
$p(\beta) = 1$	0.994	0.882

Note: OLS estimates from the regression of equation (5): $\Delta Price_{i+12m} = \alpha + \beta \times E_{2022}Inflation_{i2023} + \delta \times INFLATION_i + \theta \times (INFLATION_i \times E_{2022}Inflation_{i2023}) + X_i'\gamma + \varepsilon_i$. Dependent variable: planned price change in the next 12 months. Independent variables: respondent's inflation forecast 2023, experimental group CONTROL and INFLATION, controls if indicated, and a constant. Baseline group: CONTROL. Controls include firm controls (size groups, legal forms and 1-digit industries (WZ08 classification)), manager controls (education, position in the firm and the gender of the decision-maker) and week fixed effects. Standard errors clustered on industry and survey-week level. Standard errors in brackets. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

pre-treatment expectations when thinking about their pricing plans. β can therefore be interpreted as an estimate of the strength with which inflation expectations translate to near-term pricing strategies of firms. As our study is conducted during a high-inflation period, we expect that the inflationary environment becomes increasingly decisive in firms' pricing decisions. Unlike in low-inflation scenarios, where minor changes in expected inflation might not justify the cost of price adjustments, our focus is on a setting where the influence of inflation expectations on firm decisions is expected to be highly relevant. In this context, we argue that the advantages of adjusting prices in response to these expectations will surpass

the associated costs. Therefore, we expect β to be positive and significantly different from zero.

Indeed, the results presented in Table 3.7 support this hypothesis. The estimated coefficient is 1.002 in specification (1), without controls, and 1.038 in specification (2), with controls. The coefficient estimates are statistically different from zero. These estimates suggest a one-to-one translation of inflation expectations on pricing plans, when firms are actively reminded of their own inflation forecasts through our *active control group design*. We conclude that inflation expectations are of great importance for the pricing strategies of firms in our high-inflation environment.

The coefficient δ represents the differential intercept for the INFLATION group compared to the CONTROL group. It captures the concept that the INFLATION treatment could shift average planned prices higher or lower relative to the CONTROL group. Crucially, δ encompasses the combined effect of firms' responsiveness to the provided information and the disparity between the central bank forecast and the firms' average pre-treatment inflation expectations. Given that the majority of firms (94%) have higher inflation forecasts for 2023 compared to the central bank's prediction for the same year, we anticipate a downward adjustment in the average planned price for the INFLATION group relative to the CONTROL group. This expectation is supported by the results presented in Table 3.7, where we observe a negative, though statistically insignificant, δ coefficient, suggesting a downward level shift of price plans of 1.236 percentage points (column (1)).

The relationship between pre-treatment inflation expectations and the planned pricing of firms is best described by the coefficient θ. This coefficient captures the change in the slope of the relationship between pre-treatment inflation expectations and planned pricing for firms in the INFLATION treatment, relative to firms in the CONTROL group. In other words, θ can be interpreted as the weight firms place on the provided forecast of the central bank compared to their initial expectations when deciding on their price setting. If the supplied information does not influence the price-setting behavior of firms, then θ will equal

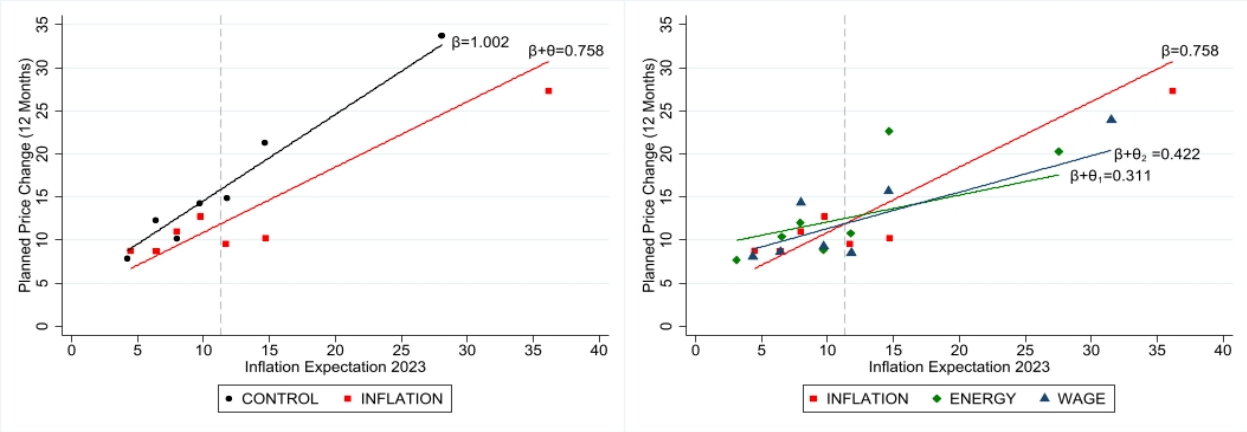
zero. This would result in the slope that connects pre-treatment expectations and pricing plans being identical to that of the CONTROL group.

A negative value of θ, however, suggests that the treatment group is attributing reduced importance to their initial inflation expectations and greater significance to the newly provided information when deciding on their planned prices. In the extreme case of $\beta + \theta = 0$, firms would put all their weight on the provided information when setting prices. The proportion of β counterbalanced by θ thus becomes the crucial measure for evaluating the shift in price setting in response to the introduction of new information. Results in Table 3.7 suggest that 24% (represented by -(θ/β)) is counterbalanced by the provided information.

Next, we connect the results in Table 3.1 and Table 3.7. Together, the coefficients δ and θ elucidate the average treatment effect (ATE) of the INFLATION treatment as observed in Table 3.1. With an average pre-treatment inflation expectation of 11.3%, the INFLATION treatment, according to specification (1) in Table 3.7, reduces the planned price by 3.98 percentage points relative to the CONTROL group. This is similar to the effect in specification (1) of Table 3.1. The slight variation in treatment effects between the tables (3.38 vs. 3.98 percentage points) is attributable to differences in the sample compositions, as specification (1) in Table 3.1 also includes firms that did not enter a value for their 2023 inflation expectation.

Finally, Panel (a) of Figure 3.3 illustrates the observed effects graphically with binned scatter plots of firms' pre-experimental inflation expectations for 2023 (x-axis) and their price-setting plans in the year ahead (y-axis). The estimated slope for the CONTROL group is 1.002 while the estimated slope for the INFLATION group is 0.758 (see Table 3.7). Further, a dashed vertical line indicates the mean inflation expectations of firms in our sample (11.3%). At this point of the x-axis, the gap between the red and the black line corresponds in absolute value to the coefficient of the INFLATION treatment (3.38) in Table 3.1.

Figure 3.3: Firms' Pre-Treatment Inflation Expectations and Price-Setting Plans



(a) CONTROL vs. INFLATION (b) INFLATION vs. ENERGY/WAGE

Note: Bin scatter plots of firms' pre-treatment inflation expectations for 2023 (x-axis) versus their post-treatment price plans in the 12 months ahead (y-axis). Panel (a) includes only CONTROL and INFLATION group, while Panel (b) contrasts INFLATION, ENERGY and WAGE treatment groups. The coefficient estimates indicate the slope of each line.

3.7.2 INFLATION vs. ENERGY/WAGE Treatment

As the average price plan reaction to our three information treatments (INFLATION, ENERGY, WAGE) is very similar (see Table 3.1), we explore the mechanisms behind this finding. In particular, we want to know whether there are differences between the groups that are not captured by the ATEs we present in Table 3.1. To this end, we modify equation (5) to include only firms from the INFLATION, ENERGY, and WAGE groups, excluding firms from the CONTROL group. In the following specification (6), we compare the three treatment groups, using INFLATION as the baseline group, as firms in the INFLATION group receive the least detailed information among the three treatment groups:

$$\begin{aligned}\Delta Price_{i+12m} = \alpha + \beta \times E_{2022}Inflation_{i2023} + \delta_1 \times ENERGY_i + \delta_2 \times WAGE_i \\ + \theta_1 \times (ENERGY_i \times E_{2022}Inflation_{i2023}) \\ + \theta_2 \times (WAGE_i \times E_{2022}Inflation_{i2023}) + X_i'\gamma + \varepsilon_i. \end{aligned} \tag{6}$$

The idea behind equation (6) is to test for the incremental effect of treating firms with the central bank's energy and labor cost projections, in addition to the central bank inflation forecasts. This allows us to investigate whether the extent and type of inflation-related information matters for firms' price-setting behavior. Specifically, the ENERGY and WAGE treatments provide information on input cost projections that is more directly relevant to firms' pricing decisions compared to the information in the INFLATION treatment. The significance of energy/material costs and labor costs in the pricing decisions of companies is evident from the responses to one of our survey questions. The data reveal that approximately 69% of firms consider energy/material costs, while 64% take labor costs into account when making pricing decisions. In comparison, other factors such as legal regulations (26%), customer demand (25%), and competitor prices (19%) appear to have less impact during our period of study.[53]

Table 3.8 presents the results of regressions from equation (6). The following observations are noteworthy. We observe a significant upward intercept shift in the ENERGY and WAGE treatments compared to the INFLATION treatment, for both specifications with and without controls. In specification (1), firms in the ENERGY and WAGE treatments exhibit intercepts that are 5.698 percentage points (δ_1) and 3.808 percentage points (δ_2) higher, respectively. This result suggests that treating firms with inflation predictions about their most important cost factors, in addition to general price predictions, makes them more sensitive to future cost increases. This leads to an upward shift in average planned prices.

At the same time, the slope coefficients θ_1 (for the ENERGY treatment: -0.447 in column (1)) and θ_2 (for the WAGE treatment: -0.336 in column (1)) are both significantly lower compared to the INFLATION treatment. This implies a weakened relationship between pre-treatment inflation expectations and planned prices, indicating a higher weight that firms place on the provided information when firms set their pricing strategies. This result suggests that including energy and labor cost projections in overall inflation forecasts makes

[53] See Appendix B.2 for details.

Table 3.8: Experimental Results - INFLATION vs. ENERGY/WAGE

Groups included:	INFLATION + ENERGY + WAGE	
Dependent Variable: $\Delta Price_{i+12m}$	(1)	(2)
Baseline: **INFLATION group**		
Infl. 2023	0.758***	0.759***
	(0.094)	(0.072)
ENERGY	5.698*	5.729**
	(2.834)	(2.642)
WAGE	3.808**	3.756**
	(1.703)	(1.654)
ENERGY × Infl. 2023	-0.447*	-0.450*
	(0.251)	(0.248)
WAGE × Infl. 2023	-0.336*	-0.340*
	(0.190)	(0.186)
Constant	3.307**	3.337**
	(1.260)	(1.545)
Controls	No	Yes
N	1424	1424
R^2	0.079	0.128

Note: OLS estimates from the regression of equation (6): $\Delta Price_{i+12m} = \alpha + \beta \times E_{2022}Inflation_{i2023} + \delta_1 \times ENERGY_i + \delta_2 \times WAGE_i + \theta_1 \times (ENERGY_i \times E_{2022}Inflation_{i2023}) + \theta_2 \times (WAGE_i \times E_{2022}Inflation_{i2023}) + X_i'\gamma + \varepsilon_i$. Split variable as indicated in the first row of the table. Dependent variable: planned price change in the next 12 months. Independent variables: respondent's inflation forecast 2023, experimental groups INFLATION, ENERGY and WAGE, controls if indicated, and a constant. Baseline group: INFLATION. CONTROL group is not included in analysis. Controls include firm controls (size groups, legal forms and 1-digit industries (WZ08 classification)), manager controls (education, position in the firm and the gender of the decision-maker) and week fixed effects. Standard errors clustered on industry and survey-week level. Standard errors in brackets. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

the treatment more relevant, as these costs are critical for the pricing decisions of firms in our survey.

Finally, Panel (b) of Figure 3.3 illustrates the observed effects graphically with bin scatters of firms' pre-experimental inflation expectations for 2023 (x-axis) and their price-setting plans in the year ahead (y-axis), this time only for the treatments INFLATION, ENERGY and WAGE. Most striking is the fitted line slope difference between the INFLATION (0.758)

and ENERGY/WAGE (0.311/0.422) treatments. This slope difference graphically illustrates the different weights that survey participants put on the provided information when indicating their price plans. Further, the fact that all lines intersect approximately at firms' *average* inflation expectations (11.3, marked with a dotted line), illustrates graphically why a difference in *average* treatment effects is absent (Table 3.1) even though the groups apparently respond in different ways to the different information treatments depending on their inflation expectations. Collectively, these results indicate a nuanced dynamic: the increase in intercepts and the reduction in slopes essentially neutralize each other. This results in a negligible overall average difference among the INFLATION, ENERGY, and WAGE treatments, as shown in Table 3.1 and discussed in Section 3.5.[54]

In terms of policy implications, these findings suggest that central banks can enhance the relevance of their communication by incorporating predictions about significant cost factors important to firms' pricing decisions, beyond just providing general price forecasts. Furthermore, our results imply that emphasizing cost factors in central banks' communication does not compromise the overarching objective of mitigating upward inflation trends in high-inflation contexts. This is demonstrated by the lack of significant differences in the pricing effects across the INFLATION, ENERGY, and WAGE treatments.

3.8 Conclusion

We provide causal survey evidence on the effect of inflation information on firms' price-setting behavior. Making use of a randomized information experiment, a subset of firms in our survey receives publicly available information on central banks' inflation forecasts. Treated firms indicate planned price increases that are 22% lower when compared to firms not receiving the information treatment. Additionally, providing firms with detailed information on projected input cost developments (energy and labor costs) in an extended treatment

[54] Using coefficients from specification (1) of Table 3.8 and the average inflation expectation for 2023 (11.3%), the calculated overall effect is 0.6469 ($5.698 - 0.447 \times 11.3$) for the ENERGY treatment and 0.0112 ($3.808 - 0.336 \times 11.3$) for the WAGE treatment.

has no incremental effect compared to the simple information treatment providing general price forecasts. In additional tests, we show that overall treatment effects are stronger when firms are less informed about *past* inflation dynamics and when they are more satisfied with economic policy. Effects further depend on the importance of labor and energy as major cost factors and on the planned frequency of price setting.

Our findings bear several key implications for monetary policy-making. First, we show that central bank communication can be an effective tool to shape firms' price-setting plans in high-inflation times. Therefore, central bank information policies targeted toward firms can effectively be used to break an inflation spiral. An improved information provision would also allow keeping interest rates on a lower path, thereby decreasing the risk of a hard landing. Second, our information treatments have a stronger impact on firms with higher (untreated) inflation expectations or limited knowledge with regard to realized inflation, which are precisely the types of firms that central bank communication aims to target during periods of high inflation. Third, we show that the provision of more detailed information on central bank expectations with regard to firms' input price developments further facilitates weakening the link between firms' pre-treatment inflation expectations and their intended price setting. These findings indicate that central banks can make their communication more relevant by including forecasts of key cost factors that significantly impact firms' pricing strategies.

Overall, our results suggest that adequate information policies towards firms can be an effective additional instrument for monetary policy allowing better guidance for firms' pricing decisions, and thereby inflation in the economy as a whole.

4 Narratives about Fiscal Policy: Redistribution or Fiscal Consolidation?

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Abstract: This paper examines how narratives shape the tax preferences of business leaders. Using a large-scale survey experiment, we assess the impact of two narrative framings commonly used in public policy debates: taxes as payments of due debts or as coverage for undue losses. We find that the key channel affecting tax preferences is debt-based framing, which increases support for taxation. A narrative emphasizing undue losses has limited impact, except in increasing support for capital gains taxes. Tax preferences are also driven by attitudes toward fiscal stimuli and self-interest. These findings have implications for fiscal policy communication.

Keywords: Tax preferences, fiscal policy, firm decision-makers, survey experiments.

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But for those making more than $1 million (...) – I would raise rates immediately on taxable income in excess of $1 million, including, of course, dividends and capital gains.

(Buffett, 2011)

4.1 Introduction

Public statements like Warren Buffet's 2011 op-ed reflect a growing phenomenon: business leaders taking an increasingly active role in publicly expressing their views on a wide range of policy issues. This trend has accelerated significantly in recent years. In 2019, 37.53% of CEOs publicly voiced opinions on policies – a dramatic rise from just 0.98% in 2011 (Mkrtchyan et al., 2024). These statements often address heavily debated political and societal topics such as taxation, regulation, or climate policy. Interestingly, some of these positions appear misaligned with the traditional profit-maximizing objectives of the companies they run. One policy area where this phenomenon is particularly striking is taxation.

Tax-related statements by firm decision-makers spark public interest as they directly touch on issues of redistribution and corporate responsibility. Taxes not only fund public goods and redistribute resources but also carry strong symbolic meaning in debates about equity and social justice. For firm decision-makers, statements on tax policies allow them to signal commitments to societal responsibility and long-term economic sustainability, even when these stances appear to conflict with their financial interests or those of their shareholders. This raises a critical question: why do firm decision-makers advocate for tax policies that seem counter to their self-interest?

One potential explanation lies in the persuasive power of narratives – the stories individuals tell themselves and others to make sense of and justify (economic) outcomes in the world around them. Narratives have been shown to significantly shape individual preferences and behaviors, particularly regarding redistributive policies (Shiller, 2017; Stantcheva, 2021; Alesina et al., 2023; Colonnelli et al., 2024). For instance, the moral or social framing of narratives can lead individuals to support higher taxes, even when these measures do not

directly serve their personal pecuniary interests.[55]

While most of this previous research has explored how narratives shape the general public's attitudes toward economic and political issues, relatively little attention has been paid to their influence on firm decision-makers[56] – a group that plays a major role in shaping corporate strategies and public policy debates (Milyo et al., 2000; Bertrand et al., 2014; Fouirnaies & Hall, 2018; Bertrand et al., 2020; Babenko et al., 2020).[57] Understanding the influence of narratives on firm decision-makers' beliefs is therefore vital to uncovering the motivations driving their public policy stances, especially in key areas like taxation and fiscal policy.

However, studying the preferences and reactions of corporate decision-makers to narratives presents a methodological challenge, as experimental surveys targeting this group are rare. We address this gap by using the GBP, a large-scale survey of corporate decision makers specifically designed to explore issues related to corporate behavior in the areas of taxation and accounting (Bischof et al., 2024). This dataset provides unique insights into typically unobservable drivers of behavior, including attitudes, preferences, and expectations.

Using this unique survey data, our main contribution is to examine how different types of narratives commonly invoked in public policy debates influence the tax preferences of firm decision-makers. Specifically, we seek to answer the following key questions: i) Is (policy) communication a determinant of the preference for paying taxes? ii) Is there awareness

[55] Studies have shown that the willingness to pay taxes is shaped not only by pecuniary motives (Allingham & Sandmo, 1972), but also by non-pecuniary factors (Luttmer & Singhal, 2014).

[56] We require that survey participants have managerial responsibilities in their respective firm. While the majority of our participants are CEOs (91%), we do not exclude participants who do not identify as CEO. Thus, we refer to the participants in our survey as firm decision-makers.

[57] Focusing on the tax preferences of firm decision-makers is crucial for several reasons. First, businesses have more opportunities than individuals to shift or underreport profits (Kleven et al., 2011), while remitting the lion's share of taxes to governments (Milanez, 2017), hence playing a central role in tax enforcement. Second, firm decision-makers have the capacity to significantly influence policy and public opinion through lobbying or corporate philanthropy (Milyo et al., 2000; Bertrand et al., 2014; Fouirnaies & Hall, 2018; Bertrand et al., 2020), meaning their preferences can directly impact policy making. Third, firm decision-makers' attitudes toward policies affect the performance of their company or the political attitudes of their employees (Babenko et al., 2020; Mkrtchyan et al., 2024). Despite the importance of understanding firm decision-makers' tax preferences, we are not aware of any studies that have addressed how these preferences are shaped.

about the link between public goods and taxes? iii) Do redistribution concerns influence the preference for paying taxes? By addressing these questions, we provide new insights into the effectiveness of narrative-based interventions in shaping tax preferences of high-level firm decision-makers.

We develop a set of questions and experimental interventions to explore how narratives influence tax preferences, collecting approximately 8,000 firm-level observations.[58] We analyze the tax preferences of firms by examining how narratives shape how much tax they would be willing to pay to finance a large fiscal stimulus package. Our study focuses on narratives surrounding the justification and implications of this large fiscal stimulus package. To make the narratives credible and relevant, we field our survey experiment shortly after the implementation of the German government's fiscal policy responses to the unexpected economic shocks caused by the outbreak of the Covid-19 pandemic. Specifically, in June 2020, Germany launched a fiscal stimulus program, part of which provided financial support to firms, financed by net borrowing of around 130 billion EUR (German Federal Ministry for Economic Affairs and Energy (BMWK), 2022). This specific context allows us to link the preference for paying taxes to concrete government expenditures and provides a situation in which the government's need for financing is credible, and future tax changes are possible. In addition, because this government program directly targets firms during the crisis, it is a relevant and relatable policy example for firm decision-makers.

Our experimental interventions rely on contextual cues from economic policy narratives to frame tax policies and tax payments. We focus on two economic policy narratives commonly used in public debates. These narratives represent two contrasting views on the role of taxation: one that emphasizes fiscal responsibility and the need for balanced government

[58] Ideally, we would examine how the attitudes and preferences expressed in the survey translate into firm decision-makers' actual behavior, such as voting or supporting petitions for higher taxes. However, such actions are inherently difficult to observe directly. Nonetheless, a substantial body of research has shown that survey responses are meaningfully correlated with actual behavior when the latter is observable (Hainmueller et al., 2015; Funk, 2016; Parker & Souleles, 2019; Epper et al., 2022; Dechezleprêtre et al., 2025). In the context of the GBP, Bischof et al. (2024) demonstrate that, for example, reported revenues in nearly 90% of cases align with externally verified data. These findings give us confidence that, in our case as well, stated behavior is a reliable proxy for actual behavior.

budgets, and another that highlights the importance of redistribution to mitigate unforeseen economic hardship. In particular, we randomly assigned survey respondents to three experimental groups: i) one receiving cues about the need for fiscal consolidation, including potential tax increases following the government's Covid-19 business support program; ii) another receiving cues about firms facing hardship through no fault of their own; and iii) a control group that received no such cues.

In the two economic policy narratives, we framed the 130 billion EUR stimulus package in contrasting ways: as tax payments for *due debts* or for *undue losses*. The *due debts* narrative emphasizes that increased government spending raises debt levels and will likely necessitate future tax hikes, as seen after the 2008/2009 financial crisis. This framing draws on public discourse that stresses the trade-off between government debt and taxes, portraying taxes as a necessary measure to maintain fiscal balance. In contrast, the *undue losses* narrative highlights that many firms experienced financial difficulties and losses due to external shocks like the pandemic, beyond their control. This treatment frames tax payments as a mechanism for redistributing income in response to uncontrollable misfortune rather than insufficient effort.

The psychological contract underlying these narratives reflects two key aspects: taxes as the price for a balanced government budget or as a tool for redistribution when income disruptions result from bad luck. These treatments are designed to address two main research questions. First, we assess whether firm decision-makers alter their attitudes toward taxes when made aware of the government's budget constraints, testing whether fiscal consolidation narratives increase the preference for paying taxes. Second, we investigate whether a narrative focused on external hardship increases the preference for taxes by fostering a stronger preference for redistribution.

After assignment to the experimental treatment group, we examine the effect of the narrative treatments on firms' preference to pay taxes. We measure this preference by asking firm decision-makers how they would like to adjust taxes (in percentage points) to

enable the government to support businesses during crises. Firm decision-makers can choose to adjust the following types of taxes: *corporate tax*, *local business tax*, *income tax*, and *capital gains tax*. These taxes are particularly important for firms in Germany, as they directly impact local business operations and influence firms' overall strategic planning and competitiveness. Additionally, we examine how our treatments affect firm decision-makers' attitudes toward the fiscal stimulus (i.e., whether they perceive the government intervention as justified) and their expectations of future medium-term tax rates (i.e., anticipated tax rate changes in percentage points over the next two years). We compare the effects of these narrative treatments on the preference to pay taxes against the results obtained in the untreated control group.

We obtain four main results. First, when firm decision-makers are made aware of the need to counter-finance the fiscal stimulus program through future higher taxes or spending cuts, their support for the program decreases. However, they show a greater preference for paying higher taxes now compared to the control group to fund fiscal stimulus programs. This result holds for all type of taxes. This suggests that firm decision-makers may overlook the potential costs and financing needs of such programs unless these are explicitly highlighted. Consistent with this, we find that firms exposed to this narrative treatment also exhibit higher tax expectations compared to those in the control group. In our context, narratives emphasizing fiscal prudence and reinforcing the implicit contract between taxpayers and the state prove effective in enhancing tax payment preferences, indicating that firm decision-makers are inclined to pay taxes to help maintain manageable levels of government debt. Our results indicate that narratives emphasizing fiscal realities are effective tools to influence tax preferences. Whereas studies at the household level have found a limited effect of quantitative information on attitudes toward tax policy (Roth et al., 2022), we find that embedding such statistical facts within narratives shifts the tax policy preferences of firm decision-makers.

Second, we find that appeals to misfortune do not significantly influence firm decision-makers' support for the fiscal stimulus program or their preference for paying most types of

taxes. Thus, unlike findings from household studies (Alesina & Angeletos, 2005; Durante et al., 2014; Gualtieri et al., 2019; Fisman et al., 2020), our results suggest that narratives centered on fairness and misfortune in the income-generating process have a limited impact on shaping tax preferences among firms, and are far less effective than highlighting the need to balance the government budget. However, we observe a notable exception: firm decision-makers express a desire to increase the *capital gains tax* when the role of bad luck in the income-generating process is emphasized. This suggests an increased willingness to redistribute wealth, as the *capital gains tax* primarily targets wealth accumulation. In a follow-up survey, participants indicated that redistributive motives were the main factor behind their preference for raising the *capital gains tax* rate. This finding aligns with the broader observation that many business owners support higher taxes on wealth (Buffett, 2011; Mohamed, 2020). In addition to redistributive motives, firm decision-makers' desire to increase the *capital gains tax* may also stem from the belief that this tax has a smaller impact on the firm's production process compared to corporate or personal income taxes. Consequently, they may prefer to avoid raising taxes that, in their view, could impede economic activity.

Third, we examine the heterogeneity in treatment effects more closely. Using a data-driven machine learning approach, we estimate the distribution of conditional average treatment effects for each decision-maker and experimental group. Analyzing this distribution allows us to verify that the treatment effects are not influenced by outliers and remain consistent for the majority of respondents within each treatment group. The analysis reveals that average treatment effects from the multi-arm causal forest closely align with those from our parametric models, confirming the robustness of the results. Additionally, our non-parametric approach highlights that while redistribution considerations in the *undue losses* narrative have a limited impact on most taxes (except *capital gains tax*), the *due debts* narrative significantly boosts tax payment preferences for most decision-makers, demonstrating the power of framing fiscal stimuli as due debt.

Fourth, building on the observed heterogeneity of conditional average treatment effects identified in the machine learning analysis, we test covariates expected to influence observed treatment effects on theoretical grounds. Specifically, we examine whether attitudes toward the fiscal stimulus and firms' tax obligations contribute to variations in the preference to pay taxes. Our findings show that stronger agreement with the stimulus correlates with a greater willingness to pay taxes. This effect is particularly pronounced when decision-makers are exposed to the *due debts* narrative. The more firm decision-makers support the government stimulus, the more willing they are to finance it, especially when they recognize the need for counter-financing. Additionally, we observe that firm decision-makers are more likely to support tax increases on taxes to which their firms are not subject, indicating that motives of self-interest, similar to findings on household climate policy preferences (Dechezleprêtre et al., 2025), do play a role. However, both narratives are effective in reducing firms' self-interested tendencies to lower the taxes they are liable to. As a result, effective communication from policymakers and tax authorities can foster greater support for tax policies.

Taken together, our study offers new perspectives on how narratives surrounding tax policy influence the tax preferences of firm decision-makers. By exploring the effects of these narratives on tax preferences, our work contributes to the understanding of corporate responses to government policy communication, which is particularly relevant in the context of unforeseen economic challenges, such as those exemplified by the Covid-19 pandemic or economic downturns when increased government spending is needed. Based on our empirical findings, we answer the three questions posed at the beginning of this introduction as follows: i) Effective communication and relevant context about government budgeting and expenditures influence how firms' preferences toward paying taxes are shaped. ii) Firm decision-makers do not seem to be aware of the relationship between increased government spending and taxes, unless the costs of the stimulus program are explicitly highlighted. iii) Redistribution narratives seem to have limited effectiveness in increasing firms' preference to pay taxes.

Related literature. We contribute to different strands of literature. First, we relate to the literature studying how individuals form beliefs about economic policies, in particular tax policy. Within this literature studies have examined the effect of narratives or anecdotes in experimental studies on economic policy perceptions or tax preferences of individuals. Stantcheva (2021) uses educational videos that focus on the redistributive consequences and efficiency costs concerning economic policies. The study reveals that videos emphasizing redistributive aspects increase support for more progressive taxation, while videos concentrating solely on efficiency considerations do not have a significant impact. Alesina et al. (2023) explore the effects of factual information versus a narrative about a hard-working immigrant on redistribution preferences through cross-country surveys, discovering that anecdotes more effectively increase support for redistribution than just hard facts. Similarly, Colonnelli et al. (2024) employ animated videos that portray positive or negative images of large US corporations and assess their impact on public support for corporate bailouts. Further, narratives have been shown to causally affect the formation of expectations about the overall economy and can explain economy-wide fluctuations (Shiller, 2017; Andre et al., 2024). Moreover, Graeber et al. (2024) show that people remember narratives better than pure statistical information. Our contribution to this literature lies in identifying which narratives often used in public policy debates influence *firms*' tax preferences – insights that cannot be inferred from tax return data alone. By adopting the perspective of policymakers and tax authorities, we seek to uncover communication strategies that can shape tax preferences across different relevant types of taxes, particularly during times of heightened financial pressure on government budgets.

Second, through the arguments presented in our narrative treatments, we contribute to the body of research exploring how beliefs about the causes of income and wealth inequality, alongside considerations of redistribution and efficiency, influence individuals' attitudes toward tax policy. With regard to the income-generating process, previous literature has shown that individuals who view wealth accumulation as driven by luck or chance tend

to support greater redistribution, especially after major economic shocks like pandemics or natural disasters (Gualtieri et al., 2019). Conversely, those who attribute wealth to personal effort often favor less redistribution (Alesina & Angeletos, 2005; Durante et al., 2014; Fisman et al., 2020). Moreover, the propensity to pay taxes can also be influenced by perceptions of public institutions' fairness and efficiency. Regarding fairness, Feld and Frey (2007) conceptualize the interaction between taxpayers and the government as a psychological contract, where tax compliance depends on the perceived fairness and legitimacy of the political process. This contract is strengthened when citizens feel treated equitably and agree with government spending decisions, fostering reciprocal attitudes (Alm et al., 1993; Fehr & Gächter, 1998; De Neve et al., 2021). In terms of efficiency, Roth et al. (2022) examine how households' beliefs about the debt-to-GDP ratio affect their attitudes toward government spending and taxation, showing that individuals tend to underestimate current debt levels. Moreover, when informed of the actual debt-to-GDP ratio, respondents tend to demand less government spending, while their overall attitudes toward taxation remain largely unchanged.

We contribute to the literature by demonstrating that, unlike households (Durante et al., 2014; Fisman et al., 2020), firm decision-makers are less influenced by redistribution-focused narratives and respond more strongly to messages emphasizing the trade-offs between government spending and taxation. An exception is *capital gains tax*, where redistribution concerns lead firm decision-makers to support higher taxes. This preference may stem from a desire for redistribution, as capital gains taxes target wealth growth, or from the perception that such taxes are less disruptive to business operations compared to corporate or personal income taxes. Additionally, transparent communication about fiscal realities and the necessity of sustainable government financing proves more effective in our setting at shaping tax preferences than the purely quantitative information utilized in previous studies (Roth et al., 2022).

Chapter 4 proceeds as follows. Section 4.2 describes the survey data used in the empirical

analysis. Section 4.3 outlines the experimental setup and gives an overview of the different experimental treatments. Section 4.4 presents the main results of our empirical analysis. Section 4.5 examines the heterogeneity regarding our main treatment effect. Section 4.6 concludes with policy implications of our results, offering guidance for policymakers on how to effectively communicate proposed changes in tax policy.

4.2 Data

The survey questions used in the experimental analysis were fielded in the first wave of the GBP, which ran from July to September 2020. The GBP is an online survey focusing on firm decision-makers, which collects data to improve the understanding of entrepreneurial behavior, mechanisms of change in the business landscape, and the impact of policy decisions on firms. In the first survey wave, 15,414 firms began the questionnaire by answering the revenue category question. This corresponds to a response rate of approximately 5% (15,414 out of 331,300 firms). A detailed overview of the survey can be found in Bischof et al. (2024).[59] For the empirical analysis, we restrict our sample to 7,848 firm decision-makers who participate in the survey experiment.[60]

The target group of participants ranges from business owners of small- and medium-sized companies to CEOs of large corporations. 91% of our participants are owners or CEOs. Furthermore, 60% hold a university degree and decision-makers are predominantly male (81%). To ensure no selection bias in our survey among firms, we compare the characteristics of participating firms to firms in Orbis[61], which did not participate in our survey experiment. We find that participating firms and non-participating firms are comparable with regard to employees, total assets and revenues (Appendix C.1.3). We also contrast our sample against the German business register of the German Federal Statistical Office for reporting year

[59] Potential participants are informed about the academic institution (*GBP*; Part of the *University of Mannheim*), which conducts the survey, and are reminded to answer the questions from the perspective of their firm.

[60] For information on summary statistics and sample sizes, we refer to Appendix C.1.2.

[61] The contact database from which firms in our sample are drawn.

2020 (Appendix C.1.4). The comparison shows that our sample population is quite close to the firm population in Germany with regard to industry composition, and slightly over-represents larger firms in terms of revenues and employees. For a comprehensive overview, we refer to Appendix C.1.

4.3 Experimental Design

We investigate whether narratives about fiscal consolidation and redistribution affect attitudes toward fiscal policies and taxes, using a survey experiment. We aim to identify communication strategies that enhance the preference for paying taxes during periods of increased financial pressure on the government budget. Our focus is on how narratives shape the relationship between firms as taxpayers and the government, with particular attention to the factors that motivate businesses to pay taxes.

Experimental setup. Our survey's experimental setup consists of two treatment groups (FISCAL and SOCIAL) and a CONTROL group. Firm decision-makers are randomly assigned to receive one of the narrative treatments (FISCAL or SOCIAL) or no treatment in the CONTROL group. At the start of the survey, all participating decision-makers answer questions about their firm's characteristics, such as legal form and annual revenues. After this, firms are randomly assigned to the SOCIAL and FISCAL treatment groups, where they receive the respective narrative treatments. The CONTROL group does not receive any narrative. To ensure comparability across the different experimental groups, we test for systematic differences in underlying characteristics. Our analysis shows that these characteristics are well-balanced across the treatment groups, as demonstrated in Appendix C.1.1.

After the experimental treatments, all survey participants are asked the following questions. First, we ask whether the firm decision-makers think it is justified for the government to intervene with a stimulus package to support the economy. Second, participants are asked about their expectations regarding future medium-term tax rates (i.e., tax rate changes in

the next two years). Third and most importantly, we ask firm decision-makers how they would like to adjust taxes such that the government can support businesses during crises. Firm decision-makers can choose to adjust the following taxes: *corporate tax*, *local business tax*, *income tax*, and *capital gains tax*.[62] For more information on the experimental design, we refer to Appendix C.2, which presents screenshots and translations of the original survey questions.

Experimental treatments. Our experiment is designed as follows. Participants in the FISCAL and SOCIAL treatment are both provided with information that the German government pledged a 130 billion EUR stimulus program during the Corona crisis (German Federal Ministry for Economic Affairs and Energy (BMWK), 2022). We explicitly choose the fiscal stimulus program as an example of government spending since this support program directly benefits firms. However, the economic policy narratives incorporated into our treatments present this government stimulus in different ways.

Participants in the **FISCAL treatment** group are informed not only about the government's 130 billion EUR stimulus package but also that the increase in government spending raises the level of government debt and will likely necessitate future tax increases. To bolster credibility, we note that many countries raised their tax rates following the 2008/2009 financial crisis. Additionally, we highlight that representatives of the governing party have already stressed the need to increase taxes to restore fiscal balance. Drawing on Roth et al. (2022), this treatment emphasizes the higher debt level of the German state resulting from increased government spending. In this context, we present the fiscal stimulus as *due debts* requiring counter-financing, thereby clarifying the contract between firms as taxpayers and

[62] The German *corporate tax* is levied on the income of incorporated firms and amounts to 15 percent of taxable income. The *local business tax* is payable by both sole proprietorships/business partnerships and corporations, and, like the *corporate tax*, is also applied as a tax on the profits of a business. On average, the local business tax rate amounts to 14 percent of taxable profits in 2021 (German Federal Statistical Office, 2021). The *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 personal income tax rates. Finally, the *capital gains tax* covers payouts (e.g., dividends) from corporations to shareholders. For investors holding less than 1 percent in a company, this tax is a maximum of 25 percent. Investors holding more than 1 percent are taxed under a partial income procedure as part of their income tax.

the government. In this framing, taxes are portrayed as the price for maintaining a low-debt government, explicitly emphasizing the inherent trade-off between taxes and government debt to firm decision-makers. In the FISCAL treatment, firm decision-makers are presented with the following text:[63]

> *The federal government has pledged support of **130 billion EUR** as part of the economic stimulus package adopted in June.*
>
> *The increased government spending and additional debt incurred in the wake of the Corona crisis could necessitate higher government revenues or spending cuts in the future.*
>
> *After the 2008/2009 financial crisis, for example, tax rates were raised in many European countries. Representatives of the CDU have already announced that the debt incurred as a result of the Corona crisis will be reduced again by 2030.*

For participants in the **SOCIAL treatment** group, we enhance the information on the size of the government stimulus by highlighting that many companies experienced hardship through no fault of their own as a result of the Corona crisis. To emphasize this point, we inform firm decision-makers that sales in the hospitality industry dropped by 75.8 percent compared to the same month in the previous year. This treatment is related to the experiments in Durante et al. (2014) and Fisman et al. (2020), which show that support for redistribution is higher among *individuals* if earnings are assigned according to a random process. We test whether this effect is present among firm decision-makers by emphasizing in this narrative that the income-generating process is influenced by bad luck from a negative exogenous shock, such as a pandemic, rather than by a lack of effort, and that the fiscal stimulus was implemented to address this involuntary emergency. If decision-makers value such government support, tax payments can be viewed as a mechanism for redistribution in

[63] Appendix C.2.3 provides a screenshot of the original treatment text in German.

response to an income-generating process shaped by external misfortune rather than by insufficient effort (Alesina & Angeletos, 2005). In the SOCIAL treatment, firm decision-makers receive the following text:[64]

> *Many companies have experienced hardship through no fault of their own as a result of the Corona crisis, with sales in the hospitality industry, for example, plummeting 75.8 percent compared to the same month last year. The federal government has pledged support of* ***130 billion EUR*** *as part of the economic stimulus package adopted in June.*

Our experimental treatments are designed to examine which types of narratives can shift the tax preferences of firm decision-makers. First, by comparing respondents from the FISCAL group and the CONTROL group, we test whether firm decision-makers alter their attitudes toward taxes and the fiscal stimulus when made aware of the government budget constraints and the need to counter-finance government spending. Specifically, we evaluate whether a narrative that presents taxes as necessary to keep government debt at reasonable levels increases the preference for paying taxes compared to an untreated CONTROL group. Second, to assess whether a narrative highlighting the role of luck in the income-generating process increases decision-makers' preference to pay taxes by fostering stronger support for redistribution, we compare the SOCIAL group to the CONTROL group.

Our study differs from previous research in two key ways. First, we use an experimental design that incorporates narratives common in public discourse, such as electoral campaigns or media debates on state fiscal financing, allowing us to critically evaluate the communication strategies employed by politicians and tax administrations that can effectively enhance firms' preferences for paying taxes. Second, we focus on the perspectives of firm decision-makers, who are significant tax contributors and can substantially influence public opinion as well as the attitudes of their employees.

[64] Appendix C.2.4 provides a screenshot of the original treatment text in German.

Experimental design. Our experimental design has several features. First, we ask participants about their expectations concerning future tax rates and their preferences regarding tax adjustments only after the experimental intervention to avoid potential priming effects. Asking respondents about tax rates beforehand might predispose them to provide consistent answers post-treatment, as they may feel compelled to maintain consistency in their responses (*consistency bias*; Falk and Zimmermann (2013)). Moreover, asking the same question twice can lead to problems related to experimenter demand effects or confuse respondents, resulting in higher dropout rates (Haaland et al., 2023). Therefore, we rely on a between-subject design to elicit the effect of our economic policy narratives.

Second, as we are interested in the effect of policy-relevant narratives on the preference to pay taxes compared to not being provided with any narrative, we opt for a pure control group (rather than an active control group design) that is not provided with any narrative cues (Haaland et al., 2023).

Third, to increase comparability between outcomes of our experimental groups in our between-subject design, we choose to measure tax expectations and the desired adjustment of tax rates quantitatively rather than qualitatively. Measuring expected and desired tax changes qualitatively would complicate between-subject comparisons, as firm decision-makers might have systematically different interpretations of terms like "moderate" or "strong" when using a verbal answer scale. Moreover, qualitative response scales can often be too vague, thereby limiting the informational content of responses (Haaland et al., 2023). Given the potential significant heterogeneity in firms' current effective tax rates, we also choose to measure tax changes relative to current tax rates rather than in absolute terms to enhance comparability.

Fourth, our experimental treatments employ a mixture of quantitative data, such as the size of the fiscal stimulus package, and qualitative narratives about the economy (Stantcheva, 2023). Given the scarcity of experimental studies utilizing anecdotes and narratives, as highlighted by Haaland et al. (2023), our research provides new evidence on the impact

of narratives on expectations and preferences. This is especially important in the realm of tax policy communication by governments, where the way narratives are framed and delivered can provoke a more significant response from taxpayers than straightforward factual information (Alesina et al., 2023).

4.4 Experimental Results

4.4.1 Attitude toward the Fiscal Stimulus Program

We begin by testing whether the experimental treatments differently affect decision-makers' opinions on the fiscal stimulus program. Several studies have shown that perceptions of the political process and government actions shape individuals' judgments of various policies (Alm et al., 1993; Fisman et al., 2020; Stantcheva, 2021). Since we emphasize redistribution considerations in the SOCIAL treatment, we expect a potentially higher share of firms to perceive the fiscal stimulus as just. Conversely, support for government spending may decline if decision-makers become more aware of the costs associated with government intervention, as emphasized in the FISCAL treatment, which might ultimately be financed by higher taxes (Roth et al., 2022).

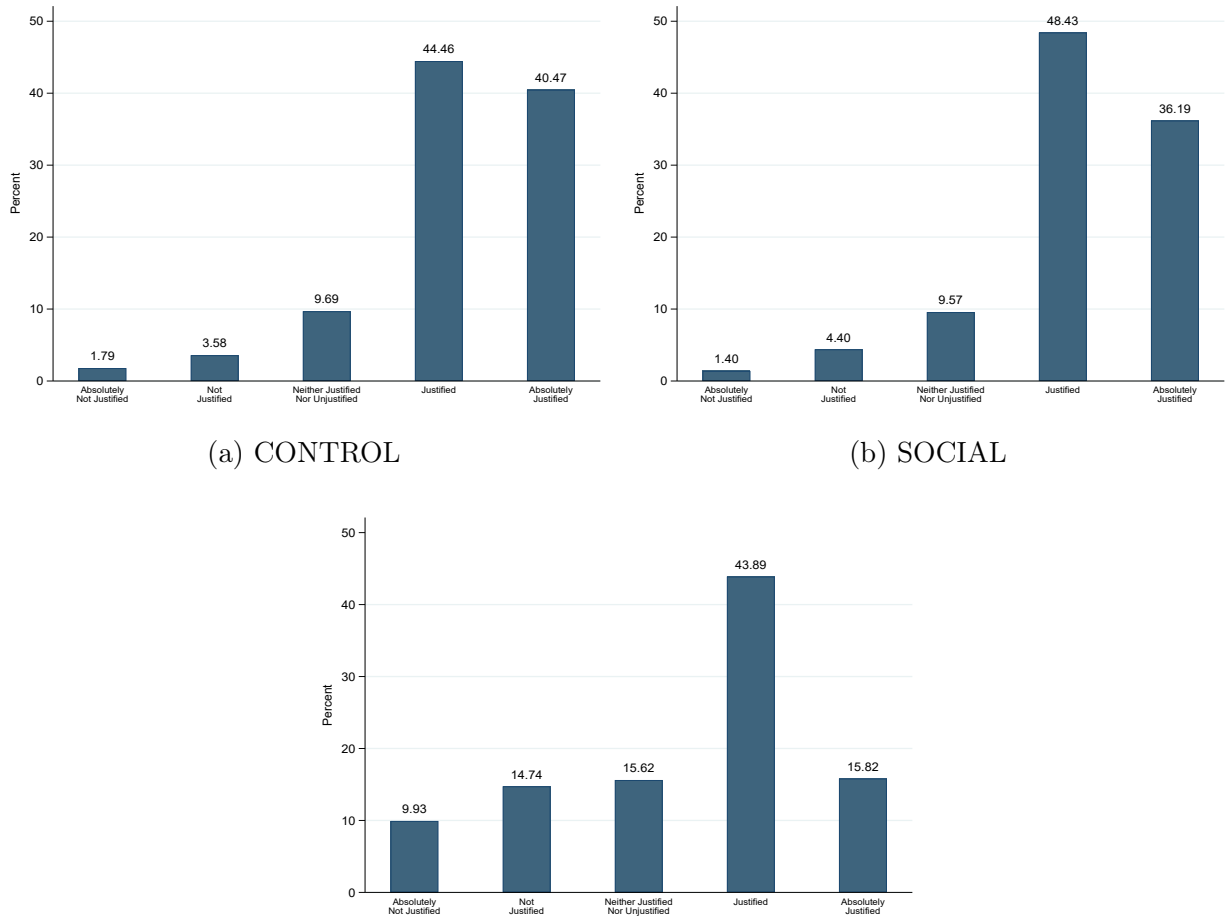
Figure 4.1 shows the distribution of responses to the question whether the fiscal stimulus program is perceived as justified by treatment group. Decision-makers can choose between the five answer options (*Absolutely Not Justified*, *Not Justified*, *Neither Justified Nor Unjustified*, *Justified*, and *Absolutely Justified*) when answering the following question:

> *"Do you think it is justified for the government to intervene with a/this stimulus package (at the taxpayer's expense)?"*

In the CONTROL group, where we do not refer to a specific stimulus package, we use the indefinite article *"a"*. If a treatment (FISCAL or SOCIAL) was given, where we explicitly mention the 130 billion EUR stimulus package, we use the demonstrative *"this"* to indicate

that we are referring to the stimulus package mentioned in the treatment. The phrase *"at the taxpayer's expense"* was used only in the FISCAL treatment to emphasize the financing side of the state intervention.

Figure 4.1: Attitude toward the Fiscal Stimulus



(a) CONTROL

(b) SOCIAL

(c) FISCAL

Note: Figure 4.1 illustrates the distributions over a 5-point Likert scale on the question *"Do you think it is justified for the government to intervene with a/this stimulus package (at the taxpayer's expense)?"* for CONTROL group ($N = 1,228$), SOCIAL group ($N = 2,998$), and FISCAL group ($N = 2,971$). Firm decision-makers could choose between the following five answer options: *Absolutely Not Justified*, *Not Justified*, *Neither Justified Nor Unjustified*, *Justified* and *Absolutely Justified*. In the CONTROL group, where no specific stimulus package is mentioned, we use the indefinite article *"a"*. In the treatment groups (FISCAL or SOCIAL), where the 130 billion EUR stimulus package is explicitly mentioned, we use the demonstrative *"this"* to refer to the specified stimulus package. The phrase *"at the taxpayer's expense"* is used exclusively in the FISCAL treatment to emphasize the financing aspect of the state intervention. For screenshots of the original survey questions, we refer to Appendix C.2.

There are three key insights when examining the answer distributions by treatment group. First, the majority of decision-makers in our sample think that the fiscal stimulus package is justified. Second, mentioning the possible future costs of the government intervention, in the form of potential tax increases or spending cuts, significantly reduces support for the stimulus program (FISCAL vs. CONTROL). Nonetheless, a large share of decision-makers in the FISCAL group (approx. 60%) still view the government intervention as justified. Third, there are no meaningful differences between participants in the SOCIAL treatment and those in the CONTROL group.

Next, we estimate the treatment effect of our experimental intervention on attitudes toward the fiscal stimulus using two different specifications of the outcome variable. First, we estimate an OLS regression using the 5-point Likert scale as the outcome variable, considering it as a continuous dependent variable. Second, we estimate a linear probability model by aggregating the outcome variable into a binary form: the dependent variable is set to 1 if the decision-maker views the stimulus as justified (either *Absolutely Justified* or *Justified*) and 0 otherwise. For the estimation, we use the following specification:

$$y_i = \beta_0 + \sum_{k=1}^{2} \beta_k \times TREATMENT_{ik} + X_i'\gamma + \varepsilon_i. \tag{7}$$

The dependent variable y_i represents the attitude toward the fiscal stimulus, defined either as a continuous 5-point Likert scale variable or as a binary outcome variable, depending on the specification. $Treatment_{ik}$ is a categorical variable indicating the experimental treatment group firm i is in (SOCIAL, FISCAL). The coefficient β_0 shows the baseline effect for the CONTROL group and is captured by the constant term. β_k denotes the effect of the experimental groups SOCIAL and FISCAL relative to the CONTROL group. We also include results for a specification with controls (X_i), where we control for firm size, industry, legal form, gender, education, and position in the company.

Results in Table 4.1 indicate that the observed effect of the treatments is not sensitive to the specification of the outcome variable. In the specification in columns (1) and (2), where the dependent variable is defined as a continuous 5-point Likert scale, we observe no significant difference between the CONTROL and SOCIAL treatment groups. In contrast, the FISCAL treatment notably reduces respondents' perception that the fiscal stimulus is just. For instance, in column (1), the perception of the stimulus being just diminishes by almost a full Likert scale point (-0.773). The findings in columns (3) and (4), where we employ a linear probability model, closely align with the results in columns (1) and (2). Again, we find no significant difference between the CONTROL group and SOCIAL group, but a significant negative difference between the CONTROL group and FISCAL group. For example, in column (3), firms in the FISCAL group have, on average, a 25 percentage point lower probability of assessing the fiscal stimulus as just compared to the CONTROL group. The results are robust to using an ordered probit model with a 5-point Likert scale as the dependent variable or a logistic regression model with a binary dependent variable (see Appendix C.3). Moreover, Table 4.1 shows a consistent and significant difference between the FISCAL and SOCIAL treatment, with a lower proportion of respondents in the FISCAL treatment believing the stimulus is justified compared to the SOCIAL treatment (p(FISCAL vs. SOCIAL)).

Overall, our findings suggest that narratives about misfortune or bad luck in the SOCIAL treatment do not change whether the fiscal stimulus program is perceived as justified by firm decision-makers. This likely reflects the fact that decision-makers are aware that many firms are struggling due to the repercussions of an unforeseen crisis and not through any fault of their own. However, stressing the higher tax burden in the FISCAL treatment, and thereby priming firm decision-makers to consider the cost of the fiscal stimulus from a taxpayer's perspective, reduces the share of decision-makers perceiving the stimulus as just.[65]

[65] In a follow-up survey conducted approximately one year later, we revisited the fiscal stimulus question with a small random subset. Of the 41 panel participants, 81% either chose the same Likert scale point or shifted by just one point upward or downward. This indicates that views on the stimulus are robust rather than short-lived.

Table 4.1: Attitude toward Fiscal Stimulus: Likert Scale and Linear Probability Model

Dependent Var.:	**Attitude toward Fiscal Stimulus**			
	5-Scale Likert	5-Scale Likert	Binary (0/1)	Binary (0/1)
	(1)	(2)	(3)	(4)
Constant	4.182***	4.236***	0.849***	0.863***
(Baseline CONTROL)	(0.025)	(0.031)	(0.010)	(0.013)
SOCIAL	-0.046	-0.054	-0.003	-0.002
	(0.030)	(0.037)	(0.012)	(0.015)
FISCAL	-0.773***	-0.760***	-0.252***	-0.237***
	(0.033)	(0.042)	(0.014)	(0.017)
Controls	No	Yes	No	Yes
N	7197	4291	7197	4291
Adj. R^2	0.113	0.125	0.079	0.079
p(FISCAL vs. SOCIAL)	0.000	0.000	0.000	0.000

Note: OLS estimates from the regression of equation (7). **Dependent variable in Columns (1) and (2)**: attitude toward fiscal stimulus measured on a 5-point Likert scale (1 = *Absolutely Not Justified*, 2 = *Not Justified*, 3 = *Neither Justified Nor Unjustified*, 4 = *Justified*, 5 = *Absolutely Justified*). **Dependent variable in Columns (3) and (4)**: dummy variable equal to 1 for firm decision-makers selecting the items *Absolutely Justified* or *Justified* and zero otherwise. **Independent variables**: experimental group, constant, and control variables if stated (size group (EC's definition for small and medium-sized enterprises (SMEs)), industry (WZ08 1-digit), legal form, gender of manager, manager education, position in company. p(FISCAL vs. SOCIAL) represents the p-value from a test of significant difference between the FISCAL and SOCIAL treatments. Robust standard errors in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

4.4.2 Tax Expectations

Next, we present results on the post-treatment tax expectations of firm decision-makers. Asking respondents about expected tax rates after having received the experimental treatment serves two distinct purposes.

First, we want to verify that firm decision-makers do not confuse *tax expectations* and *preferences about tax rate changes* by explicitly eliciting their future tax expectations before asking them about their preference to pay taxes. Second, studying tax expectations also serves as a manipulation check. By asking firm decision-makers about their tax expectations, we can verify if receiving information about the size of the fiscal stimulus alone (like

in the SOCIAL treatment) alters tax expectations and subsequently influences preferences toward taxes. For example, simply informing firm decision-makers about the size of the fiscal stimulus could lead them to anticipate higher future tax rates, even without explicitly stating that the stimulus might be counter-financed by higher taxes, as is done in the FISCAL treatment.

To examine these channels, we ask decision-makers the following question regarding their tax expectations:

> *"Based on your current tax rate, what changes in the following tax rates do you expect in the* ***medium term*** *(12-24 months)?"*

Firm decision-makers can specify a change in percentage points ranging from -20 to 20 for the *corporate tax*, *local business tax*, *income tax* and *capital gains tax*. Additionally, a *Do not know*-option is available. To quantify the treatment effect on decision-makers' tax expectations, we run an OLS regression similar to equation (7), regressing expected medium-term changes in the *corporate tax*, *local business tax*, *income tax*, and *capital gains tax* rates on treatment dummies as follows:

$$y_i = \beta_0 + \sum_{k=1}^{2} \beta_k \times TREATMENT_{ik} + X_i'\gamma + \varepsilon_i. \tag{8}$$

In equation (8), the dependent variable y_i represents the expected medium-term tax change for the respective tax in percentage points. Similar to equation (7), $Treatment_{ik}$ is defined as a categorical variable indicating the experimental treatment firm i is in (SOCIAL, FISCAL). Additionally, we include specifications with the controls (X_i) described in Section 4.4.1, which account for firm size, industry, legal form, gender, education, and position in the company. Furthermore, we extend the set of control variables to include a binary variable that indicates whether the stimulus was perceived as just or not to account for the

respective decision-maker's perception of the fiscal stimulus.[66]

Table 4.2: Expected Medium-Term Changes in Taxes

Dependent Var.:	**Medium-Term Tax Expectations**							
	Corporate Tax		**Business Tax**		**Income Tax**		**Capital Gains Tax**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	0.004	0.004	0.008***	0.010**	0.008***	0.012***	0.023***	0.025***
(Baseline CONTROL)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)
SOCIAL	-0.004	-0.004	-0.004	-0.003	-0.003	-0.006	-0.000	-0.001
	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)
FISCAL	0.022***	0.021***	0.025***	0.019***	0.024***	0.017***	0.029***	0.025***
	(0.003)	(0.005)	(0.004)	(0.005)	(0.003)	(0.005)	(0.004)	(0.005)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	3393	2183	3676	2366	3741	2391	3460	2220
Adj. R^2	0.029	0.036	0.027	0.030	0.028	0.036	0.034	0.032
p(FISCAL vs. SOCIAL)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Note: OLS estimates from the regression of equation (7): $y_i = \beta_0 + \sum_{k=1}^{2} \beta_k \times TREATMENT_{ik} + X_i'\gamma + \varepsilon_i$. **Dependent variable**: expected medium-term change in percentage points of respective tax. **Independent variables**: experimental group dummy, constant, and control variables (size group (EC's definition for small and medium-sized enterprises (SMEs)), industry (WZ08 1-digit), legal form, gender of manager, manager education, position in company, dummy equal to 1 if decision-maker finds stimulus justified and 0 otherwise) if indicated. p(FISCAL vs. SOCIAL) represents the p-value from a test of significant difference between the FISCAL and SOCIAL treatments. Robust standard errors in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

The results in Table 4.2 indicate that firms in the CONTROL group, on average, expect only very minor increases in future tax rates. The largest increase is expected for the *capital gains tax* (e.g., a 2.3 percentage point increase in column (7)), followed by the *business tax* (e.g., a 0.8 percentage point increase in column (3)) and the *income tax* (e.g., a 0.8 percentage point increase in column (5)). For the *corporate tax*, no significant change is expected. Comparing the tax expectations of firms in the SOCIAL treatment to those in the CONTROL group, we find no significant difference. Consequently, our results confirm that simply being reminded of the size of the stimulus program does not have any additional effect on expectations about future tax changes compared to not mentioning the size of the stimulus.

Considering the results for the FISCAL group, we find that explicitly emphasizing that the government stimulus raises the level of government debt and will likely lead to future tax

[66] See Section 4.4.1 for the analysis of the attitude of firm decision-makers toward the fiscal stimulus.

increases significantly raises expectations of future taxes. Decision-makers in the FISCAL group expect increases for all types of taxes. These increases are statistically significant and economically relevant. For instance, participants in the FISCAL group expect a 2.5 percentage point higher tax rate for the medium-term *business tax* compared to the CONTROL group (column (3)). Based on an average *local business tax* of 14% in 2021 (German Federal Statistical Office, 2021), this implies an 18% higher expected increase compared to the CONTROL group. The coefficients for the effect of the FISCAL treatment on the *corporate tax*, *income tax*, and *capital gains tax* are of a similar magnitude. Results remain qualitatively the same when including control variables. Also, when examining differences between the FISCAL and SOCIAL treatments, we find a significant difference between the two groups, indicating that firm decision-makers in the FISCAL treatment expect higher taxes in the medium term (p(FISCAL vs. SOCIAL)), as shown in Table 4.2.

Overall, the results from the SOCIAL treatment show that merely mentioning the stimulus program does not have any additional effect on tax expectations compared to the CONTROL group. Further, the fact that decision-makers have higher expectations about future tax rates after receiving the FISCAL treatment indicates that they do not internalize the costs of the stimulus program unless it is explicitly mentioned to them. Thus, firm decision-makers are less aware of the link between higher government expenditures today and higher tax rates tomorrow. Therefore, the proposition of Ricardian equivalence (Barro, 1974) does not seem to hold for German firm decision-makers in our setting.

4.4.3 Desired Tax Changes

In this section, we present results on decision-makers' preferences to pay taxes by asking about their preferred tax rate changes. We illustrate how these preferences change in response to emphasizing either fiscal prudence considerations or the role of misfortune in our narrative treatments.

To elicit decision-makers' preferences regarding taxes, we ask participants the following

question about their preferred tax rate changes:

> *"From your company's perspective, by how many percentage points would you want to adjust the following types of taxes based on your current tax rate to ensure that the government is able to support businesses in crises?"*

Similar to the survey question regarding tax expectations, firm decision-makers can select their preferred tax rate change for the *corporate tax*, *local business tax*, *income tax*, and *capital gains tax* on a scale from -20 to 20 percentage points, or choose the *Do not know*-option. Following the concept of a *contract* between taxpayers and the tax administration (Feld & Frey, 2007; Alm, 2019), we stress the link between paying taxes and government support measures when decision-makers decide about their desired tax rate changes. We anticipate that respondents in the SOCIAL treatment and FISCAL treatment groups will prefer higher taxes compared to those in the CONTROL group. In the SOCIAL treatment, we expect that emphasizing the role of inequalities due to misfortune will increase firm decision-makers' preference to pay taxes because of a desire for greater redistribution relative to the CONTROL group. In the FISCAL treatment, we present the fiscal stimulus as debt that requires counter-financing, thereby clarifying the contractual relationship between firms, in their role as taxpayers, and the government. If firm decision-makers value lower levels of governmental debt, we expect to observe a preference for higher taxes compared to the CONTROL group.

To estimate the effects of our experimental treatments on preferences regarding tax rates, we employ a regression model similar to equation (8). In this model, the dependent variable is specified as the desired tax rate change in percentage points for the *corporate tax*, *local business tax*, *income tax*, and *capital gains tax*, and is regressed on the respective treatment dummies. We include the respective expected tax rate in our set of control variables (see Section 4.4.2 for details) to account for potential confounding effects of our treatments on tax expectations and preferred taxes. Table 4.3 presents the results, using the CONTROL

group as the baseline, with coefficients for the SOCIAL and FISCAL treatments measuring the differences relative to the CONTROL group.

Table 4.3: Desired Adjustment Tax Rates

Dependent Var.:	**Adjustment Corporate Tax**		**Adjustment Business Tax**		**Adjustment Income Tax**		**Adjustment Capital Gains Tax**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant (Baseline CONTROL)	-0.032*** (0.003)	-0.031*** (0.004)	-0.042*** (0.003)	-0.040*** (0.004)	-0.034*** (0.003)	-0.031*** (0.004)	-0.005 (0.004)	-0.012** (0.005)
SOCIAL	-0.001 (0.004)	0.001 (0.005)	-0.001 (0.004)	0.001 (0.005)	-0.001 (0.004)	-0.002 (0.005)	0.011** (0.004)	0.012** (0.006)
FISCAL	0.025*** (0.004)	0.018*** (0.005)	0.028*** (0.004)	0.021*** (0.005)	0.028*** (0.004)	0.021*** (0.005)	0.041*** (0.004)	0.031*** (0.006)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	3794	1783	4020	1931	4036	1943	3830	1834
Adj. R^2	0.023	0.186	0.026	0.169	0.029	0.177	0.031	0.169
p(FISCAL vs. SOCIAL)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Note: OLS estimates from the regression of equation (7): $y_i = \beta_0 + \sum_{k=1}^{2} \beta_k \times TREATMENT_{ik} + X_i'\gamma + \varepsilon_{1i}$. **Dependent variable**: desired adjustment in percentage points of respective tax. **Independent variables**: experimental group dummy, constant, and control variables (size group (EC's definition for small and medium-sized enterprises (SMEs)), industry (WZ08 1-digit), legal form, gender of manager, manager education, position in company, dummy equal to 1 if decision-maker finds stimulus justified and 0 otherwise, medium-term tax expectations) if indicated. Compared to regressions in Table 4.2, our set of control variables additionally incorporates expectations over the respective tax rate in regressions that include controls (as indicated). p(FISCAL vs. SOCIAL) represents the p-value from a test of significant difference between the FISCAL and SOCIAL treatments. Robust standard errors in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

We observe that firm decision-makers in the CONTROL group want to reduce their respective tax rates relative to the current tax rate. The tax they want to reduce the most is the *business tax*, which they want to lower by 4.2 percentage points (column (3)). Further, they wish to reduce the *income tax* by 3.4 percentage points (column (5)) and *corporate tax* by 3.2 percentage points (column (1)). The large effect for the *business tax* could be driven by the fact that this tax is levied on all businesses and does not depend on the legal form of a business. Interestingly, participants in the CONTROL group do not want a substantial reduction of the *capital gains tax* (column (7)).

For the SOCIAL treatment group, we find no statistically significant differences in the desired tax rates for the *corporate tax*, *business tax*, or *income tax* compared to the CONTROL group. However, participants in the SOCIAL treatment want to increase the *capital gains tax* by 1.1 percentage points more than the CONTROL group (column (7)). Except for the

capital gains tax, respondents in the FISCAL treatment do not want to increase taxes overall but favor reductions in the *corporate tax* (-0.7 percentage points; column (1)), *business tax* (-1.4 percentage points; column (3)), and *income tax* (-0.6 percentage points; column (5)).[67] However, compared to the CONTROL group, firm decision-makers in the FISCAL treatment strongly prefer higher taxes to pay for government bailouts. This finding is consistent across different types of taxes and is statistically significant at the one percent level. The largest response is observed for the *capital gains tax*, where respondents in the FISCAL group want a 4.1 percentage point higher tax rate than respondents in the CONTROL group. Importantly, when comparing desired adjustments between firm decision-makers in the FISCAL and SOCIAL treatments, we find that decision-makers in the FISCAL treatment prefer significantly higher taxes than those in the SOCIAL treatment (p(FISCAL vs. SOCIAL) in Table 4.3). Results are robust to the inclusion of control variables.[68]

Given the level of current statutory tax rates, the treatment effects are substantial. For example, the *corporate tax* rate in Germany amounts to 15%, so a difference of 2.5 percentage points implies that respondents in the FISCAL group want to lower the tax rate by 17% less than respondents in the CONTROL group. The desired increase of the *capital gains tax* by 3.6 percentage points implies a 14% increase over the statutory *capital gains tax* of 25%.

Our experimental evidence provides three main findings. First, decision-makers in the FISCAL treatment want to decrease tax rates less than participants in the CONTROL group, even when controlling for support of the fiscal stimulus program and future tax expectations. Second, participants in the SOCIAL treatment seem to favor a higher *capital gains tax* compared to decision-makers in the CONTROL group. Otherwise, there are few differences in the responses between the CONTROL and SOCIAL groups. Third, on average,

[67] E.g., for the *corporate tax*, the average desired adjustment for the FISCAL group can be calculated as follows: -3.2 (CONTROL) + 2.5 (FISCAL vs. CONTROL) = -0.7 percentage points.

[68] Given the multiple treatments and outcomes, which can increase the likelihood of false rejections beyond the desired level, Appendix C.3.2 presents multiplicity-adjusted p-values following the approaches of List et al. (2019) and Holm (1979) (Table C.7). After accounting for multiple hypotheses testing, the results on treatment effects across the experimental groups remain robust, further strengthening confidence in the validity of the findings presented in Section 4.4.

firm decision-makers want to decrease all tax rates, except for the *capital gains tax*.

How can our results be explained? First, firm decision-makers seem to adjust their tax preferences when confronted with narratives emphasizing the need to counter-finance the government stimulus. This suggests that they do not fully account for the potential costs of the fiscal stimulus program and may not be aware of the need to finance such programs.

Second, the limited effect of the SOCIAL treatment on the preference to pay taxes suggests that, in our setting, firm decision-makers do not become more supportive of higher taxes after being exposed to narratives of misfortune in the income-generating process. This contrasts with findings from studies conducted among households (Durante et al., 2014; Fisman et al., 2020). A key difference between the FISCAL and SOCIAL treatments that might explain this result concerns the government's revenue sources. While the narrative in the FISCAL treatment makes it clear that government revenues are insufficient to cover the stimulus package expenditures, participants in the SOCIAL treatment are less aware of the need to finance the extraordinary expenses caused by the Corona crisis. Thus, decision-makers in the SOCIAL treatment might assume that government revenues will be sufficient to support firms in crises without the need for additional taxes.

Third, apart from the taxation of capital gains, firm decision-makers generally prefer to reduce tax rates. Our findings, which suggest that CEOs and business owners want to increase the *capital gains tax*, closely align with public statements by prominent business owners and CEOs advocating for a wealth tax.[69] There might be several explanations as to why decision-makers want to increase the *capital gains tax*. First, the preference to increase the *capital gains tax* might be driven by a willingness to redistribute, since the tax base for the *capital gains tax* is increments in wealth. Second, decision-makers might perceive the *capital gains tax* as less distortive to the firm production process than corporate or personal

[69] See, e.g., the public statements of business owners including Bill Gates and Warren Buffett on their preferences on tax policy and the repeated call for raising taxes on the wealthy (Buffett, 2011; Mohamed, 2020). The effects of a *capital gains tax* and a wealth tax can be considered similar, since the *capital gains tax* constitutes a tax on the returns to wealth, while the wealth tax constitutes a tax on the stock of wealth.

income taxes. Consequently, they may prefer not to increase taxes that, in their view, could potentially slow down economic activity.

To better understand the rationale underlying the wish to increase the *capital gains tax*, we conducted a small follow-up survey one month after the original survey. In the follow-up survey, a different sample of firms received the same treatment and were asked an open text question as to why they wanted to increase the *capital gains tax* if they indicated the preference to increase the *capital gains tax*. The main motive for increasing the *capital gains tax* are redistributive arguments, for example, making the wealthy pay their fair share. Given the small number of participants who filled out the open-ended text question, these results need to be interpreted with caution. Thus, we can only presume that redistribution considerations do matter for firm decision-makers' attitudes toward tax policy to some extent.

Overall, the results in this section indicate that firm decision-makers are more responsive to narratives of fiscal prudence than to those of redistribution in adjusting their preference to pay taxes in our setting. Extending previous results by Roth et al. (2022), we find that narratives are more effective in changing attitudes toward tax policy than hard facts. However, it appears that firm decision-makers only take into account considerations about redistribution to a limited extent when forming their attitudes toward tax policy. Nevertheless, there is evidence that redistribution concerns prompt decision-makers to increase taxes for the affluent, since participants of the SOCIAL treatment want to have higher *capital gains taxes* compared to firms in the CONTROL group.

4.5 Heterogeneity of Main Treatment Effect

In this section, we analyze the heterogeneity of the main treatment effect using two distinct empirical strategies. First, we employ a machine learning approach to explore treatment effect heterogeneity (Section 4.5.1). This data-driven, non-parametric method predicts treatment effects from observable characteristics in a theory-neutral framework, avoiding reliance on theoretical hypotheses for estimating cross-sectional treatment effects. Additionally, this

machine learning approach generates the distribution of conditional average treatment effects (CATEs) for each decision-maker and experimental group. This enables us to confirm that the treatment effects are not influenced by outliers and remain consistent across the majority of respondents within each treatment group. Second, we identify theoretically motivated predictions to test covariates that are expected to influence the observed treatment effects (Section 4.5.2).

4.5.1 Conditional Average Treatment Effects (CATEs) based on Causal Forests

As ATEs can conceal significant variation in treatment effects, examining treatment effect heterogeneity is crucial for enabling more precise intervention targeting and gaining a deeper understanding of the distributional impacts resulting from policy interventions (Athey et al., 2019; Nie & Wager, 2021; De Neve et al., 2021). In our context, a better understanding of treatment effect variation is essential for tax administrations. It enables them to customize enforcement actions, thereby enhancing both the precision and cost-effectiveness of their communication interventions.

To assess heterogeneity in treatment effects, we use the multiple-arm causal forest algorithm developed by Athey et al. (2019) and Nie and Wager (2021) to predict treatment effects non-parametrically. This data-driven machine learning approach offers the advantage of providing a distribution of estimated effect sizes for each participant and treatment, rather than a singular ATE point estimate. Moreover, the use of out-of-bag predictions – where models estimate outcomes for observations not included in the training data – ensures that only models not trained on the respective observations are used for effect size prediction. This approach minimizes the risk of overfitting. To conduct our analysis of treatment effect heterogeneity, we include the set of observables used in Section 4.4.3.[70] For the algorithm, we restrict the sample to all observations with non-missing values regarding our set of covariates.

[70] Set of observables: size group (EC's definition for small and medium-sized enterprises (SMEs)), industry (WZ08 1-digit), legal form, gender of manager, manager education, position in the company, a dummy equal to 1 if the decision-maker finds the stimulus justified and 0 otherwise, and medium-term tax expectations.

The basic idea of the multiple-arm causal forest is to estimate non-linear CATEs for each participant and each experimental treatment group. These non-linear CATE estimates are defined as follows:

$$\tau_k(x) = \mathbb{E}[Y_i(k) - Y_i(CONTROL) \mid X_i = x], \tag{9}$$

where $\tau_k(x)$ represents the respective CATE for the treatments SOCIAL and FISCAL ($k = \{\text{SOCIAL}, \text{FISCAL}\}$) relative to the CONTROL group, and $Y_i(k)$ and $Y_i(\text{CONTROL})$ indicate the potential outcome of respondent i depending on being in a treatment group ($Y_i(k)$, k = SOCIAL, FISCAL) or the CONTROL group ($Y_i(\text{CONTROL})$). Moreover, X_i is the covariate matrix including the set of observables described above, where x stands for the realization of the respective covariates for respondent i.

The main challenge in the estimation of equation (9) is that for a given observation, only one of the potential outcomes ($Y_i(k)$, $Y_i(\text{CONTROL})$) is observable. Nevertheless, one can estimate equation (9) by assuming *unconfoundedness* (Rosenbaum & Rubin, 1983), such that:

$$\{Y_i(k), Y_i(CONTROL)\} \perp\!\!\!\perp W_i \mid X_i. \tag{10}$$

In equation (10), W_i is a treatment indicator that denotes the experimental group to which the respondent was assigned (CONTROL, SOCIAL, or FISCAL). Given the assumption of unconfoundedness, the treatment assignment W_i can be considered independent of the potential outcome $Y_i(k)/Y_i(\text{CONTROL})$ conditional on covariates X_i. Since the treatment assignment in our setting is exogenous, the assumption of unconfoundedness is reasonable (Athey & Imbens, 2016).

Due to the assumption of unconfoundedness, we can use a causal forest algorithm (Athey

et al., 2019; Nie & Wager, 2021) that weights a collection of regression trees. The algorithm effectively performs local regressions within neighborhoods formed by these regression trees, based on observations with similar covariate characteristics. The treatment effect in a specific leaf of a regression tree is estimated by taking the average of all observations in this leaf belonging to the treatment group and subtracting the average of all observations in this leaf belonging to the control group (see equation (5) in Wager and Athey (2018)).[71] Moreover, using an adaptive nearest neighbor estimator, neighboring observations with similar covariate characteristics receive larger weights the more often they fall into the same leaf for a specific realization of covariates x. This is achieved by the causal forest averaging over the respective neighborhoods produced by the collection of regression trees. Furthermore, as we have more than one treatment group, we employ a generalization of causal forests for multi-arm causal forests developed by Nie and Wager (2021).

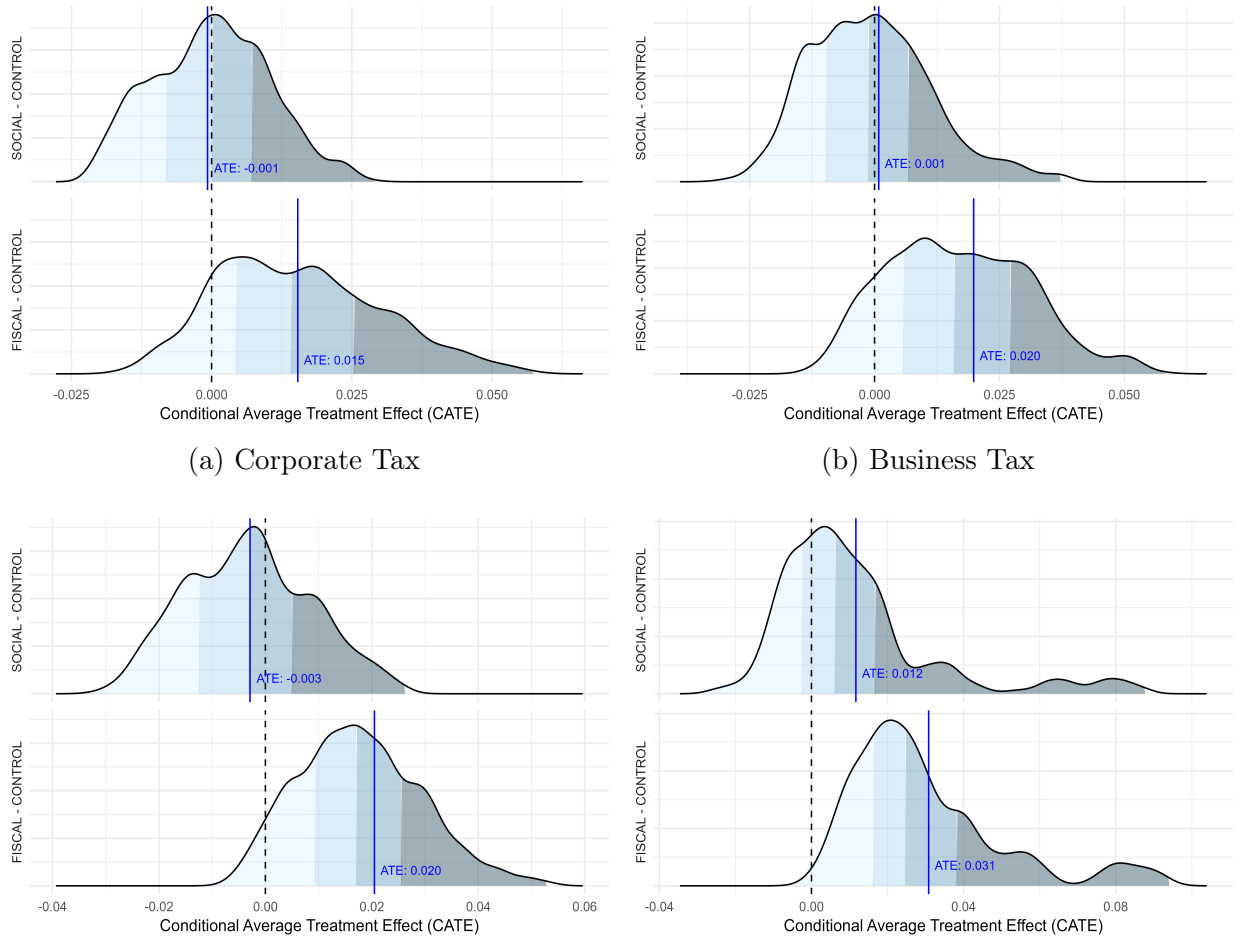
Figure 4.2 presents estimated distributions of CATEs for each respondent for the two experimental treatments, SOCIAL and FISCAL, relative to firms in the CONTROL group for the *corporate tax*, *business tax*, *income tax*, and *capital gains tax*.[72] The shaded regions represent the quartile ranges of each distribution. Figure 4.2 effectively highlights the heterogeneity in treatment effects based on the set of observables mentioned above. Moreover, controlling for the same set of covariates in a non-parametric manner in the causal forest provides valuable insights into the distributional impacts of our experimental interventions.

Three points merit attention. First, as expected, we observe that the ATEs based on the multi-arm causal forest approach are very close to the estimated ATEs based on OLS regressions for the respective taxes, as presented in columns (2), (4), (6), and (8) in Table 4.3. This gives us confidence that the estimated ATEs are robust with respect to different estimation approaches.

[71] We do not transform outcomes by applying propensity score weighting in each leaf, as in Athey and Imbens (2016), to account for variations in the expected propensity to be treated ($e(x) = \mathbb{E}[W_i \mid X_i = x]$). Instead, we follow Wager and Athey (2018) (see footnote 3) and estimate deep trees, where the leaves used to estimate treatment effects are smaller compared to the approach by Athey and Imbens (2016), making an additional transformation of our outcome variable unnecessary.

[72] The R package *grf* (Athey et al., 2019; Nie & Wager, 2021) was used to create Figure 4.2.

Figure 4.2: Distribution of Conditional Average Treatment Effects (CATEs) estimated by Causal Forest



(a) Corporate Tax

(b) Business Tax

(c) Income Tax

(d) Capital Gains Tax

Note: Figure 4.2 shows estimated distributions of conditional average treatment effects (CATEs) of the two experimental treatments SOCIAL and FISCAL relative to firms in the CONTROL group for the *corporate tax*, *business tax*, *income tax* and *capital gains tax*. The shaded regions represent the quartile ranges of each distribution. The covariates included in the causal forest estimation comprise the following variables, which are already part of the set of control variables in the Section 4.4.3: size group (EC's definition for small and medium-sized enterprises (SMEs)), industry (WZ08 1-digit), legal form, gender of manager, manager education, position in company, dummy equal to 1 if decision-maker finds stimulus justified and 0 otherwise and medium-term tax expectations. The R package *grf* (Athey et al., 2019; Nie & Wager, 2021) is used to create Figure 4.2.

Second, similar to the results in Section 4.4.3, we do not find an effect of the SOCIAL treatment relative to the CONTROL group for the *corporate tax*, *business tax*, and *income tax*. The estimated ATEs for these taxes are close to zero. Moreover, the median of the distribution of CATEs is also very close to zero, and the distribution is nearly symmet-

rically centered around a zero treatment effect. This finding supports the evidence from Section 4.4.3 that tax preferences of firm decision-makers are only influenced to a limited extent by redistribution considerations. However, the results for the *capital gains tax* indicate that redistribution concerns do matter to some extent, as decision-makers in the SOCIAL treatment want to increase the *capital gains tax* compared to participants in the CONTROL group based on comparable observable characteristics. Nearly three-quarters of the estimated CATEs are positive when comparing the difference between the SOCIAL and CONTROL group in Figure 4.2d.

Third, when investigating the difference between the FISCAL and CONTROL group, we observe that for all four types of taxes, more than three-quarters of the estimated CATEs are positive. This indicates that the majority of firm decision-makers in the FISCAL treatment have a higher preference to pay taxes compared to their counterparts in the CONTROL group. Therefore, we conclude that presenting the fiscal stimulus as a *due debt*, thereby clarifying the contractual relationship between firms and the government, leads to greater fiscal responsibility in our context.

In sum, the analysis reveals that the estimated ATEs using the multi-arm causal forest approach align closely with those from OLS regressions, confirming the robustness of our results. Our non-parametric machine learning approach demonstrates that, while redistribution considerations in the SOCIAL treatment show limited impact on most taxes (except for the *capital gains tax*), the FISCAL treatment significantly enhances tax payment preferences, indicating the effectiveness of framing fiscal stimuli as *due debt*.

4.5.2 Heterogeneous Treatment Effects based on Hypotheses

In this section, we investigate whether our experimental treatments have differential effects on tax payment preferences across different types of firms, based on two specific hypotheses.

First, we explore whether the attitude of firm decision-makers toward the fiscal stimulus program influences their preferences regarding taxes. We anticipate that this effect will

be particularly pronounced in the FISCAL treatment, where decision-makers recognize the need to counter-finance the government stimulus. Consequently, if they deem the stimulus justified and are simultaneously in favor of keeping government debt at reasonable levels, they are likely to adjust their preference to pay taxes upwards (Alm et al., 1993; Feld & Frey, 2007).

Second, we test whether preferences toward specific tax changes depend on whether firms are required to pay the respective tax. To investigate this, we examine heterogeneous effects among firms with different legal forms. For example, unlike incorporated firms, non-incorporated firms, such as sole proprietorships and business partnerships, are not subject to the *corporate tax*.[73] We therefore examine whether the desired change in the *corporate tax* depends on the incorporation status of the firm. We expect corporations to have a lower preference for paying the *corporate tax* compared to non-incorporated firms if firms display a certain degree of self-interest regarding policies that might affect them, similar to findings on household climate policy preferences (Dechezleprêtre et al., 2025). However, the narratives put forward in the SOCIAL and FISCAL treatments might dampen this effect, as redistribution and fiscal prudence considerations could offset the incentive to reduce a firm's tax burden.

To test for these differential effects, we run separate OLS regressions of the following form:

$$y_i = \alpha + \beta \times FirmType_i + \sum_{k=1}^{2} \delta_k \times TREATMENT_{ik} + \sum_{k=1}^{2} \theta_k \times FirmType_i \times TREATMENT_{ik} + \varepsilon_i. \quad (11)$$

Like before, the dependent variable y_i represents the desired tax rate adjustment of the re-

[73] In Germany, firms with legal forms such as *GmbH*, *UG*, *AG*, *SE*, or *Genossenschaft* are subject to the *corporate tax*. In contrast, firms with legal forms such as *Einzelunternehmen*, *oHG*, *GbR*, *PartG*, *KG*, and *Personengesellschaft* are non-incorporated and therefore not subject to the *corporate tax*.

spective tax. The categorical variable $FirmType_i$ is used to test for potential heterogeneous treatment effects we are interested in. To test the two channels described above, $FirmType_i$ is defined in the following two ways. First, when looking at differences in tax changes depending on the attitude toward the fiscal stimulus, $FirmType_i$ is a dummy variable equal to one if firm decision-makers find the fiscal stimulus *Justified* or *Absolutely Justified* and zero otherwise. Second, in the case of different legal forms, $FirmType_i$ is a categorical variable equal to one for corporations and zero for non-incorporated businesses. $Treatment_i \times FirmType_i$ represents the full interaction between the respective treatment received and the respective firm type.

Table 4.4 presents the group average treatment effects (GATEs) for firm decision-makers with different attitudes toward the fiscal stimulus. We observe that firm decision-makers, who find the government stimulus justified, have a higher preference to pay taxes compared to their counterparts. Further, we notice heterogeneity across treatments. While the difference between the GATEs is positive for all experimental groups, the effect is particularly strong (significant at the 1% level) for the FISCAL treatment. The economic magnitude of this effect is large. For example, with a 15% flat statutory corporate tax, a 2-percentage-point difference implies a 13% higher desired tax rate for decision-makers in the FISCAL treatment who find the stimulus justified, as compared to decision-makers in the same experimental group, who have a neutral perspective or perceive the fiscal stimulus as unjust. The strong effect of the FISCAL treatment is also present for the other three types of taxes (*business tax*, *income tax*, and *capital gains tax*).

We conclude that firm decision-makers, who support the fiscal stimulus program, have a higher preference to pay taxes. This effect is particularly strong when decision-makers realize the need for the government to counter-finance the expenditures such that the state can support firms in crises. Our findings remain robust when defining the support for the fiscal stimulus program as a continuous variable (Table C.10 in Appendix C.4.2).

In Table 4.5, we investigate if treatment effects differ with respect to the type of tax a

Table 4.4: Heterogeneous Effects by Attitude toward Fiscal Stimulus

	Estimated GATE (SE in parentheses)		
	CONTROL	SOCIAL	FISCAL
Corporate Tax			
Not Justified or Neutral	-0.037***	-0.041***	-0.019***
	(0.009)	(0.006)	(0.003)
Justified	-0.031***	-0.030***	0.002
	(0.004)	(0.002)	(0.002)
Difference: Justified - Not Justified or Neutral	0.006	0.010*	0.020***
	(0.010)	(0.006)	(0.004)
Business Tax			
Not Justified or Neutral	-0.041***	-0.050***	-0.026***
	(0.009)	(0.006)	(0.003)
Justified	-0.039***	-0.041***	-0.006**
	(0.004)	(0.002)	(0.002)
Difference: Justified - Not Justified or Neutral	0.002	0.009	0.020***
	(0.010)	(0.006)	(0.004)
Income Tax			
Not Justified or Neutral	-0.032***	-0.051***	-0.022***
	(0.009)	(0.006)	(0.003)
Justified	-0.032***	-0.032***	0.005**
	(0.004)	(0.002)	(0.002)
Difference: Justified - Not Justified or Neutral	0.001	0.019***	0.027***
	(0.010)	(0.006)	(0.004)
Capital Gains Tax			
Not Justified or Neutral	-0.019*	-0.013*	0.019***
	(0.011)	(0.006)	(0.004)
Justified	0.001	0.010***	0.048***
	(0.004)	(0.003)	(0.003)
Difference: Justified - Not Justified or Neutral	0.020*	0.023***	0.030***
	(0.012)	(0.007)	(0.005)

Note: Table 4.4 presents estimated group average treatment effects (GATEs) for the *corporate tax*, *business tax*, *income tax* and *capital gains tax* for firms varying by their attitude toward the fiscal stimulus. Estimated GATEs are based on the regression in column (1) (*corporate tax*), column (2) (*business tax*), column (3) (*income tax*) and column (4) (*capital gains tax*) shown in Table C.9 in Appendix C.4.2. The regressions are based on equation (11): $y_i = \alpha + \beta \times Justified_i + \sum_{k=1}^{2} \delta_k \times TREATMENT_{ik} + \sum_{k=1}^{2} \theta_k \times Justified_i \times TREATMENT_{ik} + \varepsilon_i$. Dependent variable: desired adjustment in percentage points of *corporate tax*, *business tax*, *income tax* and *capital gains tax*. Independent variables: experimental group dummy, dummy variable $Justified_i$ equal to one for firm decision-makers finding the fiscal stimulus absolutely justified or justified and zero otherwise, and a constant. The row *Difference* presents the difference between the GATE of firms finding the stimulus package justified compared to firms with a neutral or opposing attitude toward the stimulus package for the respective treatment groups (slight differences due to rounding). Robust standard errors are in parentheses. Significance levels are: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

firm is liable to. As mentioned above, only corporations are subject to the *corporate tax*. Therefore, we expect a lower preference to pay the *corporate tax* for corporations compared to non-incorporated firms. The GATEs presented in Table 4.5 support this hypothesis.

Incorporated firms want a significantly lower *corporate tax* compared to non-incorporated firms.[74] Over all experimental groups, this effect is highly significant and the difference between incorporated and non-incorporated firms ranges from 1.6 percentage points to 3.1 percentage points. We also observe that the absolute difference between incorporated and non-incorporated firms becomes weaker in the SOCIAL (1.6 % percentage points) and FISCAL (1.8 % percentage points) group as compared to the CONTROL group (3.1 % percentage points). This finding suggests that redistribution and fiscal prudence narratives, with which decision-makers are confronted in the SOCIAL and FISCAL treatment, can be an effective tool to dampen a firm's self-centered interest. However, across all experimental groups, we observe that self-interest plays a role, supporting household findings regarding climate policy preferences (Dechezleprêtre et al., 2025).

Table 4.5: Heterogeneous Effects by Legal Form

Corporate Tax:	Estimated GATE (SE in parentheses)		
	CONTROL	SOCIAL	FISCAL
Non-incorporated	-0.008	-0.020***	0.009
	(0.008)	(0.007)	(0.006)
Incorporated	-0.038***	-0.035***	-0.010***
	(0.004)	(0.003)	(0.002)
Difference: Incorporated - Non-incorporated	-0.031***	-0.016**	-0.018***
	(0.009)	(0.007)	(0.006)

Note: Table 4.5 presents estimated group average treatment effects (GATEs) for the *corporate tax* for firms varying by legal form. Estimated GATEs are based on the regression in column (1) shown in Table C.8 in the Appendix C.4.1. The regressions are based on equation (11): $y_i = \alpha + \beta \times Corporation_i + \sum_{k=1}^{2} \delta_k \times TREATMENT_{ik} + \sum_{k=1}^{2} \theta_k \times Corporation_i \times TREATMENT_{ik} + \varepsilon_i$. Dependent variable: desired adjustment of corporate tax in percentage points. Independent variables: experimental group dummy, dummy variable $Corportation_i$ equal to one for corporations and zero for non-incorporated firms, and a constant. The row *Difference* presents the difference between the GATE of incorporated firms compared to non-incorporated firms for the respective treatment groups (slight differences due to rounding). Robust standard errors are in parentheses. Significance levels are: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[74] Importantly, when only considering corporations, the FISCAL treatment still leads to a positive and highly significant effect for the *corporate tax* compared to the CONTROL group (difference: 2.8 percentage points; p-value: 0.000).

Moreover, in Appendix C.4, we present additional analyses investigating the heterogeneous effects on firms that received government aid or experienced different profit trajectories since the onset of the pandemic. Our findings indicate that, on average, firms that accepted government aid or faced negative profit trajectories since the pandemic began show a decreased preference for paying taxes. This may be because these firms undervalue public services, perceiving the government's crisis aid as inadequate or insufficient. Additionally, firms experiencing significant profit downturns might be facing tighter financial constraints, prompting them to seek lower tax obligations compared to their more financially stable counterparts. We do not find significant differences based on firm size.

4.6 Conclusion

Firms are important taxpayers and intermediaries, making it essential to understand the tax preferences of the people who run them. Our study sheds new light on the dynamics between tax policy narratives and the tax preferences of firm decision-makers. By showing how narratives about fiscal consolidation or redistribution shape these decision-makers' beliefs, our research contributes to the understanding of how firms respond to policy communication, particularly in the context of unexpected economic challenges like the Covid-19 pandemic.

Our findings reveal a nuanced picture of how firm decision-makers react to different types of narratives. We observe that when confronted with a fiscal consolidation narrative emphasizing fiscal prudence and the need to counter-finance government expenditures, there is a notable shift in firms' preferences to accept higher taxes. This suggests a responsiveness to narratives that highlight the fiscal realities and constraints faced by the government. Conversely, a redistribution narrative centered on fairness and misfortune in the income-generating process appears to have limited impact on altering tax preferences among firm decision-makers, except for a notable tendency to favor raising the *capital gains tax*.

Regarding our results, there are several implications. For policymakers and tax administrations, the findings underscore the importance of crafting effective communication

strategies that resonate with the realities and perceptions of businesses and their managers. These aspects gain particular importance in securing support for policy changes, during election campaigns, and in pursuing fiscal consolidation objectives. The effectiveness of the fiscal consolidation narrative in boosting firms' preference to pay taxes suggests that transparent and realistic communication about government budgets and spending can play a crucial role in fostering firms' willingness to pay taxes. At the same time, our results caution against overreliance on redistribution narratives when communicating with firms. Finally, the observed preference for an increase in the *capital gains tax* opens avenues for policymakers to consider more progressive taxation structures. This aligns with a growing global discourse on economic equity, particularly in the wake of financial crises and economic downturns.

5 Summary of Main Findings

How can survey research shed light on the inner workings of firms? This dissertation demonstrates how survey experiments can serve as a valuable tool for academic research in taxation and economics by focusing on firms – a hard-to-reach population for which survey evidence remains scarce. Drawing on empirical evidence from diverse firm-level contexts, the dissertation illustrates how targeted surveys can generate novel insights into firm behavior.

Chapter 2 explores how changes in profit taxation influence managerial decision-making – an essential channel for understanding the economic incidence of such taxes. Behavioral reactions play a central role in assessing tax progressivity and its welfare implications. Yet, despite its importance for public policy, there remains little empirical evidence on how profit tax changes are distributed across a firm's various stakeholders.

Drawing on data from a survey experiment, we find that the incidence of profit tax changes is strikingly asymmetric across adjustment margins. In the case of tax increases, the burden falls predominantly on firm owners and consumers. By contrast, tax cuts primarily benefit workers and finance new investment. Notably, the share of the burden or benefit accruing to workers rises with the magnitude of the tax change, partially offsetting the impact on firm owners.

These results imply that profit tax changes are progressive in both directions: tax hikes disproportionately affect owners, while tax reductions favor workers. Since our survey experiment focuses on the short-run adjustments of firms to profit tax changes, further research is essential to trace the dynamic effects of firm-level responses on future pre-tax profits, as this is key to assessing the full economic incidence of taxation.

Chapter 3 examines whether central bank communication – in the form of inflation forecasts – affects firms' pricing plans in a high-inflation environment. It addresses a key policy question: Is it possible to manage inflation dynamics through targeted communication with firms?

Using a randomized survey experiment with German firms in 2022, the chapter analyzes

how firms revise their planned price changes in response to official inflation forecasts from the German central bank. In the control group, firms are reminded of their own inflation expectations. In the treatment groups, firms see both their own expectations and the central bank's projections – either a general inflation forecast or one that also includes forecasts for energy or labor costs.

The results show that central bank forecasts significantly reduce firms' planned price increases. This effect holds also when additional cost information is included. Impacts are strongest for firms with high pre-treatment expectations and those previously inattentive to inflation, suggesting belief updating rather than priming. Lastly, control group firms exhibit a near one-to-one relationship between inflation expectations and pricing plans, highlighting strong pass-through in high-inflation settings. These findings suggest that inflation forecast communication can be an effective tool for curbing inflationary dynamics.

Chapter 4 investigates how economic policy narratives influence firm decision-makers' preferences for taxation. Motivated by the increasing public engagement of business leaders on policy issues, the chapter explores why some firm decision-makers support tax policies that appear to go against their financial interests. Using a survey experiment with German firms, it tests whether narratives emphasizing fiscal consolidation or redistribution can shape preferences for tax increases to finance a large fiscal stimulus package.

The results show that narratives stressing the need for fiscal responsibility – framing taxes as necessary to maintain balanced government budgets – significantly increase firms' willingness to support higher taxes. In contrast, redistribution-focused narratives emphasizing misfortune and external shocks have little overall effect, with the exception of capital gains taxes, where firms show increased support. This response appears driven by a desire to redistribute or by the lower perceived economic cost of taxing capital gains compared to other tax types.

Further analysis shows that decision-makers more supportive of the stimulus exhibit greater willingness to fund it when fiscal trade-offs are made explicit through the fiscal

consolidation narrative. Firms are also more likely to favor tax increases on taxes they do not personally bear, although both narratives tend to reduce this self-interested tendency. These findings suggest that policy communication highlighting fiscal realities is more effective than fairness-based appeals in influencing firms' tax preferences. This provides valuable guidance for crafting communication strategies in times of increased public spending and fiscal pressure.

Survey data offer unique insights, particularly into beliefs, perceptions, and expectations that administrative data cannot capture. The strength of survey data lies in making the intangible visible, enabling researchers to address questions that are difficult or impossible to study through revealed preference approaches alone (Stantcheva, 2023). By allowing researchers to design data-generating processes and introduce controlled variation, surveys open up rich possibilities for causal inference (Haaland et al., 2023). Like any method, they are most effective when used with a clear understanding of their strengths and limitations. As demonstrated throughout this dissertation, survey methods yield valuable insights into how firms respond to policies and information, offering a window into the behavioral mechanisms that shape corporate decision-making – insights that are otherwise difficult to obtain.

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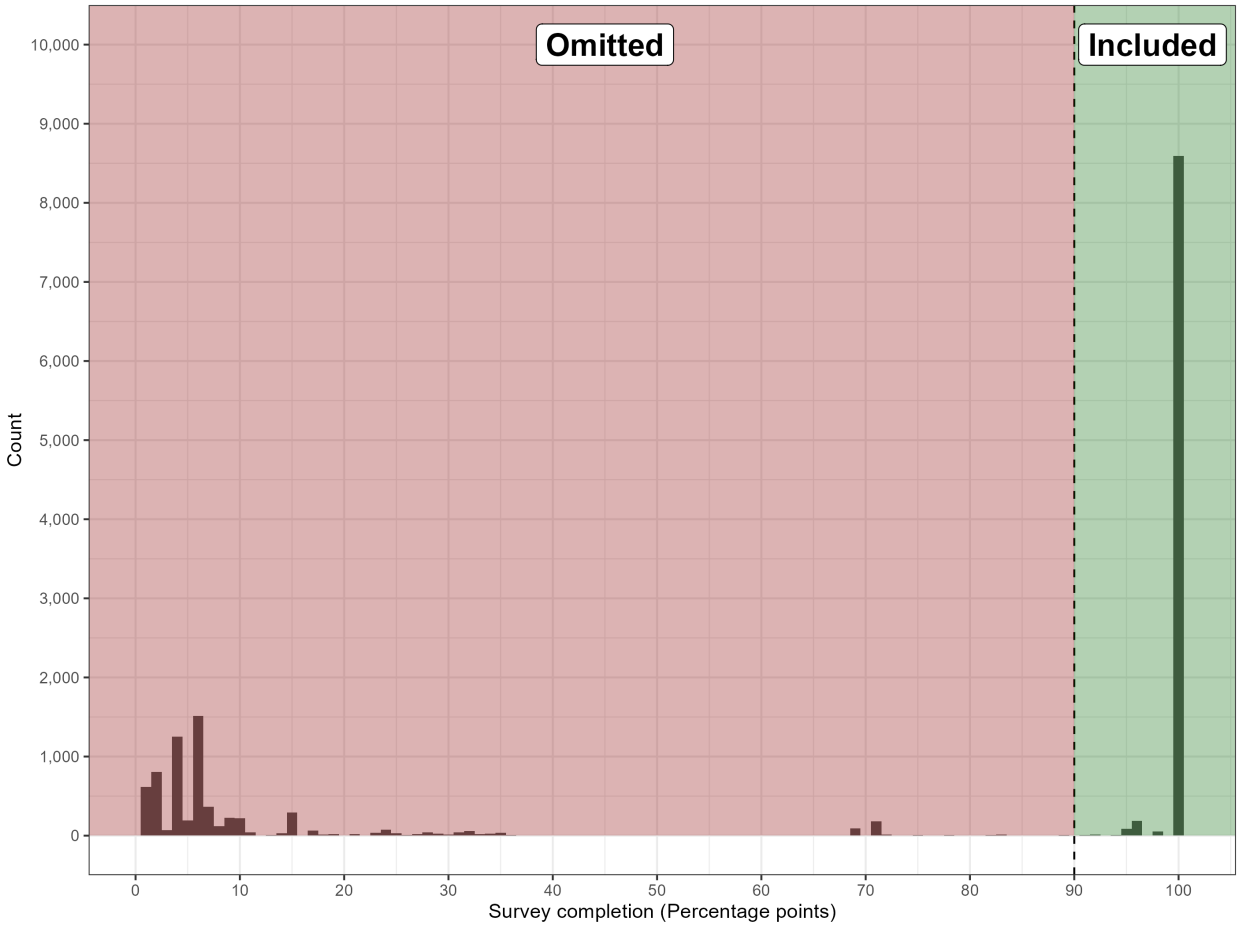
A Appendix to Chapter 2

A.1 Completion Rate

Figure A.1 illustrates the completion rates of survey respondents. Responses with a completion rate below 90% (shaded in red) are excluded from the analysis, while all responses meeting or exceeding the 90% threshold ($N = 8{,}955$) are retained (shaded in green). The final sample used in the main analysis ($N = 6{,}749$) consists of these high-completion responses, further refined to exclude observations with missing values for control and weighting variables.

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

Figure A.1: Completion Rate



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

A.2 Screenshots of Original Survey Questions

A.2.1 Tax Decrease Treatments

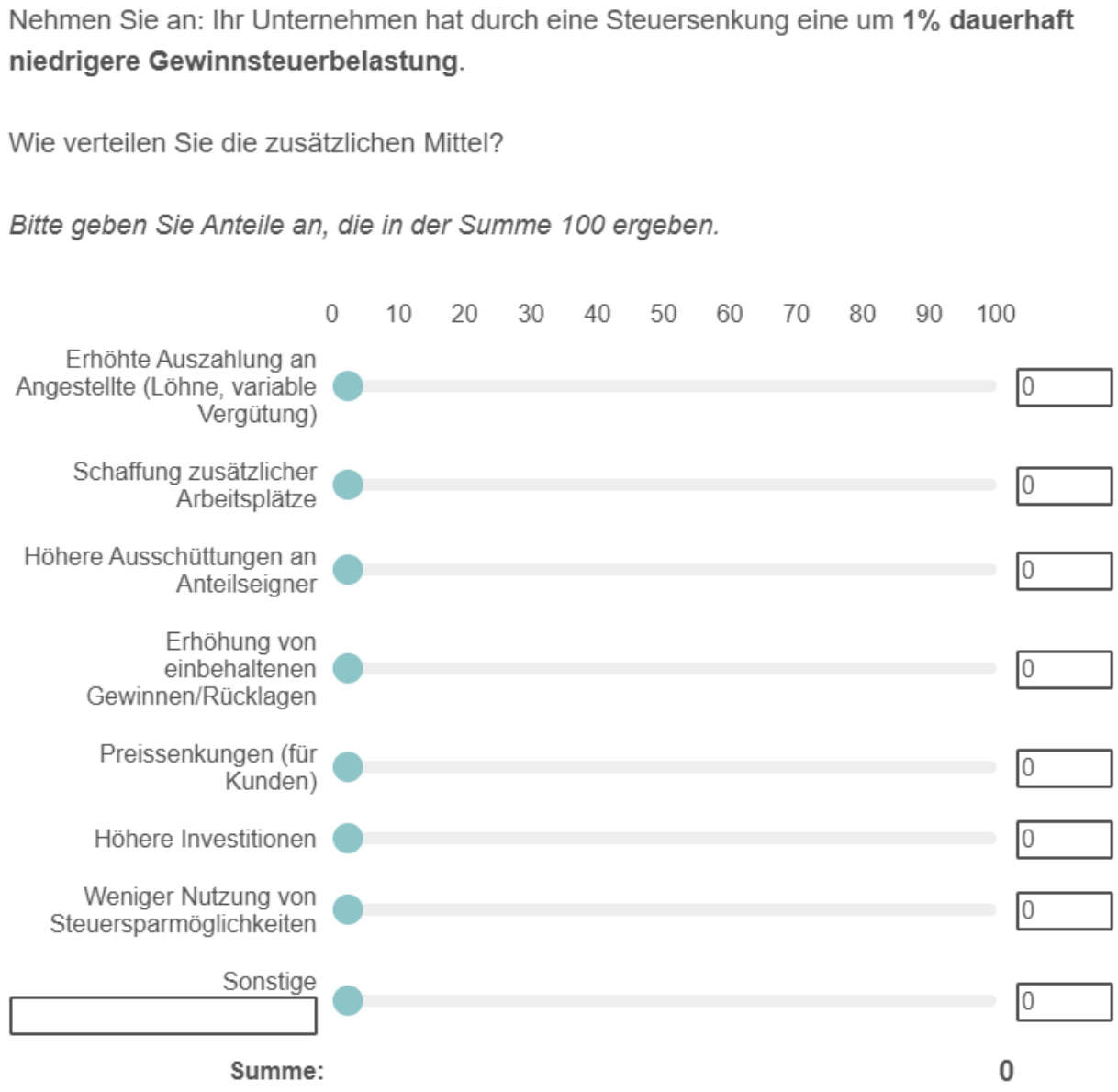
Question:

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

Answer Options:

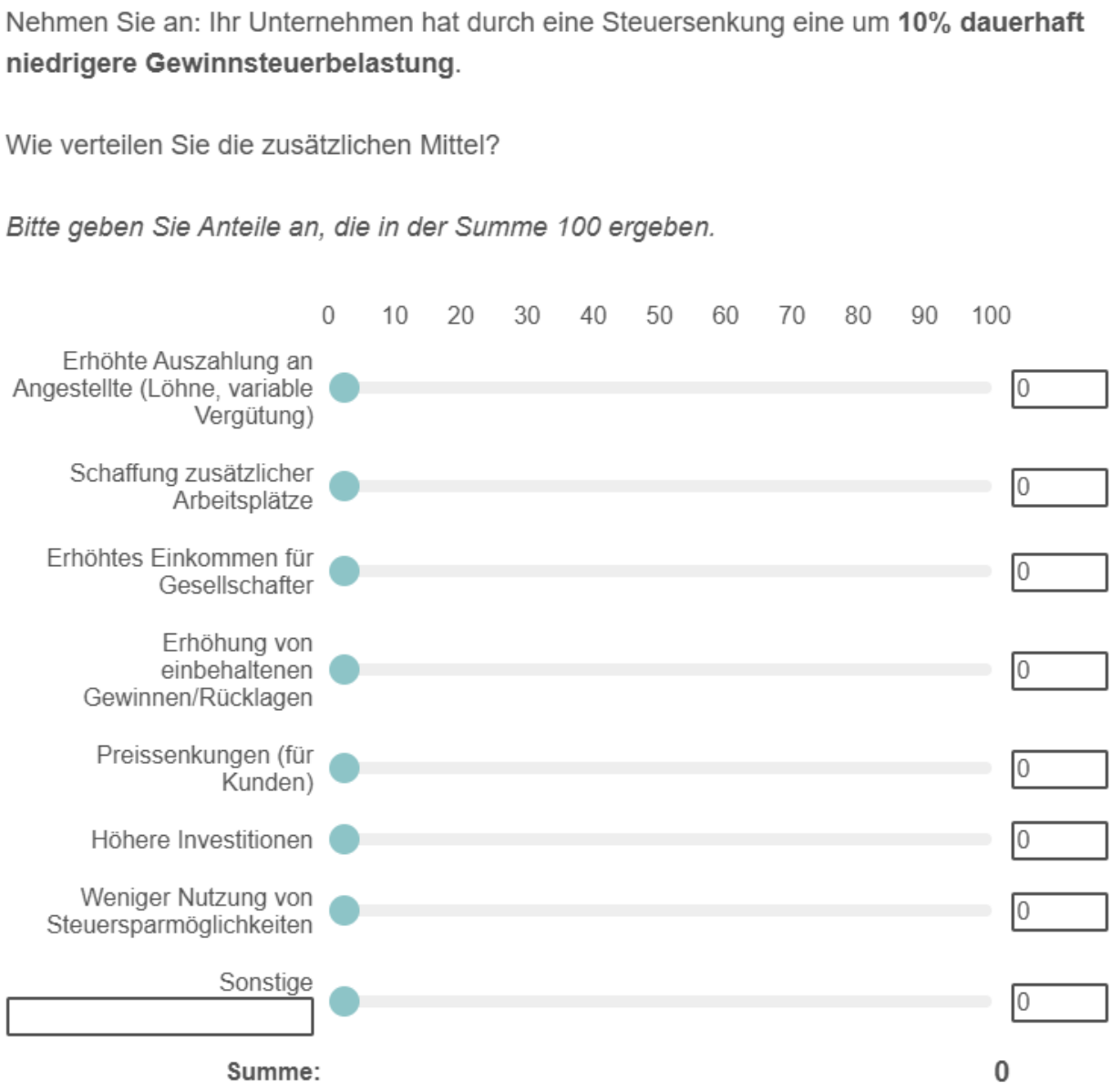
- Increased payment to employees
- Creation of additional jobs
- Higher distributions to partners (for non-corporations)
- Higher distributions to shareholders (for corporations)
- Increase in retained earnings/reserves
- Price reductions (for customers)
- Higher investments
- Less use of tax saving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

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



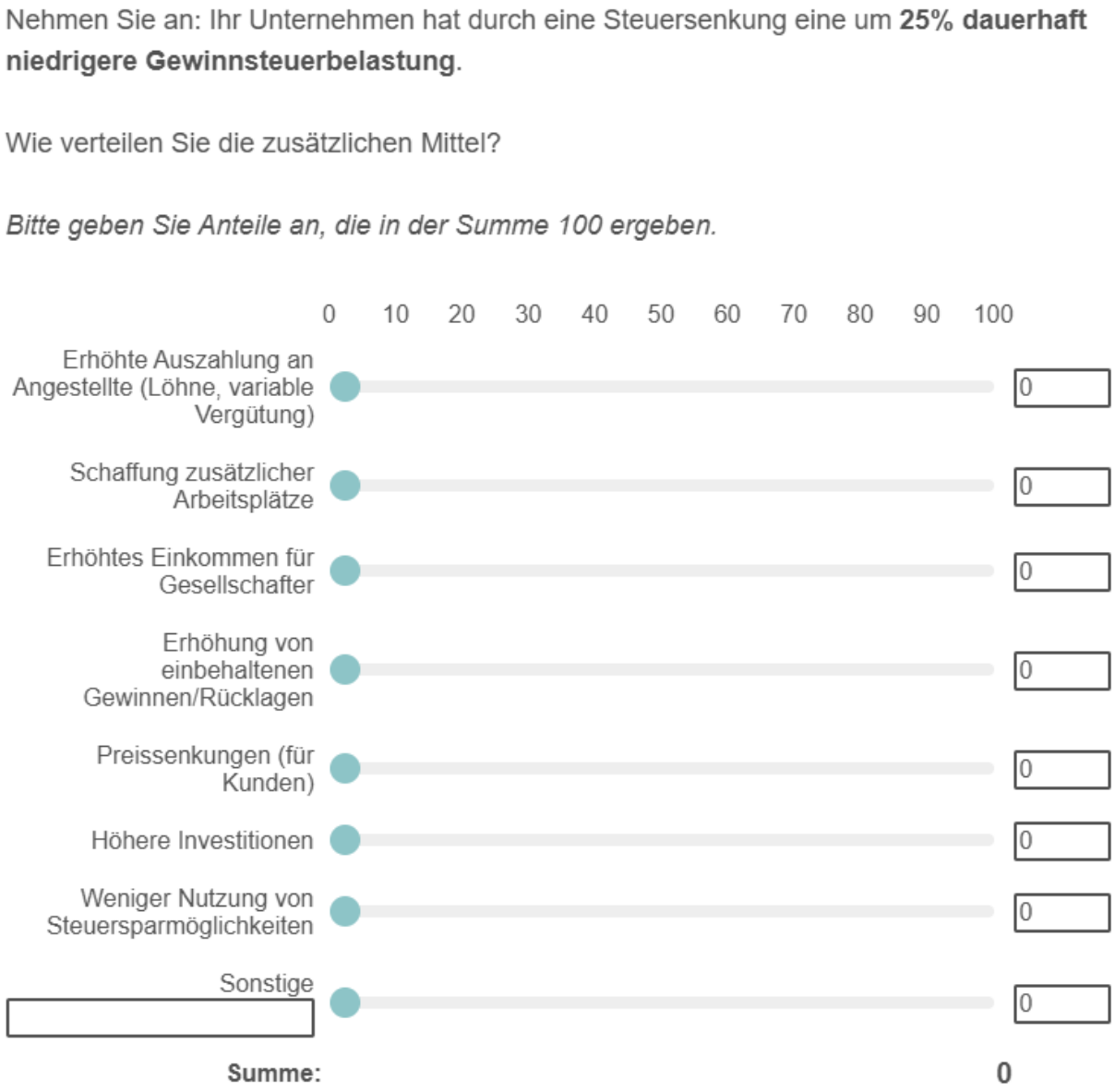
Note: Figure A.2 shows an example of the tax decrease (1%) survey experiment as appearing in the web survey of the GBP. After the hypothetical treatment (*"Assume that your company has a (1%/10%/25%)* ***permanently lower profit tax burden*** *as a result of a tax decrease. How do you distribute the additional funds?"*), the respondent was asked how the additional funds would be distributed and notified that entered shares must add up to 100. The respondent then could attribute shares to the categories listed in Table 2.1 either via adjusting the sliders or entering them directly in the boxes to the right. Shares were initially set to zero for all categories.

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



Note: Figure A.3 shows an example of the tax decrease (10%) survey experiment as appearing in the web survey of the GBP. After the hypothetical treatment (*"Assume that your company has a (1%/10%/25%)* ***permanently lower profit tax burden*** *as a result of a tax decrease. How do you distribute the additional funds?"*), the respondent was asked how the additional funds would be distributed and notified that entered shares must add up to 100. The respondent then could attribute shares to the categories listed in Table 2.1 either via adjusting the sliders or entering them directly in the boxes to the right. Shares were initially set to zero for all categories.

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



Note: Figure A.4 shows an example of the tax decrease (25%) survey experiment as appearing in the web survey of the GBP. After the hypothetical treatment (*"Assume that your company has a (1%/10%/25%)* ***permanently lower profit tax burden*** *as a result of a tax decrease. How do you distribute the additional funds?"*), the respondent was asked how the additional funds would be distributed and notified that entered shares must add up to 100. The respondent then could attribute shares to the categories listed in Table 2.1 either via adjusting the sliders or entering them directly in the boxes to the right. Shares were initially set to zero for all categories.

A.2.2 Tax Increase Treatment.

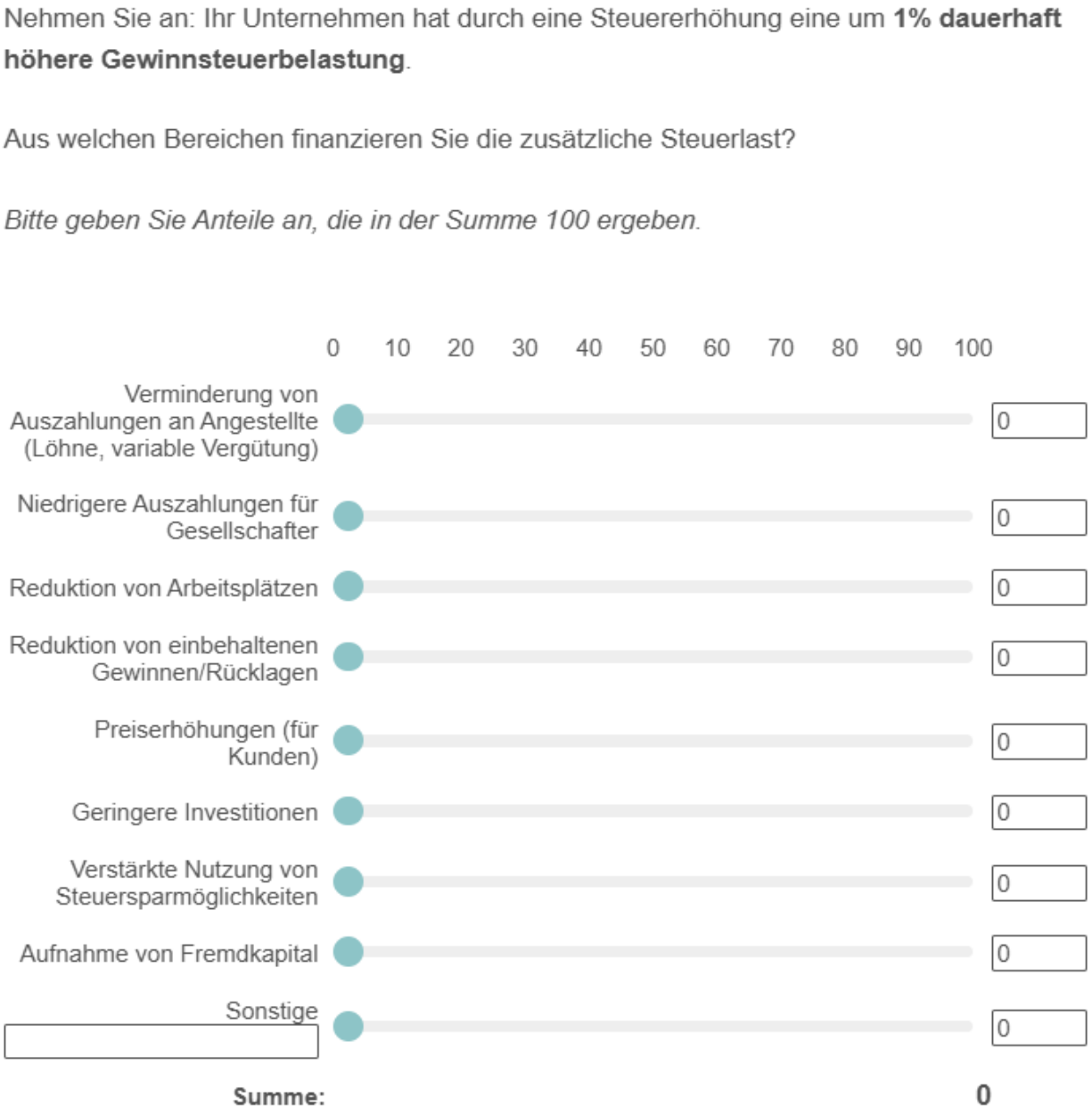
Question:

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

Answer Options:

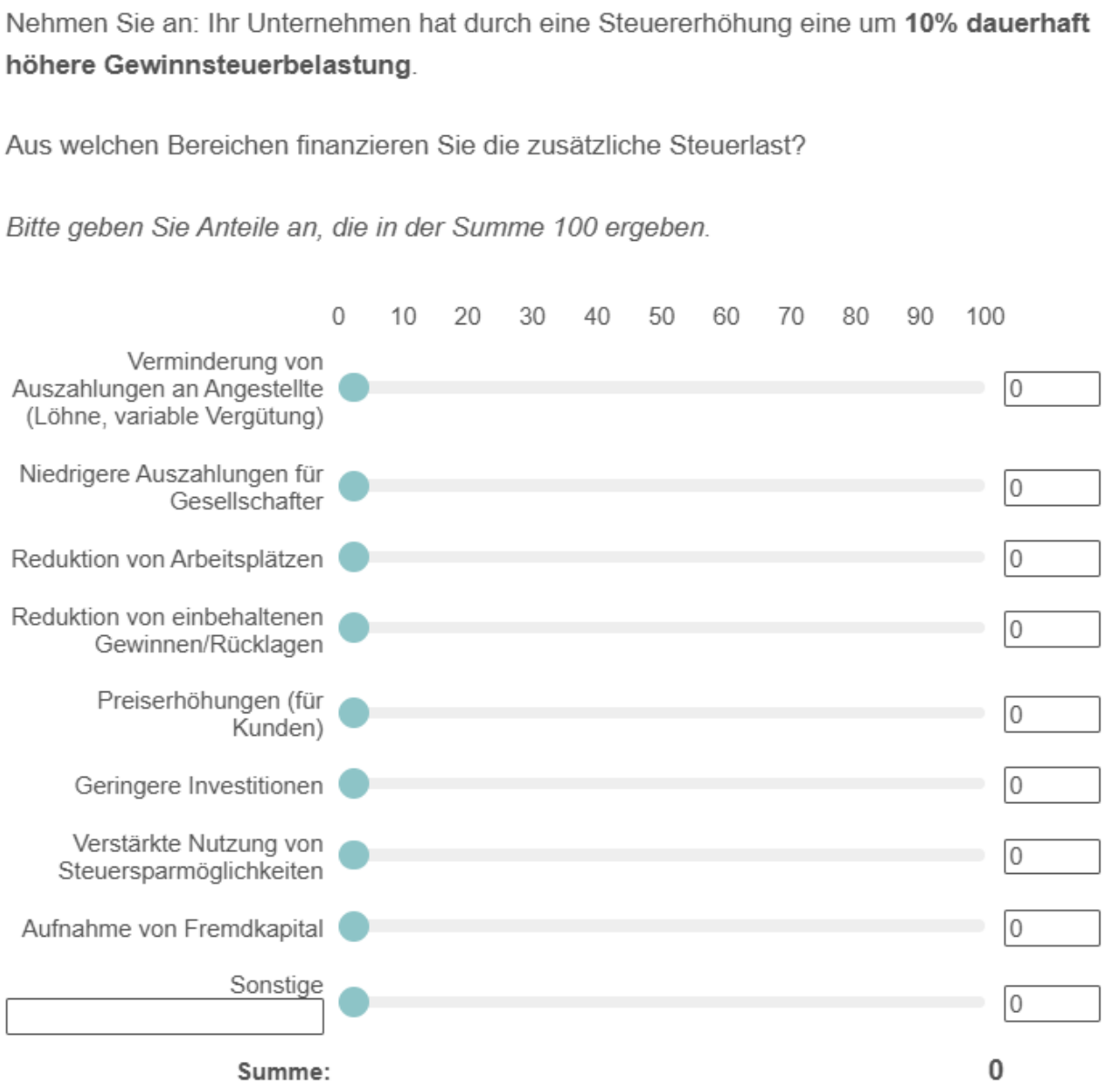
- Decreased payment to employees
- Lower distributions to partners (for non-corporations)
- Lower distributions to shareholders (for corporations)
- Reduction of jobs
- Decrease in retained earnings/reserves
- Price increases (for customers)
- Lower investments
- More use of tax saving opportunities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

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



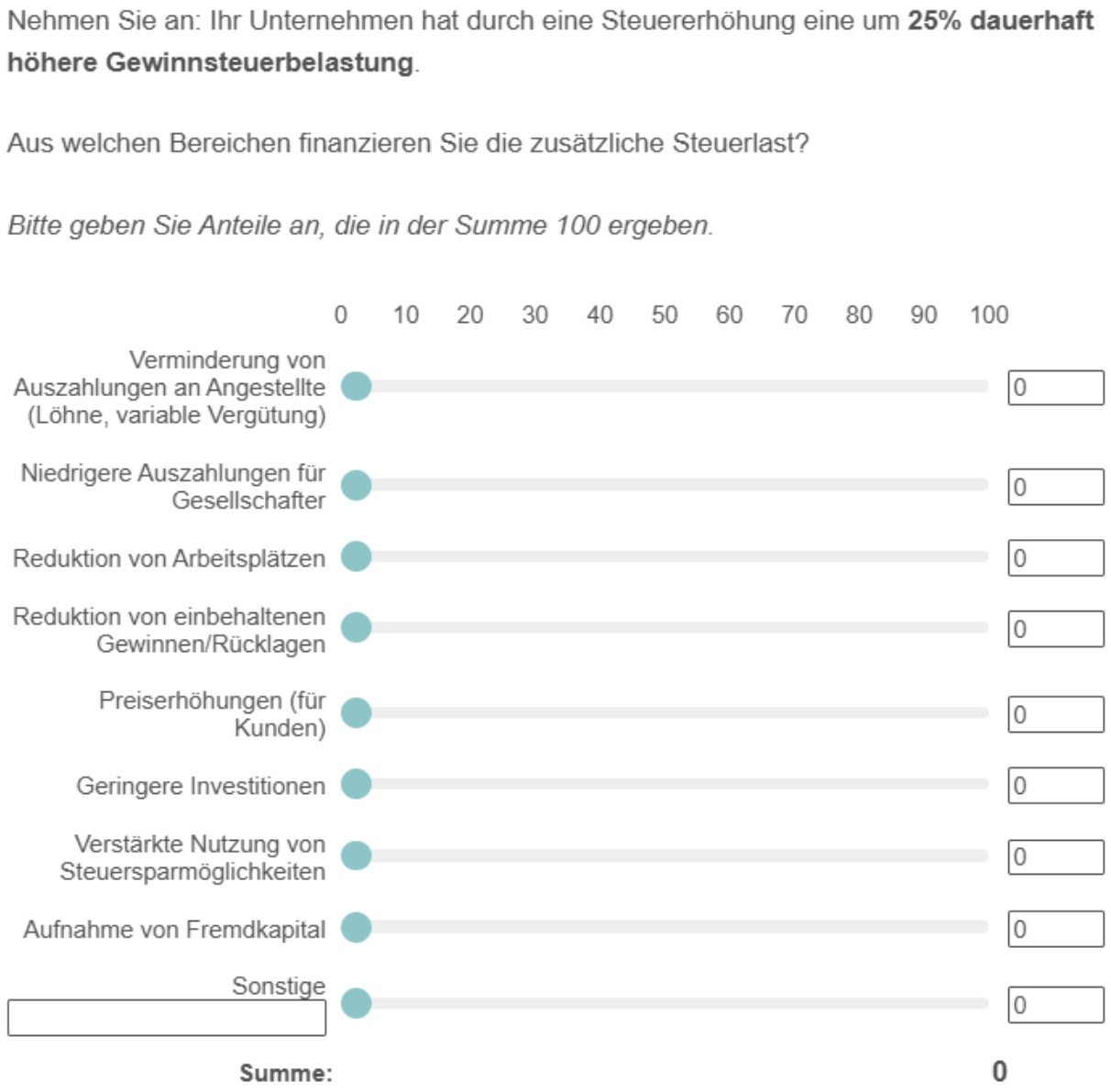
Note: Figure A.5 shows an example of the tax increase (1%) survey experiment as appearing in the web survey of the GBP. After the hypothetical treatment (*"Assume that your company has a (1%/10%/25%)* ***permanently higher profit tax burden*** *as a result of a tax increase. How do you finance the additional burden?"*), the respondent was asked how the additional funds would be distributed and notified that entered shares must add up to 100. The respondent then could attribute shares to the categories listed in Table 2.1 either via adjusting the sliders or entering them directly in the boxes to the right. Shares were initially set to zero for all categories.

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



Note: Figure A.6 shows an example of the tax increase (10%) survey experiment as appearing in the web survey of the GBP. After the hypothetical treatment (*"Assume that your company has a (1%/10%/25%)* ***permanently higher profit tax burden*** *as a result of a tax increase. How do you finance the additional burden?"*), the respondent was asked how the additional funds would be distributed and notified that entered shares must add up to 100. The respondent then could attribute shares to the categories listed in Table 2.1 either via adjusting the sliders or entering them directly in the boxes to the right. Shares were initially set to zero for all categories.

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



Note: Figure A.7 shows an example of the tax increase (25%) survey experiment as appearing in the web survey of the GBP. After the hypothetical treatment (*"Assume that your company has a (1%/10%/25%)* ***permanently higher profit tax burden*** *as a result of a tax increase. How do you finance the additional burden?"*), the respondent was asked how the additional funds would be distributed and notified that entered shares must add up to 100. The respondent then could attribute shares to the categories listed in Table 2.1 either via adjusting the sliders or entering them directly in the boxes to the right. Shares were initially set to zero for all categories.

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

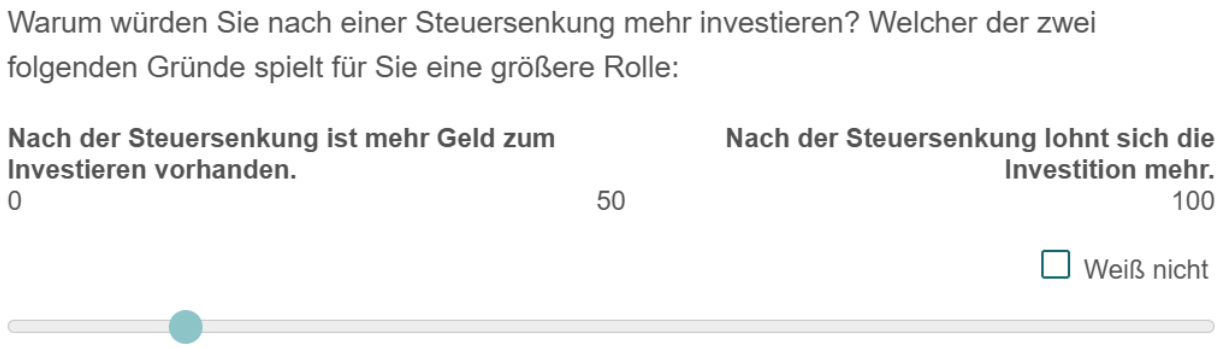
Question:

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

Answer Options - Slider: [0,100]

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

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



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

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

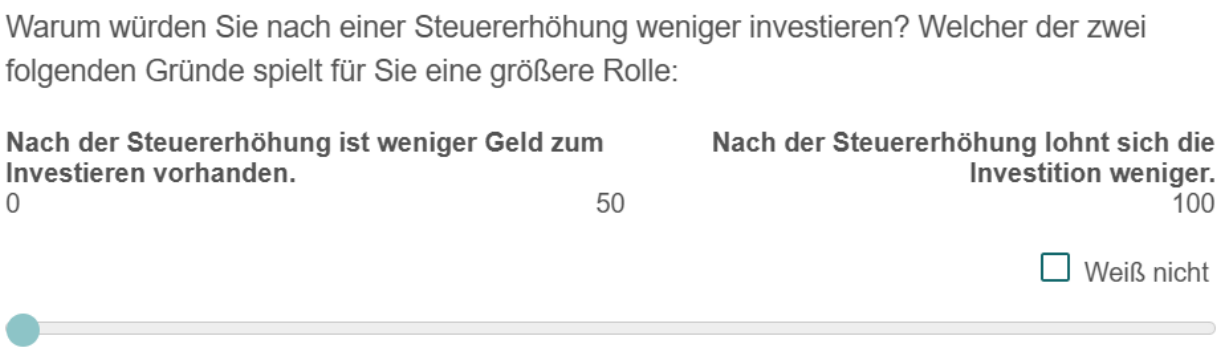
Question:

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

Answer Options - Slider: [0,100]

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

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



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

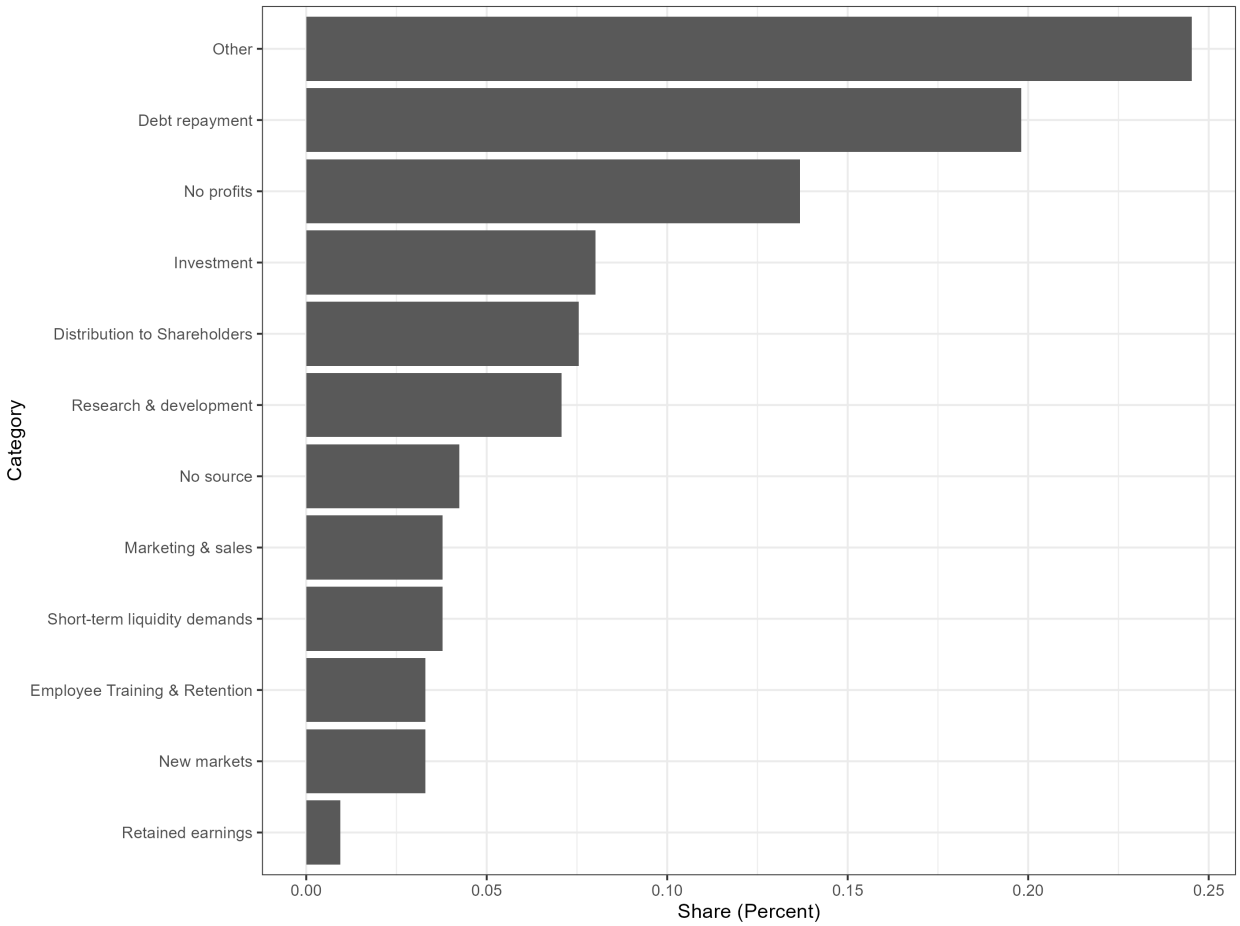
A.3 Text Entries - Other Category

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

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

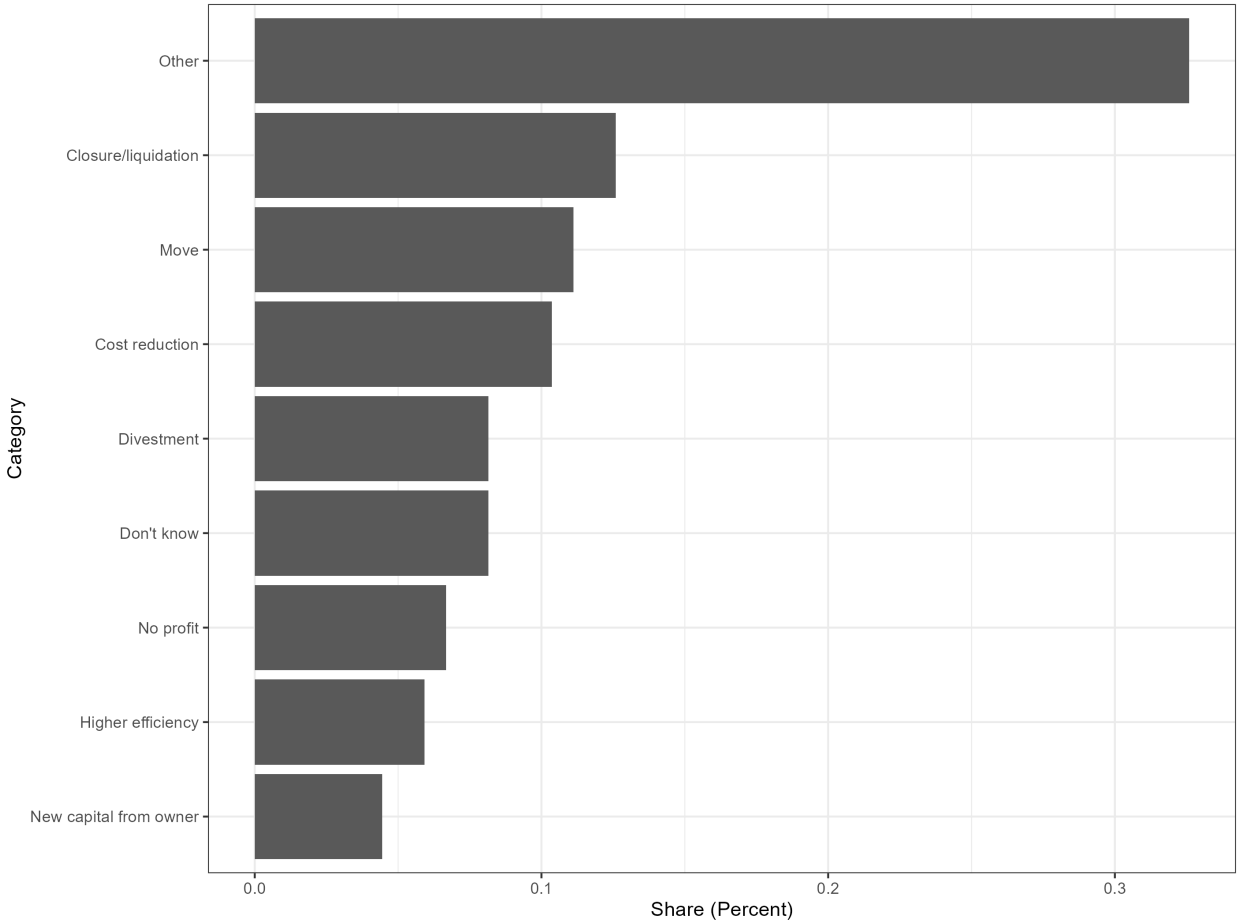
For the **tax decrease treatment**, the most frequently mentioned missing category appears to be the use of additional funds for debt repayment, as indicated by terms such as liabilities (Verbindlichkeiten), repayment (Rückzahlung), and loans (Kredite, Darlehen). Additionally, some firms noted that they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

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



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

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

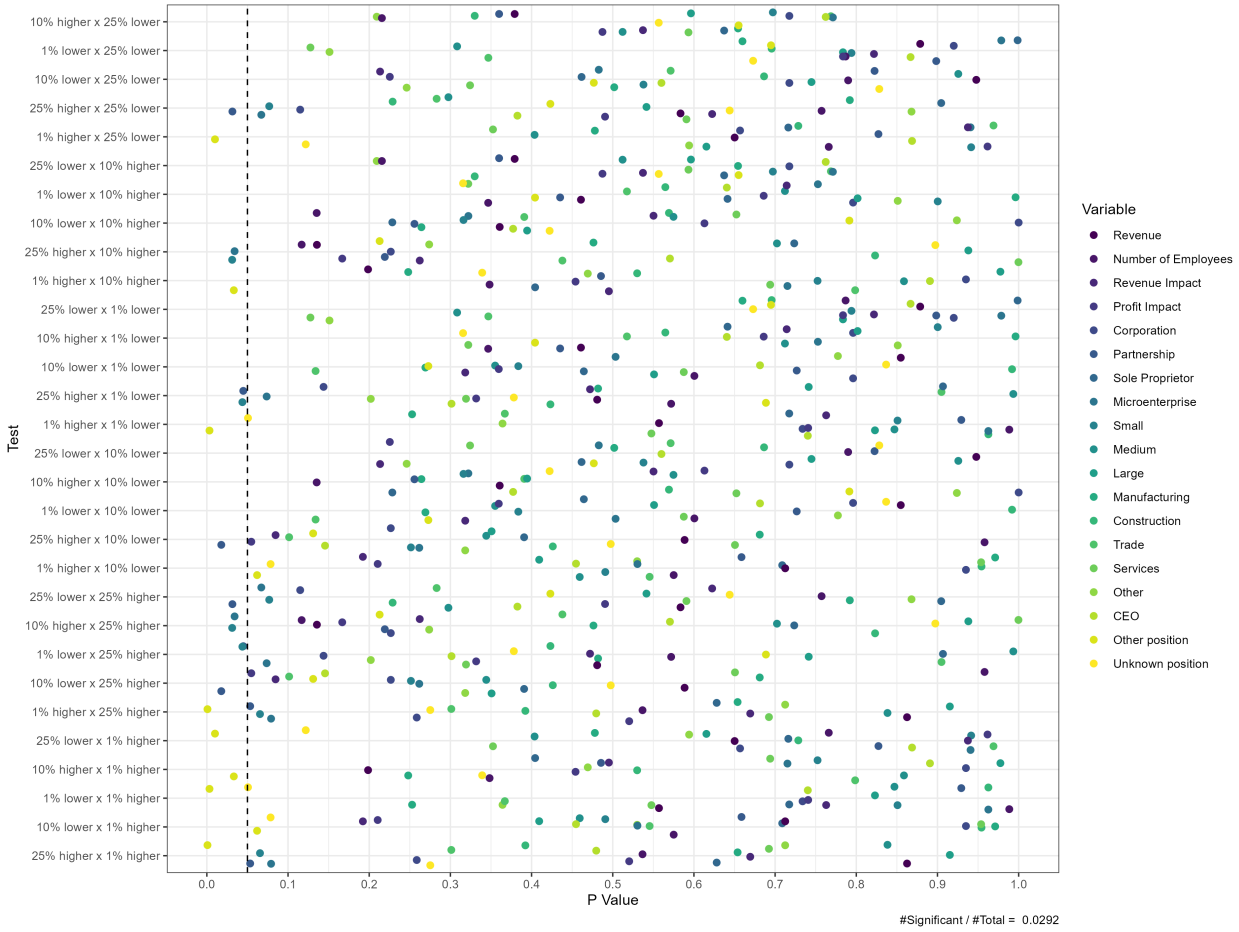


Note: Figure A.11 displays the most common categories selected by respondents in the **tax increase treatment** arm after indicating a positive share in the *Others* category.

A.4 Balance Tests

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

Figure A.12: Covariate Balance Tests across Treatments



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

A.5 Sample vs. German Firm Population

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

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

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

- **Industry sector** (1-digit WZ08 classification),[75]
- **Number of employees** (0–9, 10–49, 50–249, $\geq$ 250 employees, subject to social insurance contributions),
- **Revenue categories** (EUR 0–2 million, EUR 2–10 million, EUR 10–50 million, > EUR 50 million).

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

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

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

To prevent distortions caused by excessively high survey weights in underrepresented cells, we apply a trimming procedure, capping weights at the 5th and 95th percentiles of the distribution while ensuring that the total sum of weights remains unchanged.[76] This approach helps stabilize the variance of the survey weights and minimizes the influence of extreme values in the analysis.

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

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

Table A.1: Sample vs. German Firm Population

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

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

A.6 Orbis Comparison: Participants vs. Non-Participants

Section A.6 presents a balance table (Table A.2) comparing the observable financial characteristics of firms in our survey sample (i.e., participants), as obtained from Orbis, with those of German firms in the Orbis database that did not participate in the survey or did not complete our survey (i.e., non-participants), using financial data from the 2019 reporting year. The selection of 2019 as the reference year was based on its status as the most recent available dataset at the time of sample construction, and the revenues elicited in the survey refer to 2019. The comparison encompasses key financial indicators, including total assets, number of employees, turnover, and cost of employees, with all variables constrained to non-negative values. For each variable, we report the number of available observations within Orbis, along with the corresponding mean and median values. Furthermore, we conduct a t-test to assess differences in means between survey participants and non-participants, presenting the associated p-values. The survey sample comprises firms that explicitly consented to linking their survey responses with external data sources (N = 2,435). Overall, we find no significant differences in key financial metrics between survey participants and non-participants, suggesting that firms opting to participate and complete the survey do not systematically differ in financial characteristics from those in the Orbis database that were not included in our sample.

Table A.2: Orbis Comparison: Participants vs. Non-participants

	Participants			Non-participants			
	Obs.	Mean	p50	Obs.	Mean	p50	p-value
Total Assets	793	11,374,676.10	798,066.00	462,984	13,879,657.47	1,005,009.00	0.86
Number of Employees	1,516	23.18	6.00	901,927	28.88	3.00	0.75
Turnover	606	13,105,905.96	900,000.00	228,972	19,909,355.46	1,050,000.00	0.68
Costs of Employees	62	16,558,588.39	4,492,631.00	42,142	11,847,030.72	4,427,542.50	0.62
Taxes on Income	58	658,508.95	230,522.50	39,746	965,969.31	142,365.00	0.82

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

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

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

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

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

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

of employees in the survey itself (Mean: 43, Median: 6), reinforcing the reliability of our survey responses.

A.7 Reasons for Investment Change

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

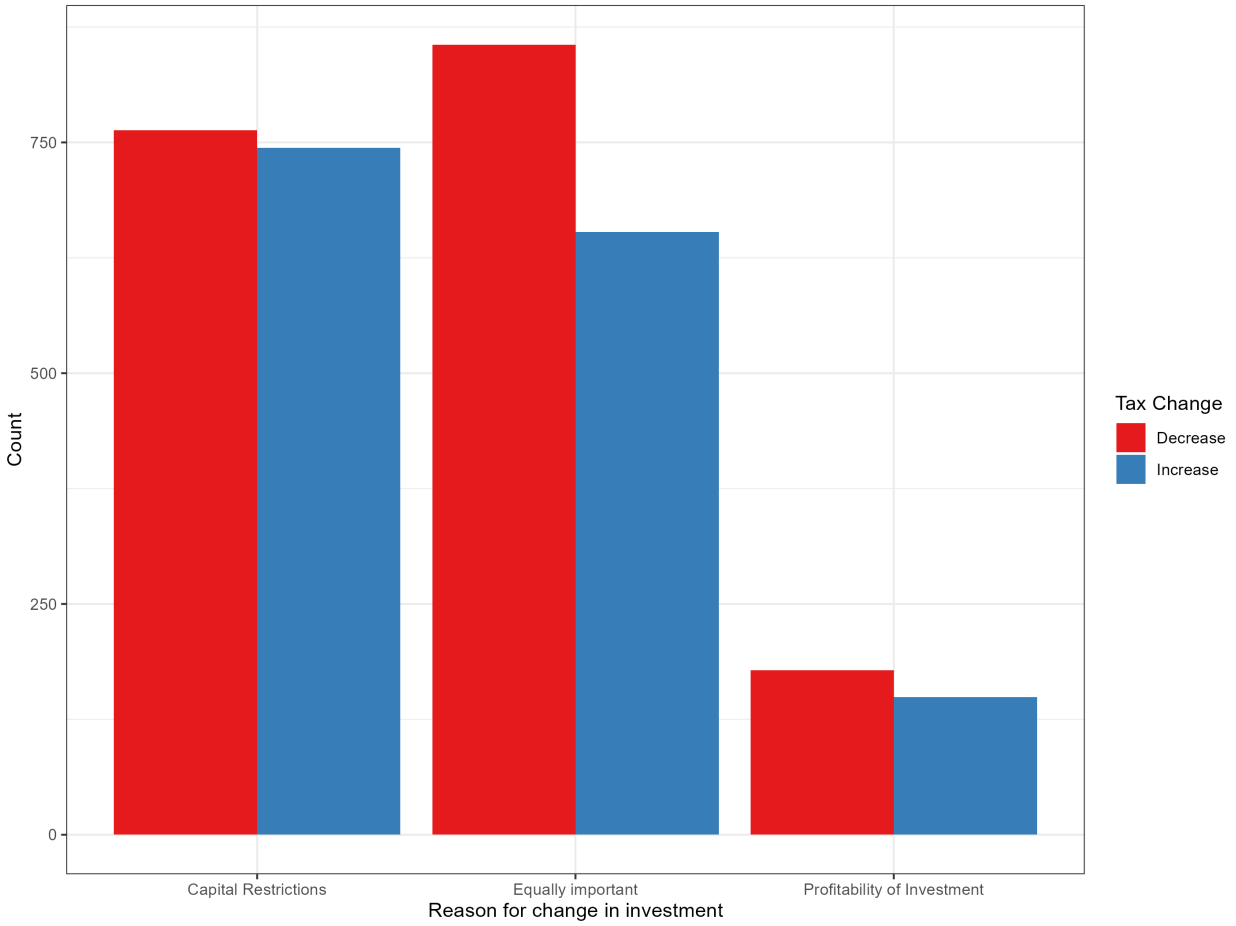
Participants rated their reasoning on a scale from 0 to 100, where 0 indicated that investment adjustments were primarily driven by changes in available funds following a tax decrease or increase, while 100 suggested that the perceived profitability of investments was the dominant factor. Lower values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

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

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

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

Figure A.13: Reasons for Change in Investment



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

A.8 Robustness Tests - Main Results

A.8.1 Ordinary Least Squares (OLS) Estimation

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

For each category of incidence (i.e., the dependent variables listed in Table A.4), we apply OLS estimation to equation (1):

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

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

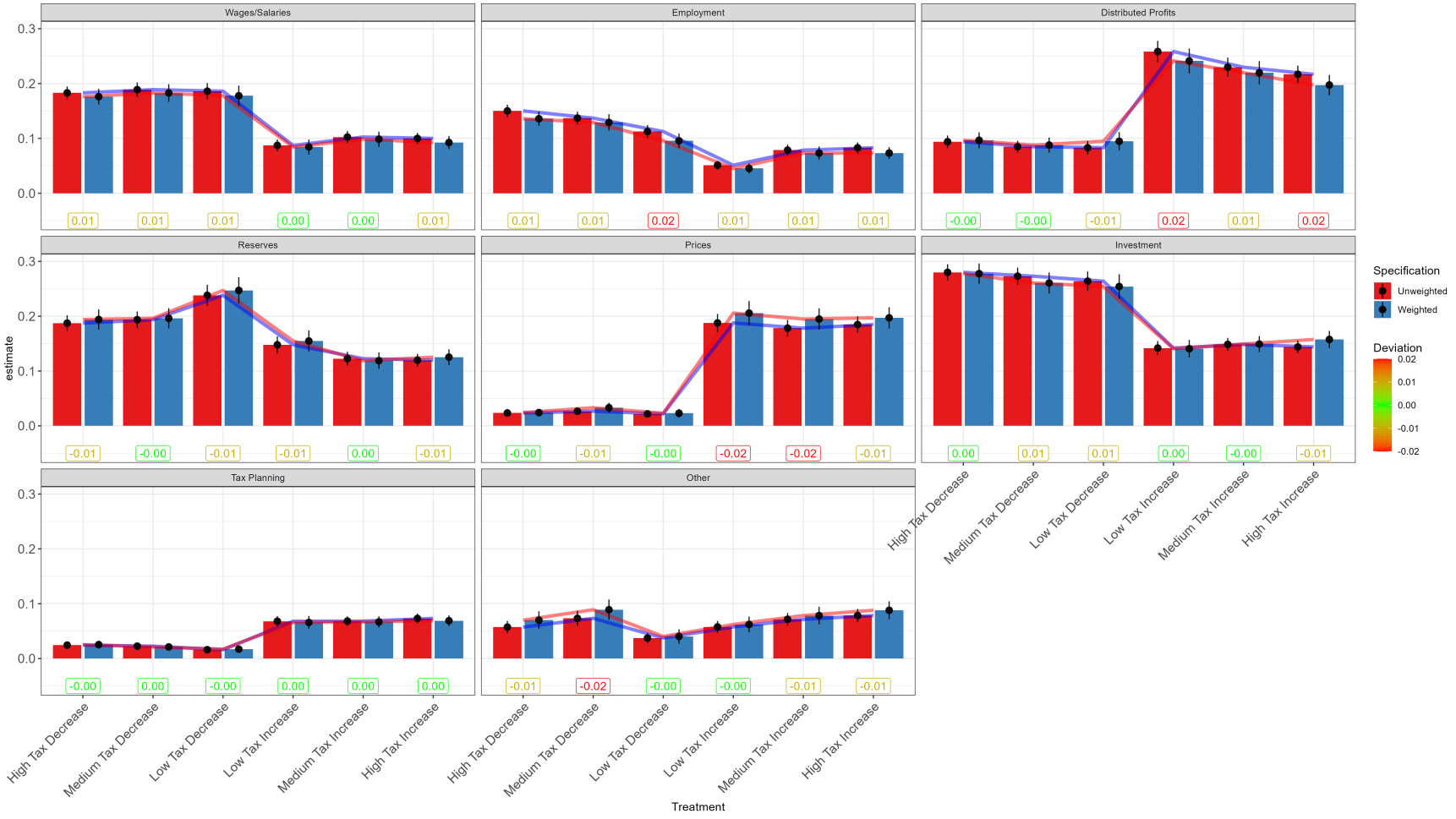
We begin by exploring the sensitivity of our main results with respect to the usage of survey weights. Figure A.14 compares our main estimates from equation (1) with weighted regressions that apply the survey weights described in Section A.5. The comparison suggests that there are only minor differences between weighted and unweighted point estimates, with none of them exceeding a two percentage point difference in estimated incidence. However, the unweighted coefficients are estimated with greater precision. Therefore, we opt for unweighted regressions throughout our paper.

As part of our robustness analysis, we incorporate additional control variables in Ta-

ble A.5 to enhance the accuracy of our estimates. These controls include industry-specific dummies for key economic sectors (Manufacturing, Construction, Trade, and Services), dummy variables for a firm's legal structure, and dummies for firm size – small, medium, and large – based on annual revenue. Additionally, we introduce an indicator identifying firms that experienced a substantial financial impact from the COVID-19 pandemic. A firm is classified as significantly affected by COVID-19 if its reported percentage change in net income is lower than the median of the distribution.

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

Figure A.14: Weighted versus Unweighted Regressions



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

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

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

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

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

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

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

A.8.2 Multivariate Fractional Logit (MFL) Estimation

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

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

$$E[y_{im}|\boldsymbol{X}] = G_m(\boldsymbol{X}; \boldsymbol{\beta}) \in (0, 1), \ m = 1, \ldots, M \tag{12}$$

$$\sum_{m=1}^{M} y_{im} = 1, \ i = 1, \ldots, N, \tag{13}$$

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

$$\Pr(y_{im} = 1|X) > 0 \ \forall m = 1, \ldots, M, \tag{15}$$

where $\boldsymbol{\beta} = (\boldsymbol{\beta}_1, \ldots, \boldsymbol{\beta}_K)$ is a $K \times M$ vector of parameters and $G_m(\boldsymbol{X}; \boldsymbol{\beta})$ a parametric conditional mean function. Equation (12) signifies the bounded nature of our outcome variables. Note that the condition as stated precludes the case in which one share obtains a boundary value $\mu \in \{0, 1\}$ for some combination of covariates $\boldsymbol{X}$.[77] Equation (13) is the unit-sum constraint, stemming from the fact that, by construction, the shares of different categories need to sum to one for each firm in the sample. Equations (14) and (15) illustrate that individual shares might attain boundary values with non-trivial probabilities, which

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

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

Like mentioned above, in our baseline specification (i.e., OLS estimation), we ignore the bounded nature of our outcome variables (equation (12)) as well as the unit-sum constraint (equation (13)) and assume a linear conditional mean function for each category m. Ignoring the underlying restrictions of our dataset has two main potential drawbacks, as pointed out, e.g., by Mullahy (2015) or Murteira and Ramalho (2016). First, similar to a linear probability model, predicted shares are not guaranteed to fall in the interval $[0, 1]$ for all combinations of covariates, and do not necessarily sum to one. Second, the model might misrepresent the partial effects of covariates.

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

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

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

as

$$E[y_{im}|\boldsymbol{X}] = G_m(\boldsymbol{X};\boldsymbol{\beta}) = \frac{\exp(\boldsymbol{x}_i'\boldsymbol{\delta}_m)}{1+\sum_{l=1}^{M-1}\exp(\boldsymbol{x}_i'\boldsymbol{\delta}_l)},\ m=1,\ldots,M, \tag{17}$$

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

$$\begin{aligned}
\hat{APE}_{mk} &= \frac{1}{N}\sum_{i=1}^{N}\hat{PE}_{mki} \\
&= \frac{1}{N}\sum_{i=1}^{N}\frac{\Delta E[y_{im}|\boldsymbol{x}_i]}{\Delta x_{ik}} \\
&= \frac{1}{N}\sum_{i=1}^{N}\frac{\exp(\boldsymbol{x}'_{-k,i}\boldsymbol{\beta}_{m,-k}+\beta_{mk})}{1+\sum_{l=1}^{M-1}\exp(\boldsymbol{x}'_{-k,l}\boldsymbol{\beta}_{l,-k}+\beta_{lk})} - \frac{\exp(\boldsymbol{x}'_{-k,i}\boldsymbol{\beta}_{m,-k})}{1+\sum_{l=1}^{M-1}\exp(\boldsymbol{x}'_{-k,l}\boldsymbol{\beta}_{l,-k}+\beta_{lk})},
\end{aligned} \tag{18}$$

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

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

Table A.6: Comparison of Average Partial Effects

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

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

A.9 Robustness Tests - Heterogeneity

In Section 2.4 of Chapter 2, we examine treatment effect heterogeneity across firm size, economic sector, legal structure, and net income impact from COVID-19. In this section, we extend the analysis from Section 2.4 by incorporating all relevant firm heterogeneity characteristics into a single estimation model, thereby controlling for other firm characteristics when testing effect heterogeneity along a specific margin. We assess variation in incidence using the following OLS regression:

$$y_i = \beta_0 + \beta_1 \text{ Increase}_i + \boldsymbol{\gamma}_1' \, \boldsymbol{x}_i^* + \boldsymbol{\gamma}_2' \text{ Increase}_i \times \boldsymbol{x}_i^* + \varepsilon_i, \tag{19}$$

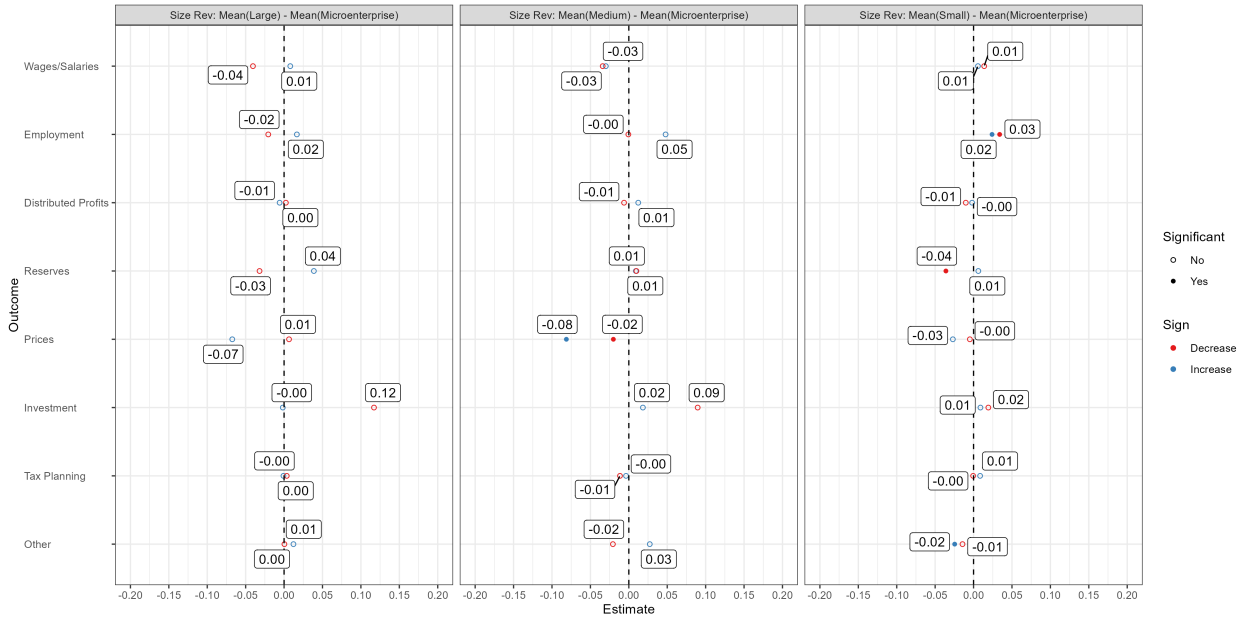
where $\boldsymbol{x}_i^*$ represents a vector of firm characteristics: dummies for firm size, economic sector, legal structure, and net income impact from COVID-19.[78] As in equation (2), we aggregate the treatment intensity groups and classify treatment solely based on whether an individual firm was part of a tax increase treatment or not, as indicated by Increase_i, a dummy variable. Since treatment intensity was randomly assigned and is, therefore, uncorrelated with firm characteristics, this approach should not introduce bias.

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

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

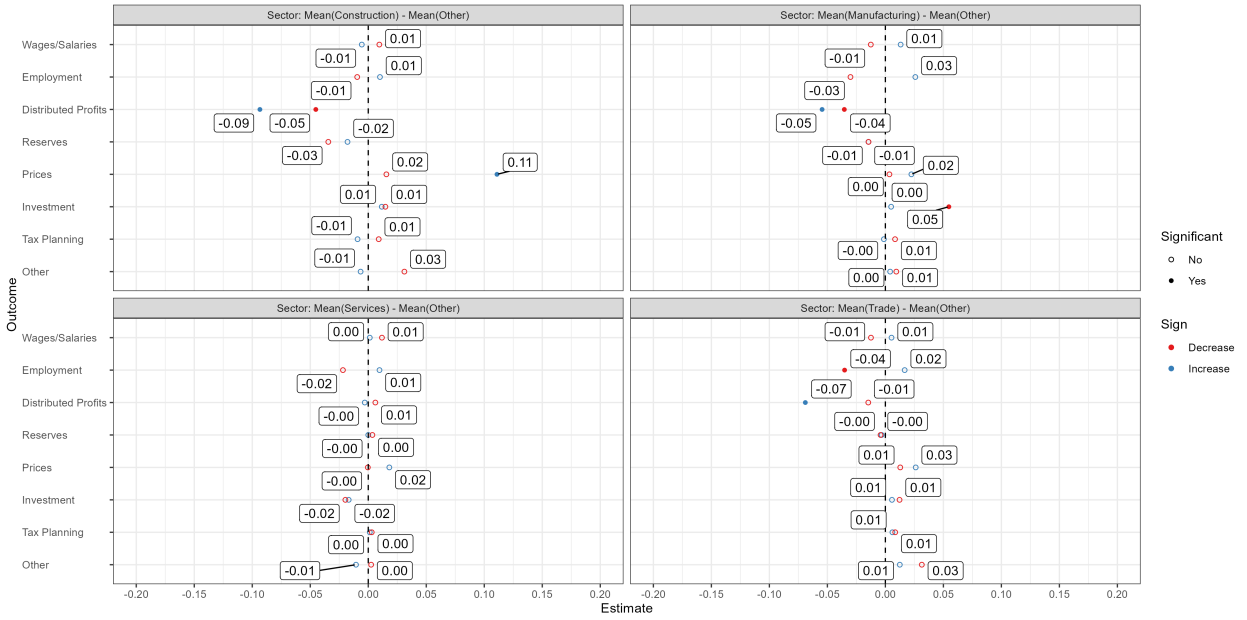
The results closely align with those in Section 2.4 for almost all heterogeneities, where we estimate separate OLS regressions for each of the four firm characteristics. The only exception is the net income impact of the COVID-19 pandemic, where the point estimates turn insignificant when including controls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

Figure A.15: Average Partial Effects of Revenue Size Category with Control Variables



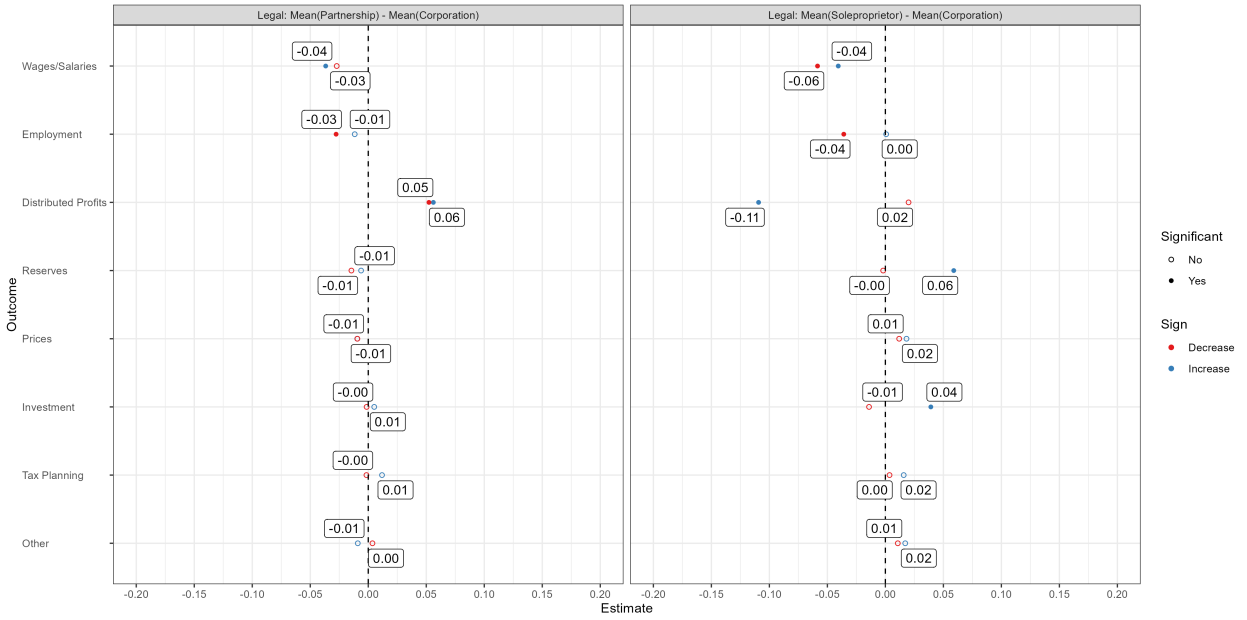
Note: Figure A.15 shows heterogeneity in incidence by company size measured by revenues. The figure shows average partial effects for each comparison with the baseline based on the estimated coefficients from equation (19). Average partial effects with a significant p-value after applying the Benjamini and Yekutieli (2001) correction are denoted by filled dots, whereas insignificant effects are illustrated by hollow circles.

Figure A.16: Average Partial Effects of Economic Sector with Control Variables



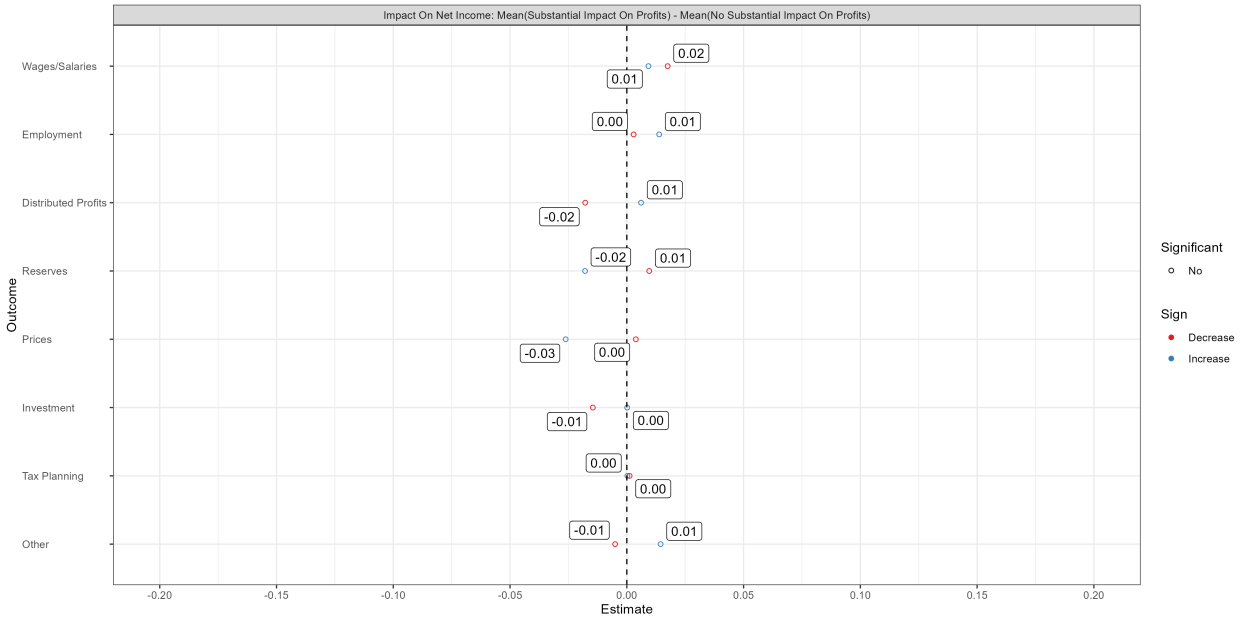
Note: Figure A.16 shows heterogeneity in incidence by economic sector. The figure shows average partial effects for each comparison with the baseline based on the estimated coefficients from equation (19). Average partial effects with a significant p-value after applying the Benjamini and Yekutieli (2001) correction are denoted by filled dots, whereas insignificant effects are illustrated by hollow circles.

Figure A.17: Average Partial Effects of Organizational Form with Control Variables



Note: Figure A.17 shows heterogeneity in incidence by company legal form. The figure shows average partial effects for each comparison with the baseline based on the estimated coefficients from equation (19). Average partial effects with a significant p-value after applying the Benjamini and Yekutieli (2001) correction are denoted by filled dots, whereas insignificant effects are illustrated by hollow circles.

Figure A.18: Average Partial Effects of COVID-19 Impact on Net Income with Control Variables



Note: Figure A.18 shows heterogeneity in incidence depending on whether the company was substantially impacted in its net income by COVID-19. The figure shows average partial effects for each comparison with the baseline based on the estimated coefficients from equation (19). 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.10 Incidence Estimates in Previous Literature

Table A.7 provides a comprehensive summary of previous studies on tax incidence. In contrast to Table 2.5, which highlights selected recent studies, Table A.7 also incorporates corporate tax incidence estimates from earlier published research and working papers.

Table A.7: Tax Incidence Estimates

Paper	Tax Variation	Tax Change	Country	Episode	Incidence on			
					Workers	Firm Owners	Consumers	Land Owners
Arulampalam et al. (2012)	Cross-company differences in tax liability	Increases and Decreases (pooled)	9 countries	1996-2003	49% (long run) 64% (short run)	51% (long run) 36% (short run)	-	-
Azémar and Hubbard (2015)	Cross-country variation in the statutory corporate tax	Increases and Decreases (pooled)	13 OECD countries	1980–2004	60%	-	-	-
Baker et al. (2023)	Variation in state corporate tax rates	Increases and Decreases (pooled)	US	2006-2017	Primary spec.: 28% Alternative: 36%	Primary spec.: 20% Alternative: 21%	Primary spec.: 51% Alternative: 43%	-
Carbonnier et al. (2022)	Large French corporate income tax credit	Decrease	France	2009-2015	50% Range: 40%-60%	50%	-	-
Carroll (2009)	Variation in states' corporate taxes	Increases and Decreases (pooled)	US	1970-2007	>100% (i.e. 250%)	-	-	-
Desai et al. (2007)	Cross-country differences in corporate taxes	Increases and Decreases (pooled)	52 countries	1989-2004	Baseline: 57%, Range: 45%-75%	Baseline: 43%, Range: 25%-55%	-	-
Dobridge et al. (2021)	Variation in the DPAD	Decrease	US	1999-2015	80%	20%	-	-
Duan and Moon (2024)	Corporate tax cuts	Decrease	Canada	2001-2017	73%, owner-workers: 39%	27%	-	-
Dwenger et al. (2019)	Federal tax cut/Variation in effective corporate tax burden	Decrease	Germany	1998–2006	19% (long-run) - 28% (short-run)	-	-	-
Felix (2007)	Variation in corporate tax rate	Increases and Decreases (pooled)	30 countries	1979–2002	>100% Range: 235%-620%	-	-	-
Felix (2009)	Variation in states' corporate taxes	Increases and Decreases (pooled)	US	1977-2005	>100%, Gravelle (2011): 141%-360%	-	-	-
Felix and Hines (2022)	State tax changes	Variation between unionized and non-unionized workers	US	2000	31% (fully unionized firm)	-	-	-

(Table continues on the next page)

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

Note: Table A.7 summarizes previous estimates of tax incidence found in the literature on workers, capital/firm owners, consumers, and land owners. "-" : Indicates that no information on the incidence for this group was provided, or that it was explicitly assumed to be 0% in the respective study.

Chapter 2 Appendix References

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B Appendix to Chapter 3

B.1 Survey and Experimental Design

Our experimental design incorporates several stages, which are visually depicted in Figure 3.1 in Chapter 3. In the initial stage, participants are requested to provide their inflation estimates for the years 2021, 2022, and 2023. This stage yields two essential pieces of information. First, we obtain participants' prior expectations regarding future inflation, specifically for the years 2022 and 2023. Notably, the expectation for 2022 is partially realized at the time of the survey, while the expectation for 2023 remains entirely in the future. Second, by soliciting firms' assessment of past inflation in 2021, we can assess the level of knowledge of each firm concerning inflation in general. This serves as a natural anchor point and enables within-subject comparisons between realized and expected inflation.

In the second stage, after indicating their inflation assessment, firms are randomly assigned to one of four groups. Depending on the assignment to one of the four groups, firms see different information displayed on the next page of the survey. The exact layout of the information provided can be seen in Figure B.1 (translated survey screen) and Figure B.2 (original survey screen in German). All firms, including the CONTROL group, see their own inflation assessment for the three years as indicated in the first question. Firms in the INFLATION, ENERGY and WAGE group see, in addition, the German central bank's inflation assessment for the respective year. Finally, the ENERGY (WAGE) group is additionally informed about the central bank's assessment of energy cost (labor cost) development for all three years.

All mentioned information is displayed adjacently for the respective group. Hence, participants can compare their own estimates to the displayed information. The CONTROL group only sees its own estimates. The INFLATION group receives the same screen as the CONTROL group with inflation forecasts added. Further, ENERGY and WAGE see the same information as the INFLATION group with single cost components added. This

step-wise addition of information allows us to measure the incremental effect of additional information.

In the third stage, we ask participants about their pricing plans for their main product in the upcoming 12 months. Thereby, we indirectly measure posterior beliefs with regard to inflation expectations via price adjustments of firms. That is, we omit the direct measurement of posterior inflation expectations. It is important to note that we purposely avoid directly measuring these post-treatment expectations due to various reasons, such as the desire to minimize potential experimenter demand effects, as explained in Section 3.3 of Chapter 3. Following the completion of the third stage, participants proceed to the remaining questions of the questionnaire of the GBP. From these questions, we extract the relevant firm and manager characteristics. A comprehensive description of these variables can be found in Table B.1 below.

Figure B.1: Screenshots - Experimental Treatment (Translated)

Below is a summary of your responses regarding inflation for the respective years.

Note:
Numbers represent the average change compared to the previous year.

Year	Your Inflation Estimates (%)
2021	5
2022	8
2023	7

(a) CONTROL

Below is a summary of your responses regarding inflation for the respective years. In addition, you can see the German Central Bank's assessments regarding the development of inflation.

Note:
Numbers represent the average change compared to the previous year.

Year	Your Inflation Estimates (%)	Central Bank Inflation Estimates (%)
2021	5	3.2
2022	8	7.1
2023	7	4.5

Source:
The Bundesbank figures are from the June 2022 monthly report.

(b) INFLATION

Below is a summary of your responses regarding inflation for the respective years. In addition, you can see the German Central Bank's assessments regarding the development of inflation and energy prices.

Note:
Numbers represent the average change compared to the previous year.

Year	Your Inflation Estimates (%)	Central Bank Inflation Estimates (%)	Central Bank Energy Price Development Estimates (%)
2021	5	3.2	10.1
2022	8	7.1	27.2
2023	7	4.5	8.5

Source:
The Bundesbank figures are from the June 2022 monthly report.

(c) ENERGY

Below is a summary of your responses regarding inflation for the respective years. In addition, you can see the German Central Bank's assessments regarding the development of inflation and wages.

Note:
Numbers represent the average change compared to the previous year.

Year	Your Inflation Estimates (%)	Central Bank Inflation Estimates (%)	Central Bank Wage Development Estimates (%)
2021	5	3.2	3.5
2022	8	7.1	4.3
2023	7	4.5	4.5

Source:
The Bundesbank figures are from the June 2022 monthly report.

(d) WAGE

Note: Translation of screenshots of the experimental information treatment in the online survey for the four experimental groups. Top left: **CONTROL** group is shown their own inflation estimates they indicated in the previous survey question. Top right: firms in baseline **INFLATION** treatment are shown their own inflation estimates contrasted with the forecasts of the German central bank (Bundesbank) at the time of the survey. Bottom left: firms in extended **ENERGY** treatment are shown their own inflation estimates contrasted with the forecasts of the German central bank (Bundesbank) on both inflation rates and energy price development at the time of the survey. Bottom right: firms in extended **WAGE** treatment are shown their own inflation estimates contrasted with the forecasts of the German central bank (Bundesbank) on both inflation rates and wage development at the time of the survey.

Figure B.2: Screenshots - Experimental Treatment (Original Survey Screens)

Im Folgenden sehen Sie eine Übersicht Ihrer Antworten bezüglich der Inflationsentwicklung für die jeweiligen Jahre.

Hinweis: Die Angaben stellen die durchschnittliche Veränderung im Vergleich zum Vorjahr dar.

Jahr	Ihre Inflations-angaben (%)
2021	5
2022	8
2023	7

(a) CONTROL

Im Folgenden sehen Sie eine Übersicht Ihrer Antworten bezüglich der Inflationsentwicklung für die jeweiligen Jahre. Zudem präsentieren wir Ihnen die Einschätzungen der Deutschen Bundesbank hinsichtlich der Entwicklung der Inflation.

Hinweis: Die Angaben stellen die durchschnittliche Veränderung im Vergleich zum Vorjahr dar.

Jahr	Ihre Inflations-angaben (%)	Bundesbank (%)
2021	5	3,2
2022	8	7,1
2023	7	4,5

Quelle: Die Zahlen der Bundesbank stammen aus dem Monatsbericht-Juni 2022.

(b) INFLATION

Im Folgenden sehen Sie eine Übersicht Ihrer Antworten bezüglich der Inflationsentwicklung für die jeweiligen Jahre. Zudem präsentieren wir Ihnen die Einschätzungen der Deutschen Bundesbank hinsichtlich der Entwicklung der Inflation und der Entwicklung der Energiepreise.

Hinweis: Die Angaben stellen die durchschnittliche Veränderung im Vergleich zum Vorjahr dar.

		Bundesbank	
Jahr	Ihre Inflations-angaben (%)	Inflation (%)	Energiepreis-entwicklung (%)
2021	5	3,2	10,1
2022	8	7,1	27,2
2023	7	4,5	8,5

Quelle: Die Zahlen der Bundesbank stammen aus dem Monatsbericht-Juni 2022.

(c) ENERGY

Im Folgenden sehen Sie eine Übersicht Ihrer Antworten bezüglich der Inflationsentwicklung für die jeweiligen Jahre. Zudem präsentieren wir Ihnen die Einschätzungen der Deutschen Bundesbank hinsichtlich der Entwicklung der Inflation und der Entwicklung der Löhne.

Hinweis: Die Angaben stellen die durchschnittliche Veränderung im Vergleich zum Vorjahr dar.

		Bundesbank	
Jahr	Ihre Inflations-angaben (%)	Inflation (%)	Lohn-entwicklung (%)
2021	5	3,2	3,5
2022	8	7,1	4,3
2023	7	4,5	4,5

Quelle: Die Zahlen der Bundesbank stammen aus dem Monatsbericht-Juni 2022.

(d) WAGE

Note: Screenshots of the experimental information treatment in the online survey for the four experimental groups. Top left: **CONTROL** group is shown their own inflation estimates they indicated in the previous survey question. Top right: firms in baseline **INFLATION** treatment are shown their own inflation estimates contrasted with the forecasts of the German central bank (Bundesbank) at the time of the survey. Bottom left: firms in extended **ENERGY** treatment are shown their own inflation estimates contrasted with the forecasts of the German central bank (Bundesbank) on both inflation rates and energy price development at the time of the survey. Bottom right: firms in extended **WAGE** treatment are shown their own inflation estimates contrasted with the forecasts of the German central bank (Bundesbank) on both inflation rates and wage development at the time of the survey.

Table B.1: Variable Overview

Panel A: Outcome Variables	
Inflation	"How high do you estimate the inflation rate for the respective years?" (2021, 2022, 2023) (*Hint: The inflation rate is defined as the change in the average price development of all goods and services that private households in Germany buy for consumption purposes. It is measured as the average change compared to the previous year.*)
Price Change	"Compared to today, how do you plan to adjust the selling price of your main product or service in the next 12 months (in %)?"
Input Cost Factors	"Which factors have the greatest influence on pricing in your company?"
Future Price Adjustment	"Compared to past years: Do you think you will adjust the price of your main product or service more or less frequently in the next 12 months?"
Panel B: Manager Characteristics	
Education	"Please indicate your highest level of education completed."
Position	"What is your current position within your organization?"
Gender	"What is your preferred salutation?"
Panel C: Firm Characteristics	
Revenues	"Please indicate the annual revenue (in EUR) of your company in the previous calendar year."
Employees	"How many employees (in full-time positions) subject to social insurance contributions does your company have?"
Legal Form	"What is the legal form of your company?"
Industry	"Please select the most important industry sector in which your company is active, by selecting the corresponding category."

Note: Questions of the fifth survey wave of the GBP used in the empirical analyses.

B.2 Descriptive Characteristics and Balancing Tests

A key assumption of randomized control trials is that a random assignment of participants to treatments leads to balanced participant characteristics across treatment groups. In this section, we investigate whether this key assumption holds for our experiment, i.e., whether firms in our different experimental groups have comparable prior inflation expectations. In other words, our tests show whether we were successful in randomizing firms in our different experimental arms. This ensures that participating firms do not exhibit systematic differences in their inflation assessment prior to receiving the information treatment.

Table B.2: Descriptive Statistics and Balancing Tests – Inflation Assessment

	Total	CONTROL	INFLATION	ENERGY	WAGE	P-value for equality across groups
Inflation 2021 (in %)						
Mean	4.68	4.53	5.03	4.37	4.74	0.25
SD	(4.93)	(3.72)	(6.28)	(4.42)	(4.76)	
N	1,872	437	496	474	465	
Inflation 2022 (in %)						
Mean	10.48	10.09	10.80	10.22	10.78	0.25
SD	(7.14)	(5.47)	(8.49)	(6.33)	(7.68)	
N	1,898	441	504	480	473	
Inflation 2023 (in %)						
Mean	11.31	10.75	11.80	10.89	11.75	0.21
SD	(10.06)	(7.58)	(12.02)	(9.32)	(10.51)	
N	1,883	440	494	477	472	

Note: Descriptive statistics for prior inflation assessment for 2021, 2022 and 2023 in % for the total sample and the experimental groups, respectively. *P*-values in the last column from a Wald chi-square test for equality of means across all four experimental groups. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table B.2 shows descriptive statistics for each experimental group's inflation assessment for 2021, 2022, and 2023. We perform a Wald chi-square test for equality of means across all four experimental groups. P-values are displayed in the last column of Table B.2 and support that inflation expectations among the four groups do not significantly differ from each other, confirming the effectiveness of our randomization procedure. Similarly, as our ENERGY and WAGE treatment inform firms about energy and wage components of the central bank's inflation forecasts, we test whether firms in the four groups differ systematically in which input

costs are important for them. Table B.3 displays the results. Approximately 69% of firms consider energy/material costs, while 64% take labor costs into account when making pricing decisions. In comparison, other factors such as legal regulations (26%), customer demand (25%), and competitor prices (19%) appear to have less impact. Importantly, this pattern holds true across all experimental groups, indicating that energy/material costs and labor costs are consistently regarded as among the most crucial factors in the price-setting process.

Table B.3: Descriptive Statistics and Balancing Tests – Factors Influencing Prices

	Total	CONTROL	INFLATION	ENERGY	WAGE	P-value for equality across groups
Which factors have the greatest influence on pricing in your company?						
Energy/Material Costs						
Mean	0.69	0.70	0.68	0.67	0.70	0.77
SD	(0.46)	(0.46)	(0.47)	(0.47)	(0.46)	
Labor Costs						
Mean	0.64	0.64	0.64	0.64	0.63	0.94
SD	(0.48)	(0.48)	(0.48)	(0.48)	(0.48)	
N	1,934	445	515	495	479	

Note: Descriptive statistics of firms naming energy and labor costs as having the greatest influencing on their price setting behavior for the total sample and the experimental groups, respectively. *P*-values in the last column from a Wald chi-square test for equality of means across all four experimental groups. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table B.4 displays descriptive statistics for firm and manager characteristics we use in our analyses by experimental group. Again, the last column of Table B.4 displays the p-values of Wald chi-square tests for equality of means across all four experimental groups for each variable. P-values demonstrate that our randomization was also successful regarding firm and manager characteristics, as the distributions do not display systematic differences.

Finally, Table B.5 shows that the industry composition of our firm sample is largely comparable to the industry composition of the overall German firm population (German Federal Statistical Office, 2021). Our sample includes more firms from the manufacturing and information sector and fewer firms from the hospitality and health service industry, in contrast to the German firm population in 2021. Moreover, firms in our sample are slightly

larger with regard to employees and revenues compared to the German firm population.

Table B.4: Descriptive Statistics and Balancing Tests – Firm and Manager Characteristics

	Total Sample	CONTROL	INFLATION	ENERGY	WAGE	P-value for equality across groups
Size groups - Revenues/Employees						
Very Small	0.68	0.65	0.69	0.67	0.70	0.36
Small	0.24	0.26	0.24	0.25	0.21	0.30
Medium	0.06	0.05	0.05	0.06	0.07	0.64
Large	0.02	0.03	0.01	0.02	0.01	0.07*
Missing	0.01	0.00	0.01	0.00	0.01	0.57
Legal Forms						
Sole Proprietorship	0.23	0.22	0.23	0.21	0.24	0.65
Partnerships	0.13	0.14	0.15	0.12	0.11	0.12
Corporations	0.56	0.56	0.52	0.59	0.58	0.13
Other	0.07	0.07	0.08	0.07	0.06	0.63
Missing	0.01	0.00	0.01	0.00	0.01	0.76
Economic Sector (1-digit WZ08)						
A Agriculture	0.01	0.02	0.01	0.02	0.01	0.54
B Mining and quarrying†	0.00	0.00	0.00	0.00	0.00	-
C Manufacturing	0.14	0.15	0.14	0.13	0.14	0.88
D Energy Supply	0.01	0.01	0.01	0.00	0.01	0.41
E Water supply	0.00	0.01	0.00	0.00	0.00	0.34
F Construction	0.10	0.10	0.11	0.09	0.09	0.44
G Trade	0.14	0.15	0.14	0.14	0.12	0.54
H Transport and Storage	0.03	0.02	0.02	0.03	0.04	0.49
I Accommodation/Food	0.04	0.04	0.04	0.03	0.04	0.93
J Information	0.08	0.08	0.05	0.08	0.10	0.07*
K Financial/Insurance	0.03	0.02	0.02	0.03	0.03	0.82
L Real Estate	0.03	0.03	0.02	0.03	0.02	0.66
M Professional, scientific, and technical activities	0.14	0.12	0.16	0.15	0.14	0.34
N Other econ. services	0.04	0.04	0.05	0.04	0.04	0.79
O Public administration	0.00	0.00	0.00	0.00	0.00	0.86
P Education	0.02	0.02	0.01	0.03	0.01	0.52
Q Health/Social Services	0.03	0.03	0.03	0.03	0.03	0.91
R Arts/Entertainment	0.03	0.02	0.04	0.03	0.02	0.66
S Other services	0.04	0.04	0.04	0.04	0.04	0.92
Missing	0.10	0.08	0.08	0.11	0.12	0.11
Gender						
Male	0.75	0.78	0.74	0.73	0.74	0.28
Missing	0.08	0.07	0.08	0.09	0.09	0.65
Education						
Apprenticeship (voc.)	0.13	0.14	0.12	0.13	0.14	0.69
Bachelor Degree	0.06	0.04	0.06	0.05	0.07	0.16
Master (voc.)	0.14	0.15	0.15	0.12	0.13	0.46
Master Degree or higher	0.37	0.40	0.39	0.38	0.32	0.05*
Missing/Other/No degree	0.31	0.27	0.28	0.32	0.34	0.08*
Position						
Clerk	0.02	0.02	0.01	0.02	0.02	0.40
Department Head	0.02	0.02	0.03	0.03	0.02	0.83
Owner/CEO	0.87	0.88	0.87	0.86	0.88	0.72
Missing/Other	0.09	0.09	0.10	0.09	0.07	0.59
N	1,944	449	515	499	481	

Note: Descriptive statistics of firm and manager characteristics for the total sample and the experimental groups, respectively. *P*-values in the last column from a Wald chi-square test for equality of means across all four experimental groups. Size groups - Revenues/Employees (SME- EU Definition 2003/361): Very small ($\leq$ 9 employees & $\leq$ 2 mio. revenues), Small ($\leq$ 49 employees & $\leq$ 10 mio. revenues), Medium ($\leq$ 249 employees & $\leq$ 50 mio. revenues), Large ($>$ 249 employees or $>$ 50 mio. revenues). The economic sector classification follows the classification of economic activities from the German statistical office (2008 edition; WZ 2008). †: Due to missing observations in the experimental group ENERGY for the sector B, no test for equality of means across experimental groups can be conducted. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table B.5: Firm Characteristics - Sample vs. Population

	Total Sample	Company Register 2021
No. of Employees		
0-9	0.72	0.87
10-49	0.22	0.10
50-249	0.04	0.02
>250	0.01	0.00
Missing	0.01	-
Revenues (in million €)		
0-2	0.80	0.93
2-10	0.13	0.06
10-50	0.04	0.01
>50	0.01	0.00
Missing	0.03	-
Economic Sector (1-digit WZ08)		
A Agriculture	0.01	†
B Mining and quarrying	0.00	0.00
C Manufacturing	0.14	0.06
D Energy Supply	0.01	0.02
E Water supply	0.00	0.00
F Construction	0.10	0.11
G Trade	0.14	0.17
H Transport and Storage	0.03	0.03
I Accommodation/Food	0.04	0.07
J Information	0.08	0.04
K Financial/Insurance	0.03	0.02
L Real Estate	0.03	0.06
M Professional, scientific, and technical activities	0.14	0.15
N Other econ. services	0.04	0.07
O Public administration	0.00	‡
P Education	0.02	0.02
Q Health/Social Services	0.03	0.08
R Arts/Entertainment	0.03	0.03
S Other services	0.04	0.06
Missing	0.10	-
N	1,944	3,390,704

Note: Firm characteristics of the total sample and the German company register for 2021 for comparison (German Federal Statistical Office, 2021). †, ‡: Information on marginal distributions for these industries not available from German company register.

B.3 Additional Results

Table B.6 presents the results of the experimental treatments on firms' planned price setting, excluding those firms with extremely high or negative inflation expectations for 2023 (greater than 30% or negative). Overall, the experimental results are comparable to those of the entire sample. Therefore, we conclude that our overall findings are not driven by outlier firms in terms of inflation expectations.

Table B.6: Robustness Check: Experimental Groups and Planned Price Changes (Outliers excluded)

			Inflation Expectations 2023			
Sample:	**All**		$E_{2022}Inflation_{i2023} <= 30$		$0 <= E_{2022}Inflation_{i2023} <= 30$	
Dependent Variable: $\Delta Price_{i+12m}$	(1)	(2)	(3)	(4)	(5)	(6)
INFLATION	-3.380**	-3.257*	-3.377**	-3.487*	-3.410**	-3.541*
	(1.421)	(1.701)	(1.495)	(1.710)	(1.495)	(1.703)
ENERGY	-2.973***	-2.693***	-2.019***	-1.871**	-2.028***	-1.919**
	(0.644)	(0.652)	(0.609)	(0.775)	(0.620)	(0.773)
WAGE	-3.313**	-3.326**	-2.778**	-2.891*	-2.830**	-2.973**
	(1.351)	(1.464)	(1.167)	(1.363)	(1.170)	(1.356)
Constant (Baseline CONTROL)	15.368***	15.268***	14.222***	14.242***	14.256***	14.298***
	(1.388)	(0.689)	(1.202)	(0.531)	(1.205)	(0.517)
Controls	No	Yes	No	Yes	No	Yes
N	1912	1912	1803	1803	1799	1799
R^2	0.004	0.051	0.004	0.050	0.004	0.051

Note: OLS estimates from the regression of firms' planned price change in the next 12 months on experimental group dummies: $\Delta Price_{i+12m} = \alpha + \beta_1 \times INFLATION_i + \beta_2 \times ENERGY_i + \beta_3 \times WAGE_i + X_i'\gamma + \varepsilon_i$. **Columns (1) and (2)** include all observations in our sample. **Columns (3) through (4)** only include firms with inflation expectations for 2023 smaller or equal to 30% ($E_{2022}Inflation_{i2023} <= 30$) answering the survey in 2022. **Columns (5) and (6)** only include firms with inflation expectations for 2023 larger or equal to 0% and smaller or equal to 30% ($0 <= E_{2022}Inflation_{i2023} <= 30$) answering the survey in 2022. Controls as indicated in each column. Controls include firm controls (size groups, legal forms and 1-digit industries (WZ08 classification)), manager controls (education, position in the firm and the gender of the decision-maker) and week fixed effects. Standard errors clustered on industry and survey-week level. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Chapter 3 Appendix References

German Federal Statistical Office. (2021). *Statistisches Unternehmensregister* (Company Register). German Federal Statistical Office.

C Appendix to Chapter 4

C.1 Summary Statistics

Section C.1 provides summary statistics and balancing tests for both firm and manager characteristics. Moreover, summary statistics for our outcome variables of interest are presented. Lastly, we contrast our sample of firms against the entire population of firms in the contact database from which our participants come from. Additionally, we compare our participating firms with those in the German Federal Statistical Office's official business registry for 2020.

C.1.1 Firm and Manager Characteristics - Summary Statistics and Balancing Tests

Table C.1 presents summary statistics of firm and manager characteristics for the overall sample of firms and for the respective experimental groups. Following the definition of small and medium-sized enterprises (SMEs) of the European Commission (EC), the majority of firms (89% – *Very Small* or *Small*) in our sample employ less than 50 employees and have less than 10 million euros in revenue. In terms of legal structures, the majority of firms are corporations (71%). A larger share of sample firms (15%) are also sole proprietorships or business partnerships. Most of our firms come from trade (16%), manufacturing (12%) or information and communication (13%). Concerning firm decision-makers' attributes, most firm representatives are male (81%), possess a university degree (60%), and serve as the firm's owner or manager (91%).

Table C.1 indicates that we successfully randomized firms into our different experimental groups. Except for industry sector K (Financial and insurance activities) and M (Professional, scientific, and technical activities), all p-values from Wald chi-square test for equality of means across the experimental groups are insignificant at conventional levels. The share of 5% (2/37) significant p-values is well in line with what one would expect in the absence of any systematic differences between groups. Nevertheless, to increase the power to precisely

estimate treatment effects and to control for these minor imbalances, we include a vector of control variables X_i as additional robustness checks in our regressions in Section 4.4 of Chapter 4.

Table C.1: Descriptive Statistics and Balancing Tests – Firm and Manager Characteristics

	Total	CONTROL	SOCIAL	FISCAL	P-value for equality across groups
Size groups - Revenues/Employees (SME-EU Definition 2003/361)					
Very Small	0.614	0.617	0.606	0.622	0.483
Small	0.272	0.263	0.282	0.266	0.299
Medium	0.082	0.090	0.080	0.079	0.462
Large	0.032	0.030	0.032	0.033	0.876
N	6,614	1,413	2,627	2,574	
Legal Forms					
Sole Proprietorship & Business Partnerships	0.149	0.152	0.147	0.150	0.901
Mixed Legal Forms (GmbH & Co. KG, KGaA)	0.099	0.099	0.096	0.101	0.820
Corporations	0.711	0.709	0.717	0.707	0.665
Other	0.040	0.040	0.039	0.042	0.889
N	7,816	1,778	3,022	3,016	
Economic Sector (1-digit WZ08)					
A Agriculture, forestry, and fishing	0.010	0.015	0.009	0.009	0.214
B Mining and quarrying	0.001	0.001	0.001	0.002	0.491
C Manufacturing	0.123	0.130	0.128	0.114	0.169
D Energy Supply	0.013	0.015	0.011	0.014	0.487
E Water supply/Sanitation/Waste/Pollution abatement	0.005	0.005	0.004	0.006	0.601
F Construction	0.073	0.071	0.070	0.078	0.514
G Trade	0.161	0.164	0.159	0.161	0.908
H Transport and Storage	0.024	0.025	0.025	0.021	0.524
I Accommodation and food service activities	0.045	0.047	0.046	0.044	0.891
J Information and communication	0.134	0.130	0.134	0.137	0.822
K Financial and insurance activities	0.032	0.033	0.026	0.037	0.087*
L Real estate activities	0.033	0.036	0.029	0.035	0.298
M Professional, scientific, and technical activities	0.108	0.090	0.119	0.107	0.010**
N Other economic service activities	0.075	0.079	0.072	0.075	0.699
O Public administration and defense/Social security	0.004	0.004	0.005	0.004	0.803
P Education	0.016	0.015	0.019	0.014	0.387
Q Health/Social Services	0.047	0.046	0.045	0.050	0.609
R Arts/Entertainment/Recreation	0.041	0.048	0.041	0.037	0.240
S Other services	0.047	0.039	0.050	0.050	0.160
T Manufacture of goods/services by private hh. for own use	0.006	0.007	0.007	0.005	0.534
U Extraterritorial organisations and entities	0.001	0.001	0.001	0.001	0.830
N	6,722	1,427	2,671	2,624	
Gender					
Male	0.808	0.795	0.809	0.813	0.517
N	4,680	951	1,879	1,850	
Education					
University Degree or PhD	0.603	0.578	0.609	0.609	0.214
Master (technical vocations)	0.136	0.141	0.129	0.142	0.439
Apprenticeships or Other	0.239	0.259	0.241	0.228	0.188
No training	0.022	0.023	0.022	0.021	0.975
N	4,829	974	1,929	1,926	
Position					
Clerk/Other	0.053	0.052	0.056	0.052	0.840
Department Head	0.035	0.034	0.039	0.031	0.367
Owner/CEO	0.912	0.914	0.905	0.918	0.391
N	4,927	998	1,975	1,954	

Note: Descriptive statistics of firm and manager characteristics for the total sample and the experimental groups, respectively. *P*-values in the last column of a Wald chi-square test for the equality of means across the three experimental groups. Size groups - Revenues/Employees (SME- EU Definition 2003/361): Very small ($\leq$ 9 employees & $\leq$ 2 mio. revenues), Small ($\leq$ 49 employees & $\leq$ 10 mio. revenues), Medium ($\leq$ 249 employees & $\leq$ 50 mio. revenues), Large ($>$ 249 employees or $>$ 50 mio. revenues). The economic sector classification follows the WZ 2008 classification of economic activities (German Federal Statistical Office, 2008). ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

C.1.2 Outcome Variables

Table C.2 presents summary statistics for the outcome variables of the empirical analysis for the overall sample and the experimental treatment groups, respectively. As described in Section 4.2 of Chapter 4, we only include firms in our empirical analysis that participate in the survey experiment and do not drop out before the experiment section was reached. Nevertheless, as we do not force participants to answer the survey questions, we observe varying numbers of observations for each respective outcome variable in our empirical analysis.

Concentrating on firms in the CONTROL group, which are unaffected by an experimental treatment, we make the following observations: First, we observe that 85% of firm decision-makers consider the fiscal stimulus program to be justified or absolutely justified. Second, firms do not expect medium-term *corporate tax* (0.4 percentage points), *business tax* (0.8 percentage points) and *income tax* (0.8 percentage points) to increase strongly. In contrast, firm decision-makers expect the *capital gains tax* to strongly increase by 2.3 percentage points, which would translate into an increase of 9% considering the current flat *capital gains tax* of 25%. Third, we observe that the *capital gains tax* is the only tax firm decision-makers do not want to reduce strongly (-0.5 percentage points). For the other type of taxes, we observe strong desired downward adjustments (*corporate tax*: -3.2 percentage points, *business tax*: -4.2 percentage points, *income tax*: -3.4 percentage points). These reductions are also economically substantial. For instance, given a current flat *corporate tax* of 15%, the desired reduction of -3.2 percentage points would translate into a reduction of 21%. Overall, from a fiscal policy perspective, increasing the capital gains tax would have the highest support from the perspective of firm decision-makers.

C.1.3 Sample Firms vs. Orbis Reference Group

To ensure that no systematic bias affects the selection of firms participating in our survey, we compare the characteristics of our participating firms with those of non-participating firms in the Orbis database (the contact database from which our sample firms were drawn).

Table C.2: Descriptive Statistics – Outcome Variables

	Total	CONTROL	SOCIAL	FISCAL
Attitude toward Fiscal Stimulus				
Absolutely Not Justified	0.050	0.018	0.014	0.099
Not Justified	0.085	0.036	0.044	0.147
Neutral	0.121	0.097	0.096	0.156
Justified	0.459	0.445	0.484	0.439
Absolutely Justified	0.285	0.405	0.362	0.158
N	7,197	1,228	2,998	2,971
Medium-term Expected Tax Changes				
Corporate Tax				
Mean	0.011	0.004	-0.000	0.027
SD	(0.072)	(0.076)	(0.075)	(0.064)
N	3,393	708	1,326	1,359
Business Tax				
Mean	0.017	0.008	0.005	0.033
SD	(0.081)	(0.086)	(0.083)	(0.072)
N	3,676	759	1,442	1,475
Income Tax				
Mean	0.017	0.008	0.006	0.033
SD	(0.077)	(0.080)	(0.080)	(0.068)
N	3,741	761	1,473	1,507
Capital Gains Tax				
Mean	0.035	0.023	0.022	0.052
SD	(0.079)	(0.079)	(0.080)	(0.073)
N	3,460	706	1,335	1,419
Desired Tax Adjustment				
Corporate Tax				
Mean	-0.022	-0.032	-0.032	-0.006
SD	(0.082)	(0.085)	(0.084)	(0.077)
N	3,794	790	1,480	1,524
Business Tax				
Mean	-0.031	-0.042	-0.043	-0.014
SD	(0.087)	(0.092)	(0.088)	(0.079)
N	4,020	846	1,564	1,610
Income Tax				
Mean	-0.023	-0.034	-0.035	-0.005
SD	(0.083)	(0.088)	(0.085)	(0.074)
N	4,036	844	1,574	1,618
Capital Gains Tax				
Mean	0.016	-0.005	0.006	0.036
SD	(0.097)	(0.099)	(0.098)	(0.092)
N	3,830	778	1,473	1,579

Note: Descriptive statistics for the outcome variables for the overall sample and the experimental groups, respectively.

Table C.3 indicates that the participating and non-participating Orbis firms are comparable with respect to the number of employees, total assets and revenues. Participating firms are on average around 3 years younger and have a slightly lower equity ratio (2 percentage points). Although these differences are statistically significant, economically they are of minor importance. Overall, there seems to be no severe selection bias with regard to firms choosing to participate in our survey.

C.1.4 Sample Firms vs. Business Register

Next, we evaluate how our sample of participating firms aligns with the broader German firm landscape in terms of revenues, employees, and industry sector. Table C.4 compares the sample of participating firms with the population of German firms for reporting year

Table C.3: Sample Firms vs. Orbis Reference Group

	Sample Firms			**Orbis Reference Group**			
	N	Mean	Median	N	Mean	Median	t-statistic
Firm Age	5,127	20.76	15.00	473,783	23.22	18.00	7.82***
Number of Employees	1,664	31.44	13.00	399,689	36.25	12.00	0.84
Total Assets	868	3,968,898.32	822,350.50	257,959	7,567,204.73	907,651.00	0.90
Equity Ratio	492	0.43	0.41	151,908	0.45	0.42	2.04**
Revenues (in €)	607	9,376,936.17	1,986,650.00	129,226	17,161,387.72	2,010,761.50	0.58

Note: Table C.3 compares the sample of participating firms with an *Orbis* reference group of German firms (reporting year 2019) regarding the variables firm age, number of employees, total assets, equity ratio and revenues. We present *t-tests* and the number of observations, the mean and median of each firm characteristic. We exclude 2,691 firm, which refuse to get their data in the survey linked with external data sources. Equity ratio is defined as one minus the ratio between current and non-current liabilities divided by total assets. For revenues, operating revenues (turnover) in Orbis is used. We require non-negative entries for current, non-current liabilities and total assets and a debt ratio lying between 0 and 1 to be included in the analysis of Table C.3. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

2020. Information about the German business universe comes from the German Federal Statistical Office (2020). With regard to revenues and employees, our sample is slightly larger compared to the firms in the overall population of firms. When we compare the distributions in terms of 1-digit WZ08 economic sector, our sample and the population of firms are mostly comparable, with minor exceptions. We slightly overestimate firms from manufacturing as well as information and communication, and underrepresent firms from construction. Overall, our sample of firms provides a comprehensive representation of the German business landscape, encompassing firms of every size and across all sectors of the economy.

Table C.4: Descriptives Sample Firms vs. Business Register 2020

Panel A: Revenues (in €)

	Sample	Business Register 2020
<= 2 Mio.	0.753	0.928
More than 2 Mio. - 10 Mio.	0.164	0.053
More than 10 Mio. - 50 Mio.	0.057	0.014
More than 50 Mio.	0.026	0.004
N	7,632	3,374,583

Panel B: Employees subject to social insurance (in full positions)

	Sample	Business Register 2020
0 - 9	0.651	0.869
10 - 49	0.259	0.105
50 - 249	0.066	0.022
More than 250	0.024	0.005
N	6,717	3,374,583

Panel C: Economic Sector (1-digit WZ08 Classification)

	Sample	Business Register 2020
A Agriculture, forestry, and fishing	0.010	
B Mining and quarrying	0.001	0.001
C Manufacturing	0.123	0.065
D Energy Supply	0.013	0.021
E Water supply/Sanitation/Waste/Pollution abatement	0.005	0.003
F Construction	0.073	0.113
G Trade	0.161	0.171
H Transport and Storage	0.024	0.032
I Accommodation and food service activities	0.045	0.069
J Information and communication	0.134	0.039
K Financial and insurance activities	0.032	0.022
L Real estate activities	0.033	0.061
M Professional, scientific, and technical activities	0.108	0.148
N Other economic service activities	0.075	0.065
O Public administration and defense/Social security	0.004	†
P Education	0.016	0.022
Q Health/Social Services	0.047	0.074
R Arts/Entertainment/Recreation	0.041	0.030
S Other services	0.047	0.066
T Manufacture of goods/services by private households for own use	0.006	†
U Extraterritorial organisations and entities	0.001	†
N	6,722	3,374,583

Note: Table C.4 presents distributions of firms with regard to revenues, number of full-time employees and economic sector (1-digit WZ08 classification (German Federal Statistical Office, 2008)) for our sample of firms and the population of firms in Germany for reporting year 2020 (German Federal Statistical Office, 2020). †: Information on marginal distributions for the respective industries not available from German company register.

C.2 Experimental Setup

In Section C.2, we detail the design of our survey experiment, present screenshots of the original experimental treatments as they appeared in the survey (in German) and provide screenshots of the original survey questions of our outcome question (also in German). In addition, we also offer English translations for all experimental treatments and outcome questions.

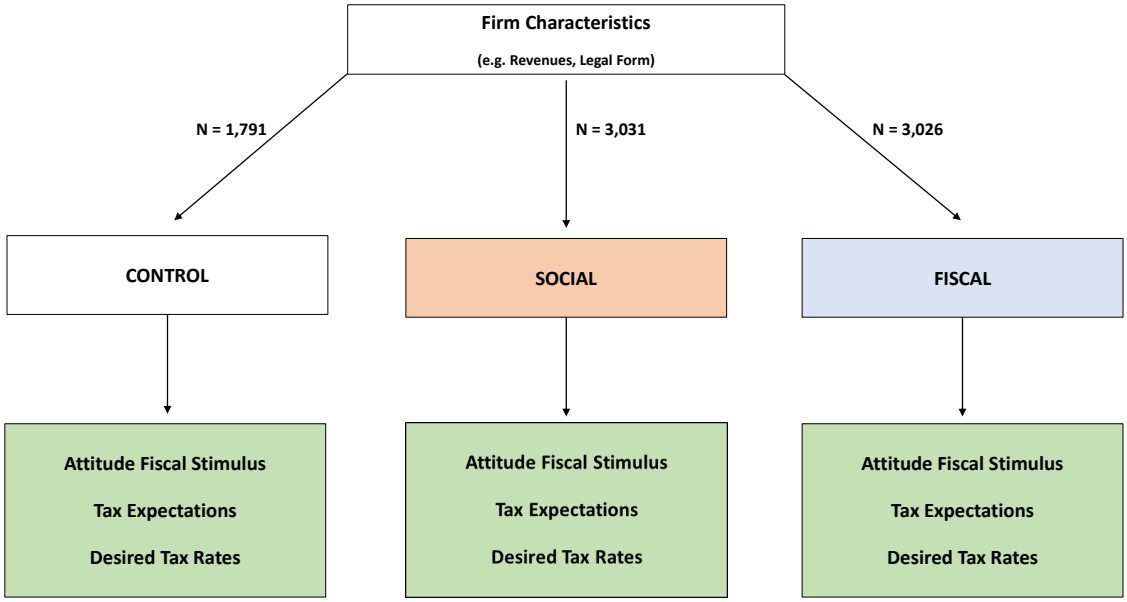
C.2.1 Experimental Design

Figure C.1 presents the design of our survey experiment. As described in Section 4.3 in Chapter 4, we start by asking all survey participants about general characteristics of the firm, such as legal form or revenues. Survey participants are then randomly assigned to one of the experimental groups (CONTROL, SOCIAL, FISCAL). Depending on the assigned experimental group, survey participants then receive different information treatments in the SOCIAL and FISCAL treatment or no text when assigned to the CONTROL group. After receiving the experimental treatment, all firm decision-makers are first asked whether they think it is justified for the government to intervene with a stimulus package to support the economy. Next, participants are asked about their expectations concerning future tax rates and their preferences regarding tax adjustments. The survey then goes on with further questions not related to the survey experiment.

C.2.2 CONTROL Group

In the CONTROL group, the survey participants do not receive an experimental treatment. Survey participants are only asked about their perspective on the fiscal stimulus package. In the CONTROL group, where we do not refer to a specific stimulus package, we use the indefinite article *"a"* when referring to the stimulus package. Figure C.2 displays the original survey question in German. We also provide the English translation of the survey question

Figure C.1: Experimental Design



Note: Figure C.1 illustrates the design of the survey experiment.

further below.

English Translation:

Do you think it is justified for the government to intervene with a stimulus package?

Answer Options:

Absolutely Justified, Justified, Neither Justified nor Unjustified, Not Justified, Absolutely Not Justified

C.2.3 FISCAL Treatment

In the FISCAL treatment, firms are presented with the text shown in Figure C.3. In this section, we also provide an English translation of the experimental treatment text. In the FISCAL treatment, we mention the size of the stimulus package as well as potential fiscal consequences of the state intervention like the need for higher government revenues or

Figure C.2: CONTROL Group - Survey Screen

Note: Figure C.2 illustrates the survey screen concerning the attitude toward the fiscal stimulus package for the CONTROL group.

spending cuts due to increased government spending in the wake of the Corona crisis. In addition, we make the prospect of tax increases more credible by giving an example from the 2008/2009 financial crisis. On the same screen, participants are also asked about their perspective on the fiscal stimulus package, similar to firms in the CONTROL group. Moreover, as we explicitly mention a 130 billion EUR stimulus package, we use the demonstrative *"this"* to indicate that we refer to the stimulus package mentioned in the treatment. Moreover, the addition *"at the taxpayer's expense"* was only used in the FISCAL treatment, as the FISCAL treatment wants to stress the need to counter-finance the state intervention.

English Translation:

Background information:

The federal government has pledged support of ***130 billion EUR*** *as part of the economic stimulus package adopted in June.*

The increased government spending and additional debt incurred in the wake of the Corona crisis could necessitate higher government revenues or spending cuts in the future.

After the 2008/2009 financial crisis, for example, tax rates were raised in many European

countries. Representatives of the CDU have already announced that the debt incurred as a result of the Corona crisis will be reduced again by 2030.

Do you think it is justified for the government to intervene with this stimulus package at the taxpayer's expense?

Answer Options:

Absolutely Justified, Justified, Neither Justified nor Unjustified, Not Justified, Absolutely Not Justified

Figure C.3: FISCAL Treatment - Survey Screen

Note: Figure C.3 illustrates the survey screen concerning the experimental treatment and the attitude toward the fiscal stimulus package for the FISCAL treatment group.

C.2.4 SOCIAL Treatment

In the SOCIAL treatment, firms are presented with the text shown in Figure C.4. Again, we also provide an English translation of the treatment text further below. Similar to the FISCAL treatment, we mention the size of the stimulus package. Furthermore, we stress that many firms are in distress due to the Corona crisis through no fault of their own. We mention the hospitality industry as a striking example of a sector where revenue decreases have been extensive, as many firms in this sector had to shut down temporarily due to public life restrictions during the Corona outbreak. Analogue to the FISCAL treatment, participants are then asked about their perspective on the fiscal stimulus package. Again, we use the demonstrative *"this"* to indicate that we refer to the stimulus package mentioned in the treatment.

English Translation:

Background information:

Many companies have experienced hardship through no fault of their own as a result of the Corona crisis, with sales in the hospitality industry, for example, plummeting 75.8 percent compared to the same month last year. The federal government has pledged support of ***130 billion EUR*** *as part of the economic stimulus package adopted in June.*

Do you think it is justified for the government to intervene with this stimulus package?

Answer Options:

Absolutely Justified, Justified, Neither Justified nor Unjustified, Not Justified, Absolutely Not Justified

Figure C.4: SOCIAL Treatment - Survey Screen

Note: Figure C.4 illustrates the survey screen concerning the experimental treatment and the attitude toward the fiscal stimulus package for the SOCIAL treatment group.

C.2.5 Tax Expectations

After the experimental intervention, we first ask the decision-makers of participating firms about their medium-term tax expectations. We ask for tax expectations first, as we want to verify that firm decision-makers do not confuse tax expectations and preferences about tax policy, i.e., their preference to pay taxes.

Figure C.5 provides a screenshot of the original survey question in German. In addition, we present the English translation below. Answers can be given on a slider ranging from -20 percentage points to 20 percentage points. Additionally, a *Do not know*-option is provided.

English Translation:

Based on your current tax rate, what changes in the following tax rates do you expect in the ***medium term*** *(12-24 months)?*

Please indicate your expectation in percentage points.

Answer Options:

Corporate Tax, Local Business Tax, Income Tax, Social Security Contributions, Solidarity Contribution, Capital Gains Tax

Answer Scale: *[-20 percentage points, 20 percentage points]; Do not know-option given*

Figure C.5: Expected Medium-Term Tax Changes - Survey Screen

Note: Figure C.5 presents the original survey question concerning the expected medium-term tax rate change.

C.2.6 Desired Adjustment Taxes

Figure C.6 presents a screenshot of the original survey question in German concerning the desired adjustment of the *corporate tax*, *local business tax*, *income tax* and the *capital gains tax*. In this section, we also provide the English translation of the survey question. Similar to the question concerning medium-term tax expectations, answers can be given on a slider ranging from -20 percentage points to 20 percentage points. Additionally, a *Do not know-*

option is provided.

English Translation:

From your company's perspective, by how many percentage points would you want to adjust the following types of taxes based on your current tax rate to ensure that the government is able to support businesses in crises?

Answer Options:

Corporate Tax, Local Business Tax, Income Tax, Social Security Contributions, Solidarity Contribution, Capital Gains Tax

Answer Scale: *[-20 percentage points, 20 percentage points]; Do not know-option given*

Figure C.6: Desired Taxes - Survey Screen

Note: Figure C.6 illustrates the original survey question concerning the desired tax adjustment.

C.3 Supplementary Analysis - Main Results

C.3.1 Robustness Analysis - Attitude Fiscal Stimulus

In Section 4.4.1 of Chapter 4, we show that, in our setting, the SOCIAL treatment does not have a significant effect compared to the CONTROL group on the perception of the stimulus program being just. However, when emphasizing the higher tax burden in the FISCAL treatment, firm decision-makers are less likely to perceive the stimulus as justified, as they become more aware of the cost of the fiscal stimulus. In this section, we demonstrate that the treatment effects are robust to considering two additional estimation methods.

First, we test the robustness of our results using an *ordered probit model*, in which the dependent variable is defined as a 5-point Likert scale variable (i.e., *Not Absolutely Justified*, *Not Justified*, *Neither Justified Nor Unjustified*, *Justified*, *Absolutely Justified*). The ordered probit model is particularly suitable for variables representing ordered categories. In particular, it accounts for the order of the categories without assuming equal distances between the categories. This is in contrast to a linear regression approach, which assumes continuous and equidistant intervals. Table C.6 presents the results of the ordered probit regression. We observe that the FISCAL treatment reduces the likelihood of perceiving the stimulus as just. Furthermore, the marginal effects presented in Table C.6 show that decision makers in the FISCAL treatment are less likely to select the *Absolutely Justified* category and are more likely to select the categories *Neither Justified Nor Unjustified*, *Not Justified*, or *Not Absolutely Justified* compared to the CONTROL group. In contrast, depending on the specification, the SOCIAL treatment either has no statistically significant impact or only a weak impact on attitudes toward the fiscal stimulus compared to the CONTROL group.

Second, we test the robustness of the treatment effects using a logistic estimation strategy, in which the dependent variable is equal to 1 for firm decision-makers selecting the items *Absolutely Justified* or *Justified* and is zero otherwise. An advantage of using a logistic regression is that the predicted probabilities lie between 0 and 1, which may not hold when

using a linear regression model with a binary dependent variable. Table C.5 shows that the odds ratios for the SOCIAL treatment group in both models are very close to 1, indicating that the SOCIAL treatment has no significant effect on the odds of firm decision-makers considering the fiscal stimulus as justified compared to decision-makers in the CONTROL group. However, the odds ratios for the FISCAL treatment group in both models are significantly less than 1. This indicates that firms in the FISCAL treatment group have substantially lower odds of considering the fiscal stimulus as justified compared to the CONTROL group (i.e., a decrease in odds of around 73% (1-0.263)).

Overall, both additional estimation methods support our experimental results presented in the main paper regarding the perception of the stimulus program.

Table C.5: Attitude toward Fiscal Stimulus: Logistic Regression

Dependent Var.:	**Attitude toward Fiscal Stimulus**	
	Binary (0/1)	Binary (0/1)
	Odd Ratios	Odd Ratios
	(1)	(2)
Baseline CONTROL		
SOCIAL	0.976	0.996
	(0.092)	(0.129)
FISCAL	0.263***	0.263***
	(0.023)	(0.032)
Controls	No	Yes
N	7,197	4,289
pseudo R^2	0.069	0.079

Note: Table C.5 presents odd ratios of a logistic regression from equation (7). **Dependent variable**: dummy variable equal to 1 for firm decision-makers selecting the items *Absolutely Justified* or *Justified* and zero otherwise. **Independent variables**: experimental group, constant, and control variables if stated (size group (EC's definition for small and medium-sized enterprises (SMEs)), industry (WZ08 1-digit), legal form, gender of manager, manager education, position in company. Robust standard errors in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table C.6: Attitude toward Fiscal Stimulus: Ordered Probit Regression

Dependent Var.: **Attitude toward Fiscal Stimulus**		
	(1)	(2)
Baseline CONTROL		
SOCIAL	-0.071*	-0.076
	(0.037)	(0.049)
FISCAL	-0.809***	-0.821***
	(0.038)	(0.050)
cut1	-2.135***	-2.381***
	(0.041)	(0.235)
cut2	-1.544***	-1.723***
	(0.036)	(0.233)
cut3	-1.060***	-1.268***
	(0.034)	(0.233)
cut4	0.247***	0.065
	(0.033)	(0.232)
Controls	No	Yes
N	7197	4291
pseudo R^2	0.043	0.052

	Average Predictions		**Marginal Effects**	
			Baseline CONTROL	
Absolutely Not Justified				
CONTROL	0.016***	0.013***		
	(0.002)	(0.002)		
SOCIAL	0.020***	0.016***	0.003**	0.003
	(0.001)	(0.002)	(0.002)	(0.002)
FISCAL	0.092***	0.078***	0.076***	0.065***
	(0.005)	(0.006)	(0.004)	(0.005)
Not Justified				
CONTROL	0.045***	0.044***		
	(0.003)	(0.004)		
SOCIAL	0.051***	0.050***	0.006*	0.006
	(0.003)	(0.003)	(0.003)	(0.004)
FISCAL	0.139***	0.143***	0.094***	0.099***
	(0.005)	(0.007)	(0.005)	(0.007)
Neither Justified Nor Unjustified				
CONTROL	0.083***	0.072***		
	(0.004)	(0.005)		
SOCIAL	0.091***	0.079***	0.008*	0.007
	(0.003)	(0.004)	(0.004)	(0.005)
FISCAL	0.170***	0.153***	0.086***	0.082***
	(0.005)	(0.007)	(0.005)	(0.006)
Justified				
CONTROL	0.453***	0.444***		
	(0.007)	(0.010)		
SOCIAL	0.463***	0.457***	0.011*	0.013
	(0.006)	(0.008)	(0.006)	(0.008)
FISCAL	0.454***	0.466***	0.001	0.022***
	(0.006)	(0.008)	(0.006)	(0.008)
Absolutely Justified				
CONTROL	0.403***	0.427***		
	(0.013)	(0.016)		
SOCIAL	0.375***	0.398***	-0.027*	-0.029
	(0.008)	(0.010)	(0.014)	(0.019)
FISCAL	0.146***	0.160***	-0.257***	-0.267***
	(0.006)	(0.007)	(0.013)	(0.017)
Controls	No	Yes	No	Yes
N	7,197	4,291	7,197	4,291

Note: Table C.6 presents results from estimating equation (7) as an Ordered Probit Regression. **Dependent variable**: attitude toward fiscal stimulus measured on a 5-point Likert scale (1 = Absolutely Not Justified, 2 = Not Justified, 3 = Neither Justified Nor Unjustified, 4 = Justified, 5 = Absolutely Justified). **Independent variables**: experimental group, constant, and control variables if stated (size group (EC's definition for small and medium-sized enterprises (SMEs)), industry (WZ08 1-digit), legal form, gender of manager, manager education, position in company. Average and marginal effects are presented. Robust standard errors in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

C.3.2 Multiple Hypotheses Testing

As our experiment involves multiple treatments (FISCAL, SOCIAL) and several outcomes of interest (e.g., corporate tax, business tax, income tax, capital gains tax), we simultaneously test several null hypotheses when comparing the effects of the treatments against the control group and against each other. However, testing multiple hypotheses at the same time increases the likelihood of false rejections beyond the preferred level. To mitigate this, we adopt the approach developed by List et al. (2019), which utilizes a bootstrap method for testing multiple hypotheses simultaneously while incorporating dependence information among the p-values – an aspect not considered by conventional methods like the Holm (1979) correction. This integration significantly enhances the power to detect truly false null hypotheses.

Table C.7 presents the multiplicity-adjusted p-values following the approach of List et al. (2019), alongside the more traditional p-values calculated using the Holm (1979) method. We compute the adjusted p-values using the STATA package *mhtexp* provided by List et al. (2019). Overall, compared to the results in Section 4.4 of Chapter 4, our findings on treatment effects across the experimental groups remain highly robust after adjusting for multiple hypotheses testing, reinforcing confidence in the validity of the reported treatment effects.

Table C.7: Multiple Hypothesis Tests - Main Results

		Multiplicity adj. *p*-Values	
	Mean Difference	List et al. (2019)	Holm (1979)
Attitude toward Fiscal Stimulus (0 - Not Justified/Neutral, 1 - Justified)			
CONTROL vs. SOCIAL	0.003	0.788	0.788
CONTROL vs. FISCAL	0.252	0.000***	0.001***
SOCIAL vs. FISCAL	0.249	0.000***	0.001***
Corporate Tax Expectations (in p.p.)			
CONTROL vs. SOCIAL	0.004	0.527	0.983
CONTROL vs. FISCAL	0.022	0.000***	0.003***
SOCIAL vs. FISCAL	0.027	0.000***	0.002***
Business Tax Expectations (in p.p.)			
CONTROL vs. SOCIAL	0.004	0.624	1.000
CONTROL vs. FISCAL	0.025	0.000***	0.002***
SOCIAL vs. FISCAL	0.028	0.000***	0.002***
Income Tax Expectations (in p.p.)			
CONTROL vs. SOCIAL	0.003	0.662	0.934
CONTROL vs. FISCAL	0.024	0.000***	0.004***
SOCIAL vs. FISCAL	0.027	0.000***	0.004***
Capital Gains Tax Expectations (in p.p.)			
CONTROL vs. SOCIAL	0.000	0.917	0.917
CONTROL vs. FISCAL	0.029	0.000***	0.003***
SOCIAL vs. FISCAL	0.030	0.000***	0.003***
Desired Adjustment Corporate Tax (in p.p.)			
CONTROL vs. SOCIAL	0.001	0.854	0.854
CONTROL vs. FISCAL	0.025	0.000***	0.003***
SOCIAL vs. FISCAL	0.026	0.000***	0.004***
Desired Adjustment Business Tax (in p.p.)			
CONTROL vs. SOCIAL	0.001	0.934	1.000
CONTROL vs. FISCAL	0.028	0.000***	0.003***
SOCIAL vs. FISCAL	0.029	0.000***	0.002***
Desired Adjustment Income Tax (in p.p.)			
CONTROL vs. SOCIAL	0.001	0.936	1.000
CONTROL vs. FISCAL	0.028	0.000***	0.002***
SOCIAL vs. FISCAL	0.029	0.000***	0.004***
Desired Adjustment Capital Gains Tax (in p.p.)			
CONTROL vs. SOCIAL	0.011	0.038**	0.048**
CONTROL vs. FISCAL	0.041	0.000***	0.003***
SOCIAL vs. FISCAL	0.030	0.000***	0.002***

Note: Table C.7 presents the multiplicity-adjusted p-values following the approaches of List et al. (2019) and Holm (1979). The multiplicity-adjusted p-values are calculated using the STATA package *mhtexp*, provided by List et al. (2019). *Mean Difference* refers to the mean differences between the experimental groups. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

C.4 Supplementary Analysis - Heterogeneity Analysis

Section C.4 extends the analysis on differential effects with regard to the preference to pay taxes for different types of firms and decision-makers. We provide the regressions underlying the GATEs presented in Section 4.5.2 in Chapter 4. In addition, we investigate whether heterogeneity with respect to the take-up of governmental aid and a firms' profit change has an influence on the preference to pay taxes.

C.4.1 Legal Forms

Table C.8 presents results of an OLS regression of equation (11). The GATEs shown in Table 4.5 in Chapter 4 are based on these regression results. As shown in Section 4.5.2, the regressions indicate that incorporated firms want a significantly lower *corporate tax* as compared to non-incorporated firms. However, we also observe that the SOCIAL and FISCAL treatment have a positive effect on corporations compared to corporations in the CONTROL group. This suggests that narratives centered on fairness and fiscal prudence, as presented in the SOCIAL and FISCAL treatments, can effectively dampen a firm's self-interested desire to reduce the taxes it faces.

C.4.2 Attitude toward Fiscal Stimulus

Table C.9 presents OLS estimations of equation (11). The results from this table serve as the basis for the GATEs detailed in Table 4.4 in Section 4.5.2. Furthermore, in Table C.10, we present OLS estimation results for equation (11), where support for the fiscal stimulus program is characterized not by a binary variable but by a continuous 5-point Likert scale variable treated as a continuous variable.

Overall, we observe heterogeneity across treatments. Firm decision-makers, who consider the government stimulus to be justified and are part of the FISCAL treatment group, have a higher preference to pay taxes compared to decision-makers who do not consider the

Table C.8: Heterogeneous Effects by Legal Form - Full Interaction

Dependent Variable:	**Adjustment Corporate Tax**
	(1)
Constant	-0.008
	(0.008)
SOCIAL	-0.012
	(0.010)
FISCAL	0.016
	(0.010)
Corporations	-0.031***
	(0.009)
SOCIAL × Corporations	0.015
	(0.011)
FISCAL × Corporations	0.012
	(0.011)
N	3328
Adj. R^2	0.030

Note: OLS estimates from the regression of equation (11): $y_i = \alpha + \beta \times Corporation_i + \sum_{k=1}^{2} \delta_k \times TREATMENT_{ik} + \sum_{k=1}^{2} \theta_k \times Corporation_i \times TREATMENT_{ik} + \varepsilon_i$. Dependent variable: desired adjustment in percentage points of corporate tax. Independent variables: experimental group, dummy variable $Corporation_i$ equal to one for corporations and zero for non-incorporated firms, and a constant. Robust standard errors in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

stimulus program to be justified and/or belong to another experimental group (CONTROL, SOCIAL). This pattern is not specific to a particular tax but present for the *corporate tax*, *business tax*, *income tax* as well as for the *capital gains tax*. We conclude that firm decision-makers' preference to pay taxes is particularly strong when decision-makers realize the need for the government to counter-finance the fiscal stimulus supporting firms during the crisis, which is stressed in the FISCAL treatment.

Table C.9: Heterogeneous Effects by Attitude toward Fiscal Stimulus - Full Interaction

Dependent Variable:	**Adjustment Corporate Tax**	**Adjustment Business Tax**	**Adjustment Income Tax**	**Adjustment Capital Gains Tax**
	(1)	(2)	(3)	(4)
Constant	-0.037***	-0.041***	-0.032***	-0.019*
	(0.009)	(0.009)	(0.009)	(0.011)
SOCIAL	-0.003	-0.009	-0.018*	0.006
	(0.011)	(0.011)	(0.011)	(0.013)
FISCAL	0.019**	0.015	0.010	0.037***
	(0.009)	(0.010)	(0.010)	(0.012)
Justified	0.006	0.002	0.001	0.020*
	(0.010)	(0.010)	(0.010)	(0.012)
SOCIAL × Justified	0.004	0.007	0.018	0.003
	(0.011)	(0.012)	(0.012)	(0.014)
FISCAL × Justified	0.014	0.018*	0.027**	0.010
	(0.010)	(0.011)	(0.011)	(0.013)
N	3620	3830	3834	3654
Adj. R^2	0.030	0.031	0.042	0.041

Note: OLS estimates from the regression of equation (11): $y_i = \alpha + \beta \times Justified_i + \sum_{k=1}^{2} \delta_k \times TREATMENT_{ik} + \sum_{k=1}^{2} \theta_k \times Justified_i \times TREATMENT_{ik} + \varepsilon_i$. Dependent variable: desired adjustment in percentage points of *corporate tax*, *business tax*, *income tax* and *capital gains tax*. Independent variables: experimental group, dummy variable $Justified_i$ equal to one for firm decision-makers finding the fiscal stimulus absolutely justified or justified, and zero otherwise, and a constant. Robust standard errors in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

C.4.3 Take-up of Governmental Aid

Section C.4.3 investigates whether firms which benefited directly from the governmental stimulus program are more or less inclined to pay higher taxes. In particular, we focus on firms which claimed at least one of the following three government relief measures: **Corona emergency relief (Corona Überbrückungshilfe)**, **interim aid (Sofortkredit)** or **KfW special program (KfW-Kredit)**. In our sample, 40% of survey participants benefited from at least one of the three measures. We concentrate on these measures because they provide direct financial assistance, unlike other Corona-era policies (e.g., credit authorization, tax payment deferrals). This direct aid might have heightened firms' appreciation for state support during crises, leading to a higher preference to pay taxes. However, firms which have claimed at least one of these measures might be firms which are hit the hardest by the crisis. Indeed, we find that the take-up of at least one of these measures is negatively

Table C.10: Heterogeneous Effects by Attitude toward Fiscal Stimulus (Cont. Variable) - Full Interaction

Dependent Variable:	**Adjustment Corporate Tax**	**Adjustment Business Tax**	**Adjustment Income Tax**	**Adjustment Capital Gains Tax**
	(1)	(2)	(3)	(4)
Constant	-0.017	-0.023	-0.033*	-0.019
	(0.019)	(0.019)	(0.019)	(0.022)
SOCIAL	-0.018	-0.016	-0.024	-0.006
	(0.022)	(0.022)	(0.022)	(0.025)
FISCAL	-0.027	-0.026	-0.019	0.002
	(0.020)	(0.020)	(0.020)	(0.023)
$Justified\_cont_i$	-0.004	-0.004	0.000	0.004
	(0.005)	(0.004)	(0.005)	(0.005)
SOCIAL × $Justified\_cont_i$	0.004	0.003	0.005	0.003
	(0.005)	(0.005)	(0.005)	(0.006)
FISCAL × $Justified\_cont_i$	0.015***	0.014***	0.013***	0.012**
	(0.005)	(0.005)	(0.005)	(0.005)
N	3620	3830	3834	3654
Adj. R^2	0.035	0.035	0.048	0.047

Note: OLS estimates from the regression of equation (11): $y_i = \alpha + \beta \times Justified\_cont_i + \sum_{k=1}^{2} \delta_k \times TREATMENT_{ik} + \sum_{k=1}^{2} \theta_k \times Justified\_cont_i \times TREATMENT_{ik} + \varepsilon_i$. Dependent variable: desired adjustment in percentage points of *corporate tax*, *business tax*, *income tax* and *capital gains tax*. Independent variables: experimental group, categorical variable treated as a continuous variable $Justified\_cont_i$ (1 = Absolutely Not Justified, 2 = Not Justified, 3 = Neither Justified Nor Unjustified, 4 = Justified, 5 = Absolutely Justified), and a constant. Robust standard errors in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

correlated (-0.30: p-value: 0.00) with having a positive profit change or no profit change compared to before the start of the pandemic. Such firms could therefore also have a lower preference to pay taxes. There are several potential explanations for this. First, firms with declining profits may, from a long-term perspective, anticipate a post-crisis economic rebound. In anticipation of future recovery, they might aim to offset pandemic losses by reducing their tax liabilities. Second, firms experiencing significant profit reductions could undervalue public benefits, possibly due to perceived insufficient governmental aid during the crisis. Third, firms with a strong profit decline might have also stronger financial constraints and therefore want to reduce their tax burden more than firms with stable or increased profits.

Table C.11 presents the GATEs for the *corporate tax*, *business tax*, *income tax* as well as for the *capital gains tax*. It differentiates based on whether firms adopted at least one of the

three measures and the experimental group they belong to. The results are based on OLS regressions in Table C.12. Overall, we observe that the adoption of these governmental aid programs does not notably increase the preference to pay taxes compared to firms that did not benefit from these governmental measures. In fact, firms that accessed at least one of these measures display a reduced preference to pay taxes compared to firms which did not take up one of these measures, corroborating our earlier arguments explaining their potential lower preference to pay taxes. Nevertheless, the information that current government spending may have to be refinanced by future tax increases in the FISCAL treatment did reduce the difference between the GATEs of these two type of firms compared to the other experimental groups. Similar to the results in Section 4.5.2 of Chapter 4, this finding suggests that firms react stronger to fiscal prudence narratives than fairness arguments in our setting.

Table C.11: Heterogeneous Effects by Take-up of Government Aid

	Estimated GATE (SE in parentheses)		
	CONTROL	SOCIAL	FISCAL
Corporate Tax			
No Take-up Aid	-0.026***	-0.025***	-0.002
	(0.003)	(0.003)	(0.002)
Take-up Aid	-0.041***	-0.045***	-0.013***
	(0.006)	(0.004)	(0.003)
Difference:	-0.015**	-0.020***	-0.011***
Take-up - No Take-up	(0.007)	(0.005)	(0.004)
Business Tax			
No Take-up Aid	-0.034***	-0.034***	-0.010***
	(0.004)	(0.003)	(0.002)
Take-up Aid	-0.054***	-0.058***	-0.019***
	(0.006)	(0.004)	(0.003)
Difference:	-0.020***	-0.023***	-0.009**
Take-up - No Take-up	(0.007)	(0.005)	(0.004)
Income Tax			
No Take-up Aid	-0.030***	-0.028***	-0.002
	(0.003)	(0.003)	(0.002)
Take-up Aid	-0.041***	-0.046***	-0.011***
	(0.006)	(0.004)	(0.003)
Difference:	-0.011*	-0.018***	-0.008**
Take-up - No Take-up	(0.007)	(0.005)	(0.004)
Capital Gains Tax			
No Take-up Aid	-0.001	0.009***	0.036***
	(0.004)	(0.003)	(0.003)
Take-up Aid	-0.011*	0.001	0.038***
	(0.006)	(0.005)	(0.004)
Difference:	-0.010	-0.008	0.002
Take-up - No Take-up	(0.008)	(0.006)	(0.005)

Note: Table C.11 presents estimated group average treatment effects (GATEs) for the *corporate tax*, *business tax*, *income tax* and *capital gains tax* for firms, varying by having taken up government aid or not. Estimated GATEs are based on the regression in column (1) (*corporate tax*), column (2) (*business tax*), column (3) (*income tax*) and column (4) (*capital gains tax*) shown in Table C.12 in the Appendix. The regressions are based on equation (11): $y_i = \alpha + \beta \times Take\_up_i + \sum_{k=1}^{2} \delta_k \times TREATMENT_{ik} + \sum_{k=1}^{2} \theta_k \times Take\_up_i \times TREATMENT_{ik} + \varepsilon_i$. Dependent variable: desired adjustment in percentage points of *corporate tax*, *business tax*, *income tax* and *capital gains tax*. Independent variables: experimental group dummy, dummy variable $Take\_up_i$ equal to one if a firm claimed at least one of the following three government relief measures **Corona emergency relief/Corona Überbrückungshilfe**, **interim aid/Sofortkredit** or **KfW special program/Kwf-Kredit**, and a constant. The row *Difference* presents the difference between the GATE of firms having taken up these government aids and the ones which have not (slight differences due to rounding). Robust standard errors are in parentheses. Significance levels are: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.12: Heterogeneous Effects by Take-up of Government Aid - Full Interaction

Dependent Variable:	**Adjustment Corporate Tax**	**Adjustment Business Tax**	**Adjustment Income Tax**	**Adjustment Capital Gains Tax**
	(1)	(2)	(3)	(4)
Constant	-0.026***	-0.034***	-0.030***	-0.001
	(0.003)	(0.004)	(0.003)	(0.004)
SOCIAL	0.001	-0.000	0.002	0.010*
	(0.004)	(0.005)	(0.004)	(0.005)
FISCAL	0.024***	0.024***	0.027***	0.037***
	(0.004)	(0.004)	(0.004)	(0.005)
$Take\_up_i$	-0.015**	-0.020***	-0.011*	-0.010
	(0.007)	(0.007)	(0.007)	(0.008)
SOCIAL $\times$ $Take\_up_i$	-0.006	-0.003	-0.007	0.002
	(0.008)	(0.008)	(0.008)	(0.010)
FISCAL $\times$ $Take\_up_i$	0.003	0.011	0.003	0.012
	(0.008)	(0.008)	(0.008)	(0.009)
N	3785	4011	4027	3822
Adj. R^2	0.031	0.036	0.035	0.031

Note: OLS estimates from the regression of equation (11): $y_i = \alpha + \beta \times Take\_up_i + \sum_{k=1}^{2} \delta_k \times TREATMENT_{ik} + \sum_{k=1}^{2} \theta_k \times Take\_up_i \times TREATMENT_{ik} + \varepsilon_i$. Dependent variable: desired adjustment in percentage points of *corporate tax*, *business tax*, *income tax* and *capital gains tax*. Independent variables: experimental group, dummy variable $Take\_up_i$ equal to one if a firm claimed at least one of the following three government relief measures **Corona emergency relief/Corona Überbrückungshilfe**, **interim aid/Sofortkredit** or **KfW special program/Kwf-Kredit**, and zero otherwise, and a constant. Robust standard errors in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

C.4.4 Profit Change

Table C.13 examines whether firms' tax preferences vary based on how their profits were affected by the crisis. To this end, we conduct a sample split to identify whether there is a significant difference in desired tax rates between firms which experienced an increase in profits during the crisis (or experienced no change in profits) and firms which suffered a decrease in profits during the crisis. Overall, 45% of firms experienced a profit increase or no change in profits in our sample. Table C.13 shows that firms, which experienced an increase in profits during the crisis, opted, on average, for a significantly smaller reduction in desired tax rates than their counterparts that experienced a decrease in corporate profits. This pattern holds for all type of taxes. Moreover, the information that current government spending may have to be refinanced by tax increases in the FISCAL treatment did not reduce the difference between GATEs of these two type of firms compared to the other experimental groups significantly (except for the *capital gains tax*).

As mentioned in Section C.4.3, this finding can potentially be explained by the three following reasons. First, firms witnessing profit declines might, with a long-term view in mind, expect a post-crisis economic upturn. To counterbalance their pandemic-induced losses, they may seek to minimize tax liabilities in the future. Second, companies with marked profit reductions might undervalue public services, potentially feeling the government's crisis aid fell short. Lastly, firms facing significant profit downturns could be grappling with tighter financial constraints, leading them to pursue lower tax obligations compared to their more financially stable counterparts.

Table C.13: Heterogeneous Effects by Profit Change

	Estimated GATE (SE in parentheses)		
	CONTROL	SOCIAL	FISCAL
Corporate Tax			
Profit Decrease	-0.045***	-0.044***	-0.017***
	(0.004)	(0.003)	(0.003)
Profit Increase/Stable	-0.017***	-0.019***	0.007***
	(0.004)	(0.003)	(0.003)
Difference:	0.028***	0.025***	0.024***
Profit Increase/Stable - Profit Decrease	(0.006)	(0.004)	(0.004)
Business Tax			
Profit Decrease	-0.055***	-0.055***	-0.025***
	(0.004)	(0.003)	(0.003)
Profit Increase/Stable	-0.026***	-0.028***	0.001
	(0.005)	(0.003)	(0.003)
Difference:	0.029***	0.027***	0.025***
Profit Increase/Stable - Profit Decrease	(0.007)	(0.004)	(0.004)
Income Tax			
Profit Decrease	-0.047***	-0.046***	-0.014***
	(0.004)	(0.003)	(0.003)
Profit Increase/Stable	-0.020***	-0.021***	0.005**
	(0.004)	(0.003)	(0.002)
Difference:	0.028***	0.025***	0.019***
Profit Increase/Stable - Profit Decrease	(0.006)	(0.004)	(0.004)
Capital Gains Tax			
Profit Decrease	-0.016***	-0.003	0.033***
	(0.005)	(0.004)	(0.003)
Profit Increase/Stable	0.009*	0.018***	0.041***
	(0.005)	(0.004)	(0.003)
Difference:	0.024***	0.022***	0.007
Profit Increase/Stable - Profit Decrease	(0.007)	(0.005)	(0.005)

Note: Table C.13 presents estimated group average treatment effects (GATEs) for the *corporate tax*, *business tax*, *income tax* and *capital gains tax* for firms, varying by having taken up government aid or not. Estimated GATEs are based on the regression in column (1) (*corporate tax*), column (2) (*business tax*), column (3) (*income tax*) and column (4) (*capital gains tax*) shown in Table C.14 in the Appendix. The regressions are based on equation (11): $y_i = \alpha + \beta \times Profit\_Increase/Stable_i + \sum_{k=1}^{2} \delta_k \times TREATMENT_{ik} + \sum_{k=1}^{2} \theta_k \times Profit\_Increase/Stable_i \times TREATMENT_{ik} + \varepsilon_i$. Dependent variable: desired adjustment in percentage points of *corporate tax*, *business tax*, *income tax* and *capital gains tax*. Independent variables: experimental group dummy, dummy variable $Profit\_Increase/Stable_i$ equal to one if a firm experienced a profit increase or no change in their profits since January 2020 (before the Corona pandemic started), and zero otherwise, and a constant. The row *Difference* presents the difference between the GATE of firms having experienced a positive profit change or exhibit stable profits and firms with a profit decline (slight differences due to rounding). Robust standard errors are in parentheses. Significance levels are: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.14: Heterogeneous Effects by Profit Change - Full Interaction

Dependent Variable:	**Adjustment Corporate Tax**	**Adjustment Business Tax**	**Adjustment Income Tax**	**Adjustment Capital Gains Tax**
	(1)	(2)	(3)	(4)
Constant	-0.045***	-0.055***	-0.047***	-0.016***
	(0.004)	(0.004)	(0.004)	(0.005)
SOCIAL	0.001	-0.000	0.001	0.012*
	(0.005)	(0.005)	(0.005)	(0.006)
FISCAL	0.028***	0.030***	0.033***	0.049***
	(0.005)	(0.005)	(0.005)	(0.006)
$Profit\_Increase/Stable_i$	0.028***	0.029***	0.028***	0.024***
	(0.006)	(0.007)	(0.006)	(0.007)
SOCIAL × $Profit\_Increase/Stable_i$	-0.002	-0.002	-0.003	-0.002
	(0.007)	(0.008)	(0.007)	(0.009)
FISCAL × $Profit\_Increase/Stable_i$	-0.004	-0.004	-0.008	-0.017**
	(0.007)	(0.008)	(0.007)	(0.009)
N	3602	3804	3815	3632
Adj. R^2	0.046	0.050	0.049	0.038

Note: OLS estimates from the regression of equation (11): $y_i = \alpha + \beta \times Profit\_Increase/Stable_i + \sum_{k=1}^{2} \delta_k \times TREATMENT_{ik} + \sum_{k=1}^{2} \theta_k \times Profit\_Increase/Stable_i \times TREATMENT_{ik} + \varepsilon_i$. Dependent variable: desired adjustment in percentage points of *corporate tax*, *business tax*, *income tax* and *capital gains tax*. Independent variables: experimental group, dummy variable $Profit\_Increase/Stable_i$ equal to one if a firm experienced a profit increase or no change in their profits since January 2020 (before the Corona pandemic started), and zero otherwise, and a constant. Robust standard errors in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Chapter 4 Appendix References

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Short Curriculum Vitae

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