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Downward Revision of Investment Decisions after Corporate Tax Hikes

Abstract

This paper estimates the causal effect of corporate tax hikes on firm investment based on more than 1,400 local tax changes. By observing planned and realized investment volumes in a representative sample of German manufacturing firms, we can study how tax hikes induce firms to revise their investment decisions. On average, the share of firms that invest less than previously planned increases by three percentage points after a tax hike. This effect is twice as large during recessions.

JEL-Codes: G110, H250, H320, H710, O160.

Keywords: investment, corporate taxation, state dependence, business cycle.

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1 INTRODUCTION

The effect of corporate taxes on firm investment is a central question in macroeconomics and public finance. Corporate tax reforms like the US Tax Cuts and Jobs Act (TCJA) are often motivated by the argument that high corporate tax rates inhibit firm investment and growth (CEA, 2017). Standard theories of corporate taxation indeed predict that firms cut on investment projects if their after-tax net present value is reduced by tax increases (Hall and Jorgenson, 1967). To what degree corporate taxation affects investment, however, is ultimately an empirical question. Credible evidence on it is still scarce, as estimating the causal effect of corporate taxes on investment is challenging.

On the one hand, attributing cross-country discrepancies in investment behavior to differences in corporate tax rates is difficult to justify, as the timing of tax reforms often correlates with other macroeconomic determinants of firm investment. On the other hand, studies exploiting within-country variation need a valid control group and face the problem that many national-level tax reforms such as the TCJA change several parameters of the tax system simultaneously. For these reasons, quasi-experimental evidence on the response of investment to changes in the corporate tax burden originates predominantly from targeted tax deductions, which provide exogenous variation in exposure to tax decreases across firms of different size or in different industries (e.g., Garrett et al., 2020; Ohrn, 2018; Zwick and Mahon, 2017). However, to what extent the effects of such specific policies generalize to changes in the corporate tax *rate* remains unclear.

This paper addresses this gap by combining the specific system of business taxation in Germany with unique data on firm-level investment plans and their realizations. Our identification strategy builds on two pillars. First, we exploit the decentralized design of the German local business tax (LBT): While tax base and liability criteria are set by the federal government, municipalities each year autonomously decide on the statutory tax rates.[1] We can therefore distinguish tax rate variation from potential changes in the tax base. Furthermore, municipalities adjust their taxes frequently. Restricting the analysis to tax increases, which are much more common than tax cuts, our identifying variation consists of 1,443 tax hikes between 1980 and 2018. The large number of tax hikes allows us to control for potentially heterogeneous time trends across regions or industries.

Second, we estimate the investment response of firms to these tax changes by leveraging panel data on both planned and realized investment volumes among a large, representative survey of on average 1,500 German manufacturing firms. The unique feature of our data is that each fall, firms report the planned volume of investment for the subsequent year. Municipalities announce tax changes for the subsequent year typically in December, i.e.,

[1] This variation has been used by Fuest et al. (2018) to study the wage incidence of corporate taxation and Isphording et al. (2021) to assess the effects on R&D spending.

after firms have reported their investment plans. In consequence, firms are surprised by the tax changes and have not included this information in their investment plans. At the same time, investment plans arguably incorporate all other (partially unobserved) private and public information of the firms that determine investment in the subsequent year.

Focusing on the revision of investment plans, i.e., the difference between the investment volume planned prior to the tax change and the investment volume ultimately realized, is advantageous from several perspectives.[2] Most importantly, investment revisions allow us to estimate the effect of corporate taxes on firm investment under weaker assumptions than usually possible. Because investment plans incorporate all relevant firm-level information, our results would still be unbiased if, for example, the occurrence of tax hikes were endogenous to local economic conditions. Moreover, considering revisions avoids problems with sensitivity in estimates due to the lumpy nature of investment, and hedges against potential bias in two-way fixed effects models (de Chaisemartin and D'Haultfœuille, 2022).

Our results show economically large and statistically significant investment responses for firms experiencing a tax increase. On average, the share of firms that invest less than previously planned increases by approximately 3 percentage points after a tax hike. In terms of magnitudes, a 1 percentage point increase in the LBT rate is associated with a decrease in the ratio of realized over planned investment by 2.3-3.8 percent, depending on the empirical specification. As firms on average invest approximately as much as previously planned, this maps into a semi-elasticity of investment with respect to the LBT rate of around 3. We verify our identification approach with an event study design, demonstrating that firms only deviate from the baseline probability for revising an investment decision in the year of the tax hike. While our baseline specification exploits variation in statutory tax rates (as previous literature on LBT in Germany did, see, e.g., Fuest et al., 2018; Isphording et al., 2021), we find similar effects when relying on effective tax rates that are more common in studies for other countries and settings.

The magnitude of the investment response varies substantially over the business cycle. Compared to our baseline estimates, the share of firms that invest less than previously planned in response to a tax hike is twice as large if taxes are increased during a recession. We discuss three potential explanations for this state dependence of tax shocks, relating to uncertainty about expected returns to investments, cashflow sensitivity, and tax incidence.

Our main contribution is to investigate the impact of hikes in the corporate tax rate on firm investment.[3] While we are not the first to study this important question, we add

[2]Comparing planned to realized quantities connects to the macro literature exploiting deviations from forecasts for identification (e.g. Romer and Romer, 2004).

[3]Other studies investigate firm-level responses to the corporate income tax along other margins (e.g. Auerbach, 2006; Fuest et al., 2018; Garrett et al., 2020; Ljungqvist and Smolyansky, 2018; Suárez Serrato and Zidar, 2016). We add to this literature by providing new evidence on the investment response.

to the literature along two dimensions. First and foremost, by using a novel identification strategy based on revisions of investment plans to investigate firms' investment response, we can eliminate concerns about omitted variable bias that have not been fully resolved in most of the previous literature. When using realized investments as outcome variable instead, results could be biased if tax policy responds to economic conditions. For example, Giroud and Rauh (2019) and Ivanov et al. (2022) investigate the effects of changes in US state level taxes on firm level outcomes.[4] To be precise, the former paper studies the effects of tax changes on the reallocation of labor and capital across states while the latter looks at corporate leverage as the main outcome. In additional analyses, both papers also investigate—among others—effects on firms' capital stock. For identification, both papers rely on parallel trends between US states.[5] While in our context the variation is on a morel local level (municipalities within states), the key advantage is that assuming parallel trends is well justified by the fact that firms' ex ante planned volume of investment—–i.e., the counterfactual level of investment in absence of a tax hike—–should incorporate all firm-level information besides the tax shock that is relevant for investment in the subsequent year. That is, we do not require flat pre-trends in realized investment levels but only in terms of revisions of investment plans, which is much less demanding.

An alternative way to overcome endogeneity concerns is to focus on targeted tax deductions or accelerated depreciation allowances, giving rise to arguably exogenous variation in exposure to tax decreases for firms in different sectors and industries (Curtis et al., 2021; Garrett et al., 2020; Guceri and Albinowski, 2021; House and Shapiro, 2008; Maffini et al., 2019; Ohrn, 2018; Xu and Zwick, 2022; Zwick and Mahon, 2017). However, the extent to which the effects of such specific policies generalize to changes of the tax rate remains unclear. Studying these targeted policies can therefore not substitute for a direct evaluation of the investment effects of changing the corporate tax rate, which affects all corporate firms at the same time and independently of their investment behavior. To the best of our knowledge, the only other paper using firm-level data and quasi-experimental variation to study the investment responses to a change in the universal corporate income tax rate is Harju et al. (2022).[6] However, as the Finish corporate tax cut also entailed

[4]Mertens and Ravn (2013) use aggregate data and combine a narrative approach with a structural VAR model to exploit changes in US federal corporate taxes.

[5]Both studies provide an extension using a narrative approach in the spirit of Romer and Romer (2010) that classifies arguably exogenous tax changes. Yet, these approaches only exclude a small number of potentially endogenous tax changes from the analysis and hence some concerns remain. Furthermore, the sample in Giroud and Rauh (2019) is restricted to large multi-state firms, and Ivanov et al. (2022) study tax decreases, whereas we focus on tax hikes.

[6]In the German context, Dobbins and Jacob (2016) compare the differential investment responses of domestically and foreign-owned firms after a cut in the federal corporate tax rate in 2008. Lerche (2022) estimates the effects of an investment tax credit in East Germany on firms' production behavior.

an increase in dividend taxation, they cannot consistently disentangle the effects of both channels. Moreover, the German setting has the advantage to offer substantially larger variation especially in terms of the number of tax rate changes.

In addition, our findings of higher investment responses during recessions relate to an ongoing debate about the state dependence of fiscal multipliers (Auerbach and Gorodnichenko, 2013; Ghassibe and Zanetti, 2022; Ramey and Zubairy, 2018) and the state dependence of investment effects in response to tax changes more specifically (Demirel, 2021; Hayo and Mierzwa, 2021; Jones et al., 2015; Ljungqvist and Smolyansky, 2018; Winberry, 2021). We complement this macroeconomic evidence by means of firm-level microdata and a distinct research design, showing that investment reacts much stronger to tax increases during recessions.

The remainder of the paper is structured as follows. Section 2 describes the municipality-level data on local business tax rates and the survey data on firm-level investment plans and their realizations. Section 3 presents our empirical strategy, while Section 4 documents the results. Section 5 concludes.

2 INSTITUTIONAL BACKGROUND AND DATA

To investigate the effects of corporate tax rate changes on firm investment, we merge municipality-level data on local business tax rates with unique data on firm-level investment plans and their realizations.

2.1 The German Local Business Tax

Institutional Background. The local business tax (LBT) is one of three types of taxes on business income in Germany. It is applied to the operating profits of both corporate and non-corporate firms. While tax base and liability criteria of the LBT are set at the federal level, municipalities decide autonomously on the tax rate. The tax rate consists of two components: a basic rate, which is determined by the federal government, and a local scaling factor, which is set at the municipal level. Each year, the municipal council has to vote on next year's scaling factor, even if it remains unchanged. As it is common practice to decide on next year's local scaling factor jointly with the adoption of the budget in the year's last meeting of the municipal council, tax changes are typically announced in December.[7] Municipalities in our sample are approximately ten times more likely to increase rather than decrease their local scaling factor. In consequence, the identifying variation in our setting is too weak to consistently estimate the effect of tax decreases

[7] Appendix Figure A.1 substantiates this empirically, showing that newspaper coverage of LBT hikes indeed peaks each year in December.

on investment.[8] We thus restrict the analysis to tax changes induced by municipalities increasing their local scaling factors, henceforth referred to as a tax hike. This implies that the tax reforms exploited in this paper affect investment exclusively via increases in the tax rate, and not via changes in the tax base. Taxable profits of firms with establishments in more than one municipality are divided between municipalities according to formula apportionment based on the payroll share. Appendix A provides additional details on the institutional setting.

Variation in Business Tax Rates. We use information on municipal tax scaling factors from the Statistical Offices of the German Federal States for the years 1980 to 2018. We enrich these data with information on municipality budgets and local economic conditions from several administrative data sources, leaving us with a panel of all German municipalities with extensive information on taxes, revenues and expenditures. To avoid capturing structural changes of the German reunification, and as data for East Germany are only available since 1990, we restrict our sample to West German municipalities (excl. West-Berlin). We furthermore exclude the few municipalities that underwent a municipal merger during the period of consideration, as we cannot determine their exact tax rates.[9]

There is substantial variation in LBT rates across municipalities and over time.[10] As shown in Panel (A) of Figure 1, average tax rates differ strongly between municipalities, ranging from 12 to 34 percent. Panel (B) displays the identifying variation we rely on, i.e., the number of tax hikes between 1980 and 2018. Only few municipalities never increased the LBT in this period, while the median municipality increased the LBT rate three times and the median duration between two tax hikes in our sample is 13 years. The distribution of tax hikes is rather stable over time in terms of average size and dispersion (see Appendix Figure B.3). Importantly, past increases in the LBT contain very little predictive power for future tax hikes, as shown in Appendix Figure B.4.

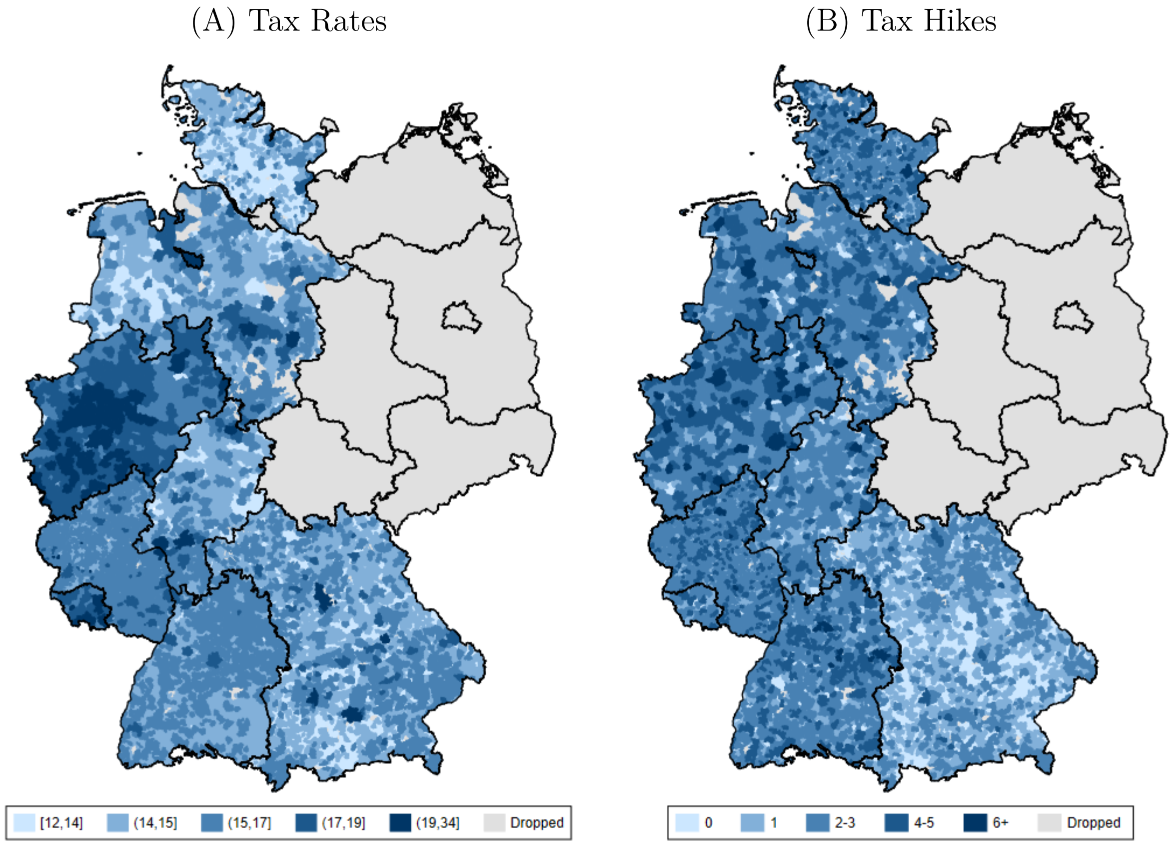
After combining the municipality-level data on LBT rates with the firm-level investment data described in Section 2.2, we can exploit large parts of this variation in LBT rates. As summarized in the left panel of Table 1, our empirical strategy outlined in Section 3 relies on 1,443 tax hikes in 802 municipalities. The average tax hike amounts to 0.92 percentage points, corresponding to a 6 percent increase on average. The right

[8] The number of tax decreases that could in principle be used in the analysis is very low. Combining the municipality-level data on LBT rates and the firm-level data from the IVS, our analysis could only exploit 236 firm-year observations (0.7% of all observations) that face a tax drop in a given year despite spanning a time frame of almost four decades.

[9] Municipal mergers were very frequent in East Germany after 1990 and this rule would also lead to an exclusion of many municipalities in East Germany.

[10] See Appendix B.1 for a more detailed description and investigation of the variation in LBT rates.

Figure 1 – Variation in Local Business Tax Rates (1980-2018)



Notes: This figure shows the cross-sectional and time variation in municipal scaling factors of the German local business tax (LBT). Panel (A) plots the average LBT rate (in percent) induced by different scaling factors for the period 1980-2018. Panel (B) indicates the number of tax hikes, defined as an increase of the scaling factor. Municipalities in light grey areas are dropped from the sample as they are either located in East Germany or underwent a change of boundaries due to a merger. Moreover, we exclude observations where a tax hike was followed or preceded by another tax hike in the next or last two years.

panel summarizes the variation in tax hikes across firms. On average, approximately 7 percent of firms are exposed to a tax hike each year.

2.2 Firm-level Data on Revisions of Investment Plans

We use micro data on firms' investment behavior from the ifo Investment Survey (IVS, 2019). The IVS is conducted biannually (spring and fall) by the ifo Institute on behalf of the European Commission and covers a representative sample of incorporated firms in the German manufacturing sector.[11] The main purpose of the IVS is to obtain timely

[11] Appendix B.2 and Sauer and Wohlrabe (2020) provide additional information on the purpose and design of the survey, its representativeness, data access, and the wording of the survey questions used in the paper. The IVS micro data have been extensively used in recent research, e.g., Bachmann et al. (2017), Bachmann and Zorn (2020), and Link et al. (2023).

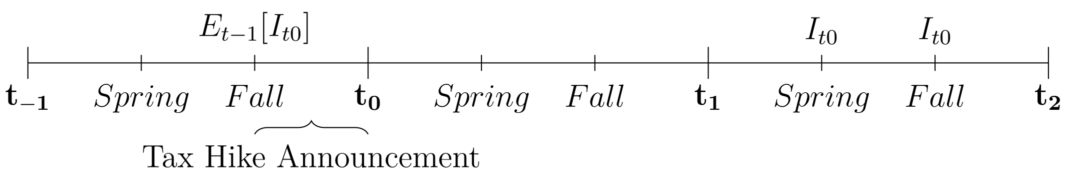
Table 1 – Tax Hikes across Municipalities and Firms: Summary Statistics

	Municipalities with Tax Hikes			Firm Observations with Tax Hikes		Firm Observations without Tax Hikes	
	N	Mean	SD	N	Share of Downward Revisions	N	Share of Downward Revisions
1980-1984	119	1.05	0.83	265	0.50	2655	0.45
1985-1989	131	0.92	0.53	340	0.49	4940	0.45
1990-1994	266	1.09	0.54	546	0.58	4831	0.54
1995-1999	228	0.94	0.47	385	0.51	4560	0.51
2000-2004	178	1.06	0.55	269	0.60	4711	0.58
2005-2009	106	0.78	0.51	161	0.66	4446	0.59
2010-2014	263	0.74	0.42	413	0.58	4118	0.58
2015-2018	152	0.69	0.38	248	0.63	2422	0.60
Full Sample	1443	0.92	0.54	2627	0.56	32683	0.54

Notes: This table reports summary statistics of the final sample used in the main analysis, i.e., after combining the municipality-level data on LBT rates and the firm-level data from the IVS. The left panel depicts the number of tax hikes at the municipality level that can be exploited in the empirical analysis along with the average size and standard deviation of these hikes. The right panel summarizes the number of firm observations that face a tax hike in a given year or not, as well as the average share of downward revisions of investment plans ($\frac{I_{i,t}}{E_{i,t-1}(I_{i,t})} < 1$) for each of these groups.

information on investment activity at disaggregated industry levels.[12] To achieve this goal, the IVS does not only elicit quantitative information on ex post realizations, but also on the planned volume of investment for the subsequent year. Thus, the panel structure of the IVS allows measuring how firms have revised their investment plans. In addition, survey participants provide quantitative information on revenues and the number of employees. The survey is usually completed by high-level management personnel at the firms' controlling departments (Sauer and Wohlrabe, 2020).

The timing of the survey is as follows:



In the fall of year t_{-1}, firms report how much they plan to invest in equipment and buildings (in Euro) in the subsequent year t_0, denoted $E_{t-1}[I_{t0}]$. The realized investment

[12]The German Federal Statistical Office releases information on realized investment at the levels of disaggregated industries only with a time lag of two years.

volume of year t_0, I_{t0}, is elicited in the spring and fall survey of year t_1.[13] By comparing planned investment $E_{t-1}[I_{t0}]$ to realized investment I_{t0}, we observe whether firms in year t_0 invested more, less, or the same amount as previously planned. As municipalities announce the LBT rate for year t_0 at the end of year t_{-1}, i.e., after the fall survey, firms' investment plans for year t_0 reported to the IVS do not include information about changes in the LBT.

The investment data of the IVS have been shown to be very accurate. For instance, Bachmann and Zorn (2020) show that aggregate investment growth calculated from the microdata of the IVS is highly correlated with manufacturing investment growth reported by the Federal Statistical Office, and Sauer and Wohlrabe (2020) report that the average absolute deviation of the former from the latter is less than two percentage points. Moreover, Bachmann et al. (2017) present a series of stylized facts on the cross-sectional and time-series properties of revisions of investment plans, i.e., the difference between ex ante planned and ex post realized investment volumes, showing that these deviations are meaningful along many dimensions. For example, they document that the overall distribution of revisions is not systematically skewed, while their cross-sectional average is procyclical.[14] This indicates that participants provide accurate investment plans given their current level of knowledge at the time of the survey.

We restrict the sample to firms that report both planned and ex post realized volumes of investment, referring to all years t_0 for which we have information on LBT rates in the municipality of their location available. Following the protocol deposited in Appendix B.3.1, we keep those firms for which we can observe revisions in investment plans for at least five years and, following Fuest et al. (2018), drop firms with legal forms that are exempted from the LBT. Our final sample consists of 35,310 firm-year observations in years $t_0 \in \{1980, 2018\}$ that are spread across 1,192 municipalities in West Germany. According to the descriptive statistics presented in greater detail in Appendix B.3.2, the median firm in our sample is a typical representative of the "German Mittelstand" employing 264 workers, generating annual revenues of 45 million Euro (CPI inflation-adjusted and—if denominated in German marks—converted to 2015 Euros), and investing 1.4 million Euro each year. For each firm, we can rely on information on reported planned and realized investment volumes in, on average, 17 years. In the final sample, firms report zero investment in only 0.7% of all observations.

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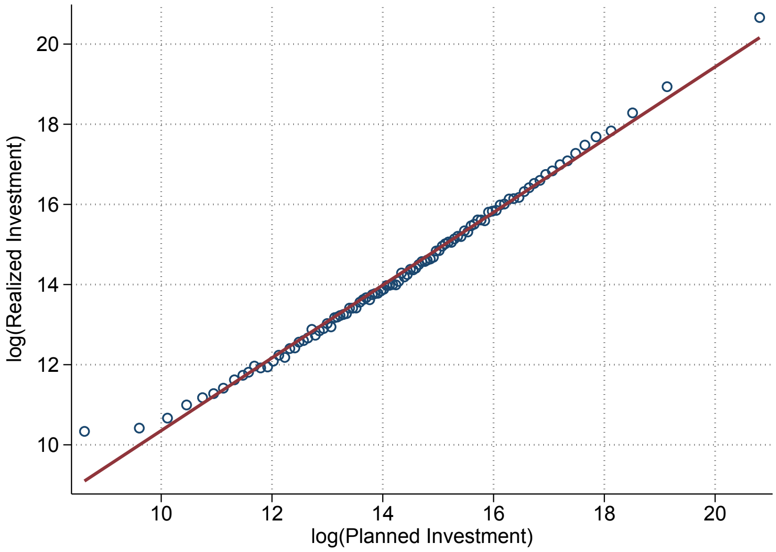
[13] Following Bachmann et al. (2017), we take the average of I_{t_0} if firms report it in both waves of year t_1 and drop the observation if these reports deviate more than 20% from the mean (see Appendix B.3.1 for details). The results are similar once we restrict the analysis to I_{t0} reported in the fall wave.

[14] Relatedly, Appendix Figure B.7 shows that the investment plans are more frequently and more strongly revised downward during recessions.

Figure 2 – Relationship between Planned and Realized Investment



Notes: This figure shows a binned scatter plot between ex ante planned and ex post realized levels of investment in year t_0 (both in logs) as reported by firms to the IVS in years t_{-1} and t_1, respectively. The red line depicts a linear fit of the data. The sample is restricted to observations in years without tax changes.

Importantly, investment plans for the next year contain a large amount of information that is highly predictive for the level of investment that is subsequently realized and that is changing within firms from year to year. The binned scatter plot depicted in Figure 2 demonstrates that the relationship between ex ante planned and ex post realized volumes of investment, i.e., $E_{t-1}[I_{t0}]$ and I_{t0} (both in logs), is highly linear and virtually corresponding to the 45 degree line. As depicted in Appendix Table B.3, 84% of the unconditional variation in (log) realized investment is explained by the investment plans for the respective year. Appendix B.3.3 presents a more detailed investigation of this relationship that, *inter alia*, demonstrates that investment plans, $E_{t-1}[I_{t0}]$, are much more strongly correlated with ex post realized investment, I_{t0}, than the realized level of investment in the previous year, I_{t-1}, and that these patterns even hold when controlling for firm fixed effects. Taken together, investment plans contain accurate information on subsequent year's investment that goes beyond the extrapolation of the level of investment that was realized in the year these plans are reported to the IVS.

The raw data provide a first indication of the main result of the paper, i.e., that firms revise investment decisions downwards after tax hikes. For each five-year interval of the data, the right panel of Table 1 depicts the average share of downward revisions of investment plans separately for firms in municipalities with and without tax hikes. The share of downward revisions is—at least weakly—larger among treated firms than untreated firms in each time interval. We investigate this effect more systematically in the remainder of the paper.

3 EMPIRICAL STRATEGY

Research Design. We seek to identify the average treatment effect of an increase in the statutory LBT rate on firm investment. We consider a firm as treated in year t_0 if residing in a municipality that increased its LBT scaling factor from year t_{-1} to t_0. The hypothesis guiding our analysis is that firms surprised by the announcement of a tax hike in December of t_{-1} will on average invest less in year t_0 than previously planned. We therefore expect downward revisions of planned investment to be more frequent in municipalities that increased their local scaling factors. At the same time, firms' investment plans elicited in the fall should incorporate all other, potentially unobserved, information influencing investment in the subsequent year.

Our identification strategy thus eliminates concerns about omitted variable bias. When using realized investment as outcome variable, results could be biased if tax policy responded to economic conditions, even after controlling for unit and time fixed effects, violating the parallel trends assumption. In our context this is quite different, as we observe the ex ante planned volume of investment—i.e., the counterfactual level of investment in absence of a tax hike—in addition to the ex post realized level of investment directly in our data. Using investment revisions instead of realized investment, we have a strong theoretical argument why we can extrapolate a (flat) pre-trend into the post-treatment period.

Hence, compared to using realized investment as dependent variable, our analysis only requires the weaker assumption that there are no unobserved factors that are both (i) correlated with investment and local tax policy in year t_0, and (ii) not in the information set of the firm when forming investment plans in the fall of year t_{-1}. The only scenario that could violate this assumption would be a *local* shock that hits after firms have reported their investment plans, and that induces municipalities to implement a tax hike *within a few weeks*. Given the municipal decision structures and the "speed" of German bureaucracy, however, such an immediate response is highly unlikely. Relatedly, Fuest et al. (2018) show that changes in the LBT are typically not triggered by shocks to economic variables, and Blesse et al. (2019) demonstrate that tax setting of the municipalities substantially deviates from theoretically optimal behavior. As in the US (Robinson and Tazhitdinova, 2022), regional variation in corporate tax rates seems to be to a large extent idiosyncratic and not readily explained by standard theories of tax setting. Overall, we are therefore confident that omitted variables do not threaten identification in our setting.

Instead, a potential limitation of our identification strategy is that some firms may put a positive probability on the scenario that taxes will be increased in the subsequent year, whereas our analysis implicitly assumes that firms expect taxes to remain constant. To the extent that this was not true, there would exist two potential sources of bias pointing in opposite directions. A downward bias originating from the treatment group (where some firms revise investment less strongly), and an upward bias originating from the control group (where some firms upward-revise investment if taxes are *not* increased). As long as the expected probability of a tax hike does not differ between treatment and control group, both biases will cancel out on average. This likely holds in practice. If at all, we would expect a higher expected probability for tax hikes in the treatment group. Because some firms may have private information about future tax hikes before investment plans are reported in the fall (Riedel and Simmler, 2021), they will—at least partially—incorporate this information into their investment plans and hence revise their investment decisions less strongly on average. If anything, we hence tend to underestimate the investment response to a tax hike.[15] In our data, however, we do not find evidence for a downward bias: Private information about future tax hikes should be more prevalent in smaller municipalities, where social ties to the municipality council are more likely, but treatment effects are not significantly different between cities and rural municipalities (see Figure 6, Panel B).

Measurement and Estimation. We use two variables to measure investment revisions. The first is an indicator for revising investment decisions downwards, defined as:

$$\text{Downward Revision:} \qquad \mathbb{1}\left(\frac{I_{i,t}}{E_{i,t-1}(I_{i,t})} < 1\right)$$

The downward revision indicator is attractive due to its robustness against outliers and non-linear investment responses. The second variable is the log revision ratio and takes

[15]Moreover, the investment response might be underestimated due to the fact that firms pay the LBT according to the payroll share attributable to each municipality. As firm investment reported in the IVS refers to all domestic plants, the tax hike variation is measured with error for firms with plants in multiple municipalities. Although the IVS data lacks information on the prevalence and the payroll share of multi-establishments, the resulting attenuation bias is arguably small as only 7% of firms in the manufacturing sector were operating in multiple municipalities in 2017 according to aggregated, administrative LBT data (*Gewerbesteuerstatistik*). Furthermore, Panel A of Figure 6 demonstrates that treatment effects do not differ by firm size, a proxy for being a multi-plant company.

the magnitude of each revision into account. It is defined as the natural logarithm of the ratio between realized and planned investment volumes:

$$\text{Log Revision Ratio:} \qquad ln\left(\frac{I_{i,t}}{E_{i,t-1}(I_{i,t})}\right)$$

We choose the logarithmic form due to the lumpy nature of investment, which means that the distribution of investment volumes is skewed and the revision ratio can get very large for small denominators. Moreover, the resulting estimates directly translate into the semi-elasticity of investment with respect to the tax rate, the relevant quantity of interest that we can directly compare to other estimates in the literature. As firms invest approximately as much as previously planned, the ratio of realized over planned investment is equal to one on average. As furthermore the revision ratio and realized investment are measured in logarithmic form, a tax hike which decreases the log revision ratio by 0.01 implies that both the revision ratio and realized investment decrease by 1%.[16]

In our main analysis, we estimate the following linear model by OLS:

$$\text{InvestmentRevision}_{i,m,t} = \gamma \text{TaxHike}_{m,t} + \mu_i + \phi_{l,t} + \psi_{s,t} + \varepsilon_{i,t}, \tag{1}$$

which explains the investment revision of firm i in municipality m and year t by municipality level tax hikes $\text{TaxHike}_{m,t}$ that take one of the following two forms:

$$\text{Tax Hike Indicator:} \qquad \mathbb{1}\left(\Delta tax_{m,t} > 0\right)$$

$$\text{Tax Hike in Percentage Points:} \qquad \Delta tax_{m,t}$$

The tax hike indicator equals one if at time t municipality m increased the LBT. In addition, $\Delta tax_{m,t}$ denotes the tax change in percentage points. As discussed above, the focus on deviations of realized investment from the planned value should by itself rule out omitted variable bias.[17] Still, some specifications additionally include firm fixed effects (μ_i) and year fixed effects at the level of industries ($\psi_{s,t}$) and federal states ($\phi_{l,t}$) to flexibly control for any time-invariant heterogeneity or systematic time trends in the probability of investment revisions and the frequency of tax hikes. In these specifications, we obtain

[16] In more recent years, firms tend to invest on average slightly less than previously planned (compare Appendix Figure B.7). For this reason, the constant for the log revision ratio in Table 2 is not exactly zero, and the constant for the share of downward revisions is slightly larger than 0.5. As such, the semi-elasticity of the revision ratio with respect to an LBT hike will slightly understate the semi-elasticity of investment with respect to an LBT hike.

[17] While Section 2.2 demonstrates that the investment plans reported to the IVS contain valuable information that is highly predictive for ex post realized investment volumes, these variables might be elicited imprecisely. The resulting measurement error in the dependent variable should thus decrease the precision of our estimates without resulting in attenuation bias.

a (generalized) Difference-in-Differences (DiD) estimate.[18] Standard errors are clustered at the municipality level.

4 RESULTS

We present our results in three steps: first, we show our baseline results and their robustness along various dimensions. Second, we discuss how the magnitude of the effects relates to other estimates in the literature, before third, documenting effect heterogeneity over the business cycle.

4.1 Revision of Investment Plans after Tax Hikes: Main Results

The baseline results presented in Table 2 reveal that firms affected by a tax hike strongly downward revise their investment decisions in the year this change is enacted. Panel (A) displays the estimates for the downward revision indicator. In Column (1), we compare the share of firms investing less than previously planned between municipalities where a tax hike is enacted and municipalities where the LBT rate did not change, without including any controls. We find that the share of firms that revise their investment decisions downwards is 2.7 percentage points higher in affected municipalities (Panel A1). The estimates presented in the remaining columns demonstrate that the point estimates for the tax hike indicator are barely affected by sequentially adding fixed effects at various dimensions, indicating that firms' investment plans already largely absorb regional and industry-specific shocks. For the size of the tax change (Panel A2), the inclusion of fixed effects tends to slightly increase the estimated coefficients. In Column (5), where we impose the most restrictive set of fixed effects, the effects of the tax hike indicator and the percentage change in the LBT on the probability of downward revising investment decisions are estimated at 3.3 and 2.4 percentage points, respectively.

Panel (B) repeats the analysis using the log revision ratio as dependent variable. The estimated coefficients are negative in all specifications of Panel B1, indicating that firms invest less than previously planned in response to a tax hike. While the effects are estimated less precisely compared to Panel A1, the point estimates are largely unaffected by the choice of the control vector. Again focusing on the most restrictive specification in Column (5), we find that the ratio of realized over planned investment decreases by

[18] Note that using investment revisions as the outcome of interest implies that treatment effects realize exclusively in the treatment period. Due to this lack of treatment effect dynamics, the recent concerns about bias in two-way fixed effects models (e.g., de Chaisemartin and D'Haultfœuille, 2022) do not apply in this setting, as discussed below in more detail. It is, however, relevant in a setting when using realized investment (instead of revisions) as the outcome variable in Section 4.2. Inspired by Dube et al. (2022), firms are then assigned to another firm identifier in the middle between two tax hikes in order to ensure that there is only one treatment for each unit and to allow for different long-run trends.

Table 2 – Difference-in-Differences: Investment Revisions after a Tax Hike

	(1)	(2)	(3)	(4)	(5)
Panel (A): Downward Revision					
A1: Tax Hike Indicator: $\mathbb{1}(\Delta tax_{m,t} > 0)$					
	0.027^{**}	0.028^{***}	0.026^{**}	0.028^{***}	0.033^{***}
	(0.011)	(0.010)	(0.011)	(0.011)	(0.011)
Constant	0.536^{***}	0.536^{***}	0.536^{***}	0.536^{***}	0.535^{***}
	(0.005)	(0.005)	(0.001)	(0.001)	(0.001)
A2: Tax Hike in Percentage Points: $\Delta tax_{m,t}$					
	0.012	0.018^{*}	0.017^{*}	0.021^{**}	0.024^{**}
	(0.009)	(0.009)	(0.010)	(0.009)	(0.010)
Constant	0.537^{***}	0.536^{***}	0.536^{***}	0.536^{***}	0.536^{***}
	(0.005)	(0.005)	(0.001)	(0.001)	(0.001)
Observations	35310	35310	35310	35310	35310
Panel (B): Log Revision Ratio					
B1: Tax Hike Indicator: $\mathbb{1}(\Delta tax_{m,t} > 0)$					
	-0.031^{*}	-0.033^{**}	-0.025	-0.029^{*}	-0.036^{**}
	(0.016)	(0.015)	(0.017)	(0.016)	(0.017)
Constant	-0.033^{***}	-0.033^{***}	-0.033^{***}	-0.033^{***}	-0.032^{***}
	(0.006)	(0.006)	(0.001)	(0.001)	(0.001)
B2: Tax Hike in Percentage Points: $\Delta tax_{m,t}$					
	-0.023^{*}	-0.032^{**}	-0.028^{*}	-0.034^{**}	-0.038^{**}
	(0.014)	(0.013)	(0.014)	(0.014)	(0.016)
Constant	-0.033^{***}	-0.033^{***}	-0.033^{***}	-0.033^{***}	-0.032^{***}
	(0.006)	(0.006)	(0.001)	(0.001)	(0.001)
Observations	34421	34421	34421	34421	34421
Firm FE	-	-	✓	✓	✓
Year FE	-	✓	-	✓	-
Year × State FE	-	-	-	-	✓
Year × Industry FE	-	-	-	-	✓

Notes: This table reports estimates from linear regressions of Equation (1). "Downward Revision" is an indicator that is one if the fraction of realized investment over planned investment is below one. "Log Revision Ratio" is the natural logarithm of this ratio. "Tax Hike Indicator" is an indicator that is one if the local corporate tax rate is higher than in the year before. "Tax Hike" is the change in the local corporate tax rate in percentage points compared to the previous year. Industry fixed effects refer to the ifo industry classification, comparable to two-digit NACE industries. Standard errors in parentheses are clustered at the municipality level. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.6 percent in response to a tax hike. Taking the magnitude of tax changes into account in Panel B2, the estimate in Column (5) implies that a 1 percentage point increase in the LBT rate is associated with a decrease in the revision ratio by 3.8 percent. Since in the absence of a tax hike firms invest approximately as much as they have planned, the ratio of realized over planned investment is close to one (and the log of the ratio is close to zero, as visible from the constant). Hence, our estimates directly map into a semi-elasticity of investment with respect to the LBT of around 3.

Overall, we find a clear and statistically significant negative investment response of firms to increases in corporate tax rates in all estimated models.

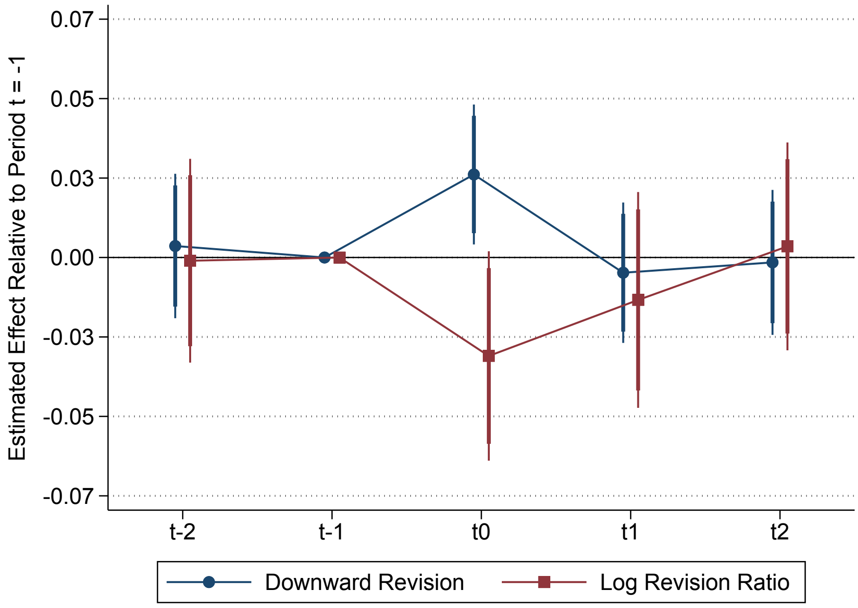
Economic Size of the Investment Response. The estimated investment response is economically sizable. To illustrate this, we conduct a back-of-the-envelope calculation, described in detail in Appendix D. According to our estimated semi-elasticity of 3, each additional Euro of tax revenues raised comes with a loss in firm investment of 2.12 Euro in the first year after a tax hike. If we also consider that lower firm investment reduces tax revenues in the medium run due to lower profits, the approximated investment loss for each additional Euro of tax revenue increases to an estimate between 2.14 Euro and 2.28 Euro, depending on the assumed strength of the second-round effect. This indicates that the marginal value of public funds (MVPF) in the spirit of Hendren and Sprung-Keyser (2020) of increasing the LBT is greater than one. While these projections rely on a series of simplifying assumptions, they still illustrate that the foregone volume of investment is non-negligible.

Validity of the Identifying Assumptions. Next, we estimate an event study to test a central implication of our identifying assumptions: an increase in investment revisions should only occur in the year of the tax hike (t_0), while no effect should be visible in the years before, when the tax hike could not have been anticipated, i.e., we should observe parallel trends before the tax change. Moreover, a tax hike implemented in January of year t_0 should be known to the firm (a) when reporting its investment plans for year t_1 to the IVS (in the fall wave of year t_0) and (b) when reporting the actual volume of investment for year t_1 (in the spring or fall wave of year t_2). Hence, investment revisions should also not be systematically higher in any period after year t_0.

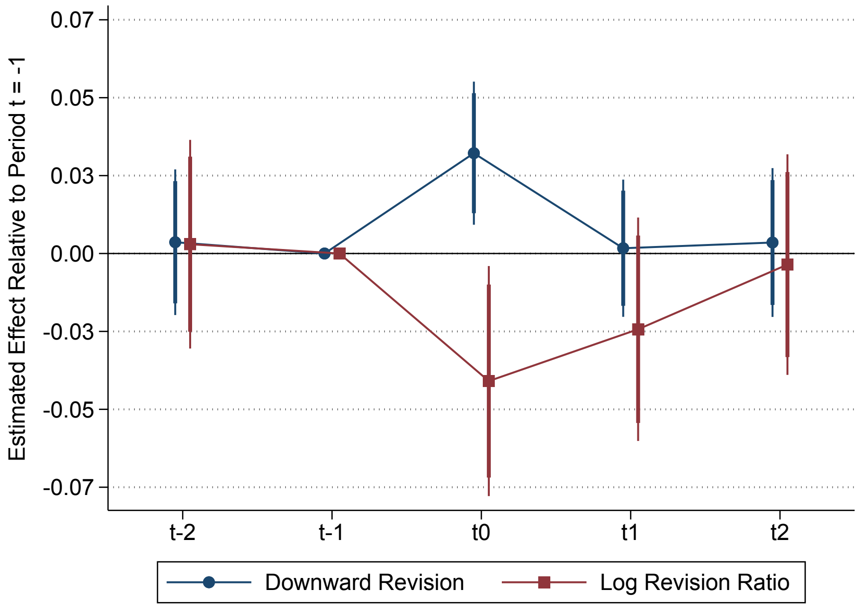
The results of the event study regression presented in Figure 3 confirm that investment revisions occur immediately in t_0 when the tax hike is enacted. In contrast, the point estimates are (close to) zero in all other years, supporting the validity of our identifying

Figure 3 – Event Study: Investment Revision Effect after a Tax Hike

(A) Year and Firm-Level Fixed Effects



(B) Full Set of Fixed Effects



Notes: This figure shows the estimates of the following event-study regression: $\text{InvestmentRevision}_{i,t} = \sum_{j=-2}^{2} \gamma_j \text{TaxHike}^{j}_{m,t} + \varepsilon_{i,t}$. In Panel (A), we additionally include year and firm fixed effects. In Panel (B), industry-year, state-year, and firm fixed effects are included. The reference period is t_{-1}. The dependent variable is based on the ratio of realized investments over planned investments (elicited in the fall of the previous year). "Downward Revision" is an indicator that is one if the ratio is below one. "Log Revision Ratio" is the natural logarithm of this ratio. Industry fixed effects are at the ifo industry classification level that is comparable to two-digit NACE industries. The confidence intervals refer to the levels of 90% (thick line) and 95% (thin line).

assumptions. Appendix Figure C.1 shows that these patterns also hold when extending the time window covered by the estimation to four years prior and post treatment.[19]

Moreover, recent research in econometrics calls for caution when estimating two-way fixed effects models in generalized DiD settings with multiple treatment groups and periods (see, e.g., the survey by de Chaisemartin and D'Haultfœuille, 2022), as these estimators only provide an unbiased DiD estimate if the treatment effect is constant between groups and over time. This problem is less relevant in our setting given that the estimated treatment effects are not dynamic, i.e., do not evolve over time (as shown above). Still, to demonstrate that the recent critique does not apply here, we repeat the event study using the imputation estimator proposed by Borusyak et al. (2021) as well as the interaction-weighted estimator by Sun and Abraham (2021). As shown in Appendix Figure C.2, results are very similar to Figure 3.

Robustness of Main Results. Next, we demonstrate that our main results are robust along various dimensions. We start by highlighting that the fact that the estimates summarized in Table 2 are barely affected by sequentially adding fixed effects at various dimensions provides a first indication for the robustness of our main results. If confounding local shocks were important, estimates should vary across these different specifications, which they do not.[20] This pattern suggests that firms' investment plans already incorporate shocks along various dimensions that might simultaneously affect firm investment and the municipalities' decisions to increase the LBT. Hence, focusing on deviations of realized investment from investment plans reported prior to the tax hike should by itself rule out many potential channels of omitted variable bias.

Nevertheless, attributing investment revisions to increases in the LBT could be problematic if tax hikes were accompanied by changes in municipality expenditures. If municipalities re-invested the additional tax revenue in local infrastructure, tax hikes would not only lead to higher tax payments on profits, but could also increase the value of local amenities for firms. If this created incentives for investment, this would counteract the direct effect of the tax hike and the true investment response would be underestimated. While this scenario is not implausible in general, we cannot detect concurrent expenditure shocks in our data. In line with evidence from Fuest et al. (2018) and Isphording et al.

[19]The reason why we restrict ourselves to two pre- and post-event periods in Figure 3 is sample size. Because we always require that no other tax change happened in the pre- and post-event periods, extending the number of periods would shrink the size of the estimation sample considerably.

[20]Relatedly, Appendix Table C.4 shows that treatment effects are not heterogeneous for firms experiencing large revenue drops (compared to those firms who do not) as a proxy for a local (or even firm-specific) shock. This provides further empirical evidence that local shocks are not driving our results. This is not surprising, given that we analyze manufacturing firms whose products are tradable across municipalities and hence are less reliant on local markets.

(2021), Appendix Figure C.3 shows that, on average, municipalities do not increase their expenditures jointly with the LBT.

Moreover, our results are robust to excluding the years after the German reunification from our sample. Although we only focus on firms located in West Germany, many of these firms were affected by this particularly turbulent economic time and their investment decisions were potentially affected by many investment subsidies that were introduced with the aim to foster investment in East Germany. Indeed, the estimated effect size is slightly, but not substantially, larger when excluding the time period after the German reunification (Appendix Table C.1).

As a final, more general robustness check, we conduct a permutation test by randomly assigning tax hikes to municipalities and, for each permutation, estimate Model (1) with both dependent variables, the downward revision indicator and the log revision ratio, along with the full set of fixed effects. Appendix Figure C.4 plots the cumulative distribution function of these placebo treatment coefficients. The non-parametric p-values obtained from this exercise are 0.0005 for the downward revision indicator and 0.0115 for the log revision ratio, and thus in the same order of magnitude as in our baseline regression.

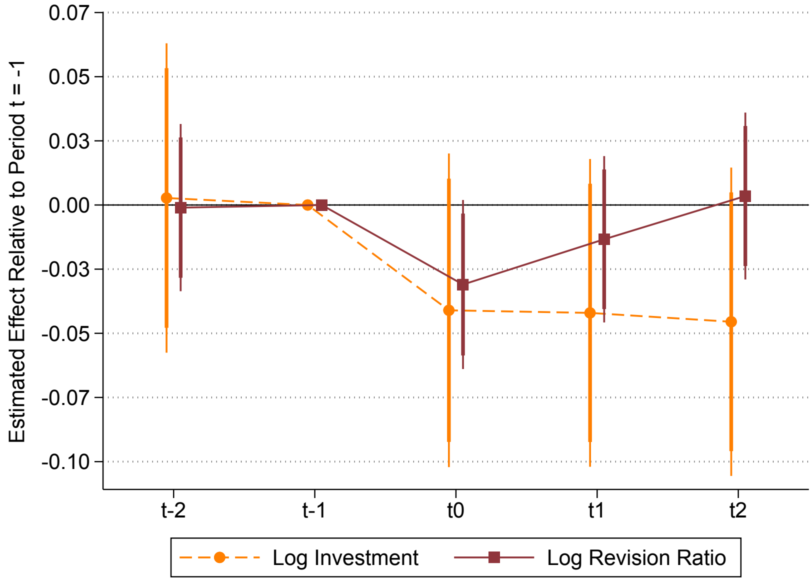
4.2 Magnitude of Effect Size in Comparison to the Literature

While previous literature focuses on the effect of tax changes on the realized level of investment, a key novelty of our paper is studying the revision of investment plans. In order to facilitate the comparison of our result to previous findings, this section first demonstrates that our results regarding the downward revision of investment decisions can indeed be interpreted in terms of a reduction in realized investment of equal size. In a second step, we convert the identifying variation in the statutory LBT rate to changes in effective tax rates or the user cost of capital to show how our results compare to studies that rely on these frequently used specifications.

Effect of Tax Hikes on Realized Investment. As argued in Section 3, we can directly interpret a one percent decrease in the log revision ratio as a one percent decrease in the realized level of investment because firms on average invest as much as previously planned. In order to demonstrate this empirically, Figure 4 plots the coefficients of two event study regressions using either the log revision ratio or the realized volume investment (in logs) as dependent variable. As expected, the point estimates of the investment responses in t_0, i.e., the year of the tax hike, are of comparable size and indicate that both realized investment and the revision ratio drop by approximately 3–4% in response to a tax hike.

The investment effect is estimated more precisely in the specification that uses investment revisions. This demonstrates a distinct advantage of our empirical strategy.

Figure 4 – Effect of Tax Hike on Investment Realizations and Revisions



Notes: This figure shows event-study estimates of log realized investment (orange, dashed lines) and the log revision ratio (red, solid lines) on the tax hike indicator and fixed effects at the levels of firm identifiers and years. The reference period is t_{-1}. "Log Revision Ratio" is the natural logarithm of the ratio between the ex post realized and ex ante planned volume of investment. Inspired by Dube et al. (2022), when estimating the effects with respect to log realized investment, firms are assigned to another firm identifier after the year that is in the middle between two tax hikes in order to ensure that there is only one treatment for each unit and to allow for different long-run trends. The confidence intervals refer to the significance levels of 90% (thick lines) and 95% (thin lines).

When directly estimating the effect of tax hikes on the level of realized investment, any variation in investment that is uncorrelated with the tax hike (e.g., due to outliers) but only imperfectly controlled for in the regression imposes noise in the dependent variable that blurs the coefficients. In contrast, investment revisions directly incorporate the counterfactual scenario as investment plans allow to directly absorb any other, potentially unobserved, information that might influence investment in the subsequent year besides the effect of the tax hike. In consequence, the short-run investment response can be estimated more precisely and under much weaker assumptions compared to just using the realized volume of investment as dependent variable.

Further, the results suggest that increases in the LBT have lasting effects on firms' investment decisions. While—given that the tax hike is also incorporated into firms' ex ante investment plans for years t_1 and thereafter—the coefficient on the log revision ratio returns to zero in the years following t_0, the point estimates regarding the level of investment remain negative. Appendix Figure C.1 shows that these patterns also hold when extending the time window covered by the estimation to four years pre and post treatment, which, by construction, relies on substantially fewer observations.

Effect Sizes Expressed in Terms of Effective Tax Rates. Our main specification estimates the investment response to changes in statutory tax rates, i.e., the parameter that is directly set by policy and that hence can be evaluated empirically without imposing further assumptions. However, large parts of the literature estimate treatment effects in relation to changes in effective tax rates. To better compare our estimates with these studies, we thus run alternative specifications of our baseline estimation based on variation in effective tax rates. Importantly, expressing changes in the LBT in terms of changes in effective tax rates requires additional assumptions on, *inter alia*, (i) firms' discount rate and (ii) the distribution of the total volume of investment among categories subject to different depreciation schedules, i.e., investment in machinery or buildings. The choice of the adequate discount rate is not innocuous in our setting, given that our analysis covers a period of almost four decades during which interest rates have fluctuated strongly (see Appendix Figure E.1). Moreover, the composition across investment categories can only be roughly approximated by either relying on yearly aggregates of the entire manufacturing sector or using time-invariant shares of firm-level investment in machinery and buildings, as firm-specific investment shares are not available in every year.

Our procedure to calculate effective tax rates τ_{eff}, which is described in detail in Appendix E, follows the framework of Hall and Jorgenson (1967), as, e.g., recently applied by Furno (2022), and relies on information on depreciation schedules for machinery and buildings obtained from the Oxford Corporate Tax Database. We compute four different versions of τ_{eff} based on two sets of assumptions regarding the discount rate and the relative share of investment in machinery and buildings, each. In the first and second specification, we follow Zwick and Mahon (2017) in assuming a time-constant discount rate of 7% when calculating the present discounted value of depreciation, while the remaining specifications use time-varying interest rates on loans for discounting. Further, the first and third specification rely on information on the average share of investment in machinery and buildings obtained from administrative aggregate data, while the others use the firm-specific share of investment in machinery and buildings reported to the ifo Investment Survey whenever available.[21] Appendix Figure E.3 shows that across all specifications, the variation captured by changes in effective tax rates is strongly associated with the underlying changes in the LBT rate. This is not surprising, as all tax base rules of the LBT are set at the federal level (and potential changes of those over time are largely absorbed by year fixed effects) and no specific tax credits exist. Hence, apart from its scale, the identifying variation exploited in the empirical estimation does not differ strongly between the different approaches.

[21] We use the firm-specific mean across all years if firms reported machinery and building investments at least three times to the IVS and replace missing values by the aggregate data used in the first specification, see Appendix E.

Table 3 – Investment Revisions after a Tax Hike: Effective Tax Rates

	Log Revision Ratio			
	(1)	(2)	(3)	(4)
Panel A: Variation in Effective Tax Rate				
Effective Tax Hike	-0.132**	-0.124**	-0.136**	-0.133**
	(0.059)	(0.057)	(0.067)	(0.066)
Constant	-0.033***	-0.033***	-0.033***	-0.033***
	(0.001)	(0.001)	(0.001)	(0.001)
Panel B: Variation in User Cost of Capital				
User Cost Hike	-0.120**	-0.107**	-0.121**	-0.111*
	(0.054)	(0.050)	(0.061)	(0.057)
Constant	-0.033***	-0.033***	-0.033***	-0.033***
	(0.001)	(0.001)	(0.001)	(0.001)
Assumptions:				
Interest Rate	0.07	0.07	time-varying	time-varying
Spec. Investment Share	I	II	I	II
Observations	34421	34421	34421	34421
Firm FE	✓	✓	✓	✓
Year × State FE	✓	✓	✓	✓
Year × Industry FE	✓	✓	✓	✓

Notes: This table reports estimates from linear regressions of the log revision ratio on the size of the tax changes. In Panel A, the estimation is based on variation in effective tax rates (τ_{eff}) calculated as described in Appendix E. Panel B runs separate regressions exploiting changes in the user cost of capital (multiplied by 100). The respective specifications rely on different assumptions regarding the calculation of the present discounted value of depreciation, either assuming a time constant interest rate of 7% (as, e.g., in Zwick and Mahon, 2017), or based on time-varying interest rates on firm loans as depicted in Appendix Figure E.1. Further, τ_{eff} (and $UserCost$) is either calculated based on the average share of investment in machinery and buildings based on aggregate data from the Federal Statistical Office of Germany (Specification "I") or on the firm-specific share of investment in machinery and buildings reported to the IVS whenever available (Specification "II"). All regressions apply firm fixed effects, as well as industry-by-year and state-by-year fixed effects. Standard errors in parentheses are clustered at the municipality level. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results presented in Panel A of Table 3 show that the estimated effect sizes are largely comparable across the different specifications of τ_{eff}. For better comparison with our baseline results, the estimates have to be rescaled, as the effective tax rates are on average much smaller than the statutory ones. In the first specification, rescaling takes into account that the average LBT rate is 16.79% and that—assuming a discount rate of 7%—the average effective tax rate amounts to 3.82%. The size and precision of the estimated coefficient shown in Column (1) is remarkably close to the respective baseline specification using variation in the statutory LBT rate ($-0.132 * 3.82/16.79 = -0.030$ vs. -0.038). When relying on time-varying interest rates, e.g., in Column (3), the rescaled point estimate implies a slightly lower effect size, which is, however, still in the same order of magnitude ($-0.136 * 2.9/16.79 = -0.023$).

Panel B of Table 3 repeats this exercise using hikes in the tax term of the user cost of capital as explanatory variable, again aiming to produce estimates comparable to other parts of prior literature. Given the linear relationship between the user cost of capital and the effective tax rate depicted in Appendix Figure E.4 ($\tau_{eff} = 1 - UserCost^{-1}$), this approach does not impact the results apart from rescaling the coefficients. Across all specifications, the estimated effects of a hike in the user cost of capital on the log revision ratio are statistically significant and range between -0.107 and -0.121.

Comparison to the Literature. How do the investment effects documented in our paper compare to findings in other studies? The earlier public finance literature (e.g., surveyed by Hassett and Hubbard, 2002) typically estimated the effect of changes in the tax term of the user cost of capital on investment, measured relative to the lagged capital stock (I/K). For the sake of comparability, the coefficient of -0.12 depicted in Panel (B) of Table 3, which can be interpreted as the effect on log investment, as demonstrated above, hence needs to be expressed in terms of I/K. As information on the capital stock is not available in our data, we use the information on I/K documented by Zwick and Mahon (2017) to rescale our estimate. Accordingly, a one unit change in the user cost of capital is associated with a decrease in the ratio of investment over the lagged capital stock by 1.2 percentage points in our setting.[22] This transformation suggests that the investment response documented in our paper is slightly stronger compared to the estimates summarized in Hassett and Hubbard (2002) that range between -0.5 and -1, but smaller than the comparatively large estimate of -1.6 found by Zwick and Mahon (2017).[23] Furthermore, our semi-elasticity of 3 can be compared to Ohrn (2018) who reports a semi-elasticity of 4.7 for a tax decrease induced by a specific tax provision for the manufacturing sector. Besides differences in research design and institutional setting, these larger responses documented in more recent studies might be due to targeted policies being more effective at stimulating investment than statutory tax rate cuts as rationalized by Chen et al. (2022).

Two recent studies also estimate how firms respond to changes in universal corporate tax rates. Giroud and Rauh (2019) study how firm-level variables react to changes in US state level corporate taxes, including changes in the capital stock. They focus on a selected sample of large multi-state firms, for which they find a semi-elasticity of 0.24.

[22]Mean I/K in Zwick and Mahon (2017) amounts to 0.1. Hence, our estimate can be expressed in terms of I/K as follows: $-0.12/0.1 = -1.2$.

[23]Ohrn (2018, p. 296) also derives, after rescaling his estimates (Appendix J), an effect size for the same policy that is remarkably close to Zwick and Mahon (2017).

As pointed out by the authors, this very small elasticity might arise due to measurement error. Mertens and Ravn (2013) use aggregate data and combine a narrative approach with a structural VAR model to exploit changes in US federal corporate taxes. They find semi-elasticities between 2.1 and 4, comparable to the responses documented in this paper.

Our results furthermore complement evidence from Isphording et al. (2021), showing that tax hikes in the German LBT reduce plant-level R&D spending by around 2 to 3 percent in the year of implementation. As R&D spending constitutes a (small) part of firm investment, we can directly compare the estimate to our semi-elasticity of investment of around 3. While both estimates suggest comparable effect sizes, our results are obtained for a different sample of firms and under less restrictive identifying assumptions.

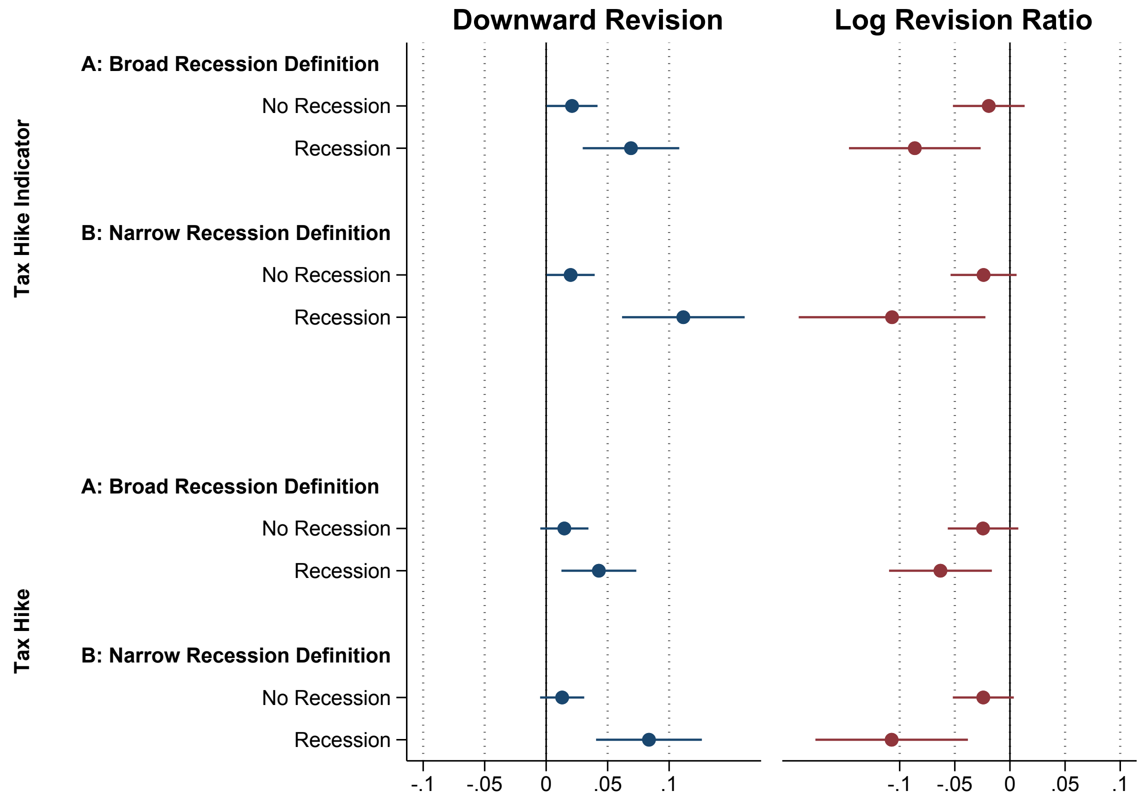
4.3 State Dependence and Heterogeneity

State Dependence. Next, we exploit the long time dimension of our data to analyze potential heterogeneity in effect sizes over the business cycle. While a large literature in macroeconomics studies the state dependence of fiscal policy, there is not yet a consensus on whether effects of corporate tax changes are state dependent (Demirel, 2021; Hayo and Mierzwa, 2021; Jones et al., 2015; Ljungqvist and Smolyansky, 2018; Winberry, 2021). As most quasi-experimental evaluations of the effect of corporate taxes on investment behavior rely on few tax changes or just a single tax reform, the treatment variation is typically not large enough to distinguish effect size heterogeneity along the business cycle. In contrast, the long time dimension of our data in combination with the occurrence of multiple local tax changes in each given year allows us to evaluate whether the treatment effect is state dependent.[24]

The effect of tax hikes on revisions of investment plans are substantially stronger during recessions compared to normal times. Figure 5 presents the estimation results of interacting the tax hike treatment with indicators capturing periods of recession and normal times. To this end, Panel A classifies t_0 as a recession year if at least one quarter of that year is defined as a recession by the German Council of Economic Experts. The average effect that we estimated in Table 2 masks substantial heterogeneity over the business cycle. For instance, while in normal times the share of firms that invest less than previously planned increases by 2 percentage points in years with a tax hike, this figure triples to 6 to 7 percentage points in recessions. The same pattern also holds for the remaining specifications and the results tend to become even stronger when using

[24]As shown in Appendix Figure B.2, municipalities are as likely to raise taxes in recessions as in normal times. The reasons why municipalities increase taxes (also in recession) are diverse, ranging from growing budget requirements to electoral cycles (Foremny and Riedel, 2014) and rent extraction (Langenmayr and Simmler, 2021); see also the discussion in Section 2.

Figure 5 – Investment Revisions after a Tax Hike: State Dependence



Notes: This figure estimates how the probability of investing less than previously planned (left panel) or the log revision ratio (right panel) change in response to a tax hike separately for recession and non-recession years by including respective interaction terms in Equation (1). In Panel A, recession years are defined following the classification of the German Council of Economic Experts and refer to 1980-1982, 1992-1993, 2001-2003, and 2008-2009. Panel B classifies recessions as years with negative real GDP growth according to World Bank data (https://data.worldbank.org/indicator/NY.GDP.MKTP.KD.ZG?locations=DE), resulting in a smaller set of recession years (1982, 1993, 2002, 2003, and 2009). The estimation purges for firm fixed effects, as well as year fixed effects at the levels of federal states and industries. Standard errors are clustered at the municipality level. Confidence intervals refer to the 90% level. The full regression output is disclosed in the even columns of Appendix Table C.2.

a narrower classification of recession periods, defined as years with negative real GDP growth in Panel B.[25]

Mechanisms of State Dependence. While our baseline estimates are in line with the predictions of standard theories of investment (Hall and Jorgenson, 1967), theory fails to

[25]Despite the fact that the effects during the recession period can only be estimated relatively imprecisely due to the small sample size, the estimated effects during recessions are statistically different from those during expansions in half of the specifications, while being close to approaching significance in the remaining specifications (see Appendix Table C.2).

explain why the effect of tax hikes should be state dependent. In the following, we discuss three channels which could explain the stronger effect during recessions.

The first channel relates to the fact that investment projects are risky. As investments are only partially deductible from the tax base, profits and losses are treated unequally by the tax authorities.[26] In expectation, tax hikes thus lead to stronger decreases in the net present value of those investment projects with a higher variance of expected returns as first formalized by Domar and Musgrave (1944).[27] During recessions, the expected return to many investment projects becomes more uncertain, as it is unknown when the economy will recover again. Tax hikes should therefore lead to stronger behavioral responses in economic downturns when a higher share of planned investments is risky. While we cannot test this conclusively in our data, we can assess whether firms with more volatile revenue paths react stronger to tax hikes. For this purpose, we calculate the standard deviation of yearly revenue growth for each firm and construct an indicator for having above median volatility. Appendix Table C.3 shows the regression results when the tax hike effect is interacted with this volatility indicator. While the effects are estimated imprecisely and are sensitive to the specified model, they indeed show slightly larger responses of firms with more volatile revenue paths, suggesting that one reason for the state dependence of tax shocks may be the heightened uncertainty about returns to investment during recessions.

Second, firm investment is sensitive to cashflow (Almeida et al., 2004). Corporate taxes decrease the cashflow for profitable firms and therefore lower investment. At the same time, Almeida et al. (2004) show that cashflow sensitivity is higher in recessions. During recessions, firms expect a higher probability of being cash constrained in the future and therefore retain more earnings for profitable investment opportunities. Taken together, this could give rise to an interaction effect, which reduces investment disproportionally if taxes are increased during recessions. Two regularities in our data support such a mechanism. First, we find that profitable firms react stronger to tax hikes during recessions. We use an indicator for a revenue drop by more than 10 percent compared to the previous year as a proxy for no longer being profitable. While firms that experience a large revenue drop in general revise investments downwards, the revision effect after a tax hike is smaller compared to firms without a large revenue drop during a recession

[26]As discussed by Fuest et al. (2018), costs of debt financing are usually fully deductible from the LBT, while costs of equity financing are not and loss offset is restricted. Moreover, due to depreciation rules, investment costs are split over several years while the revenues are fully taxed in each year.

[27]While Domar and Musgrave (1944) refer to the personal income tax, the same logic applies to the corporate tax and has been tested in the data. For state-level corporate tax rates in the US, Ljungqvist et al. (2017) show that in response to a tax increase the average firm reduces risk as measured by their earnings volatility. Langenmayr and Lester (2017) find similar results in a cross-country panel and among small Spanish firms.

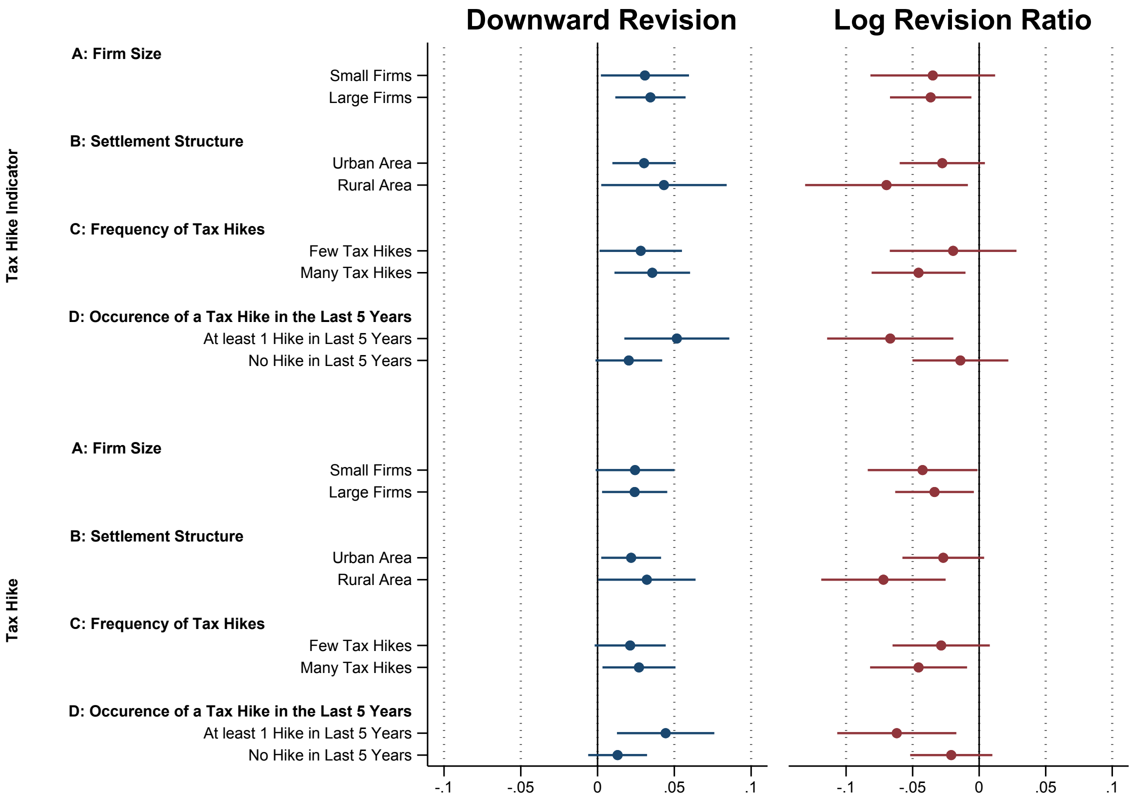
(Appendix Table C.4). Firms with a large decline in revenues might still be profitable if they reduce their labor costs significantly. Appendix Table C.5 shows that the results hold when we exclude firms with a reduction in the number of employees by more than 5 percent as a robustness check. Second, if an adverse financing situation is reported to be a factor for a strong slowdown in investment volumes, the revision effect tends again to be larger in recessions (Appendix Table C.6). Both findings provide suggestive evidence that the stronger investment response in recessions may relate to cashflow sensitivity.

Finally, the stronger investment response to tax hikes in recessions could result from diminished possibilities to shift the tax burden to third parties. Fuest et al. (2018) show that workers bear approximately half of the incidence of the LBT in Germany. However, as wages are nominal downward rigid (e.g., Barattieri et al., 2014), firms often cannot decrease wages in response to adverse economic conditions. This lower bound bites predominantly in recessions, especially given that collective bargaining agreements are still the norm in the German manufacturing sector and bargained wages usually slow down only with a considerable time lag as depicted in Appendix Figure C.5. This could suggest that during recessions, (cashflow sensitive) firms reduce their investment disproportionally as downward rigid wages do not allow shifting the tax burden on workers. Consistent with such a channel, Fuest et al. (2018) report a lower wage incidence for less profitable firms. In our data, we do not observe wages, preventing us from investigating this issue further.

Testing for Further Heterogeneity. While the sample size of our data does not permit a comprehensive heterogeneity analysis, we perform the main estimation for a number of additional sample splits emphasized in the literature. For example, the investment effects of accelerated depreciation allowances in the corporate tax code have often been found to be much stronger among small (liquidity-constrained) firms (e.g., Zwick and Mahon, 2017). Figure 6 summarizes the results, showing that treatment effects do not differ by firm size when splitting our sample into firms with more or less than 250 employees (Panel A). We also fail to detect significant differences between rural and urban municipalities (Panel B), suggesting that a downward bias in our estimates due to private information of the firms—which should be more relevant in rural municipalities—is not a major concern.

In Panel C, we split the sample into municipalities with few (≤ 3) and many (> 3) tax hikes over the entire sample period. If firms in municipalities, which often increase the LBT, expected a tax hike with a higher probability, then downward revisions of investment should be less likely among these firms. However, effect sizes are again very similar for both groups. An alternative way to investigate whether tax setting dynamics at the municipality level correlate with the effect sizes is to split the sample by the occurrence of a tax hike in the last years. The results depicted in Panel D suggest that

Figure 6 – Testing for Further Heterogeneity



Notes: This figure estimates how the probability of investing less than previously planned (left panel) or the log revision ratio (right panel) change in response to a tax hike separately for different groups of firms by including respective interaction terms in Equation (1). Panel A provides separate estimates for small and large firms (split at the threshold of 250 employees). Panel B sorts firms according to their location using the definition of urban and rural areas of the Federal Institute for Research on Building, Urban Affairs, and Spatial Development (BBSR) that is mainly based on population density. In Panel C, we split the sample into municipalities with few (≤ 3) and many (> 3) tax hikes over the entire sample period. In Panel D, the tax hike treatment is split into cases where at least one tax hike has already occurred in the previous five years and where no tax hike occurred in the previous five years in the respective municipality. The estimation purges for firm fixed effects, as well as year fixed effects at the levels of federal states and industries. Standard errors are clustered at the municipality level. Confidence intervals refer to the 90% level. The full regression output is disclosed in the even columns of Appendix Tables C.7 and C.8.

having experienced a tax hike in the last five years is plausibly associated with a larger investment response, although the estimates are not statistically different from each other in any specification (Appendix Table C.8). This result could be consistent with the notion that higher policy uncertainty triggers a stronger response after tax hikes.

Overall, Figure 6 demonstrates that other than the strong effect heterogeneity with respect to the business cycle, the effect of tax hikes on the investment behavior of firms is rather homogeneous across other important partitions of our data.

5 CONCLUSION

This paper provides novel empirical evidence on the effect of corporate taxation on firm investment. Our research design allows us to address several concerns that often complicate identification of an investment response. By considering 1,443 tax changes of the German local business tax between 1980 and 2018, we draw on extensive treatment variation and average out idiosyncratic characteristics of single tax reforms. By observing both planned and realized investment volumes, we can control for ex ante investment plans when estimating the effect of tax hikes on firm investment, eliminating a wide set of further potentially confounding factors.

We find significant and economically large investment responses for firms experiencing a tax shock. The share of firms that invest less than previously planned increases by 3 percentage points after a tax hike, with strong heterogeneity along the business cycle. While in normal times the share of firms that revise their investment decisions downwards increases by 2 percentage points in response to a tax hike, this figure triples to over 6 percentage points if taxes are increased during a recession. These findings have direct policy implications that support the countercyclical Keynesian notion of "do not increase taxes during recessions". While we find suggestive evidence that the state dependence of tax shocks could plausibly be related to uncertainty about expected returns to investments, cashflow sensitivity, and tax incidence, more research is needed to disentangle the channels behind this finding.

Overall, our results confirm the view that investment decreases substantially in the corporate tax burden. While our estimates were obtained for increases in the statutory corporate tax rate, prior studies have often evaluated targeted tax policies which were deliberately designed to stimulate investment. We look forward to future research comparing the effects of both types of policies within a unified framework.

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Downward Revision of Investment Decisions after Corporate Tax Hikes

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APPENDIX

This appendix contains all additional information referenced in the main text, as well as further supplementary material. Appendix A describes the institutional setting of business taxation in Germany. Appendix B is the data appendix and provides a detailed description of our data sources, sample selection, and summary statistics. Appendix C reports additional figures and tables. Appendix D explains the assumptions behind our back-of-the-envelope calculation. Finally, Appendix E describes the calculation of effective tax rates in detail.

A BUSINESS TAXATION IN GERMANY

In Germany, business profits are subject to two different taxes. At the national level, profits are either taxed under the personal income tax or under the corporate income tax, depending on the legal form of the firm. In addition, both corporate and non-corporate firms are subject to the local business tax (LBT) at the municipality level.

Corporate Income Tax. Profits of incorporated firms are subject to the national corporate income tax (*Körperschaftssteuer*). The rate of the corporate income tax is currently 15 percent. Until 2000, a split rate imputation system existed in Germany, where retained profits were subject to a tax rate of 40-45 percent, whereas distributed profits were taxed at a rate of 30 percent. From 2001 to 2007, all profits were equally taxed at 25 percent. In all years since 1991, a so-called solidarity surcharge (*Solidaritätszuschlag*) of 5.5 percent of the corporate tax rate was added, dedicated to financing the costs of the German reunification.

Personal Income Tax. Profits of non-corporated firms are subject to the progressive income tax (*Einkommensteuer*). The top marginal tax rate of the personal income tax is currently 45 percent but has been higher in the past, with a maximum of 56 percent in the 1980s. Since 2001, sole proprietors and partners in a partnership have been able to partially offset LBT payments tax against their income tax. This regulation, limiting the bite of the LBT, is however not relevant in our setting, as it only applies to unincorporated businesses, whereas we focus exclusively on the corporate sector.

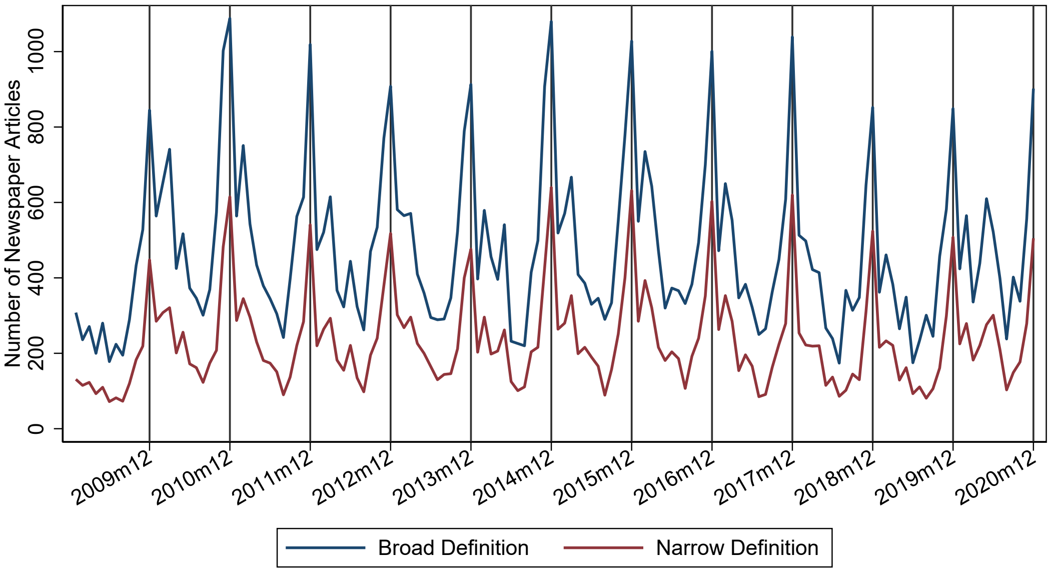
Local Business Tax. In addition, both corporate and non-corporate firms are subject to the LBT (*Gewerbesteuer*). As the corporate tax and the personal income tax, the LBT is a federal tax. For this reason, tax base and liability criteria of the LBT are set at the federal level. The tax rate, in turn, falls under the discretion of the municipalities. More precisely, municipalities decide autonomously on a scaling factor that is then multiplied with a uniform basic tax rate. This results in the following formula:

$$Local\ Business\ Tax\ Rate = Basic\ Federal\ Tax\ Rate \times Municipal\ Scaling\ Factor$$

The basic rate, which is fixed at the national level, has been constant with exception to a change in 2008, when it was decreased from 5.0 to 3.5 percent. This means that for the median municipal scaling factor of 3.2, the resulting LBT rate was 16 percent before 2008. After 2008, the tax rate for the median scaling factor of 3.5 was 12.25 percent.

Each year, the municipal council has to vote on next year's municipal scaling factor, even if it remains unchanged. The decision on next year's local scaling factor is taken

Figure A.1 – Timing of Tax Hike News



Notes: This figure provides evidence on the point in time when firms typically learn about a tixe hike by displaying the number of monthly newspaper articles covering increases in the LBT, obtained from the German press database Genios. Under the broad definition, we counted search matches for "gewerbesteuer erhöh*", under the narrow definition for "gewerbesteuer (erhöht* || angehob* || erhöhung) (beschl* || entschei*)".

jointly with the adoption of the budget in the year's last meeting of the municipal council. For this reason, tax hikes are typically announced in December. In Figure A.1, we substantiate this empirically, showing that newspaper coverage of municipal tax hikes in a given year indeed peaks in December. This holds for both a narrower definition (in red) and a broader definition (in blue) of newspaper coverage of a hike in the LBT. As documented in Appendix B.1, a decision to increase the LBT sends no clear signal about the likelihood of future tax changes.

Around three quarters of the revenues of the LBT accrue directly to the municipalities, whereas one quarter is transferred to the federal government. Taxable profits of firms with establishments in more than one municipality are divided between municipalities according to formula apportionment based on the payroll share. As a consequence, profit shifting between municipalities requires the actual re-allocation of the employees (or wages) of a firm, and is thus associated with relatively high costs. The revenues from the LBT are of key importance for municipal budgets, as the LBT constitutes the most important original source of revenue for municipalities in Germany. Besides own tax revenues, municipal budgets are strongly dependent on fiscal transfers from the federal government or the federal states. As the municipalities cannot directly influence these fiscal transfers, the rate of the LBT is the central budget parameter under their control.

B DATA APPENDIX

This appendix provides comprehensive information on the data sets used in the empirical analysis (including the translated wording of the relevant survey questions from the ifo Investment Survey), explains how we obtain our analysis sample, and reports summary statistics and aggregate time series of our final sample.

B.1 Administrative Data at the Municipality Level

The administrative data on tax rates and municipality revenues and expenditures used in this paper cover the period from 1980 to 2018. The data largely correspond to the municipality data underlying the analysis in Fuest et al. (2018), comprising the period 1993 to 2018. Data for the period from 1980 to 1992 were obtained by filing individual requests to the respective Statistical Offices of the German Federal States. For the state of Schleswig-Holstein, data were not available in the year 1980. For Bremen and Saarland, data are only available since 1990. As these are the two smallest states of Germany in terms of GDP and population, jointly comprising less than 2% of the German population, this does not substantially change the composition of our sample. For all years, the data contain information on scaling factors of the LBT. In addition, we know the full municipality budget, that is all categories of expenditures and revenues, for most years. For a more detailed description of the data, we refer to Fuest et al. (2018) and Isphording et al. (2021).

There is substantial variation in LBT rates across municipalities and over time. To document this variation, we use the subset of municipalities, where we observe at least one firm during our sample period in the ifo Investment Survey. Figure B.1 plots the raw data of the local scaling factors for each municipality in Western Germany (excl. Berlin) over time, demonstrating that there is a lot of variation in local business taxes in any given year. Municipalities tend to increase the LBT approximately ten times as frequently as decreasing it. In consequence, the statistical power of this variation is too low to investigate the effect of tax drops in our data, and the analysis is thus restricted to tax hikes.[1] Accordingly, Figure B.2 shows that the share of municipalities that increased the LBT in a given year is relatively stable over time and does not differ between recessions and expansions. Moreover, Panel (A) of Figure B.3 plots the fraction of municipalities that underwent a given number of tax hikes in the period between 1980 and 2018. The median municipality experienced three tax hikes, while taxes were never increased in only

[1]The number of tax decreases that could in principle be used in the analysis is very low. If we followed the protocol in Appendix B.3.1 to combine the municipality-level data on LBT rates and the firm-level data from the IVS, our analysis could only exploit 236 firm observations (0.7% of all firm-year observations) that face a tax drop in a given year despite spanning a time frame of almost four decades.

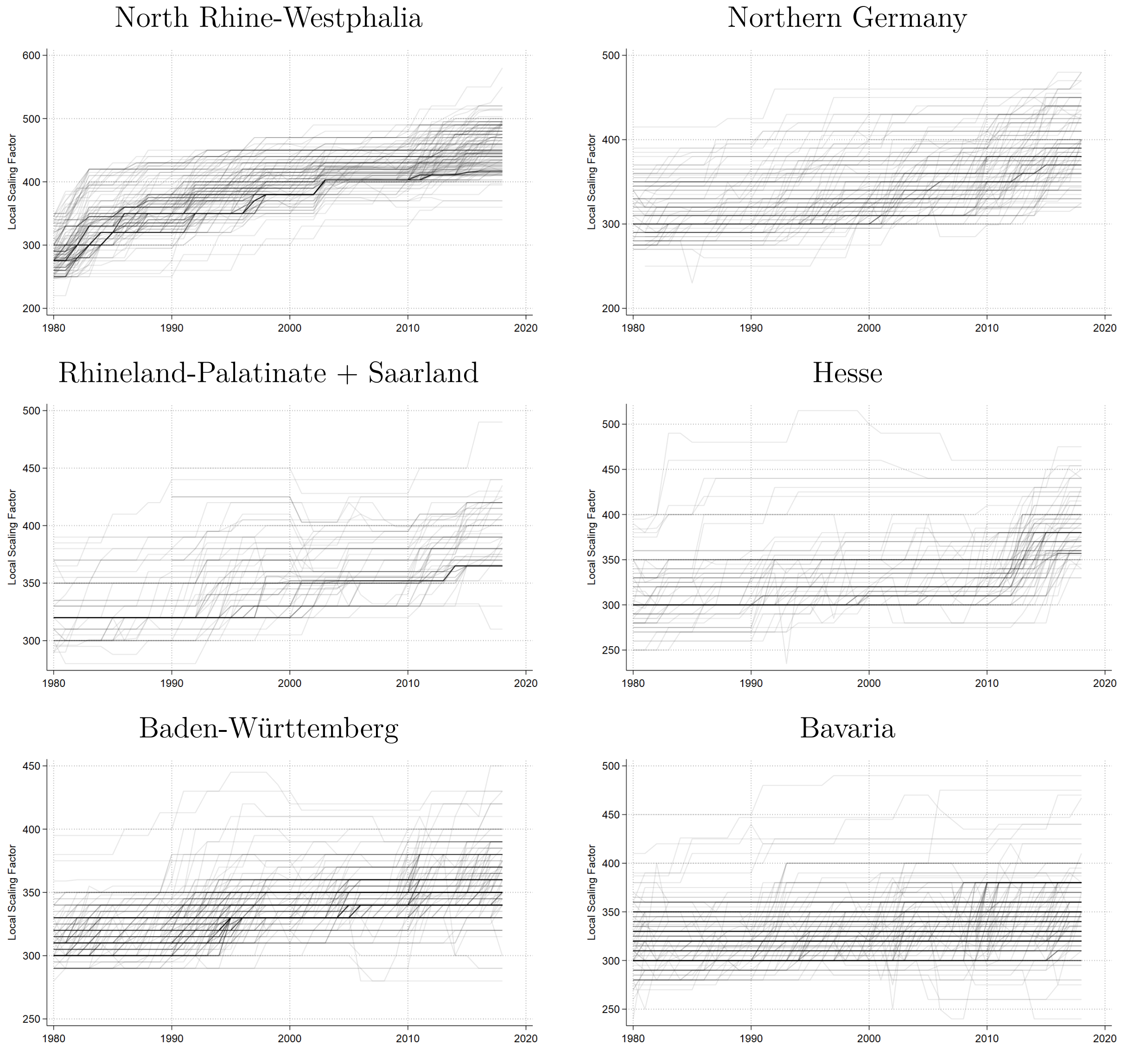
7% of municipalities. The average duration between two tax hikes in our sample is 14.6 years, the median duration 13 years. Panel (B) displays the mean and various percentiles of the size of tax hikes over time. The distribution of tax hikes is rather stable over time in terms of average size and dispersion. If anything, tax hikes were slightly larger in the early 1980s and slightly lower in the 2010s.

To shed light on the dynamic aspects of tax hikes, Figure B.4 documents how a tax hike in year t_0 influences the probability for future tax hikes in the same municipality. Specifically, the figure displays the coefficients of separate regressions of the following form

$$TaxHike_{m,t+x} = \beta TaxHike_{m,t} + \mu_m + \epsilon_{m,t} \quad \forall x = \{1, 20\},$$

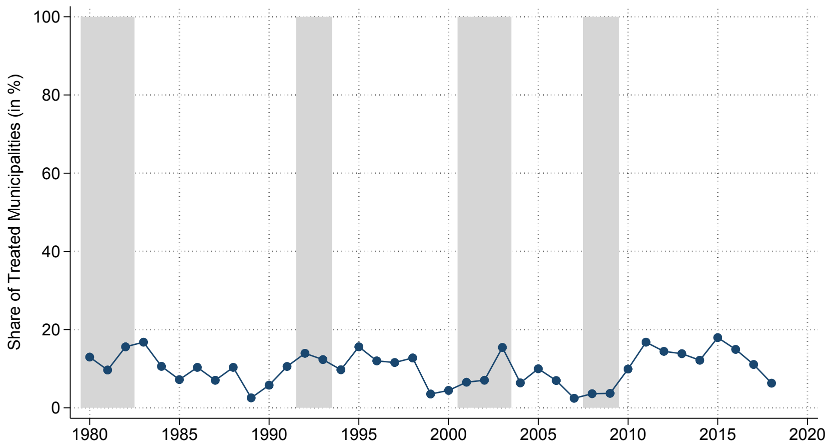
where $TaxHike_{m,t+x}$ is an indicator for a tax hike occurring x years after a tax hike in the same municipality m in year t that is estimated separately for each year in the future $x \in \{1, 20\}$. In the right panel, we include municipality fixed effects. The results show that tax hikes contain little predictive power for future tax hikes. While the unconditional probability for future tax hikes is slightly elevated if a tax hike has recently been enacted, the association is very weak and completely vanishes when including municipality fixed effects, which corresponds to the tax rate variation exploited in our main analyses (that applies firm fixed effects which are themselves nested within municipalities).

Figure B.1 – Time Series of Local Scaling Factors by Municipality



Notes: This figure shows the local scaling factors underlying the LBT for each municipality in West Germany (excl. West-Berlin) over the period between 1980 and 2018. "Northern Germany" summarizes the states of Schleswig-Holstein, Hamburg, Bremen, and Lower Saxony.

Figure B.2 – Share of Municipalities Increasing the LBT over Time

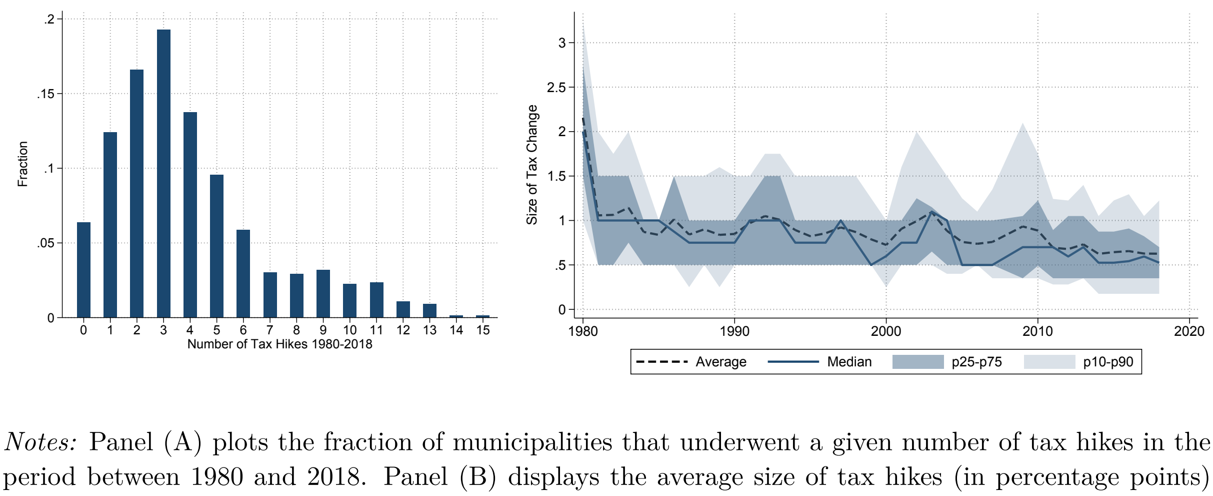


Notes: This figure shows the share of municipalities that increased the LBT in a given year. Gray shaded areas indicate recessions as defined by the German Council of Economic Experts.

Figure B.3 – Number of Tax Hikes and Distribution of Tax Changes

(A) Number of Tax Hikes per Municipality

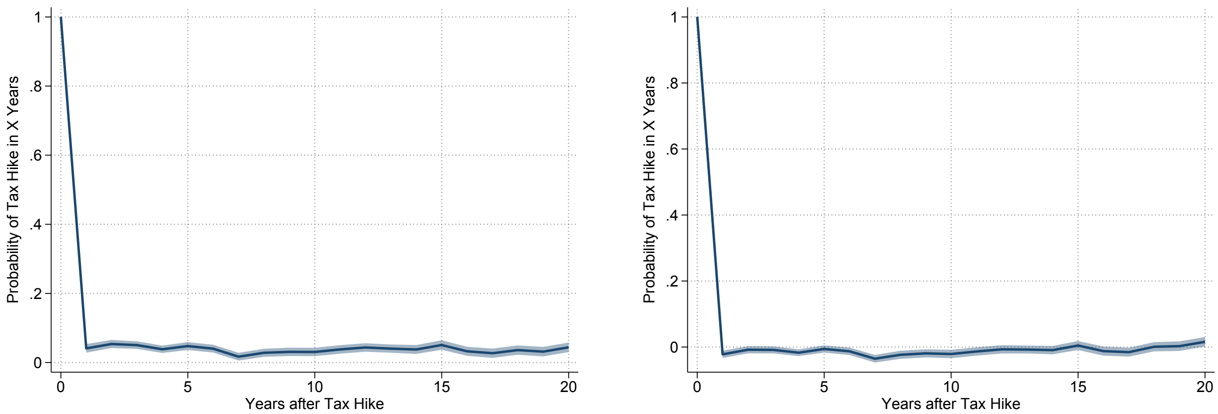
(B) Distribution of Tax Hikes over Time



Notes: Panel (A) plots the fraction of municipalities that underwent a given number of tax hikes in the period between 1980 and 2018. Panel (B) displays the average size of tax hikes (in percentage points) along with various distributional parameters, i.e., the median, the interquartile range, and the range between the 10th and 90th percentile of tax hikes in a given year.

Figure B.4 – Predictability of Tax Hikes as a Function of Past Tax Hikes in the Same Municipality

(A) Plain Relation (B) Municipality Fixed Effects



Notes: This figure reports how a tax hike in year t_0 influences the probability for future tax hikes in the same municipality, by showing the estimates of separate regressions with tax hike indicators x years in the future as dependent variable and a tax hike indicator for the current year as explanatory variable: $TaxHike_{m,t+x} = \beta TaxHike_{m,t} + \mu_m + \epsilon_{m,t}\ \forall x = \{1, 20\}$. In the right panel, we include municipality fixed effects, so that the graph shows the probability of future tax hikes conditional on knowing the institutional and political economy patterns of the own municipality.

B.2 The ifo Investment Survey

General Information. The ifo Investment Survey (IVS, 2019) is a firm-level survey of the German manufacturing sector. Since its inception in 1955, it is conducted biannually by the ifo Institute, with survey waves in spring and fall of each year. The aim of the IVS is to supplement investment data collected by the German Statistical Office, which is only available with a time lag of two years, with more recent data by means of extrapolations at the industry level. The results are part of the European Commission's sponsored investment surveys in its member countries. The aggregated investment volume of the participants of the IVS represents approximately 56% of overall investment in the manufacturing sector (see Sauer and Wohlrabe, 2020, p. 145).

The repeated panel structure of the ifo Investment Survey allows tracking approximately 1,500 firms over time. As outlined in greater detail below, the questionnaire elicits three types of questions, covering (i) the planned volume of investment, (ii) the realized volume of investment, and (iii) investment objectives. Realized investment is always reported for the previous year. Next to these investment-related variables, firms also report annual revenues and the number of employees. For all model specifications which include year fixed effects at the industry level, we rely on the ifo industry classification that maps firms into 34 industries over the entire sample period. The ifo industry classification is slightly more granular than, but largely comparable to two-digit NACE industries. All items of the questionnaire refer to the firms' plants located in Germany. Sauer and Wohlrabe (2020) provide a comprehensive overview and detailed description of this data source. The data can be accessed via the LMU-ifo Economics & Business Data Center (`https://www.ifo.de/en/ebdc`).

The survey is usually completed by high-level management personnel at the firms' controlling departments (Sauer and Wohlrabe, 2020). The ifo Institute incentivizes the participation to the survey by automatically providing the participants with the survey results free of charge as a thank-you for their cooperation. In order to create an additional incentive for participation in the investment survey, this reporting includes more detailed information, e.g., at more disaggregate sectoral levels, compared to the results that are reported publicly.

Representativeness and Accuracy. In Table B.1, we demonstrate the representativeness of the ifo Investment Survey by comparing it to the distribution of firms in administrative data by industry and firm size. The numbers depicted in the table display the percentage share of firms in the respective cells. For instance, 17.3% of firms in the 2018 ifo Investment Survey are in the basic metals and fabricated metal products industry (2-digit WZ08: 24 and 25). This is in between the share of firms by

Table B.1 – Distribution of Firms in the IVS by Industry and Size

		ifo Investment Survey				Actual Germany by			
WZ08	Industry	Small	Medium	Large	Total	Count	Employees	GVA	Payroll
10-12	Food, beverages, and tobacco	1.1	3.6	3.6	8.2	14.0	12.4	7.8	7.0
13-15	Textiles, apparel, and leather	1.2	1.8	1.0	4.1	4.2	1.8	1.1	1.1
16-18	Wood/paper products and printing	3.0	5.7	3.5	12.2	11.8	5.5	4.3	4.0
19	Coke and refined petroleum	-	-	0.3	0.6	0.0	0.3	0.8	0.5
20	Chemicals	-	1.1	3.4	4.7	1.5	4.6	6.9	6.0
21	Pharmaceuticals, medicinal chemical, and botanical	-	0.4	1.0	1.4	0.3	1.9	3.1	3.0
22+23	Rubber/plastic products, and other non-metallic	1.4	6.4	6.4	14.2	8.1	9.0	7.6	7.7
24+25	Basic and fabricated metal products	2.1	6.8	8.4	17.3	21.9	15.7	13.2	13.5
26	Computers, electronics, and optical products	-	1.0	2.4	3.6	3.7	4.8	5.4	5.5
27	Electrical equipment	-	1.3	3.8	5.3	2.9	6.4	7.0	7.5
28	Machinery and equipment	0.5	5.3	11.1	17.0	7.7	15.7	16.7	18.0
29+30	Transport equipment	-	0.8	4.0	4.9	1.9	13.2	19.0	19.1
31-33	Other, and installation of machinery and equipment	1.2	2.1	3.2	6.4	21.9	8.6	7.0	7.0
Total		11.4	36.5	52.2	100	100	100	100	100
Actual GER by Count		89.7	7.7	2.6	100				
Actual GER by Employees		19.1	18.6	62.3	100				
Actual GER by Gross Value Added (GVA)		10.6	13.2	76.1	100				
Actual GER by Payroll		10.0	13.9	76.1	100				

Notes: This table compares the distribution of firms in the ifo Investment Survey to the distribution of firms in administrative data by industry and firm size. The ifo Investment Survey data is based on the year 2018. The administrative data is based on the 2018 Statistics on Small and Medium-sized Enterprises *("Statistik für kleine und mittlere Unternehmen")* provided by the Federal Statistical Office (EVAS Code 48121). Definition of size classes: small: 0-49 employees; medium: 50-249 employees; large: 250+ employees. Cells are empty if there are less than 4 observations due to data protection.

count (21.9%) and weighted by employees (15.7%) in the administrative data. The share of firms by gross value added and payroll in this industry is around 13% in population. Overall, the industry-composition of the ifo Investment Survey is very close to the distribution in administrative data. Regarding the distribution across firm size, the ifo Investment Survey covers a substantial share in each size category. Around a third of firms have between 50 and 249 employees. Thereby, the survey slightly oversamples medium-sized firms while still being representative for small and large firms, since the share of firms is in between the population share of firms by count on the one hand, and by employees, gross value added, or payroll on the other hand.

In general, the accuracy of the IVS data appears to be quite high, as the average deviations of the survey results from the data of the Federal Statistical Office for the manufacturing sector as a whole are only relatively minor. For instance, Bachmann and Zorn (2020) show that aggregate investment growth calculated from the microdata of the ifo Investment Survey is highly correlated with manufacturing investment growth reported by the Federal Statistical Office. Similarly, benchmarking the investment growth rates calculated from the survey against official statistics from the German Statistical Office for the period 1980 to 2016, Sauer and Wohlrabe (2020) report an average absolute estimation error of less than two percentage points. Sauer and Wohlrabe (2020) stress that it should be borne in mind that, at the time investments were recorded in the survey, the balance sheets of some of the companies may not yet be final, while the official results, on the other hand, are based on the final balance sheet figures.

Lastly, and in line with evidence presented in Appendix B.3.2, Bachmann et al. (2017) present a series of stylized facts on the cross-sectional and time-series properties of revisions of investment plans, i.e., the difference between ex ante planned and ex post realized investment volumes, showing that these deviations are meaningful along many dimensions. For example, they document that the overall distribution of revisions is not systematically skewed, while their cross-sectional average is procyclical. This indicates that participants provide accurate investment plans given their current level of knowledge at the time of the survey.

Wording of Questions in the IVS Used in the Paper In the following, we present the translated wording of the questions of the IVS that we use in the paper.

Fall Questionnaire

1. General company information on the current financial year

   Employees (as of Sept. 30th): ________ Total revenue (TEUR): ________

2. Gross fixed capital formation (equipment and buildings) in TEUR

	last year	this year	next year
Total (equipment + buildings):	________	________	________

3. Investment targets this year and next year

   Our domestic investment activity is influenced positively/negatively by the following factors:

		inducement		no	hampering	
		strong	little	influence	little	strong
This year:	a) Financing situation	☐	☐	☐	☐	☐
	b) ...					
Next year:	a) Financing situation	☐	☐	☐	☐	☐
	b) ...					

Spring Questionnaire

1. General company information on the last financial year

Financial year from: ______ to: ______ Focus of production: ___________
Employees (as of Sept. 30th): _________ Total revenue (TEUR): _________

2. Gross fixed capital formation (equipment and buildings) in TEUR

	two years ago	last year	this year
Buildings:	___________	___________	___________
Equipment:	___________	___________	___________
Total (buildings + equipment):	___________	___________	___________

B.3 Construction and Descriptive Statistics of the Merged Dataset

B.3.1 Protocol for Construction of Merged Dataset

In constructing the final sample used for analysis, we have aimed at establishing a valid control group to analyze corporate tax hikes over time, and at cleaning the data to ensure that the results are not driven by outliers. To obtain our final sample, we follow the protocol outlined below:

- We restrict our sample to West Germany and, as Fuest et al. (2018), drop all municipalities which underwent municipal mergers in the observation period. As most of these municipalities were located in East Germany anyway, this does not substantially restrict our sample further (less than 1% of municipalities affected).
- We drop observations in a window of two years before and after a tax hike, if another tax hike occurred in that window.
- We drop all observations for which a tax decrease was enacted, as well as the two years before and after the tax decrease. Fuest et al. (2018) find that while tax hikes are arguably exogenous to shocks to economic variables, a potential endogeneity to economic conditions cannot be ruled out for tax decreases. In addition, only 13.5% of tax changes in the sample are tax decreases. In our setting, we do not have enough statistical power to separately analyze tax decreases.
- In total, the outlined sample selection above reduces the sample size from 8,522 municipalities and 326,274 municipality × year observations to 8,266 municipalities and 283,846 municipality × year observations.
- In the firm survey, for variables that are elicited both in the spring and the fall (last year's number of employees, revenues, and total investment volume), we follow Bachmann et al. (2017) and compute a yearly value by taking the average. We drop the observation if both values deviate more than 20% from the mean.
- As Fuest et al. (2018), we drop firms with legal forms which are exempt from paying the LBT (this affects only 6.2% of the observations).
- We drop firms for which we observe revisions in investment plans in less than 5 years.
- To construct the Log Revision Ratio, we calculate the ratio of realized investments over planned investments, take the natural logarithm, and drop outliers (all values smaller/larger than p1/p99 in each year).

- Matching the municipal and firm-level samples, the final sample consists of 35,310 observations that are spread across 1,192 municipalities.

- We express all nominal variables, i.e. the amounts of revenues and investments, in real terms of constant 2015 Euro by converting German Mark to Euro and adjusting for inflation using the German Consumer Price Index (CPI).

B.3.2 Firms in the Merged Dataset: Descriptive Statistics

Table B.2 displays summary statistics for the firms in our sample. For each firm, we can rely on information on reported planned and realized investment volumes in 17 years on average. The median firm is a typical representative of the "German Mittelstand" employing 264 workers, generating annual revenues of 45 million Euro (CPI inflation-adjusted and—if denominated in German marks—converted to 2015 Euros), and investing 1.4 million Euro each year. As described in Appendix B.2, the IVS covers firms of all sizes. While slightly oversampling medium-sized firms, it is still representative for small and large firms. Accordingly, 10% of firms in our sample have at most 38 employees, annual revenues of 5.2 million Euro and invest as little as 88,000 Euro per annum. In contrast, the 10% largest firms employ at least 1,950 workers and have annual revenues of almost half a billion Euro and total annual investment of at least 19 million Euro. As shown in Figure B.5, the firm size is consequently highly skewed according to the number of employees (Panel A), while the distribution of its logarithm displays a bell-shape (Panel B).

Documenting variation in investment over time, Figure B.6 displays a calendar time graph of the investment plans and investment realizations. Relatedly, Figure B.7 presents the share of downward revisions of investment (blue, solid) and the average log revision ratio (red, dashed) over time. The gray shaded areas indicate recession periods. During recessions, the share of downward revisions increases and the log revision ratio decreases. In addition, there might be a slight time trend towards a higher share of firms that revise their investments downwards. Note, however, that this potential trend does not affect our analysis since we include year fixed effects in the regressions and thus rely on differences between firms in a given year for identification.

Lastly, Figure B.8 shows the share of firms that report a decline in revenues by more than 10% compared to the previous year. In normal times, we observe that around 10% of firms experience such a revenue drop. In recessions, this share spikes up to 60%. This variable is used in Section 4.3, where we discuss potential channels of state-dependence in the effect of tax hikes on investment revisions.

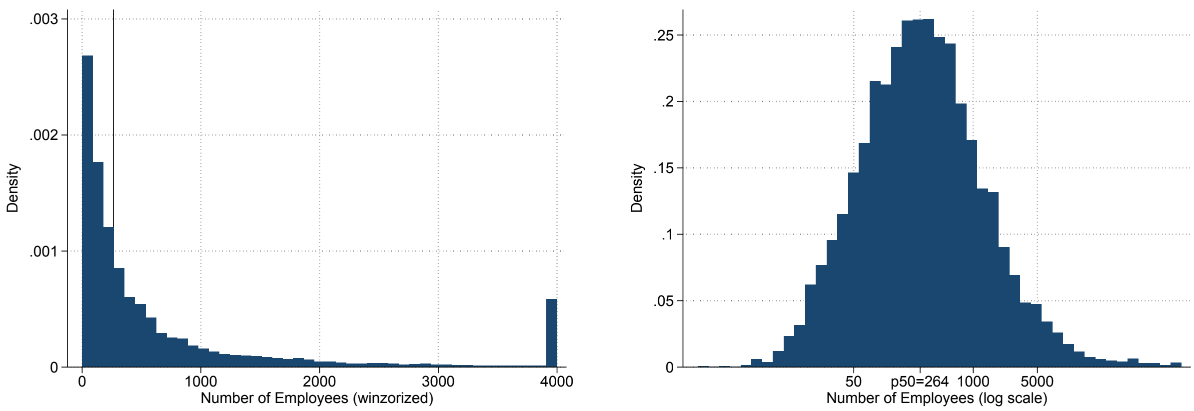
Table B.2 – Summary Statistics of Firms in the Sample

	p10	p50	p90	Mean
Employees	38	264	1,950	1,361
Revenues (in 1000s)	5,194	44,901	451,899	418,842
Investment (in 1000s)	88	1,435	19,163	17,751
Observations per Firm	7	16	29	17

Notes: This figure shows the 10th, 50th, and 90th percentile, and the mean of employees, revenues, and realized investment for the firms in our sample. "Observations per Firm" refers to the number of years a firm is observed in our sample, i.e, the number of years for which firms report both ex ante planned and ex post realized volumes of investment. Revenues and investment are displayed in thousands of Euro.

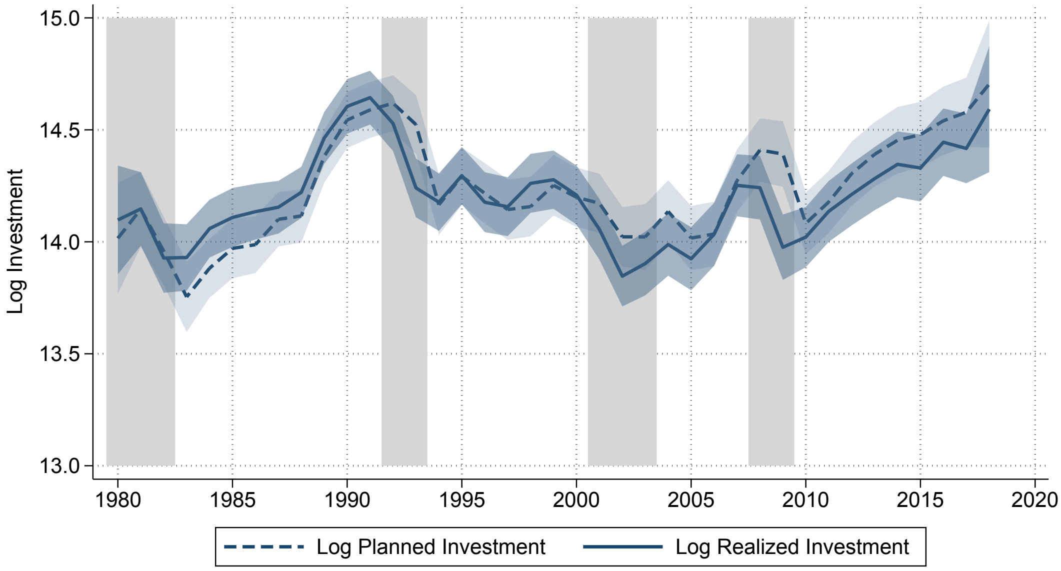
Figure B.5 – Distribution of Firms by Number of Employees

(A) Linear Scale (B) Log Scale



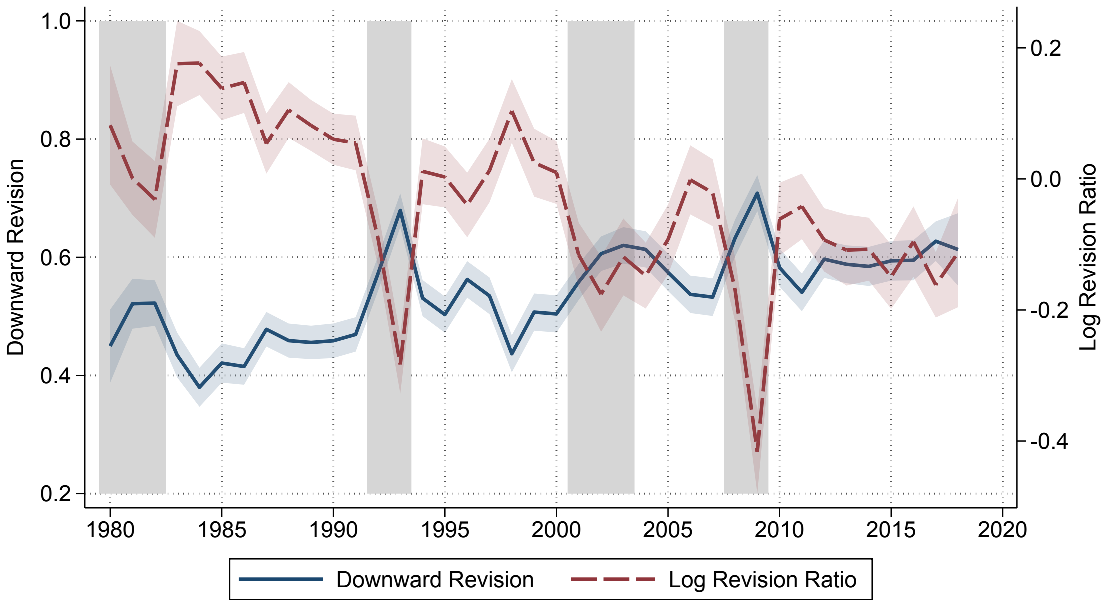
Notes: Panel (A) shows a histogram of the number of employees for the firms in our sample. The distribution is winsorized at a value of 4,000 employees. The vertical line denotes the median number of employees, which is 264. Panel (B) shows a histogram of the natural logarithm of the number of employees for the firms in our sample. The labels on the x-axis refer to the absolute number of employees.

Figure B.6 – Time Series of Investment Plans and Realizations



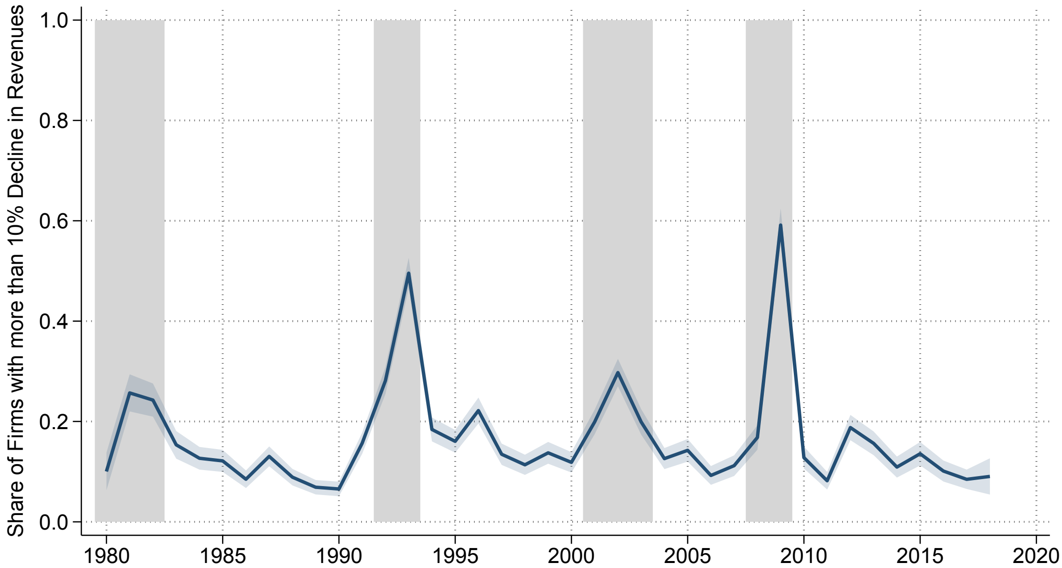
Notes: This figure shows time trends of log planned investment and log realized investment in the period 1980 to 2018, for all firms with a non-missing log revision ratio. The shaded areas indicate 95% point-wise confidence intervals. Gray shaded areas indicate recessions as defined by the German Council of Economic Experts.

Figure B.7 – Time-Series of Investment Revisions



Notes: This figure shows time series of the Log Revision Ratio (right axis), defined as the logarithm of the ratio between realized and planned investment, and the downward revision dummy (left axis), indicating whether a firm has invested less than planned, for the period 1980 to 2018 in our sample. Blue and red shaded areas indicate 95% point-wise confidence intervals. Gray shaded areas indicate recessions as defined by the German Council of Economic Experts.

Figure B.8 – Time-Series of Share of Large Revenue Drops



Notes: This figure depicts the time series of the share of firms with large revenue drops, defined as a year-to-year decline in revenues of more than 10%, over the period 1980 to 2018. Blue shaded areas indicate 95% point-wise confidence intervals, while gray shaded areas indicate recessions as defined by the German Council of Economic Experts.

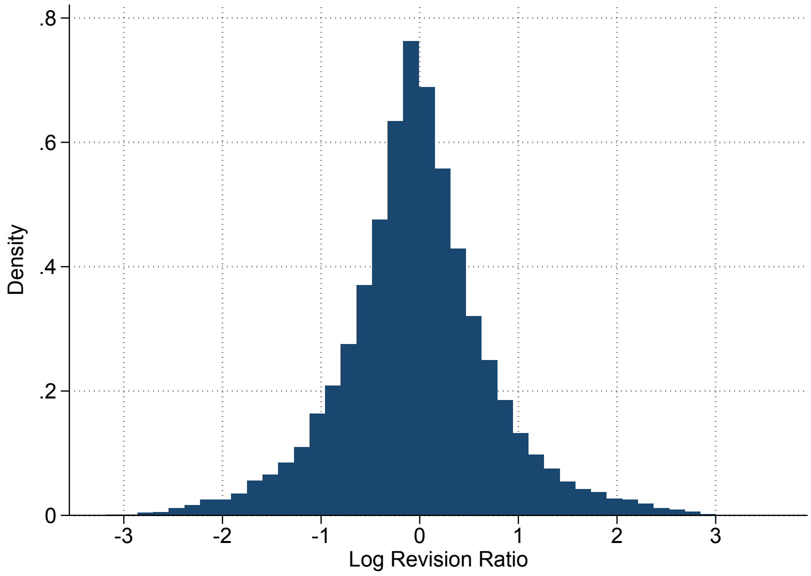
B.3.3 Relationship between Planned and Realized Investment

Our identification approach relies on the investment plans of firms. In the following, we display the distribution of the investment revision ratio and illustrate the strong explanatory power of investment plans for actual investments.

Figure B.9 shows the distribution of the log revision ratio, trimmed at the first and 99th percentile. The log revision ratio is centered around zero, which means that on average, firms invest as much as they have previously planned. Overall, the approximately normal distribution in Figure B.9 indicates that firms revise investments frequently and similarly upwards and downwards.

Next, we provide further evidence that investment plans are highly informative for subsequently realized investment volumes. As shown in Figure 2 in the main part of the paper, the relationship between planned and realized investment volumes is highly linear and virtually corresponding to the 45 degree line. According to the corresponding regression output presented in Column (1) of Table B.3, 84% of the unconditional variation in (log) realized investment is explained by the investment plans for the respective year ($R^2 = 0.84$). The estimated slope is 0.91 and thus close to one. Moreover, the horse-race regression depicted in Column (2) demonstrates that planned investment regarding year t is much more strongly correlated with the ex post realizations in t than with realized levels in the previous year. As shown in Columns (3) and (4), these patterns even hold when controlling for firm fixed effects and investment plans are still strongly positively associated with ex post realized investment. Taken together, investment plans appear to contain accurate information on subsequent year's investment that goes beyond the extrapolation of the level of investment that was realized in the year these plans are reported to the IVS.

Figure B.9 – Distribution of the Log Revision Ratio



Notes: This figure shows a histogram of the Log Revision Ratio in our sample. The Log Revision Ratio is defined as the logarithm of the ratio between realized and planned investment and constitutes one of the two main variables used in the analysis. For exhibitional reasons, the outliers below p1 and above p99 are not depicted here.

Table B.3 – Information Content of Investment Plans for Realized Investment

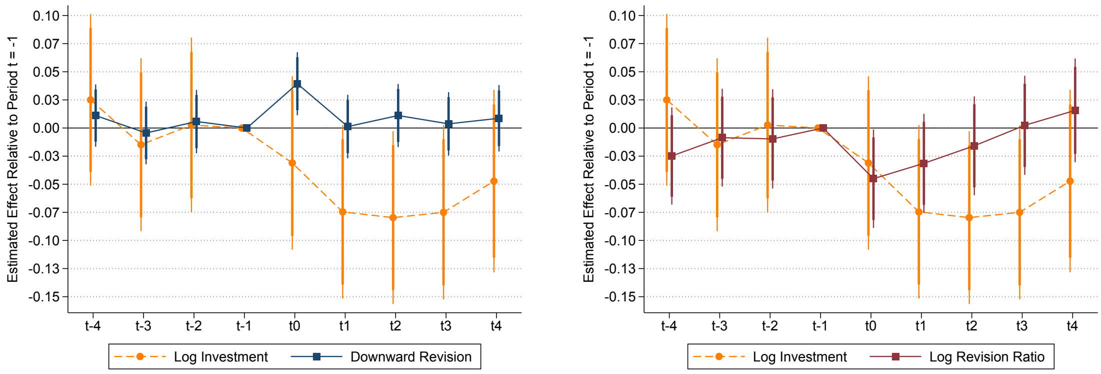
	Log(Realized Investment)			
	(1)	(2)	(3)	(4)
Log(Planned Investment)	0.908***	0.552***	0.574***	0.462***
	(0.005)	(0.011)	(0.012)	(0.012)
L.Log(Realized Investment)		0.395***		0.195***
		(0.011)		(0.011)
Constant	1.276***	0.731***	6.064***	4.886***
	(0.067)	(0.047)	(0.165)	(0.164)
Observations	25282	25282	25282	25282
R^2	0.84	0.87	0.89	0.89
R^2 (within)	-	-	0.27	0.30
Firm FE	-	-	✓	✓

Notes: This table reports estimates from linear regressions of log realized investment in year t_0 (I_{t0}) on log planned investment ($E_{t-1}(I_{t0})$) and log realized investment in the previous year (I_{t-1}). Columns (3) and (4) in addition purge for fixed effects at the firm-level. The sample is restricted to observations in years without changes in the LBT. Robust standard errors in parentheses. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C SUPPLEMENTARY FIGURES AND TABLES

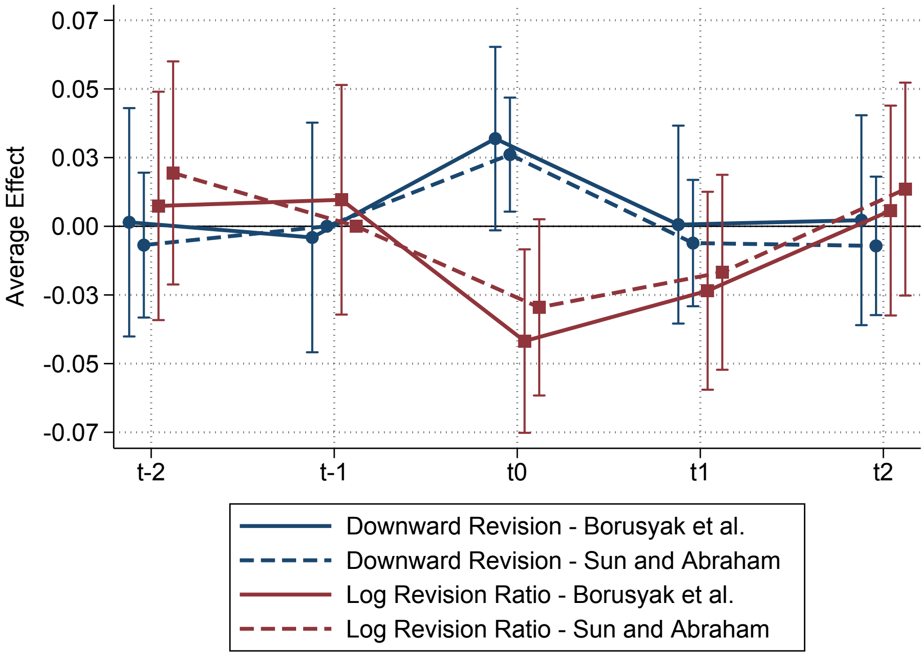
Figure C.1 – Long Event Study: Investment Revisions and Realizations

(A) Downward Revision (B) Log Revision Ratio



Notes: This figure shows event-study estimates of realized investment (orange, dashed lines), the downward revision indicator (Panel A) and the log revision ratio (Panel B) on the tax hike indicator and fixed effects at the levels of firm identifiers and years. The reference period is t_{-1}. "Downward Revision" is an indicator that is one if the ratio of realized investments over planned investments (elicited in the fall of the previous year) is below one. "Log Revision Ratio" is the natural logarithm of this ratio, which is truncated at the first and 99th percentile. "Investment" is the natural logarithm of the realized investment volume. When estimating the effects with respect to log realized investment, firms are assigned to another firm identifier after the year that is in the middle between two tax hikes in order to ensure that there is only one treatment for each unit and to allow for different long-run trends. The confidence intervals refer to the levels of 90% (thick line) and 95% (thin line).

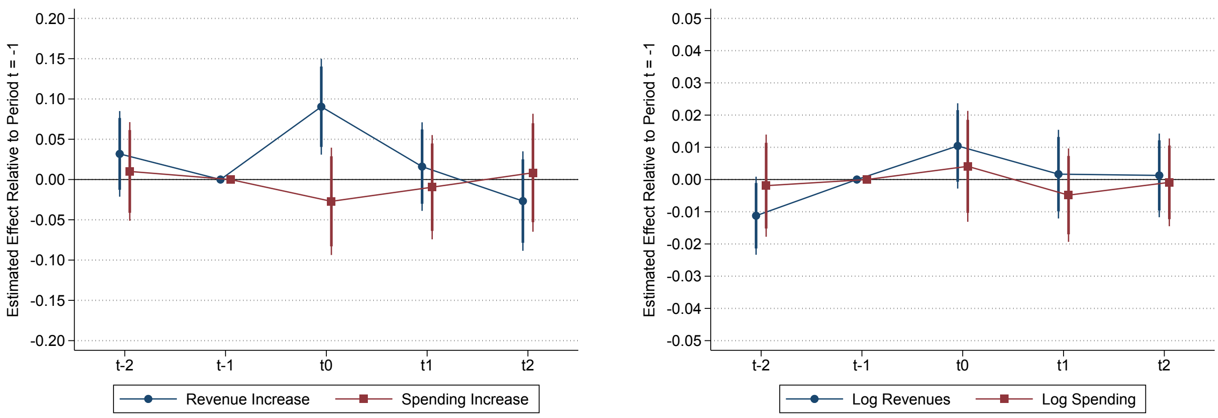
Figure C.2 – Investment Revision Effect after a Tax Hike: Alternative Estimators



Notes: This figure shows the estimates of the imputation estimator introduced by Borusyak et al. (2021) (solid lines) and the interaction-weighted estimator by Sun and Abraham (2021) (dashed lines). The dependent variable is based on the ratio of realized investments over planned investments (elicited in fall the year before). "Downward Revision" is an indicator that is one if the ratio is below one (blue/circle). "Log Revision Ratio" is the log of the ratio (red/square). The treatment "Tax Hike Indicator" is an indicator that is one if the local corporate tax rate is higher than in the previous year. Time fixed effects and firm fixed effects are absorbed in the estimation. Firms are split into multiple observations in the middle between two tax hikes to ensure that there is only one treatment for each unit. This is feasible due to the nature of the dependent variable, which indicates deviations from investment plans that rule out a long-run effect. Confidence bands refer to the 95% level.

Figure C.3 – Event Study: Expenditures and Revenues of Municipalities

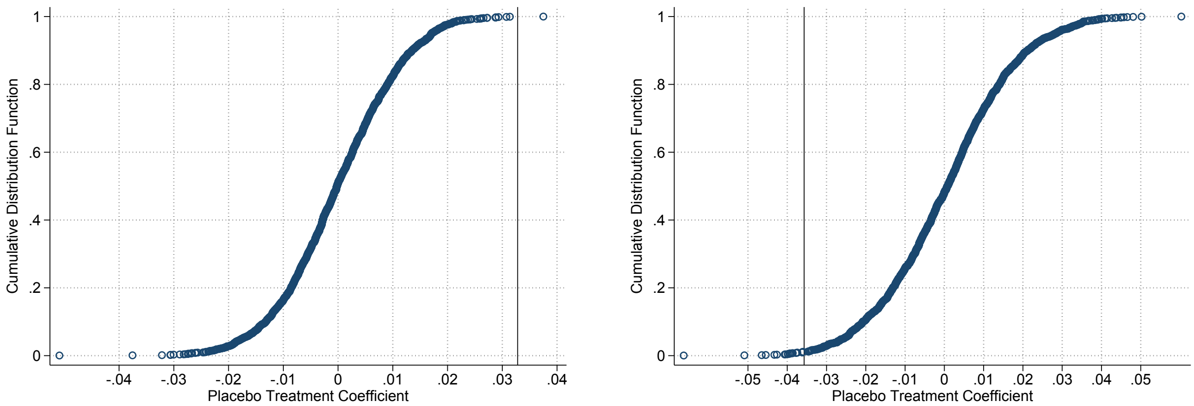
(A) Indicator (B) Logarithm



Notes: This figure shows the estimates of the following event-study regression: $Y_{m,t} = \sum_{j=-2}^{2} \gamma_j \text{TaxHike}_{m,t}^{j} + \mu_i + \phi_{l,t} + \psi_{s,t} + \varepsilon_{i,t}$, where μ_i are firm fixed effects, $\psi_{s,t}$ year fixed effects at the industry level, and $\phi_{l,t}$ state-year fixed effects. In the left panel, $Y_{m,t}$ represents an indicator that is one when municipal revenues/spending increases compared to the previous year. In the right panel, $Y_{m,t}$ represents log municipal revenues/spending. The reference period is $t-1$. Industry fixed effects are at the ifo industry classification level that is comparable to the level of two-digit NACE industries. Standard errors are clustered at the municipality level. The thick and thin confidence bands refer to the levels of 90% and 95%.

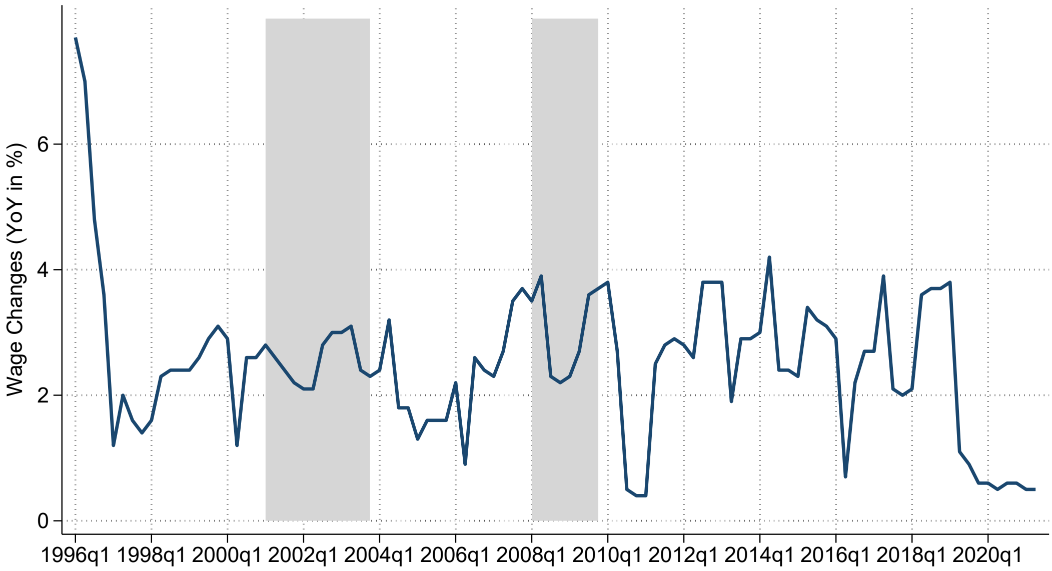
Figure C.4 – Investment Revisions after a Tax Hike: Permutation Test

(A) Downward Revision (Full FEs) (B) Log Revision Ratio (Full FEs)



Notes: This figure reports the empirical cumulative distribution functions of estimates from 2000 placebo tests. In a Monte Carlo exercise, tax hikes ($\mathbb{1}(\Delta tax_{m,t} > 0)$) are randomly allocated to municipalities by holding the share of treated municipalities constant. Then, Model (1) is estimated with the full set of fixed effects. In Panel (A), the dependent variable is $\mathbb{1}\left(\frac{I_{i,t}}{E_{i,t-1}(I_{i,t})} < 1\right)$, i.e., an indicator that is one if the fraction of realized investment over planned investment is below one. In Panel (B), the dependent variable is $ln\left(\frac{I_{i,t}}{E_{i,t-1}(I_{i,t})}\right)$, i.e., the natural logarithm of the investment revision ratio. The vertical lines correspond to the baseline estimates from Column 5 in Panels A1 and B1 of Table 2. In Panel (A), 0.05% of the estimates are equal or larger than the baseline estimate. In Panel (B), 1.15% of the estimates are equal or smaller than the baseline estimate.

Figure C.5 – Collectively Bargained Wage Growth in Manufacturing



Notes: This figure shows year-on-year changes of the index of hourly earnings in the manufacturing sector without special payments obtained from the German Statistical Office. Grey shaded areas indicate recessions as defined by the German Council of Economic Experts.

Table C.1 – Robustness: Baseline Estimates Excl. Reunification Period

	(1)	(2)	(3)	(4)	(5)
Panel (A): Downward Revision					
A1: Tax Hike Indicator: $\mathbb{1}\left(\Delta tax_{m,t} > 0\right)$					
	0.026**	0.031**	0.033**	0.039***	0.049***
	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)
Constant	0.540***	0.539***	0.539***	0.539***	0.538***
	(0.005)	(0.005)	(0.001)	(0.001)	(0.001)
A2: Tax Hike in Percentage Points: $\Delta tax_{m,t}$					
	0.008	0.019*	0.022*	0.030**	0.038***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)
Constant	0.541***	0.540***	0.540***	0.540***	0.539***
	(0.005)	(0.005)	(0.001)	(0.001)	(0.001)
Observations	25960	25960	25911	25911	25911
Panel (B): Log Revision Ratio					
B1: Tax Hike Indicator: $\mathbb{1}\left(\Delta tax_{m,t} > 0\right)$					
	-0.039**	-0.049***	-0.035*	-0.046**	-0.062***
	(0.020)	(0.019)	(0.020)	(0.020)	(0.022)
Constant	-0.039***	-0.038***	-0.039***	-0.038***	-0.037***
	(0.007)	(0.007)	(0.001)	(0.001)	(0.001)
B2: Tax Hike in Percentage Points: $\Delta tax_{m,t}$					
	-0.031*	-0.051***	-0.047***	-0.062***	-0.073***
	(0.017)	(0.016)	(0.018)	(0.018)	(0.021)
Constant	-0.039***	-0.038***	-0.038***	-0.037***	-0.037***
	(0.007)	(0.007)	(0.001)	(0.001)	(0.001)
Observations	25310	25310	25255	25255	25255
Firm FE	-	-	✓	✓	✓
Year FE	-	✓	-	✓	-
Year × State FE	-	-	-	-	✓
Year × Industry FE	-	-	-	-	✓

Notes: This table re-estimates our baseline results from Table 2, excluding the years between the reunification of Germany in 1990 and the end of the government of Helmut Kohl in 1998, i.e., a period when many subsidy programs for investment, especially in East Germany, were in place that might have influenced investment decisions of West German firms. "Downward Revision" is an indicator that is one if the fraction of realized investment over planned investment is below one. "Log Revision Ratio" is the natural logarithm of this ratio. "Tax Hike Indicator" is an indicator that is one if the local corporate tax rate is higher than in the year before. "Tax Hike" is the change in the local corporate tax rate in percentage points compared to the previous year. Industry fixed effects refer to the ifo industry classification, comparable to two-digit NACE industries. Standard errors in parentheses are clustered at the municipality level. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.2 – Treatment Effect Heterogeneity: State Dependence

	Downward Revision				Log Revision Ratio			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (A): Baseline Recession Definition by the German Council of Economic Experts								
Tax Hike Indicator ×								
No Recession	0.018	0.021*			-0.011	-0.019		
	(0.012)	(0.013)			(0.018)	(0.020)		
Recession	0.062***	0.069***			-0.084**	-0.086**		
	(0.022)	(0.024)			(0.034)	(0.036)		
Tax Hike ×								
No Recession			0.013	0.015			-0.019	-0.024
			(0.011)	(0.012)			(0.016)	(0.019)
Recession			0.037**	0.043**			-0.064**	-0.063**
			(0.016)	(0.018)			(0.027)	(0.028)
Constant	0.536***	0.535***	0.536***	0.536***	-0.033***	-0.032***	-0.033***	-0.032***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
H0: Coefficients Equal	0.069	0.074	0.201	0.195	0.059	0.105	0.16	0.266
Panel (B): Alternative Recession Definition by Negative Year-on-Year Real GDP Growth								
Tax Hike Indicator ×								
No Recession	0.019	0.020*			-0.021	-0.024		
	(0.011)	(0.012)			(0.017)	(0.018)		
Recession	0.089***	0.112***			-0.079*	-0.107**		
	(0.028)	(0.030)			(0.047)	(0.051)		
Tax Hike ×								
No Recession			0.013	0.013			-0.024*	-0.024
			(0.010)	(0.011)			(0.015)	(0.017)
Recession			0.066***	0.084***			-0.089**	-0.107**
			(0.024)	(0.026)			(0.039)	(0.042)
Constant	0.536***	0.535***	0.536***	0.536***	-0.033***	-0.032***	-0.033***	-0.032***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
H0: Coefficients Equal	0.017	0.004	0.038	0.012	0.243	0.126	0.12	0.065
Observations	35310	35310	35310	35310	34421	34421	34421	34421
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	-	✓	-	✓	-	✓	-
Year × State FE	-	✓	-	✓	-	✓	-	✓
Year × Industry FE	-	✓	-	✓	-	✓	-	✓

Notes: This table reports estimates from linear regressions based on Equation (1), where the tax hike treatment is split into recession and non-recession years. In Panel (A), 1980-1982, 1992-1993, 2001-2003, and 2008-2009 are classified as recession years as defined by the German Council of Economic Experts. In Panel (B), 1982, 1993, 2002, 2003, and 2009 are classified as recession years as these years showed negative real GDP growth according to World Bank data: https://data.worldbank.org/indicator/NY.GDP.MKTP.KD.ZG?locations=DE). The dependent variable is based on the ratio of realized investments over planned investments (elicited in fall the year before). "Downward Revision" is an indicator that is one if the ratio is below one. "Log Revision Ratio" is the log of the ratio. "Tax Hike Indicator" is an indicator that is one if the local corporate tax rate is higher than in the year before. "Tax Hike" is the change in the local corporate tax rate in percentage points compared to the previous year. Industry fixed effects are at the ifo industry classification level that is comparable to the level of two-digit NACE industries. The p-values at the bottom of each panel indicate whether the coefficients are statistically different from each other. Standard errors in parentheses are clustered at the municipality level. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3 – Treatment Effect Heterogeneity: Volatility of Revenue Growth

	Downward Revision				Log Revision Ratio			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tax Hike Indicator ×								
Low Revenue Growth Volatility	0.029*	0.034**			-0.012	-0.020		
	(0.015)	(0.015)			(0.021)	(0.022)		
High Revenue Growth Volatility	0.029*	0.032**			-0.045*	-0.050**		
	(0.015)	(0.016)			(0.023)	(0.024)		
Tax Hike Indicator ×								
Low Revenue Growth Volatility			0.022	0.024*			-0.016	-0.019
			(0.013)	(0.014)			(0.018)	(0.020)
High Revenue Growth Volatility			0.022*	0.025*			-0.052**	-0.054**
			(0.013)	(0.014)			(0.020)	(0.023)
Constant	0.536***	0.535***	0.536***	0.536***	-0.033***	-0.033***	-0.033***	-0.033***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
H0: Coefficients Equal: p-value	0.993	0.912	0.987	0.967	0.275	0.331	0.18	0.227
Observations	35155	35151	35155	35151	34281	34277	34281	34277
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	-	✓	-	✓	-	✓	-
Year × State FE	-	✓	-	✓	-	✓	-	✓
Year × Industry FE	-	✓	-	✓	-	✓	-	✓

Notes: This table reports estimates from linear regressions based on Equation (1), where the tax hike treatment is split into firms with low and high revenue growth volatility (split at median of firm-level standard deviation in revenue growth elicited in the ifo Investment Survey). The dependent variable is based on the ratio of realized investments over planned investments (elicited in fall the year before). "Downward Revision" is an indicator that is one if the ratio is below one. "Log Revision Ratio" is the log of the ratio. "Tax Hike Indicator" is an indicator that is one if the local corporate tax rate is higher than in the year before. "Tax Hike" is the change in the local corporate tax rate in percentage points compared to the previous year. Industry fixed effects are at the ifo industry classification level that is comparable to the level of two-digit NACE industries. The p-values at the bottom of each panel indicate whether the coefficients are statistically different from each other. Standard errors in parentheses are clustered at the municipality level. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.4 – Treatment Effect Heterogeneity: Current Revenue Growth I

	Downward Revision				Log Inv. Revision			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tax Hike Indicator ×								
No Recession ×								
No Strong Revenue Drop	0.018	0.024*			-0.013	-0.022		
	(0.013)	(0.013)			(0.019)	(0.021)		
Strong Revenue Drop	0.009	-0.002			0.010	0.009		
	(0.034)	(0.034)			(0.053)	(0.054)		
Recession ×								
No Strong Revenue Drop	0.072***	0.080***			-0.085**	-0.088**		
	(0.026)	(0.029)			(0.040)	(0.042)		
Strong Revenue Drop	0.032	0.037			-0.067	-0.066		
	(0.037)	(0.038)			(0.064)	(0.068)		
Strong Revenue Drop	0.105***	0.094***	0.105***	0.094***	-0.190***	-0.172***	-0.191***	-0.173***
	(0.008)	(0.009)	(0.008)	(0.008)	(0.014)	(0.014)	(0.014)	(0.014)
Tax Hike ×								
No Recession ×								
No Strong Revenue Drop			0.014	0.017			-0.021	-0.027
			(0.011)	(0.013)			(0.017)	(0.020)
Strong Revenue Drop			0.007	-0.006			-0.004	0.002
			(0.030)	(0.031)			(0.046)	(0.048)
Recession ×								
No Strong Revenue Drop			0.047**	0.054**			-0.077**	-0.077**
			(0.019)	(0.022)			(0.031)	(0.034)
Strong Revenue Drop			0.004	0.008			-0.019	-0.017
			(0.031)	(0.033)			(0.052)	(0.056)
Constant	0.518***	0.520***	0.519***	0.520***	-0.002	-0.004*	-0.002	-0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
H0: Coefficients Equal: p-value	0.379	0.354	0.243	0.256	0.817	0.787	0.347	0.376
Observations	35138	35138	35138	35138	34257	34257	34257	34257
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	-	✓	-	✓	-	✓	-
Year × State FE	-	✓	-	✓	-	✓	-	✓
Year × Industry FE	-	✓	-	✓	-	✓	-	✓

Notes: This table reports estimates from linear regressions based on Equation (1), where the tax hike treatment effect is estimated separately for each combination of recession and non-recession years and indicators of strong and weak revenue drops. A strong revenue drop is defined as a decline in revenues by more than 10% compared to the previous year. The dependent variable is based on the ratio of realized investments over planned investments (elicited in fall the year before). "Downward Revision" is an indicator that is one if the ratio is below one. "Log Revision Ratio" is the log of the ratio. "Tax Hike Indicator" is an indicator that is one if the local corporate tax rate is higher than in the year before. "Tax Hike" is the change in the local corporate tax rate in percentage points compared to the previous year. 1980-1982, 1992-1993, 2001-2003, and 2008-2009 are classified as recession years as defined by the German Council of Economic Experts. Industry fixed effects are at the ifo industry classification level that is comparable to the level of two-digit NACE industries. The p-values at the bottom of each panel indicate whether the coefficients are statistically different from each other. Standard errors in parentheses are clustered at the municipality level. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5 – Treatment Effect Heterogeneity: Current Revenue Growth II

	Downward Revision				
	(1)	(2)	(3)	(4)	(5)
Tax Hike Indicator ×					
No Recession ×					
No Strong Revenue Drop	0.024**	0.018	0.024*	0.027	0.034*
	(0.012)	(0.013)	(0.013)	(0.017)	(0.018)
Strong Revenue Drop	0.001	0.009	-0.002	-0.035	-0.009
	(0.033)	(0.034)	(0.034)	(0.057)	(0.061)
Recession ×					
No Strong Revenue Drop	0.058**	0.072***	0.080***	0.078**	0.078**
	(0.026)	(0.026)	(0.029)	(0.035)	(0.039)
Strong Revenue Drop	0.011	0.032	0.037	-0.036	-0.020
	(0.034)	(0.037)	(0.038)	(0.073)	(0.076)
Strong Revenue Drop	0.122***	0.105***	0.094***	0.087***	0.072***
	(0.008)	(0.008)	(0.009)	(0.013)	(0.014)
Constant	0.515***	0.518***	0.520***	0.497***	0.498***
	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)
N	35139	35138	35138	21255	21193
Firm FE	-	✓	✓	✓	✓
Year FE	✓	✓	-	✓	-
Year × State FE	-	-	✓	-	✓
Year × Industry FE	-	-	✓	-	✓
Exclude Labor Drop	-	-	-	Yes, > 5%	Yes, > 5%

Notes: This table reports estimates from linear regressions based on Equation (1), where the tax hike treatment effect is estimated separately for each combination of recession and non-recession years, as well as indicators of strong and weak revenue drop observations. A strong revenue drop is defined as a decline in revenue by more than 10% compared to the previous year. In Columns (4) and (5), we drop firm observations that have a decrease in employees by more than 5% compared to the previous year. "Downward Revision" is an indicator that is one if the ratio of realized investments over planned investments (elicited in fall the year before) is below one. "Tax Hike Indicator" is an indicator that is one if the local corporate tax rate is higher than in the year before. 1980-1982, 1992-1993, 2001-2003, and 2008-2009 are classified as recession years as defined by the German Council of Economic Experts. Industry fixed effects are at the ifo industry classification level that is comparable to the level of two-digit NACE industries. Standard errors in parentheses are clustered at the municipality level. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.6 – Treatment Effect Heterogeneity: Financial Constraints

	Downward Revision				Log Inv. Revision			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Tax Hike Indicator ×								
No Recession ×								
No Fin. Constr.	0.025*	0.024			-0.009	-0.011		
	(0.015)	(0.016)			(0.022)	(0.024)		
Fin. Constr.	-0.004	-0.017			-0.051	-0.045		
	(0.055)	(0.057)			(0.116)	(0.121)		
Recession ×								
No Fin. Constr.	0.024	0.039			-0.040	-0.061		
	(0.029)	(0.030)			(0.044)	(0.047)		
Fin. Constr.	0.126*	0.153**			-0.066	-0.095		
	(0.065)	(0.070)			(0.125)	(0.127)		
Fin. Constr.	0.113***	0.111***	0.115***	0.113***	-0.225***	-0.214***	-0.226***	-0.214***
	(0.017)	(0.017)	(0.017)	(0.017)	(0.031)	(0.032)	(0.031)	(0.031)
Tax Hike ×								
No Recession ×								
No Fin. Constr.			0.026*	0.021			-0.023	-0.018
			(0.014)	(0.016)			(0.023)	(0.025)
Fin. Constr.			-0.025	-0.039			-0.048	-0.045
			(0.050)	(0.053)			(0.101)	(0.108)
Recession ×								
No Fin. Constr.			0.013	0.024			-0.019	-0.030
			(0.023)	(0.025)			(0.036)	(0.040)
Fin. Constr.			0.066	0.089			-0.056	-0.066
			(0.064)	(0.077)			(0.107)	(0.116)
Constant	0.550***	0.550***	0.551***	0.551***	-0.054***	-0.054***	-0.054***	-0.054***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
H0: Coefficients Equal: p-value	0.168	0.138	0.449	0.426	0.849	0.802	0.746	0.767
Observations	23661	23640	23661	23640	23123	23101	23123	23101
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	-	✓	-	✓	-	✓	-
Year × State FE	-	✓	-	✓	-	✓	-	✓
Year × Industry FE	-	✓	-	✓	-	✓	-	✓

Notes: This table reports estimates from linear regressions based on Equation (1), where the tax hike treatment effect is estimated separately for each combination of recession and non-recession years, as well as indicators on whether the financing situation is reported to be a factor for a strong slowdown in investment volumes or not. To construct the financing indicator, we use a question from the fall survey (available since 1989), where firms rate on a scale from 1 (strong stimulus) to 5 (strong slowdown) different factors that influence investments in the current year, see Appendix B for the exact wording. We construct an indicator that is one if a firm reports the highest category (5). The dependent variable is based on the ratio of realized investments over planned investments (elicited in fall the year before). "Downward Revision" is an indicator that is one if the ratio is below one. "Log Revision Ratio" is the log of the ratio. "Tax Hike Indicator" is an indicator that is one if the local corporate tax rate is higher than in the year before. "Tax Hike" is the change in the local corporate tax rate in percentage points compared to the previous year. 1980-1982, 1992-1993, 2001-2003, and 2008-2009 are classified as recession years as defined by the German Council of Economic Experts. Industry fixed effects are at the ifo industry classification level that is comparable to the level of two-digit NACE industries. The p-values at the bottom of each panel indicate whether the coefficients are statistically different from each other. Standard errors in parentheses are clustered at the municipality level. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.7 – Treatment Effect Heterogeneity: Firm Size and Settlement Structure

	Downward Revision				Log Revision Ratio			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (A): Heterogeneity by Firm Size								
Tax Hike Indicator ×								
Small Firms	0.029*	0.031*			-0.031	-0.035		
	(0.017)	(0.017)			(0.027)	(0.028)		
Large Firms	0.028**	0.034**			-0.027	-0.036*		
	(0.014)	(0.014)			(0.017)	(0.019)		
Tax Hike ×								
Small Firms			0.021	0.024			-0.040*	-0.043*
			(0.015)	(0.016)			(0.023)	(0.025)
Large Firms			0.022*	0.024*			-0.030*	-0.034*
			(0.012)	(0.013)			(0.016)	(0.018)
Constant	0.536***	0.535***	0.536***	0.536***	-0.033***	-0.032***	-0.033***	-0.032***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
H0: Coefficients Equal	0.951	0.869	0.972	0.989	0.899	0.96	0.73	0.756
Panel (B): Heterogeneity by Settlement Structure								
Tax Hike Indicator ×								
Urban Area	0.027**	0.030**			-0.020	-0.028		
	(0.012)	(0.013)			(0.018)	(0.019)		
Rural Area	0.037	0.043*			-0.069**	-0.070*		
	(0.023)	(0.025)			(0.034)	(0.037)		
Tax Hike ×								
Urban Area			0.019*	0.022*			-0.023	-0.027
			(0.011)	(0.012)			(0.016)	(0.019)
Rural Area			0.029*	0.032*			-0.072***	-0.072**
			(0.017)	(0.019)			(0.026)	(0.028)
Constant	0.536***	0.535***	0.536***	0.536***	-0.033***	-0.032***	-0.033***	-0.032***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
H0: Coefficients Equal	0.688	0.641	0.64	0.649	0.199	0.314	0.106	0.184
Observations	35310	35310	35310	35310	34421	34421	34421	34421
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	-	✓	-	✓	-	✓	-
Year × State FE	-	✓	-	✓	-	✓	-	✓
Year × Industry FE	-	✓	-	✓	-	✓	-	✓

Notes: This table reports estimates from linear regressions based on Equation (1). In Panel (A), the tax hike treatment is split into small ($<$ 250 employees) and large ($\geq$ 250 employees) firms. In Panel (B) the treatment variables are interacted with indicators of urban and rural areas following the classification of the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR) that is mainly based on population density. The dependent variable is based on the ratio of realized investments over planned investments (elicited in fall the year before). "Downward Revision" is an indicator that is one if the ratio is below one. "Log Revision Ratio" is the log of the ratio. "Tax Hike Indicator" is an indicator that is one if the local corporate tax rate is higher than in the year before. "Tax Hike" is the change in the local corporate tax rate in percentage points compared to the previous year. Industry fixed effects are at the ifo industry classification level that is comparable to the level of two-digit NACE industries. The p-values at the bottom of each panel indicate whether the coefficients are statistically different from each other. Standard errors in parentheses are clustered at the municipality level. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.8 – Treatment Effect Heterogeneity: Tax Hike Dynamics

	Downward Revision				Log Revision Ratio			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel (A): Heterogeneity by the Frequency of Tax Hikes								
Tax Hike Indicator ×								
Few Tax Hikes	0.024	0.028*			-0.014	-0.020		
	(0.015)	(0.016)			(0.026)	(0.029)		
Many Tax Hikes	0.031**	0.036**			-0.038*	-0.046**		
	(0.015)	(0.015)			(0.020)	(0.021)		
Tax Hike ×								
Few Tax Hikes			0.021*	0.021			-0.030	-0.029
			(0.013)	(0.014)			(0.019)	(0.022)
Many Tax Hikes			0.021	0.027*			-0.038**	-0.046**
			(0.013)	(0.014)			(0.019)	(0.022)
Constant	0.536***	0.535***	0.536***	0.536***	-0.033***	-0.032***	-0.033***	-0.032***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
H0: Coefficients Equal:								
p-value	0.721	0.733	0.998	0.775	0.464	0.468	0.762	0.584
Observations	35310	35310	35310	35310	34421	34421	34421	34421
Panel (B): Heterogeneity by Occurence of a Tax Hike in the Last 5 Years								
Tax Hike Indicator ×								
≥ 1 Hike in Last 5 Years	0.039**	0.052**			-0.050*	-0.067**		
	(0.019)	(0.021)			(0.026)	(0.029)		
No Hike in Last 5 Years	0.019	0.020			-0.012	-0.014		
	(0.013)	(0.013)			(0.021)	(0.022)		
Tax Hike ×								
≥ 1 Hike in Last 5 Years			0.030*	0.044**			-0.043*	-0.062**
			(0.016)	(0.019)			(0.022)	(0.027)
No Hike in Last 5 Years			0.013	0.013			-0.023	-0.021
			(0.011)	(0.012)			(0.017)	(0.019)
Constant	0.540***	0.540***	0.541***	0.541***	-0.040***	-0.039***	-0.039***	-0.039***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
H0: Coefficients Equal:								
p-value	0.358	0.195	0.386	0.155	0.257	0.145	0.489	0.204
Observations	33220	33201	33220	33201	32375	32356	32375	32356
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	-	✓	-	✓	-	✓	-
Year × State FE	-	✓	-	✓	-	✓	-	✓
Year × Industry FE	-	✓	-	✓	-	✓	-	✓

Notes: This table reports estimates from linear regressions based on Equation 1. In Panel (A), the tax hike treatment variable is interacted with dummies splitting the sample into municipalities with few (≤ 3) and many (> 3) tax hikes over the entire sample period. In Panel (B), the tax hike treatment is split into cases where at least one tax hike has already occurred in the previous five years and where no tax hike occurred in the previous five years in the respective municipality. The dependent variable is based on the ratio of realized investments over planned investments (elicited in fall the year before). "Downward Revision" is an indicator that is one if the ratio is below one. "Log Revision Ratio" is the log of the ratio. "Tax Hike Indicator" is an indicator that is one if the local corporate tax rate is higher than in the year before. "Tax Hike" is the change in the local corporate tax rate in percentage points compared to the previous year. Industry fixed effects are at the ifo industry classification level that is comparable to the level of two-digit NACE industries. Standard errors in parentheses are clustered at the municipality level. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D BACK-OF-THE-ENVELOPE CALCULATION

In the following, we present the assumptions underlying the back-of-the-envelope calculation used to approximate the investment loss for each additional Euro of tax revenue.

The median firm in our sample generates yearly revenues of 45 million Euro. Among the subsample of firms that can be linked to information on the cashflow/revenue ratio balance sheet data, the median profit margin is 4.4%. Assuming that this figure corresponds to all firms in the sample, this translates into 1.98 million Euro of aggregate profits. A one percentage point increase in the LBT increases the tax burden of the median firm—and thus overall tax revenues—by 19,800 Euro. Moreover, the median investment-revenue ratio amounts to 3% in the microdata of the ifo Investment Survey. Hence, the median firm invests approximately 1.4 million Euro each year. Given the estimated semi-elasticity of 3 (see Section 4), a one percentage point increase in the LBT is associated with decreased investment of the median firm by roughly 42,000 Euro. Finally, dividing 42,000 by 19,800 gives that 2.12 Euro of investment volume is lost for each additional Euro of tax revenue. In crisis years, we estimate a semi-elasticity of investments with respect to the LBT rate of 6. Assuming that the relation between the profit margin and investment-revenue ratio is the same in a recession, investments even decrease by 4.24 Euro for each additional Euro of tax revenue.[2]

To interpret these figures as an estimate for the marginal value of public funds (MVPF) in the spirit of Hendren and Sprung-Keyser (2020), we further need to take the (long-term) behavioral response of firms into account: as tax increases decrease firm investment, future firm profits should also be reduced resulting in lower tax revenues of the municipalities. Unfortunately, we cannot estimate the elasticity of firms' profits with respect to changes in investment based on our data. We circumvent this constraint by separately calculating the MVFP for reasonable lower and upper bounds of this elasticity, i.e., assuming that foregone investment maps into foregone future profits with half of the median profit margin (2.2%) or with five times the median profit margin (22%). For the median firm, which lowered investment by 42,000 Euro, this translates into lower profits between 924 Euro and 9,240 Euro. As the average LBT rate is approximately 15%, this leads to an additional reduction in tax revenues between 139 Euro and 1,386 Euro. Taken together, we approximate that incorporating the behavioral response increases the investment loss for each additional Euro of tax revenue from 2.12 Euro to an estimate in the range between 2.14 Euro (42000/(19800-139)) and 2.28 Euro (42000/(19800-1386)).

[2] In fact, the profit margin decreases slightly more than the investment-revenue ratio in recessions. Incorporating this relation in the calculation would lead to an even higher loss of investments for each additional Euro of tax revenue in recessions.

E CALCULATION OF EFFECTIVE TAX RATES

This appendix describes how we calculate the effective tax rates used in the alternative specification presented in Section 4.2 based on the statutory LBT rates used in the main specification. Our procedure is guided by the classic framework of Hall and Jorgenson (1967), as, e.g., recently applied by Furno (2022). We proceed as follows:

- We first obtain depreciation schedules separately for machinery m and buildings b, the two main types of investment for which different depreciation rules apply. We take this information from the Oxford Corporate Tax Database (`https://oxfordtax.sbs.ox.ac.uk/cbt-tax-database`). Over our sample period of almost 40 years, the depreciation rules have changed repeatedly.

- We then calculate the present discounted value (PDV) of a depreciation, denoted by z_m and z_b, respectively. As the choice of the adequate discount rate is not innocuous in our setting, we employ the following two different specifications, whose resulting z_m and z_b are depicted in Figure E.2:

  I. We follow Zwick and Mahon (2017) and set the discount rate to 7%.

  II. We use time-varying interest rates for discounting to accommodate for the fact that over the sample period the interest rates on firm loans have been declining substantially, from close to 10% in 1980 to less than 2% in recent years, with considerable variation in between.[3] In contrast to most other studies in the literature that rely on a single or few tax reforms within shorter time periods, time-variation in interest rates may have large implications for the PDV of a depreciation in our analysis that covers a period of almost 40 years.

- Next, we calculate z for each firm, i.e., a weighted average of z_m and z_b based on firms' respective share of investment in machinery and buildings. As we do not observe these investment shares in machinery and buildings for each firm in all years of the survey, we must impute these values. We consider two distinct specifications.

  I. We assign the average share of investment in machinery and buildings based on aggregate data from the Federal Statistical Office of Germany (only available since 1990, imputed for the years before). This way, the investment shares vary over time, but are the same for all firms in our sample in a given year. Across years, the average share of investment in machinery amounts to 88%.

[3] The time series of average interest rates on firm loans displayed in Figure E.1 builds on three different charts provided by the Deutsche Bundesbank (German Central Bank), as the effective interest rate for non-financial corporations is only available since 2003. The breaks are indicated by the dashed vertical lines. Over our entire sample period, the average interest rate according to this graph has been 5.1%.

II. We use the firm-specific share of investment in machinery and buildings reported to the ifo Investment Survey. As this information is provided less frequently to the IVS compared to the overall level of investment, we use the firm-specific mean across all years if firms reported machinery and building investments at least three times. To retain the sample size, we replace missing values by the values obtained from method I.

- The effective tax rate is then given by $\tau_{eff} = 1 - \frac{1-\tau}{1-z*\tau}$, which only depends on z and the statutory LBT rate τ. This simple version of the formula can be applied in our setting because there are no relevant tax credits in the German system of local business taxation that would complicate the calculation.

- Finally, we calculate the change in the effective tax rate if a tax hike takes place in a given year. Here, we set z in both years (t_0 and t_{-1}) equal to the value of the tax hike year. Thereby, we isolate the effect of tax changes by making the arguably reasonable assumption that firms know the z value of the next year when forming their investment plans. Across all specifications, the variation captured by changes in effective tax rates is strongly associated with the underlying changes in the LBT rate as plotted in Figure E.3.

In alternative specifications, we also express changes in the costs for investment in terms of the user cost of capital instead of effective tax rates. This only requires a simple transformation:

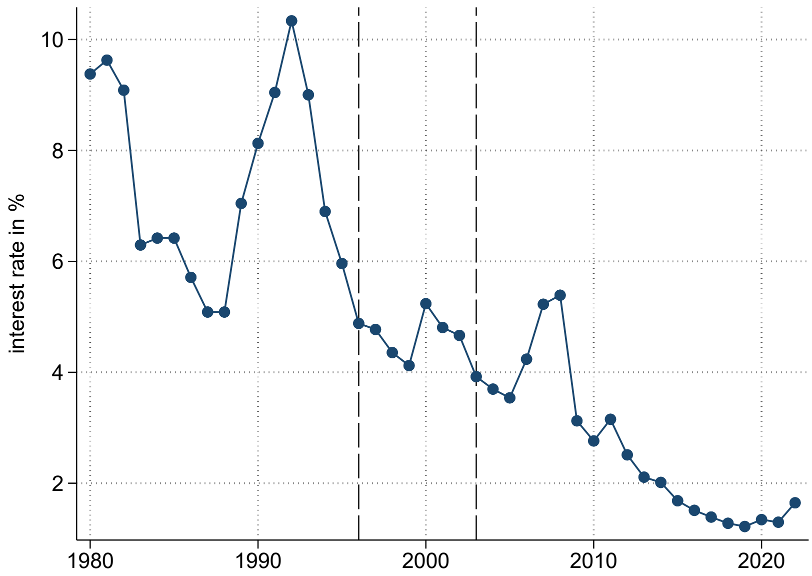
$$UserCost = \frac{1 - z * \tau}{1 - \tau}$$

Hence,

$$\tau_{eff} = 1 - UserCost^{-1},$$

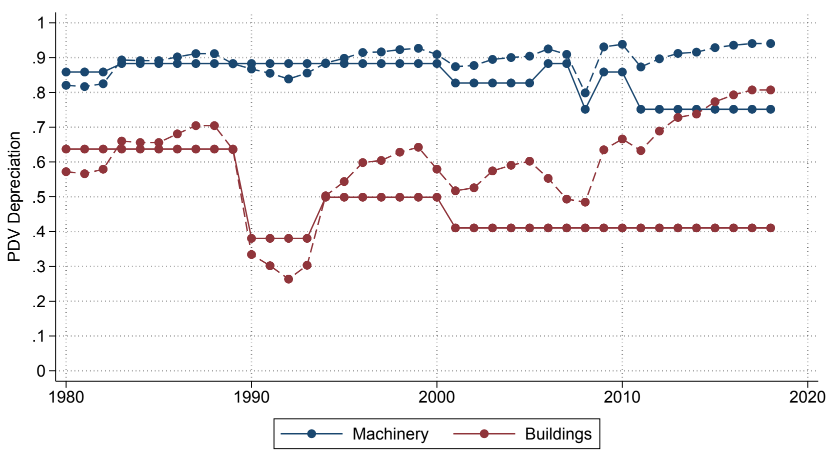
which means that switching from effective tax rates to a user cost approach will not impact our results apart from rescaling the magnitude of the coefficients. That the user cost of capital yields virtually the same results as using effective tax rates is also visible in Figure E.4, which plots the change in the user cost of capital against the change in effective tax rates for all tax hikes in our sample.

Figure E.1 – Time Series of Average Interest Rate on Loans for Firms



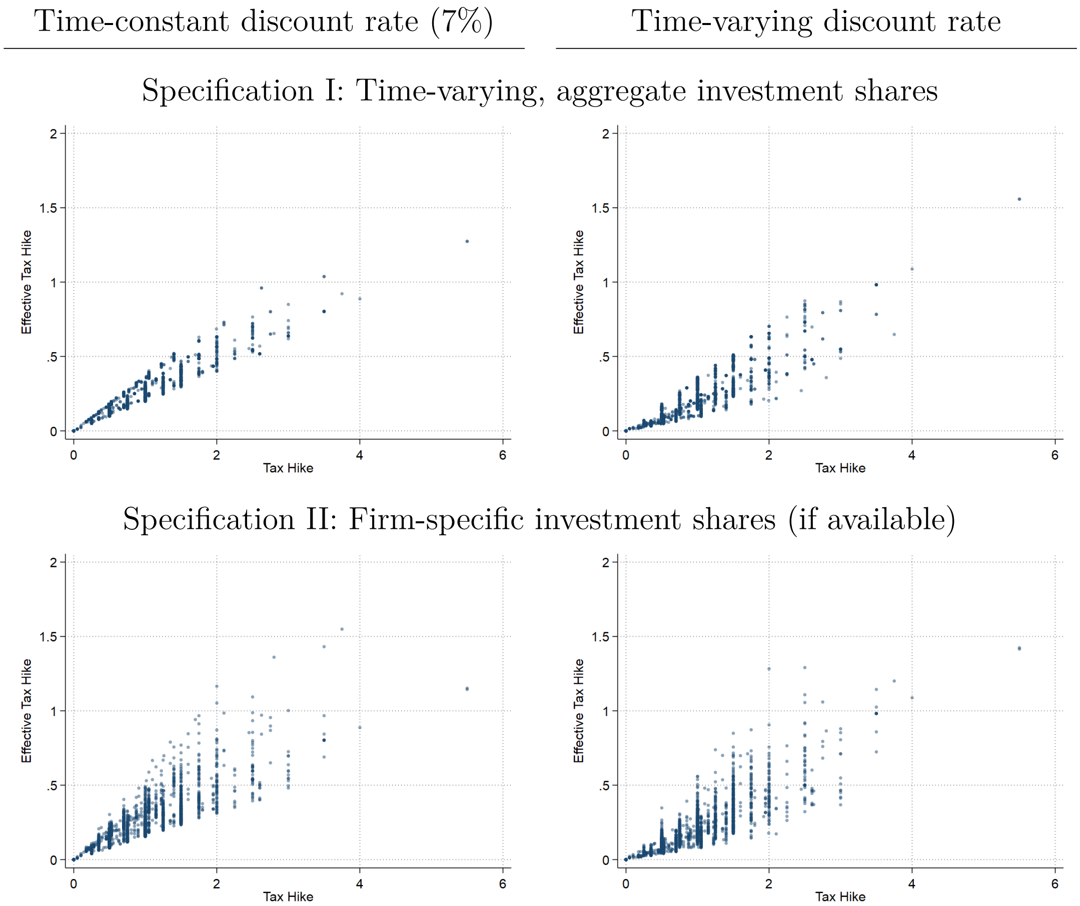
Notes: This figure shows a time series of the average lending rate for firms. From 2003 onward, the effective interest rate for non-financial corporations is used. For the year 1997 to 2002, the effective interest rate to firms for loans between 500,000 and 5 million Euro is used and adjusted upwards (roughly 1 p.p.) to ensure a smooth transition in 2003. For the years 1980 to 1996, the discount rate of the Deutsche Bundesbank is used and adjusted upwards (roughly 4 p.p.) to ensure a smooth transition in 1997. The two dashed vertical lines indicate the breaks in the time series. Source: Deutsche Bundesbank.

Figure E.2 – Present Discounted Value of Depreciation: 7% vs Time-Varying Interest Rate



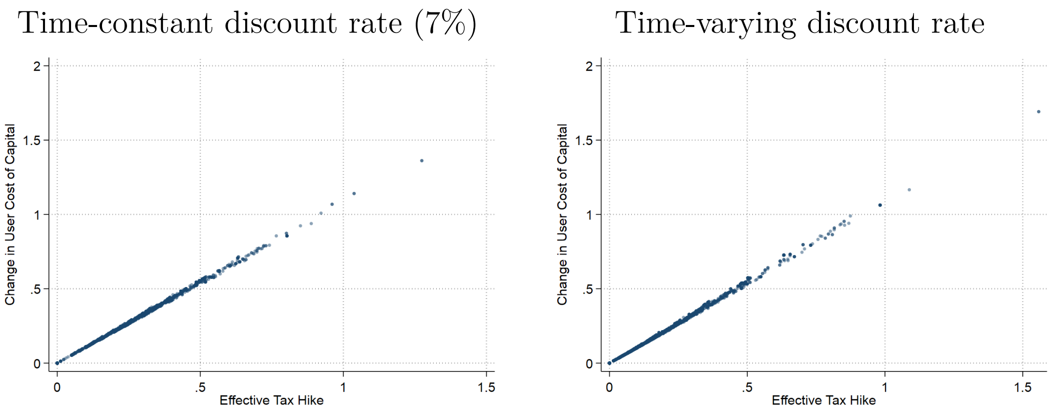
Notes: This figure shows values of the present discounted value of depreciation for machinery (z_m) and buildings (z_b) in the period 1980 to 2018. Depreciation schedules are obtained from the Oxford Corporate Tax Database. The solid line assumes a time-constant discount rate of 7% following Zwick and Mahon (2017), the dashed line calculates the PDV based on the time-varying interest rate on firm loans as displayed in Figure E.1.

Figure E.3 – Relation of Changes in LBT Rate and Changes in Effective Tax Rates



Notes: For each tax hike in our sample, this figure plots its size in terms of an effective tax hike (τ_{eff}; y-axis) against its size as a statutory tax hike (x-axis). As we do not observe the investment shares in machinery and buildings for each firm in all years of the survey, we must impute these values. We consider two distinct specifications in which τ_{eff} is either calculated based on the average share of investment in machinery and buildings based on aggregate data from the Federal Statistical Office of Germany (Specification "I") or on the firm-specific share of investment in machinery and buildings reported to the ifo Investment Survey whenever available (Specification "II"). In the left panel, we assume a time-constant discount rate of 7% following Zwick and Mahon (2017), in the right panel we calculate the PDV based on the time-varying interest rate on firm loans as displayed in Figure E.1.

Figure E.4 – Relation of Changes in Effective Tax Rates and Changes in User Cost of Capital



Notes: This figure plots the change in the user cost of capital (multiplied with 100) against the change in effective tax rates for all tax hikes in our sample, assuming that the share of investment allocated to machinery and buildings is constant across firms, but varying over time (Specification I). In the left panel, we assume a time-constant discount rate of 7% following Zwick and Mahon (2017), in the right panel we calculate the PDV based on the time-varying interest rate on firm loans as displayed in Figure E.1.

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