

## Downward Revision of Investment Decisions after Corporate Tax Hikes<sup>†</sup>

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*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 3 percentage points after a tax hike. This effect is twice as large during recessions. (JEL D22, E32, G31, H25, H32, H71, L60)*

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

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provide exogenous variation in exposure to tax decreases across firms of different size or in different industries (e.g., Zwick and Mahon 2017; Ohn 2018; Garrett, Ohn, and Suárez Serrato 2020). 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.<sup>1</sup> 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—that is, 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—that is, the difference between the investment volume planned prior to the tax change and the investment volume ultimately realized—is advantageous from several perspectives.<sup>2</sup> 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'Haultfoeuille 2023).

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

<sup>1</sup> This variation has been used by Fuest, Peichl, and Siegloch (2018) to study the wage incidence of corporate taxation and by Lichter et al. (forthcoming) to assess the effects on R&D spending.

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

average, invest approximately as much as previously planned, this maps into a semielasticity of investment with respect to the LBT rate of around 3. The corresponding elasticity of investment with respect to the net-of-tax rate is of similar magnitude. 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, for example, Fuest, Peichl, and Sieglösch 2018; Lichter et al., forthcoming), 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, cash flow sensitivity, and tax incidence.

Our main contribution is to investigate the impact of hikes in the corporate tax rate on firm investment.<sup>3</sup> While we are not the first to study this important question, we add 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, Pettit, and Whited (2022) investigate the effects of changes in US state-level taxes on firm level outcomes.<sup>4</sup> 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.<sup>5</sup> While in our context the variation is on a more 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—that is, 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.

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

<sup>4</sup>Mertens and Ravn (2013) use aggregate data and combine a narrative approach with a structural vector autoregression (VAR) model to exploit changes in US federal corporate taxes.

<sup>5</sup>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 multistate firms, and Ivanov, Pettit, and Whited (2022) studies tax decreases, whereas we focus on tax hikes.

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 (House and Shapiro 2008; Zwick and Mahon 2017; Ohn 2018; Maffini, Xing, and Devereux 2019; Garrett, Ohn, and Suárez Serrato 2020; Curtis et al. 2021; Gucer and Albinowski 2021; Xu and Zwick 2022). 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, Koivisto, and Matikka (2022).<sup>6</sup> However, as the Finnish corporate tax cut also entailed 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; Ramey and Zubairy 2018; Ghassibe and Zanetti 2022) and the state dependence of investment effects in response to tax changes more specifically (Jones, Olson, and Wohar 2015; Ljungqvist and Smolyansky 2018; Demirel 2021; Hayo and Mierzwa 2021; 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 I describes the municipality-level data on LBT rates and the survey data on firm-level investment plans and their realizations. Section II presents our empirical strategy, while Section III documents the results. Section IV concludes.

## I. Institutional Background and Data

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

### A. The German LBT

*Institutional Background.*—The LBT is one of three types of taxes on business income in Germany. It is applied to the operating profits of both corporate and noncorporate 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

<sup>6</sup>In the German context, Dobbins and Jacob (2016) compares 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.

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.<sup>7</sup> 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 on investment.<sup>8</sup> 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. Online 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 (excluding 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.<sup>9</sup>

There is substantial variation in LBT rates across municipalities and over time.<sup>10</sup> 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—that is, 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 online Appendix Figure B3). Importantly, past increases in the LBT contain very little predictive power for future tax hikes, as shown in online Appendix Figure B4.

After combining the municipality-level data on LBT rates with the firm-level investment data described in Section IB, we can exploit large parts of this variation in LBT rates. As summarized in the left side of Table 1, our empirical strategy outlined

<sup>7</sup>Online Appendix Figure A1 substantiates this empirically, showing that newspaper coverage of LBT hikes indeed peaks each year in December.

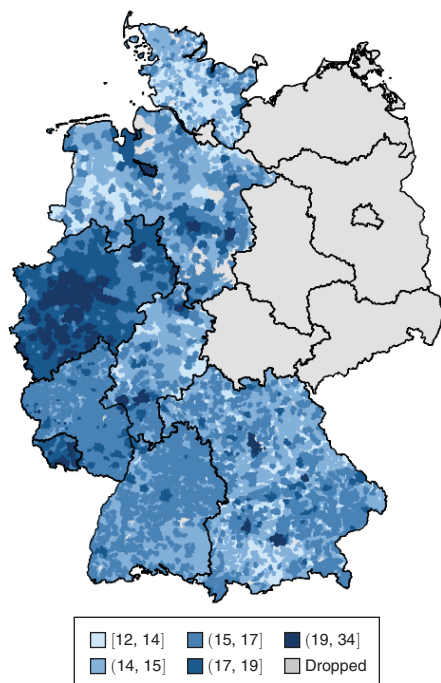
<sup>8</sup>The number of tax decreases that could, in principle, be used in the analysis is very low. Combining the municipality-level data on LBT rates and the firm-level data from the IVS, our analysis could only exploit 236 firm-year observations (0.7 percent of all observations) that face a tax drop in a given year, despite spanning a time frame of almost 4 decades.

<sup>9</sup>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.

<sup>10</sup>See online Appendix B1 for a more detailed description and investigation of the variation in LBT rates.



Panel A. Tax rates



Panel B. Tax hikes

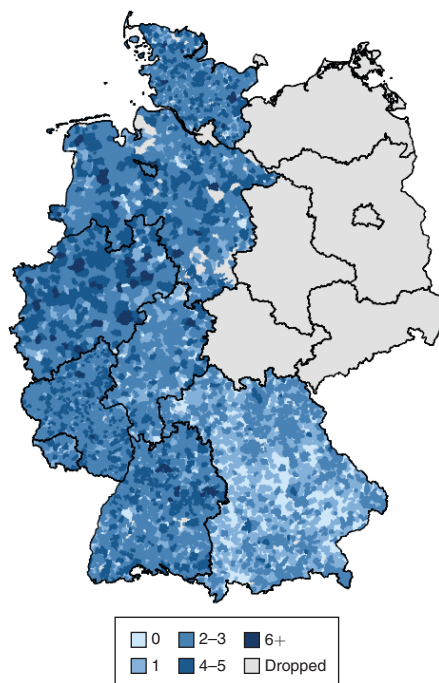


FIGURE 1. VARIATION IN LBT RATES (1980–2018)

*Notes:* This figure shows the cross-sectional and time variation in municipal scaling factors of the German 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.

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

### B. Firm-Level Data on Revisions of Investment Plans

We use micro data on firms' investment behavior from the ifo Investment Survey (IVS) (IVS-IND 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.<sup>11</sup> The main purpose of the IVS is to obtain timely information on investment activity at

<sup>11</sup> The online appendix 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 microdata have been extensively used in recent research—for example, Bachmann, Elstner, and Hristov (2017); Bachmann and Zorn (2020); and Link et al. (2023).

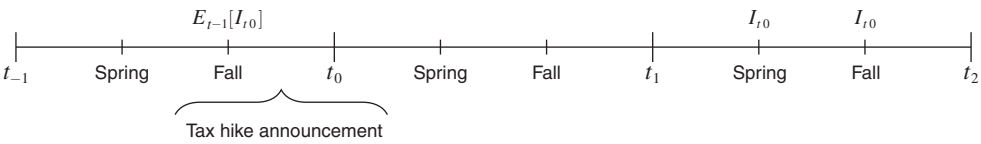
TABLE 1—TAX HIKE ACROSS MUNICIPALITIES AND FIRMS: SUMMARY STATISTICS

	Municipalities with tax hikes			Firm observations			
				With tax hikes		Without tax hikes	
	Observations	Mean	SD	Observations	Share of downward revisions	Observations	Share of downward revisions
1980–1984	119	1.05	0.83	265	0.50	2,655	0.45
1985–1989	131	0.92	0.53	340	0.49	4,940	0.45
1990–1994	266	1.09	0.54	546	0.58	4,831	0.54
1995–1999	228	0.94	0.47	385	0.51	4,560	0.51
2000–2004	178	1.06	0.55	269	0.60	4,711	0.58
2005–2009	106	0.78	0.51	161	0.66	4,446	0.59
2010–2014	263	0.74	0.42	413	0.58	4,118	0.58
2015–2018	152	0.69	0.38	248	0.63	2,422	0.60
Full sample	1,443	0.92	0.54	2,627	0.56	32,683	0.54

Notes: This table reports summary statistics of the final sample used in the main analysis—that is, after combining the municipality-level data on LBT rates and the firm-level data from the IVS. The left side 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 side 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 ( $(I_{i,t}) / (E_{i,t-1}[I_{i,t}]) < 1$ ) for each of these groups.

disaggregated industry levels.<sup>12</sup> 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 for 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).<sup>13</sup>

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 euros) in the subsequent year  $t_0$ , denoted  $E_{t-1}[I_{t0}]$ . The realized investment volume of year  $t_0$ ,  $I_{t0}$ , is elicited in the spring and fall survey of year  $t_1$ .<sup>14</sup>

<sup>12</sup>The German Federal Statistical Office releases information on realized investment at the levels of disaggregated industries, only with a time lag of two years.

<sup>13</sup>As noted on the cover letter of the survey, the ifo Institute guarantees compliance with strict data security criteria, that the data is evaluated in anonymous form, and that the survey results are only made available at the aggregate industry level. The participants therefore know that the firm-specific information reported to the IVS neither is available to their stakeholders, nor can be related to the municipality they are located in, and where the LBT is set. Therefore, incentives to strategically misreport the company’s future investment activity are limited and unrelated to the variation in the LBT.

<sup>14</sup>Following Bachmann, Elstner, and Hristov (2017), we take the average of  $I_{t0}$  if firms report it in both waves of year  $t_1$  and drop the observation if these reports deviate more than 20 percent from the mean (see online Appendix B3 for details). The results are similar once we restrict the analysis to  $I_{t0}$ , reported in the fall wave.

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  $t_{-1}$ —that is, 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) shows that aggregate investment growth calculated from the microdata of the IVS is highly correlated with manufacturing investment growth reported by the German Federal Statistical Office, and Sauer and Wohlrabe (2020) reports that the average absolute deviation of the former from the latter is less than 2 percentage points. Moreover, Bachmann, Elstner, and Hristov (2017) presents a series of stylized facts on the cross-sectional and time-series properties of revisions of investment plans—that is, 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.<sup>15</sup> 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 online Appendix B3, we keep those firms for which we can observe revisions in investment plans for at least five years and, following Fuest, Peichl, and Siegloch (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 online Appendix B3, the median firm in our sample is a typical representative of the German *Mittelstand* employing 264 workers, generating annual revenues of €45 million (CPI inflation adjusted and—if denominated in German marks—converted to 2015 euros), and investing €1.4 million 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 percent of all observations.

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—that is,  $E_{t-1}[I_{t0}]$  and  $I_{t0}$  (both in logs)—is highly linear and virtually corresponding to the 45 degree line. As depicted in online Appendix Table B4, 84 percent of the unconditional variation in (log) realized investment is explained by the investment plans for the respective year. Online Appendix B3 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

<sup>15</sup> Relatedly, online Appendix Figure B7 shows that the investment plans are more frequently and more strongly revised downward during recessions.



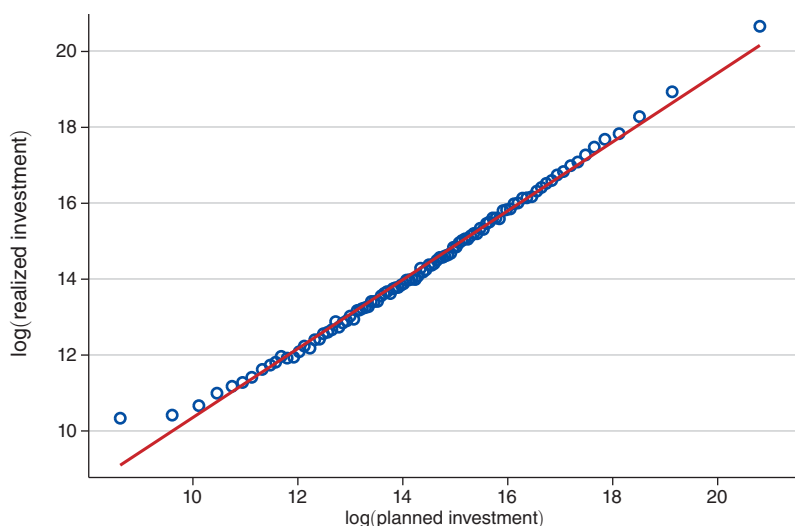


FIGURE 2. RELATIONSHIP BETWEEN PLANNED AND REALIZED INVESTMENT

*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.

investment  $I_{t_0}$  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—that is, that firms revise investment decisions downward after tax hikes. For each five-year interval of the data, the right side 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.<sup>16</sup> We investigate this effect more systematically in the remainder of the paper.

## II. Empirical Strategy

### A. 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.

<sup>16</sup>In the pretreatment year  $t_{-1}$ , the averages of both main outcome variables (log revision ratio and downward revision indicator) are not statistically different for firms that eventually are affected by a tax hike in year  $y_0$  and firms ending up in the control group; see panel B of online Appendix Table B3.

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—that is, 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 posttreatment period.

Hence, compared to using realized investment as a 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, Peichl, and Siegloch (2018) shows that changes in the LBT are typically not triggered by shocks to economic variables, and Blesse, Doerrenberg, and Rauch (2019) demonstrates that tax setting of the municipalities substantially deviates from theoretically optimal behavior. As in the United States (Robinson and Tazhitdinova 2023), 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. Furthermore, any systematic and time-constant differences in the expected probability to be treated between firms and municipalities are inconsequential once we include firm fixed effects. However, private information about the likelihood of tax hikes may lead to systematically different beliefs in  $t_{-1}$ , the year before the LBT is increased (Riedel and Simmler 2021). In particular, some firms may receive signals on the likely occurrence of a tax hike in the subsequent year even before investment plans are reported in the fall. In this case, they will—at least partially—incorporate this information into their investment plans and, hence, revise

their investment decisions less strongly on average thereafter. If the private information helped firms to better predict the occurrence or absence of tax hikes, we hence should, if anything, **tend to underestimate the investment response to a tax hike**.<sup>17</sup> 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).

### B. Measurement and Estimation

We use two variables to measure investment revisions. The first is an indicator for revising investment decisions downward, defined as

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

The downward revision indicator is attractive due to its robustness against outliers and nonlinear investment responses. The second variable is the log revision ratio and takes the magnitude of each revision into account. It is defined as the natural logarithm of the ratio between realized and planned investment volumes:

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

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 semielasticity 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 1, on average. As, furthermore, the revision ratio and realized investment are measured in logarithmic form, a tax hike that decreases the log revision ratio by 0.01 implies that both the revision ratio and realized investment decrease by 1 percent.<sup>18</sup>

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

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

<sup>17</sup> 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 lack information on the prevalence and the payroll share of multiestablishments, the resulting attenuation bias is arguably small as only 7 percent 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 multiplant company.

<sup>18</sup> In more recent years, firms tend to invest on average slightly less than previously planned (compare online Appendix Figure B7). 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 semielasticity of the revision ratio with respect to an LBT hike will slightly understate the semielasticity of investment with respect to an LBT hike.

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

$$Tax\ Hike\ Indicator: \quad \mathbf{1}(\Delta tax_{m,t} > 0)$$

$$Tax\ Hike\ in\ Percentage\ Points: \quad \Delta tax_{m,t}.$$

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.<sup>19</sup> Still, some specifications additionally include firm fixed effects ( $\mu_i$ ) 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 a (generalized) difference-in-difference (DiD) estimate.<sup>20</sup> Standard errors are clustered at the municipality level.

### III. 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.

#### A. 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 downward 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

<sup>19</sup>While Section IB 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.

<sup>20</sup>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'Haultfoeuille 2023) 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 IIIB. Inspired by Dube et al. (2023), 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—DiD: INVESTMENT REVISIONS AFTER A TAX HIKE

	(1)	(2)	(3)	(4)	(5)
<i>Panel A. Downward revision</i>					
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	35,310	35,310	35,310	35,310	35,310
<i>Panel B. log revision ratio</i>					
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	34,421	34,421	34,421	34,421	34,421
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 industries in the statistical classification of economic activities in the European community (NACE). Standard errors in parentheses are clustered at the municipality level.

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 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 semielasticity 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 online Appendix D. According to our estimated semielasticity of 3, each additional euro of tax revenues raised comes with a loss in firm investment of €2.12 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 a range between €2.14 and €2.28, depending on the assumed strength of the second-round effect. While these projections rely on a series of simplifying assumptions, they still illustrate that the foregone volume of investment is nonnegligible.

From this approximation of the behavioral response, we can also derive the marginal value of public funds (MVPF) in the spirit of Hendren and Sprung-Keyser (2020), given as

$$MVPF = \frac{\text{Beneficiaries' Willingness to Pay}}{\text{Net Cost to Government}}.$$

In our setting, firms are the beneficiaries and their willingness to pay is equal to the change of the tax burden. The net cost of the government equals the change of tax revenues plus the additional revenue changes via the behavioral response. According to this, our estimates point at an MVPF in the range between 1.01 and 1.08—that is, slightly above 1.<sup>21</sup> However, given that investment is not the only margin of adjustment through which firms may react to increases in the LBT, the true MVPF is presumably larger. For example, Fuest, Peichl, and Sieglöcher (2018) finds that workers bear about half of the total burden of the LBT via lower wages while employment is unaffected. The corresponding loss in payroll tax revenues would show up in the denominator of the MVPF formula and, thus, further raise the MVPF. Likewise, the cross-base elasticity with respect to the corporate income tax base is presumably positive, so that corporate income tax revenues accruing to the federal government will decline as well. The MVPF should be even higher once these fiscal externalities were taken into account.

<sup>21</sup> The calculation is:  $(19,800)/(19,800 - 139) = 1.01 \geq MVPF \leq 1.08 = (19,800)/(19,800 - 1,386)$ , where the value of €19,800 refers to the increase in overall tax revenues paid by the median firm in response to a one percentage point increase in the LBT, and the values of €139 and €1,386 denote the assumed upper and lower bound of the behavioral response, as described in online Appendix D.



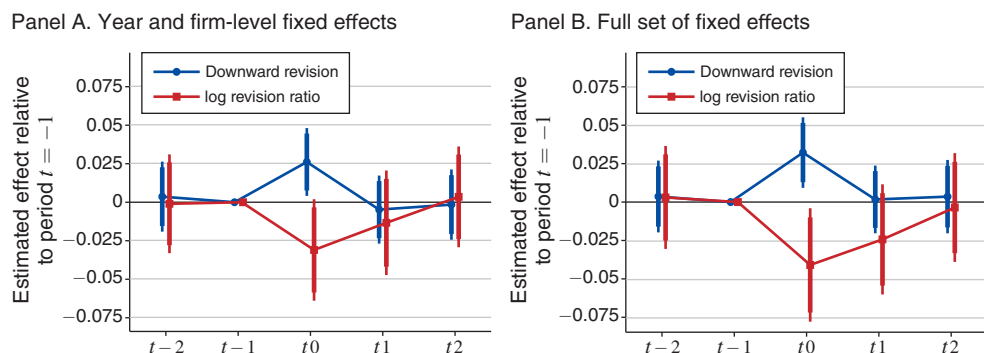


FIGURE 3. EVENT STUDY: INVESTMENT REVISION EFFECT AFTER A TAX HIKE

Notes: This figure shows the estimates of the following event-study regression:  $InvestmentRevision_{i,t} = \sum_{j=-2}^2 \gamma_j TaxHike_{m,t}^j + \varepsilon_{i,t}$ . 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 1 if the ratio is below 1. 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 percent (thick line) and 95 percent (thin line).

*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—that is, 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 (i) when reporting its investment plans for year  $t_1$  to the IVS (in the fall wave of year  $t_0$ ) and (ii) 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 assumptions. Panel A of online Appendix Figure C1 shows that these patterns also hold when extending the time window covered by the estimation to four years prior to and posttreatment.<sup>22</sup>

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'Haultfoeuille 2023), 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

<sup>22</sup> The reason why we restrict ourselves to two pre- and postevent periods in Figure 3 is sample size. Because we always require that no other tax change happened in the pre- and postevent periods, extending the number of periods would shrink the size of the estimation sample considerably.

dynamic—that is, 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, Jaravel, and Spiess (forthcoming) as well as the interaction-weighted estimator by Sun and Abraham (2021). As shown in online Appendix Figure C2, 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.<sup>23</sup> 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 reinvested 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, Peichl, and Siegloch (2018) and Lichter et al. (forthcoming), online Appendix Figure C3 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 (online Appendix Table C1).

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. Online Appendix Figure C4 plots the cumulative distribution function of these placebo treatment coefficients. The nonparametric *p*-values obtained from this exercise are 0.0005

<sup>23</sup> Relatedly, online Appendix Table C5 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.

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.

### B. 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 the net-of-tax rate, the effective tax rate, 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 II, we can directly interpret a 1 percent decrease in the log revision ratio as a 1 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 three event-study regressions using either the log revision ratio (red), the log realized investment volume (yellow), or the log planned investment volume (green) as the dependent variable. As expected, the point estimates of both realized investment and the revision ratio are of comparable size in  $t_0$ —that is, the year of the tax hike—and indicate a drop by approximately 3 percent. Accordingly, investment plans fall to a persistently lower level only one period later in  $t_1$ , when firms have incorporated the tax hike of year  $t_0$  into their information set.

Note that Figure 4 is estimated on a different sample than our baseline estimates: because treatment effects on investment levels are persistent and do not return to zero after one year, heterogeneous treatment effects may bias the estimates without further restrictions (e.g., de Chaisemartin and D'Haultfoeuille 2023). Inspired by Dube et al. (2023), we therefore assign firms that experienced several tax hikes into distinct episodes to distinct firm identifiers, such that each spell contains a window around one tax hike only. For each of these spells, we assume that treatment effects have stabilized after three years and trim more distant periods. We further require that no other tax hikes took place during these periods, which—in total—reduces the sample size by 40 percent. In consequence, the coefficients are estimated less precisely than in our baseline specification. Note that despite these necessary sample restrictions, the coefficient for the log revision ratio in Figure 4 is still identified under much weaker assumptions than the coefficients for investment plans and realizations.

Overall, 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. Panel B of online Appendix Figure C1 shows that these patterns also hold when extending the time window covered by the

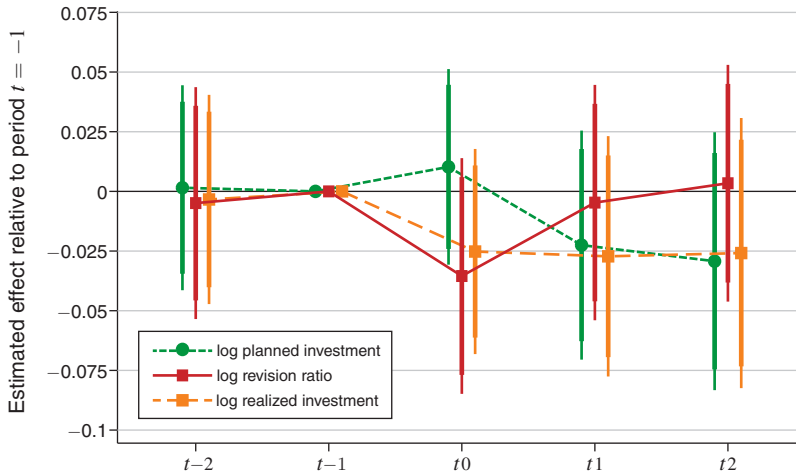


FIGURE 4. EFFECT OF TAX HIKE ON INVESTMENT PLANS, REALIZATIONS, AND REVISIONS

*Notes:* This figure shows event-study estimates of log planned investment (green, short dashed lines), log realized investment (orange, long 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  $t-1$ . In addition, end periods  $t-3$  and  $t+3$  are binned and not shown. The sample is trimmed outside the event window. 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. (2023), when estimating the effects with respect to log planned and 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 percent (thick lines) and 95 percent (thin lines).

estimation to four years before and after the treatment, which, by construction, relies on substantially fewer observations.

*Effect Sizes Expressed in Terms of Net-of-Tax Rates.*—A common quantity of interest in the public finance literature is the elasticity of investment with respect to the net-of-tax rate. To interpret our finding through the lens of this literature, online Appendix Table C3 reestimates our baseline results regarding the log revision ratio after expressing the variation in the LBT in terms of changes in the net-of-tax rate, defined as  $\log(1 - \tau_t) - \log(1 - \tau_{t-1})$ . The resulting elasticity of investment with respect to the net-of-tax rate ranges between two and three, depending on the choice of the control vector. In the most restrictive specification, which includes the full set of fixed effects, a 1 percent increase in the net-of-tax rate increases the log revision ratio and, thus, investment by 3 percent.

*Effect Sizes Expressed in Terms of Effective Tax Rates.*—Our main specification estimates the investment response to changes in statutory marginal tax rates—that is, the LBT parameter that is directly set by municipal policymakers 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 marginal tax rates—that is, also accounting for deductions—including depreciation rules, and other exemptions or tax credits (of which there are very few in the German context).

To better compare our estimates with these studies, we thus also run such alternative specifications of our baseline estimation.

Our procedure to calculate effective marginal tax rates  $\tau_{eff}$ , which is described in detail in online Appendix E, follows the framework of Hall and Jorgenson (1967), as, for example, recently applied by Furno (2022). Under this framework, the effective marginal tax rate is given by  $\tau_{eff} = 1 - ((1 - \tau)/(1 - z \times \tau))$  and depends only on the present discounted value (PDV) of the depreciation  $z$  and the statutory LBT rate  $\tau$ .<sup>24</sup> To obtain  $z$ , we rely on information on depreciation schedules for machinery and buildings obtained from the Oxford Corporate Tax Database. Due to lack of the respective information, expressing changes in the LBT in terms of changes in effective marginal 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—that is, 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 online Appendix Figure E1). 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.

To account for this, we compute four different versions of  $\tau_{eff}$  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 percent when calculating the PDV 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 aggregate data from the German Federal Statistical Office, while the others use the firm-specific share of investment in machinery and buildings reported to the IVS whenever available.<sup>25</sup> Online Appendix Figure E3 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.

The results presented in panel A of Table 3 show that the estimated effect sizes are largely comparable across the different specifications of  $\tau_{eff}$ . 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

<sup>24</sup> This simplified version of the formula can be applied in our setting because there are no relevant tax credits in the German LBT that would complicate the calculation.

<sup>25</sup> 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 online Appendix E.

TABLE 3—INVESTMENT REVISIONS AFTER A TAX HIKE: EFFECTIVE TAX RATES

	log revision ratio			
	(1)	(2)	(3)	(4)
<i>Panel A. Variation in effective tax rate</i>				
Effective tax hike	−0.132 (0.059)	−0.124 (0.057)	−0.136 (0.067)	−0.133 (0.066)
Constant	−0.033 (0.001)	−0.033 (0.001)	−0.033 (0.001)	−0.033 (0.001)
<i>Panel B. Variation in user cost of capital</i>				
User cost hike	−0.120 (0.054)	−0.107 (0.050)	−0.121 (0.061)	−0.111 (0.057)
Constant	−0.033 (0.001)	−0.033 (0.001)	−0.033 (0.001)	−0.033 (0.001)
Assumptions:				
Interest rate	0.07	0.07	time-varying	time-varying
Specification investment share	I	II	I	II
Observations	34,421	34,421	34,421	34,421
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 ( $\tau_{eff}$ ) calculated as described in online 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 PDV of depreciation, either assuming a time constant interest rate of 7 percent (as, e.g., in Zwick and Mahon 2017), or based on time-varying interest rates on firm loans as depicted in online Appendix Figure E.1. Further,  $\tau_{eff}$  (and *UserCost*) is either calculated based on the average share of investment in machinery and buildings based on aggregate data from the German Federal Statistical Office (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.

takes into account that the average statutory marginal tax rate of 16.79 percent is considerably larger than the average effective marginal tax rate (assuming a discount rate of 7 percent). 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 (compare  $-0.132 \times 3.82/16.79 = -0.030$  versus  $-0.038$ ). When relying on time-varying interest rates—for example, in column 3—the rescaled point estimate implies a slightly lower effect size, which is, however, still in the same order of magnitude ( $-0.136 \times 2.9/16.79 = -0.023$ ).

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 online Appendix Figure E4 ( $\tau_{eff} = 1 - UserCost^{-1}$ ), 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  $-0.121$ .



TABLE 4—SUMMARY OF ESTIMATED (SEMI)ELASTICITIES

	log revision ratio	$I/K$
Change in tax rate	−0.038 (0.016)	—
Change in net-of-tax rate (in %)	3.032 (1.312)	—
Change in effective tax rate	−0.133 (0.066)	—
Change in user cost of capital	−0.111 (0.057)	−1.11 (0.57)

*Notes:* This table summarizes the main (semi)elasticities reported in Section IIIB, relating the change in corporate tax rates to changes in the log revision ratio, and, as explained in the main text, to changes in realized investment. We always report the results of the most restrictive specification that controls for firm fixed effects as well as industry-year and state-year fixed effects. The  $I/K$  estimate is not directly estimated in our data, but obtained by rescaling the estimate for the log revision ratio with mean  $I/K$  of 0.1 reported by Zwick and Mahon (2017).

*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 1 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.<sup>26</sup> Table 4 summarizes the main (semi)elasticities reported in this section.

The  $I/K$  transformation in the last column 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).<sup>27</sup> Given that the investment stimulus studied in the latter paper was an explicitly recessionary policy, our evidence on state dependence in the effect of tax hikes on firm investment delivers a potential explanation for the larger effect size found in their setting. Zwick and Mahon (2017) also find that loss-making firms are less responsive to tax shocks, in line with our evidence on smaller treatment effects among firms facing large revenue drops documented below (online Appendix Table C6). Furthermore, our semielasticity of 3 can be compared to Ohn (2018) who reports a semielasticity 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

<sup>26</sup> 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:  $-0.12/0.1 = -1.2$ .

<sup>27</sup> Ohn (2018, 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).

more effective at stimulating investment than statutory tax rate cuts, as rationalized by Chen et al. (2023).

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 multistate firms, for which they find a semielasticity of 0.24. As pointed out by the authors, this small elasticity might arise due to measurement error. Furthermore, the elasticity of capital can be temporarily lower than the elasticity of investment due to adjustment costs along the transition path in the firms' dynamic response. 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 semielasticities between 2.1 and 4, comparable to the responses documented in this paper.

In general, the elasticities reported in our paper are, of course, specific to the institutional setting of corporate taxation in Germany and the sample of firms affected. For instance, the elasticities we estimate for the manufacturing sector might differ from elasticities in other sectors, as manufacturing is more capital intensive than the overall economy. For the United States, Cloyne, Kurt, and Surico (2023) argues that tax cuts stimulate real investment activity mainly in the manufacturing sector, whereas firms in the services sector are more prone to adjust dividends instead.

Finally, our results complement evidence from Lichter et al. (forthcoming), 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 semielasticity 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.

### *C. 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 (Jones, Olson, and Wohar 2015; Ljungqvist and Smolyansky 2018; Demirel 2021; Hayo and Mierzwa 2021; 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.<sup>28</sup>

<sup>28</sup> As shown in online Appendix Figure B2, 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 I.

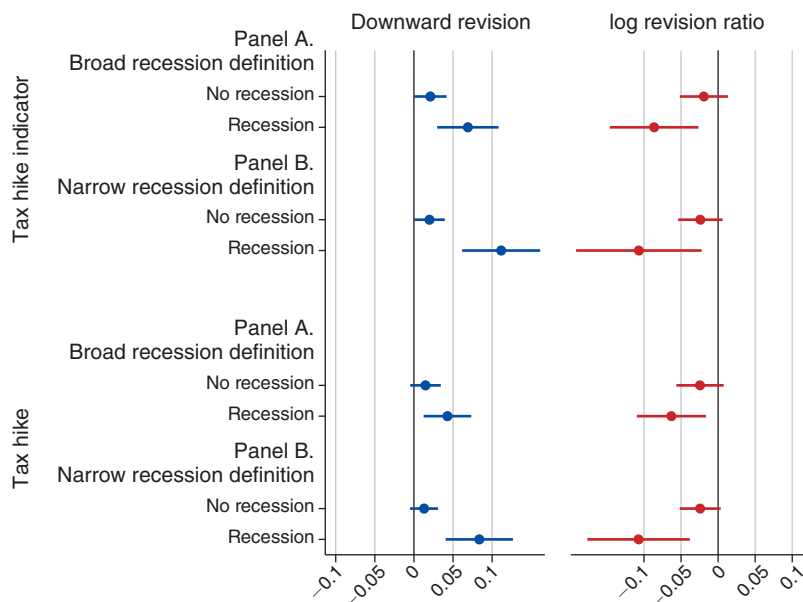


FIGURE 5. INVESTMENT REVISIONS AFTER A TAX HIKE: STATE DEPENDENCE

*Notes:* This figure estimates how the probability of investing less than previously planned or the log revision ratio change in response to a tax hike separately for recession and nonrecession 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 and 1993, 2001–2003, and 2008 and 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 percent level. The full regression output is disclosed in the even columns of online Appendix Table C.2.

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 a narrower classification of recession periods, defined as years with negative real GDP growth in panel B.<sup>29</sup>

<sup>29</sup> 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 online Appendix Table C2).

*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 explain why the effect of tax hikes should be state dependent. In the following, we discuss three channels that 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.<sup>30</sup> 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).<sup>31</sup> 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. Online Appendix Table C4 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 cash flow (Almeida, Campello, and Weisbach 2004). Corporate taxes decrease the cash flow for profitable firms and, therefore, lower investment. At the same time, Almeida, Campello, and Weisbach (2004) shows that cash flow 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 disproportionately 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 downward, the revision effect after a tax hike is smaller compared to firms without a large revenue drop during a recession (online Appendix Table C5). Firms with a large decline in revenues might still be profitable if they reduce their labor costs significantly. Online Appendix Table C6 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

<sup>30</sup> As discussed by Fuest, Peichl, and Sieglöcher (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.

<sup>31</sup> 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 United States, Ljungqvist, Zhang, and Zuo (2017) show that, in response to a tax increase, the average firm reduces risk as measured by their earnings volatility. Langenmayr and Lester (2018) find similar results in a cross-country panel and among small Spanish firms.

for a strong slowdown in investment volumes, the revision effect tends again to be larger in recessions (online Appendix Table C7). Both findings provide suggestive evidence that the stronger investment response in recessions may relate to cash flow 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, Peichl, and Sieglöcher (2018) shows that workers bear approximately half of the incidence of the LBT in Germany. However, as wages are nominal downward rigid (e.g., Barattieri, Basu, and Gottschalk 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 online Appendix Figure C5. This could suggest that during recessions, (cash flow sensitive) firms reduce their investment disproportionately, as downward rigid wages do not allow shifting the tax burden on workers. Consistent with such a channel, Fuest, Peichl, and Sieglöcher (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).<sup>32</sup> 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 ( $\leq 3$ ) 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 having experienced a tax hike in the last five years is plausibly associated with a larger investment response, although the estimates

<sup>32</sup> Given that firm size is often been used as a measure of cash flow sensitivity, this finding might seem inconsistent with the aforementioned suggestive evidence that the stronger investment response in recessions may relate to cash flow sensitivity. There are at least two possible explanations for this discrepancy. First, the share of very small manufacturing firms, which arguably suffer most from adverse financing conditions, in our sample is small (online Appendix Table B2). Second, online Appendix Figure C6 demonstrates that small firms are not only more likely to be cash flow sensitive but also more likely to underinvest due to a weak earnings situation. As the LBT taxes profits and less profitable firms should thus react less sensitively to tax hikes, the latter mechanism could counteract the former.

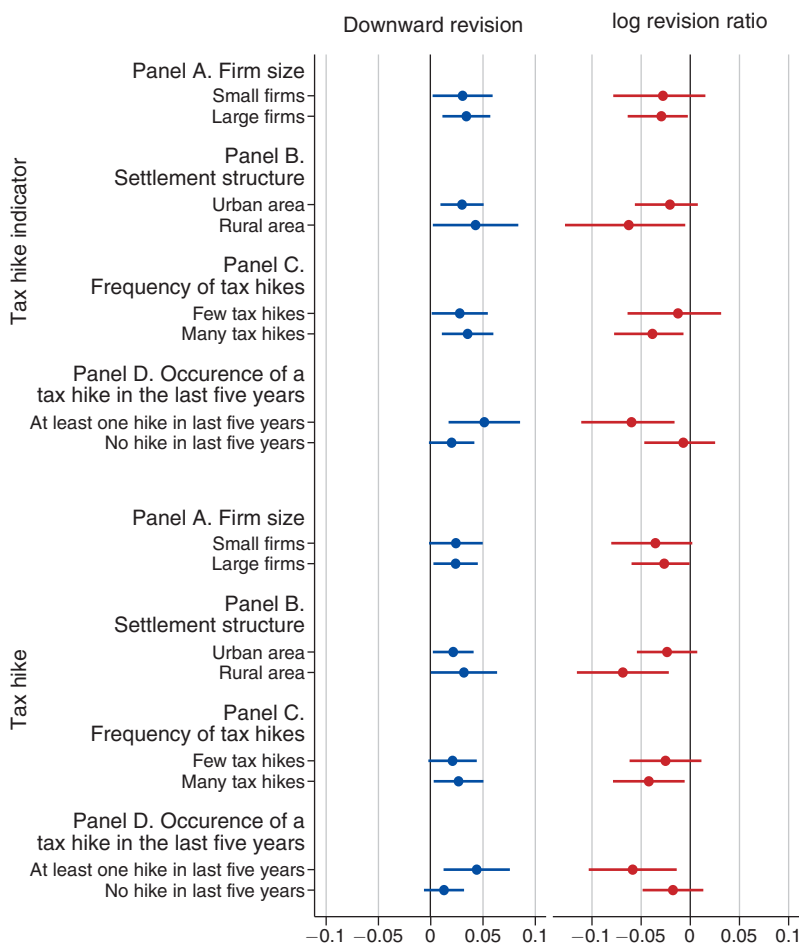


FIGURE 6. TESTING FOR FURTHER HETEROGENEITY

*Notes:* This figure estimates how the probability of investing less than previously planned or the log revision ratio 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 that is mainly based on population density. In panel C, we split the sample into municipalities with few ( $\leq 3$ ) 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 percent level. The full regression output is disclosed in the even columns of online Appendix Tables C8 and C9.

are not statistically different from each other in any specification (online Appendix Table C9). 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.



#### IV. 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 LBT 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 downward 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, cash flow 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 that 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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