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DISCUSSION PAPER

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Profit Taxation, R&D Spending, and Innovation

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Profit Taxation, R&D Spending, and Innovation

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Abstract. We study how profit taxation affects plants' R&D spending and innovation activities. Relying on geocoded survey panel data which approximately covers the universe of R&D-active plants in Germany, we exploit around 7,300 changes in the municipal business tax rate over the period 1987–2013 for identification. Applying event study models, we find a negative and statistically significant effect of an increase in profit taxation on plants' R&D spending with an implied long-run elasticity of -1.25. Reductions in R&D are particularly strong among more credit-constrained plants. In contrast, homogeneity of effects across the plant size distribution questions policy makers common practice to link targeted R&D tax incentives to plant size. We further find lagged negative effects on the (citation-weighted) number of filed patents.

Keywords: corporate taxation, firms, R&D, innovation, patents
JEL Codes: H25, H32, O31, O32

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1 Introduction

Innovation has long been emphasized as a key driver of economic growth (Solow, 1957, Romer, 1990). In this process, firms serve as the cradle of most groundbreaking new technologies and products. For firms, research and development (R&D) serves as an instrument to expand in size and to increase productivity (Balasubramanian and Sivadasan, 2011), assimilate knowledge from competitors (Aghion and Jaravel, 2015), and secure long-term growth (Kogan et al., 2017). However, from a societal perspective, private investments in R&D are generally considered to be below the social optimum (Jones and Williams, 1998). This is due to positive knowledge spillovers, which make it difficult for firms to monetize the benefits of new ideas in full (Bloom et al., 2013).

Against this backdrop, many countries across the world have implemented targeted tax policy instruments, such as R&D tax credits or extended deduction possibilities for R&D expenses, to spur firm-level R&D. These policy instruments reduce the burden of business taxes for specific firms conditional on engaging in R&D. A recent body of research, discussed in more detail below, shows that targeted R&D tax incentives work, i.e., notably increase firms' innovative activities. Compared to this consistent finding, there is very little systematic empirical evidence on the potential disincentive effects of general business taxes on firms' R&D activities. This lack of evidence is problematic for at least three reasons. First, unlike targeted R&D tax incentives, business taxes are in place in almost every country across the world. Second, knowledge about the size of possible disincentive effects is needed to optimally design targeted tax incentives for R&D. Third and related, while targeted tax incentives typically apply to specific types of R&D-active firms only, general business taxes affect all firms. Knowledge about possible effect heterogeneity across the distribution of (research-active) firms can be used to improve the accuracy and precision of targeted R&D policies.

In this paper, we aim to fill this gap by analyzing the impact of profit taxation on plant-level R&D spending and innovation output.[1] We contribute to the small literature studying the effect of general business taxes on R&D activities by exploiting the unique institutional setting of profit taxation in Germany, where municipalities autonomously set their local business tax (LBT) each year. Exploiting around 7,300 local tax changes over the period from 1987–2013, we draw upon detailed, geocoded biennial panel data that approximately covers the universe of R&D-active plants in Germany. The dataset, which forms the key basis for Germany's official R&D reporting to EU authorities and the OECD, allows us decomposing plants' overall R&D spending along various margins, e.g., by distinguishing expenses on internally- vs. externally-conducted R&D projects and testing for heterogeneous treatment effects along plant characteristics (liquidity and size in particular). To test for potential accompanying effects on innovation output, we further enrich the data with administrative information on plants' patenting activities from the European Patent Office.

We set up a simple theoretical model that builds upon earlier contributions by Chetty and Saez (2010) and Chen et al. (2021) to guide our empirical analysis. The model provides three key hypotheses. First, an increase in the local business tax rate should reduce plants' R&D investments because it lowers their after-tax returns. Second, negative effects should be particularly strong

[1] Our empirical analysis is at the level of the plant. We explicitly test for differences between single- and multi-plant firms. Throughout the paper, we use the term "firm" in general contexts, i.e., when we refer to mechanisms discussed in the literature or institutional features that apply to the firm rather than the plant.

for types of R&D expenditures that are less costly to adjust at the margin. Third, an increase in the local business tax rate is expected to cause stronger reductions in R&D spending among more credit-constrained firms. This latter hypothesis draws upon the institutional feature that the costs of debt financing can be deducted from a plant's tax base, its operating profits, whereas the costs of equity financing cannot. This distinct tax treatment distorts plants' financing decisions towards debt. However, investments in R&D are generally highly uncertain and come with substantial information asymmetries between the innovator and financial backers. Moreover, unfinished R&D projects have little residual value and often lack collateral (Hall, 2002, Brown et al., 2009, Hall and Lerner, 2010, Bakker, 2013)—factors that may make access to debt prohibitively expensive. We thus expect credit-constrained plants to respond particularly strongly to a given tax increase.

Our empirical results are in line with theoretical priors. Based on an event study model with staggered treatment that flexibly controls for varying regional trends, local business cycles, and political conditions, we show that an increase in the local business tax rate has a negative and statistically significant effect on plants' total R&D expenditures.[2] We derive a long-term elasticity of R&D expenditures with respect to the business tax rate of -1.25. This effect size is comparable with estimates reported in the context of targeted R&D tax credits or subsidies in other settings (see below for details). We further show that the negative R&D expenditure response is entirely driven by reductions in internal R&D spending, while the scale of outsourced R&D activities remains unaffected. We argue that this finding is consistent with steeper marginal adjustment costs for external R&D spending, e.g., through longer-term partnerships and contracts that are more costly to alter at the margin. Third, we find that reductions in R&D spending are indeed particularly strong among credit-constrained plants—as approximated by plants' age and their non-current liabilities to sales ratio. In contrast, we do not observe heterogeneity by firm size (employees and sales).

In a second step, we show that tax-induced reductions in R&D expenditures are accompanied by lower innovation output—both in terms of raw patent application numbers and when accounting for quality-differences in innovations by weighting each patent according to the number of citations it received. The effect materializes with a temporal lag of around two years after the initial tax-induced decline in R&D spending. The corresponding long-term elasticity with respect to the tax rate is -0.9, an estimate close to the findings of Akcigit et al. (2021). We further find very similar effects for single- and multi-plant firms, which indicates that higher business taxes have real negative effects on plants' innovation output and not only cause tax avoidance behavior of multi-plant firms; e.g., through the transfer of intangible assets to low-tax countries (see, e.g., Dischinger and Riedel, 2011, Karkinsky and Riedel, 2012, Griffith et al., 2014, for corresponding evidence).

Our findings are robust to different specifications of the event study model. In our preferred specification, we regress plant-level outcome variables on leads and lags of local business tax increases while controlling for plant fixed effects, sector-by-year fixed effects, federal state-by year as well as finely-grained commuting zone-by-year fixed effects. Post-treatment effects are stable and robust to the inclusion of varying regional trends. Pre-trends and additional robustness checks do not point to the presence of reverse causality or unobserved confounding effects. We also show that varying local economic conditions, population movements or government expenditures coinciding

[2] Throughout the paper, we focus on tax increases because less than 10% of all tax changes are decreases (see Section 2).

with a given change in the tax rate do not drive our results (see Fuest et al., 2018, for corresponding earlier evidence). Moreover, our findings remain unchanged when explicitly accounting for possible heterogeneous treatment effects across cohorts by applying the estimators of Sun and Abraham (2020) and Borusyak et al. (2021).

We take our estimates to assess the efficiency of general business taxes to affect firm-level innovation in comparison with targeted R&D tax policies. Our estimates imply that R&D expenses decrease by around 0.34 EUR for a tax increase worth 1 EUR in business tax revenues. Equivalent measures of targeted R&D tax incentives for the UK are much higher—between 1 to 1.7 GBP of increased R&D spending for each pound decrease in revenues (Dechezleprêtre et al., 2016, Guceri and Liu, 2019). Thus, targeted tax incentives for R&D expenditures appear to be the more efficient policy instrument altogether. However, our results question policy makers' common practice to link firms' eligibility for targeted R&D tax incentives to their size. Smaller plants do not react stronger to an increase in the LBT. Galaasen and Irarrazabal (2021) rather show that size-based eligibility thresholds for R&D tax incentive may even hinder aggregate economic growth: by promoting the R&D activities of small firms, size-dependent R&D tax incentives may boost the expansion of relatively unproductive firms and thus mitigate firm selection.

Last, we use our empirical set-up to carefully assess the role of innovation for economic growth and the importance of tax policy in this relationship. In line with the results by Kogan et al. (2017), we provide suggestive evidence that local innovation has a positive and lasting effect on municipal economic growth. In contrast, an increase in the local business tax considerably reduces local GDP. Combining these two pieces of evidence with the estimated plant-level elasticity of innovation output with respect to the LBT, these findings imply that around eight percent of the total negative effect of profit taxation on local growth are due to tax-induced reductions in innovation.[3] We take these results as additional evidence for the important role of innovation for economic growth.

Related Literature. Our results contribute to various strands of the literature. We first add to the small and recent literature that exploits variation in sub-national tax policy settings to study the effect of (corporate) taxation on innovation. Moretti and Wilson (2017) provide evidence on the geographic mobility of "star scientists" in response to tax policy changes at the level of the U.S. states. They find that star inventors are quite responsive to tax incentives, with long-run mobility elasticities amounting to around 1.8 (-1.7) for personal and corporate income taxes (tax credits). Akcigit et al. (2021) use U.S. state-level panel data on corporate and personal income tax rates as well as on patents over the entire 20th century to study the effect of tax policy on innovation. They find that higher taxes reduce the quantity and quality of innovations and affect the geographic spread of innovative activities. Moreover, they find corporate inventors to react more to changes in taxes than individual inventors. Exploiting the same variation in U.S. state-level tax rates over a shorter time period, Mukherjee et al. (2017) offer similar evidence in showing that increases of the corporate tax rate reduce firms' R&D investments and patenting. We contribute to these studies in two important dimensions. First, we make use of official survey data that approximately covers the

[3] The local business tax can affect growth through other channels than innovation, e.g., by reducing non-R&D investments, triggering the re-location of businesses, or lowering wages (Fuest et al., 2018).

universe of R&D-active plants in Germany, which enables us studying detailed plant-level responses to changes in the local business tax rates. The rich data allows drawing important insights on the underlying mechanisms, e.g., by differentiating between internal vs. external R&D spending. Second, the German institutional set-up of profit taxation offers ample variation in tax rates with thousands of tax changes of substantial sizes. In addition, the local nature of the tax rate variation allows accounting for possible regional shocks at very fine geographical levels.

The paper further speaks to the recent literature that analyzes the effects of targeted R&D tax credits, deduction possibilities, and subsidies. Dechezleprêtre et al. (2016) and Guceri and Liu (2019) exploit a 2008 reform in the UK's corporate tax scheme that increased R&D-related deduction possibilities for medium-sized firms relative to larger ones and document large and positive effects on R&D spending. Agrawal et al. (2020) exploit a 2004 reform of the Canadian R&D tax credit scheme for very small firms and also find substantial positive effects. Chen et al. (2021) show that a Chinese tax policy that awarded corporate income tax cuts to firms with R&D investments over a certain threshold stimulated R&D activity. Bronzini and Iachini (2014) evaluate a 2003 reform in Northern Italy, which introduced R&D subsidies for certain industrial research projects. They find that small firms significantly increased their R&D investments in response to the subsidy, whereas larger firms remained unresponsive. All studies provide clean causal evidence by exploiting policy cut-offs to establish quasi-experimental research designs. At the same time, the estimates are clearly local in nature, referring to firms around the respective thresholds. The data and identification strategy in our paper allows estimating treatment effects along the full distribution of R&D-active plants. Hence, we are able to identify average effects but also to test for heterogeneous responses along various plant characteristics. We can further rule-out mis-reporting effects as documented in Chen et al. (2021), given that our policy instrument, the LBT, does not specifically target plants' R&D spending.

Finally, we connect to a large literature that is concerned with market failures reducing private R&D activities below socially desirable levels. R&D embodies characteristics of a public good (Nelson, 1959, Arrow, 1962), such that the social rate of return to innovation is generally well above the private return (Griliches, 1992, Jones and Williams, 1998). At the same time, expected knowledge spillovers as well as uncertainty about marketability may lead to private under-investments into R&D (Czarnitzki and Toole, 2011). Taxes on firms may further lower these private returns, while social returns remain unaffected. This, in turn, widens the gap between actual and socially desired levels of R&D in an economy (Klenow and Rodriguez-Clare, 2005).

The remainder of the paper is structured as follows. Section 2 describes the institutional background of German profit taxation, documents the policy variation we exploit for identification, and briefly theoretically discusses how an increase in the (local) business tax rate may affect firms' R&D investments. Section 3 describes the plant-level dataset, as well as the matching of patent information and additional financial variables to the set of covered plants. In Section 4, we set up our empirical research design and test the plausibility of the design's underlying identifying assumptions. Section 5 presents the corresponding estimates on plant-level R&D expenditures and innovation output. Section 6 discusses the implications of our results for tax policy and regional economic growth. Section 7 concludes.

2 Profit Taxation and R&D Incentives

2.1 The German Local Business Tax

Business profits are taxed along two different margins in Germany. At the national level, profits are either subject to the corporate or personal income tax depending upon a firm's legal status. In addition, both corporate and non-corporate firms are subject to the local business tax (*Gewerbesteuer*), which is levied at the municipality level.[4] Our analysis will exploit within-municipality variation in local business tax rates for identification.

The local business tax (LBT) is the most important source of revenue for German municipalities The tax base is operating profits, with limited loss carryforward and no loss carryback. Importantly, and unlike the tax rate, rules for the tax base are defined at the national level and cannot be altered by state or municipal governments. The tax rate is derived as the product of the basic federal tax rate (*Steuermesszahl*) and a local scaling factor (*Hebesatz*), which acts as a municipality-specific multiplier:

$$\textit{Local Business Tax Rate } \tau = \textit{Basic Federal Tax Rate} \times \textit{Municipal Scaling Factor}.$$

This scaling factor serves as municipalities' margin of adjustment. At the end of each year, municipal councils autonomously decide whether and how to adjust the scaling factor for the upcoming year. In contrast, the basic federal tax rate is set by the national government and uniformly applies to all municipalities.[5]

Figure 1 illustrates the spatial and temporal variation in local tax rates across West Germany.[6] Panel A plots, as an example, the 1995 LBT rates for each municipality. We observe substantial differences across the country, with tax rates varying between zero and 45 percent (first percentile: 12.5%; 99th percentile: 22.5%). In addition, we see that tax rates are spatially correlated at the level of the federal states. This can be reconciled with varying fiscal equalization schemes across states, a feature we account for by including state $\times$ year fixed effects in the estimations (see Section 4.1 for details). Panel B highlights the variation in LBT rates within municipalities over time. On average, municipal councils decided to alter their local business tax rate three to four times between 1987 and 2013—the first and last year of tax data used in the analysis.[7] Thus, the average municipality changed its local business tax rate every eight years, which makes changes in the LBT rather rare events. Around six percent of all West German municipalities did not adjust their scaling factor during this time span at all.

Figure 2 provides additional information about the underlying variation in tax changes over time. Panel A shows that more than 90% of all tax changes during the covered period were tax increases; the majority of them being rather small. The average tax increase amounted to around one percentage point, or five percent relative to the mean. Notably, the distribution of tax changes is very similar

[4] Most firms from the agricultural sector, non-profit organizations as well as self-employed individuals in liberal professions (such as accountants, journalists or architects) are exempt from this tax.

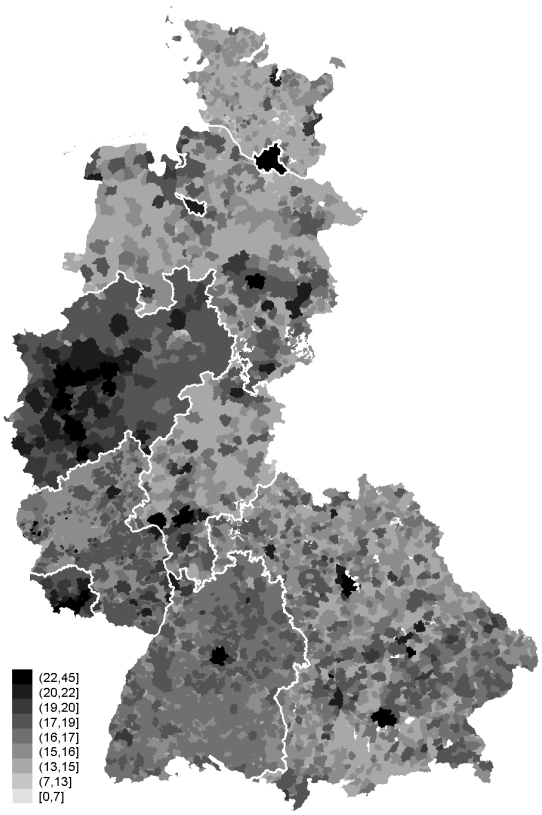
[5] The basic federal rate was 5.0% until 2007. It was reduced to 3.5% in the course of the 2008 German business tax reform.

[6] We focus on West Germany because many municipal borders were redrawn in East Germany during the 1990s and 2000s, which prevents the assignment of the exact LBT rate to affected firms. Note that East German plants account for less than five percent of the country's total R&D expenditures during the period under investigation.

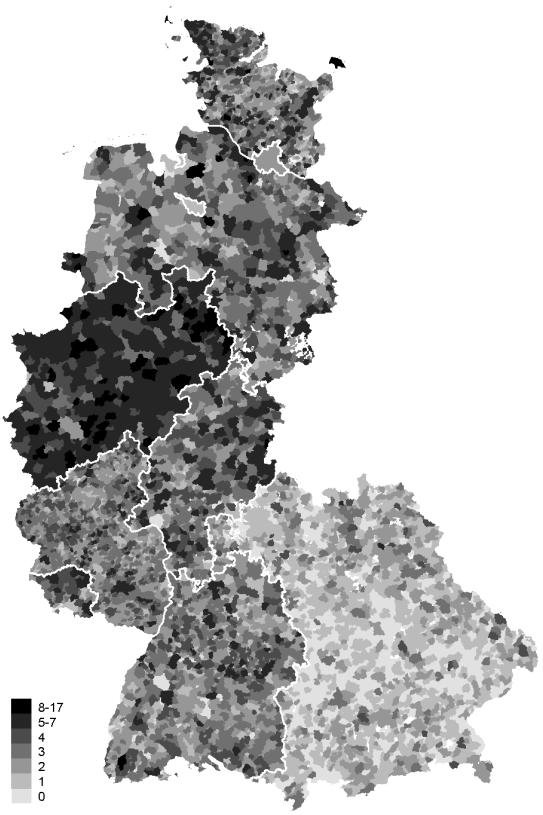
[7] We focus on outcomes from 1995 to 2007 and estimate a dynamic event study specification with a lag of eight and a lead of six years in our baseline specification (see Section 4.1 for details)

Figure 1: Spatial and Temporal Variation in Local Business Tax Rates

A. The Local Business Tax Rate in 1995 (in %)



B. Number of Tax Changes (1987–2013)



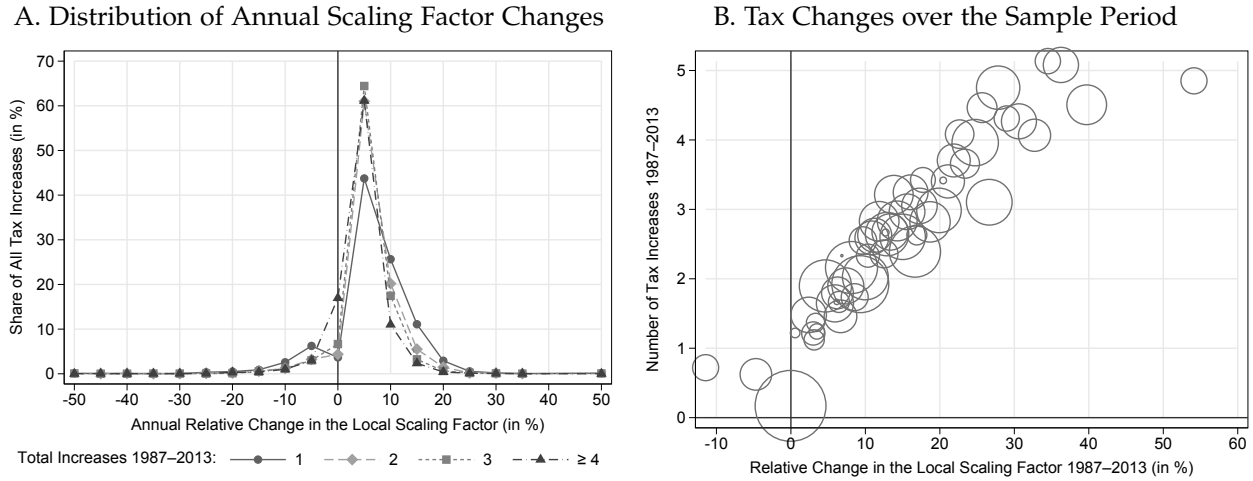
Notes: This figure illustrates the spatial and temporal variation in the local business tax rate across West German municipalities. In Panel A, the 1995 local business tax rate is plotted for each municipality. Darker colors indicate higher levels of the LBT. In Panel B, the number of total LBT changes over the period 1987–2013 is plotted for each municipality. Darker colors indicate a larger number of tax changes in a given municipality. Thick white lines indicate federal state borders. *Maps: © GeoBasis-DE / BKG 2015*.

irrespective of how often municipalities altered their LBT throughout the observation period in total. This implies that there is meaningful variation in long-run tax policies across municipalities, too. Panel B corroborates this argument. It shows that the long-term evolution of tax rates varied substantially across West German municipalities, and points to a positive link between the number of tax increases and the overall change in tax rates over the time period covered in the analysis.

The institutional features of the LBT allow us to base identification on a large number of very local tax changes while flexibly controlling for common shocks at the federal state and commuting zone level (see Section 4.1 for details on the empirical strategy pursued).[8] In addition, and in contrast to most other OECD countries, Germany offered no direct or indirect tax incentives for firms' R&D spending during the sample period (in fact until January 1, 2020). This makes the country an ideal laboratory for the research question of interest because no other tax policies need to be accounted for. Despite this institutional feature, Germany ranks among the world's most innovative countries (see, e.g., the annual Bloomberg Innovation Index). During the period from 1995 to 2007, the country's

[8] States and commuting zones are not perfectly nested.

Figure 2: Variation in LBT Scaling Factors – All West German Municipalities



Notes: This graph illustrates the variation in the local business tax rate changes across all West German municipalities. Panel A illustrates the distribution of annual scaling factor changes for municipalities with varying numbers of total tax changes throughout the effect window (1987–2013). Panel B illustrates the municipality-level relationship between the total change in the LBT rate and the number of tax changes throughout the period 1987–2013.

total R&D expenditures amounted to around 2.35% of its GDP on average, which is close to the U.S. level of 2.54% and much higher than the EU-28 average of 1.65%.[9]

2.2 Modeling Business Tax Incentives and R&D Investments

In this section, we set up a simple theoretical model of a plant to generate testable predictions about the impact of business taxation on firms' innovative activities and its underlying channels. To this end, we employ a two-period model of a plant that decides on the optimal level of R&D investments in the spirit of Chetty and Saez (2010). Our model refers to a single-plant firm in a given municipality. We suppress plant and location indices to simplify notation.

In period one, the plant decides on the level of investment I. The plant uses financial resources P to finance investments. We start by assuming that the plant collects cash by raising debt D, $P = D$. Investment decisions are made in period one, but yield a return of $R(I)$ in period two, where R is an increasing and concave function of initial R&D investments ($R' > 0,\ R'' < 0$). To simplify the model, we abstract from corporate risk-taking. The plant is subject to a profit tax in period two with a tax rate of τ. We abstract from taxation in the initial period and from policy uncertainty.

Investments generate costs in both periods. In period one, the plant has to pay per-unit investment cost c. Following Chen et al. (2021), the plant further faces convex adjustment costs $g(I)$, with $g' > 0$, $g'' > 0$, when altering the level of R&D investments. Under pure debt financing, the budget constraint in period one is thus given by $P = D = cI + g(I)$. In period two, debt D has to be repaid including

[9] Own calculations based on the OECD's Main Science and Technology Indicators database.

interest r^D. Abstracting from discounting, the plant's profits over the two periods are given by:

$$\pi = \underbrace{D - cI - g(I)}_{=\pi_1} + \underbrace{(1-\tau)(R(I) - r^D D) - D}_{=\pi_2} \tag{1}$$

Using the budget constraint from period one, we can rewrite Equation (1) to derive the plant's maximization problem:

$$\max_I \ (1-\tau)(R(I) - r^D D) - D$$
$$\text{s.t. } cI + g(I) = D.$$

The corresponding first-order condition balances marginal revenues and costs after taxes and implicitly defines the optimal level of R&D investments I^*:

$$(1-\tau)R'(I^*) = (1 + (1-\tau)r^D)(c + g'(I^*)) \qquad \text{if} \quad P = D. \tag{2}$$

Totally differentiating and rearranging terms yields the following comparative-static effect:

$$\left.\frac{\mathrm{d}I^*}{\mathrm{d}\tau}\right|_{P=D} = \frac{R'(I^*) - r^D(c + g'(I^*))}{(1-\tau)R''(I^*) - (1 + (1-\tau)r^D)g''(I^*)} < 0. \tag{3}$$

From Equation (3), we derive the following first hypothesis.

Hypothesis 1 (Investment Effect)**.** *An increase in the local business tax rate τ lowers the after-tax return on investments and leads to lower plant-level R&D investments I^*.*

Hypothesis 1 covers the main empirical test we conduct in this paper. Note that we abstract from corporate risk-taking in this simple model. Accounting for endogenous risk taking of plants would yield an additional negative effect on R&D expenses because investments in research and development are usually of high-risk nature and there is only limited loss offset in the German local business tax (Langenmayr and Lester, 2018).

Next, we investigate the role of adjustment costs. Verify that $\mathrm{d}I^*/\mathrm{d}\tau$ increases in $g''(I^*)$. In other words, the larger $g''(I^*)$, the weaker the negative effect of an increase of τ on investment. A simple example for such an adjustment cost function would be the quadratic function $g(I) = bx^2$ with scaling parameters $b > 0$; see, e.g., Chen et al. (2021) for a similar specification. For very large $g''(I^*)$ the investment effect converges to zero. Consequently, we derive our second hypothesis as follows.

Hypothesis 2 (Adjustment Costs Effect)**.** *An increase in the local business tax rate τ leads to relatively smaller reductions in plant-level R&D investments, the stronger the marginal adjustment costs increase in the level of I, i.e., for investments where $g''(I)$ is relatively high.*

Below, we will test this hypothesis by analyzing the effect of a tax increase on internal versus external R&D investments. Assuming the marginal returns for both types of investments to be identical in equilibrium, we expect that an increase in local business taxes leads to more pronounced reductions in internal than external R&D spending because of steeper marginal adjustment costs for

the latter investment type. By subcontracting R&D to external corporations or entrepreneurs, plants generally commit to longer-term partnerships and contractual arrangements, which should limit plants' ability to alter its marginal external R&D expenditures to a large extent in response to an increase in the local business tax.

Up to this point, we assumed that the plant finances its R&D expenses in period one by raising debt. In the context of R&D, credit constraints may be particularly relevant for plants. R&D investments are generally highly uncertain in their returns and come with substantial information asymmetries between the innovator and financial backers. Moreover, unfinished R&D projects have little residual value and often lack collateral (Hall, 2002, Brown et al., 2009, Hall and Lerner, 2010, Bakker, 2013)—factors that may make access to debt prohibitively expansive for some plants.

Hence, we also explore the effect of taxes on R&D investments when the plant relies upon equity financing, the alternative financing channel. Abstracting from adjustment costs, the maximization problem under pure equity financing ($P = E$) is given by:

$$\max_{I} \; (1-\tau)R(I) - (1+r^E)E \qquad (4)$$
$$\text{s.t. } cI = E.$$

The maximization problem reflects the institutional fact that the costs of equity financing cannot be deducted from the tax base, which is true for the German LBT as well as in many other corporate tax systems across the world. The corresponding first-order condition is given by:

$$(1-\tau)R'(I^*) = c(1+r^E) \qquad \text{if} \quad P = E. \qquad (5)$$

Totally differentiating and rearranging yields:

$$\left.\frac{\mathrm{d}I^*}{\mathrm{d}\tau}\right|_{P=E} = \frac{R'(I^*)}{(1-\tau)R''(I^*)} < 0. \qquad (6)$$

Recall from Equation (3) that the corresponding effect under debt financing is given by:

$$\left.\frac{\mathrm{d}I^*}{\mathrm{d}\tau}\right|_{P=D} = \frac{R'(I^*) - cr^D}{(1-\tau)R''(I^*)} < 0.$$

With $c > 0$ and $0 < r^D < 1$, it holds true that $\mathrm{d}I^*/\mathrm{d}\tau|_{P=E} < \mathrm{d}I^*/\mathrm{d}\tau|_{P=D} < 0$. Based on this result, we derive our final hypothesis.

Hypothesis 3 (Financing Effect)**.** *An increase in the local business tax rate τ leads to relatively stronger reductions in R&D investments for plants that have to finance marginal R&D projects to a larger extent via equity, for instance, when more binding credit constraints limit debt financing.*

Empirically, we test Hypothesis 3 by analyzing whether the negative effect of local business taxes on R&D investments is different for plants that are more credit constrained and therefore limited in their capacity to finance investment via debt.

3 Data

Plant-Level R&D Data. Our main data source is the biennial longitudinal dataset *Survey on Research and Development of the German Business Enterprise Sector* (henceforth: *R&D Survey*), collected and administrated by the *Stifterverband* on behalf of the German Federal Ministry of Education and Research. The survey targets all German plants engaged in R&D, and forms the key basis for the country's official reporting on its entrepreneurial R&D activities to EU authorities and the OECD.[10] The survey contains detailed information on plants' overall R&D spending, their R&D expenses by subcategories (internally- vs. externally-conducted R&D, personnel vs. non-personnel R&D spending) and their R&D staff. Moreover, it offers information on plant size, industry classification, and plants' organizational structure. Detailed information on each plant's location of residence further allows the exact assignment of the applicable LBT in each year.

Our estimation window spans the period from 1995, the earliest year of the survey, to 2007. We do not cover years beyond 2007 for two reasons. First, we bypass potential R&D effects due to the Great Recession in 2008–2009. Second, a major tax reform in 2008 altered institutional features of the LBT, lowering the basic federal tax rate from 5.0 to 3.5 percent and broadening the tax base. Besides this restriction, we constrain our baseline sample along two additional margins. First, we discard 649 plants (6% of the total sample) that report R&D activities not only for their own plant but for the entire firm (at different locations). By applying this restriction, we make sure to compare local changes in the LBT to responses of local plants only. Second, we drop 283 plants which moved during the survey period to exclude variation that is due to potentially endogenous mobility decisions.[11]

Ultimately, our baseline sample contains 31,648 plant-year observations in 2,442 municipalities. In total, these plants spent around 37 billion EUR per year on R&D, which accounts for three-quarters of Germany's total R&D expenditures during this period. In Panel A of Figure 3, we illustrate the spatial distribution of R&D-active plants across municipalities in 2007. We find R&D activity to be widespread across the country: around one-fourth of all municipalities have at least one R&D active plant. However, there are also regional clusters of R&D activity; in particular, in south-western Germany and along the rivers Rhine and Ruhr.

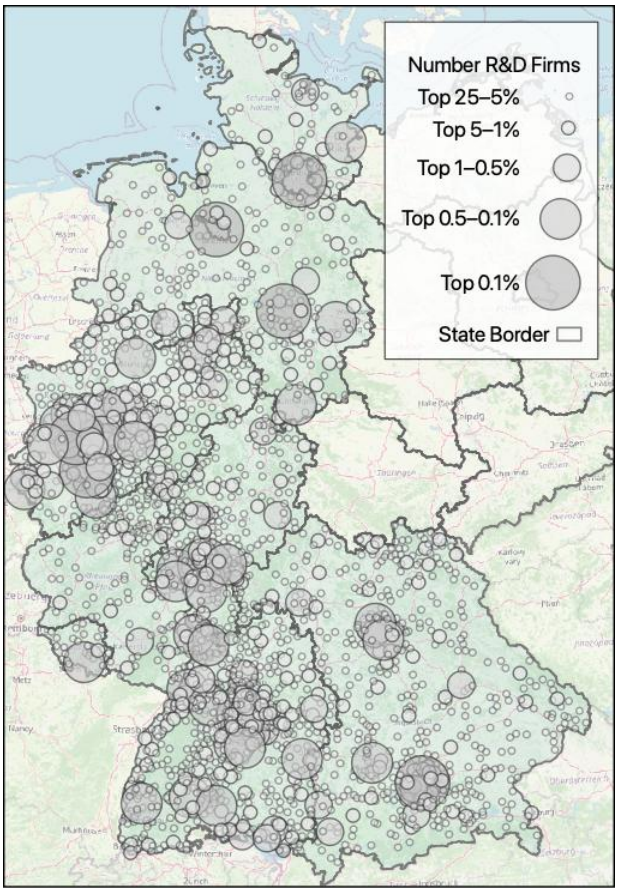
In the baseline estimation sample, plants' annual total spending on R&D varies from around EUR 46,000 (5th percentile) to around EUR 16 million (95th percentile); see Panel A of Appendix Table B.1 for detailed descriptive statistics. The R&D Survey further allows the disaggregation of plants' total R&D expenses along two margins. First, information on plants' expenses for internally- vs. externally-conducted R&D projects is given. External R&D is typically used as a strategy to acquire missing knowledge, either by engaging in licensing and outsourcing or starting strategic alliances. Whereas outsourcing allows firms to exploit economies of specialization and scale, strategic cooperation generally aims at the development of new technological capabilities (Bönte, 2003, Lokshin

[10] The survey also acts as one source of the OECD's Analytical Business Enterprise Research and Development database (*ANBERD*), which has been used in related research (see, e.g., Bloom et al., 2002). See Appendix A for more detailed information on this dataset.

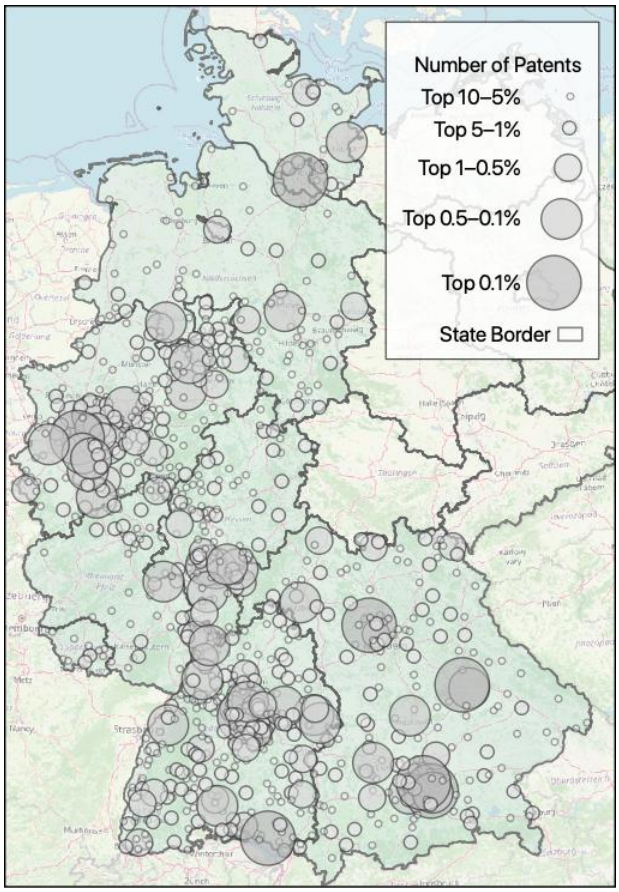
[11] We find very similar effects when including these 283 plants and assigning them the corresponding tax rates prevalent in their first observed municipality of residence (see below). Moreover, we find no evidence for selective location choice with respect to the LBT: almost half of those plants that change their location of residence actually relocate to municipalities with higher local business tax rates.

Figure 3: Spatial Distribution of R&D Firms and Patenting in West Germany

A. Distribution of R&D Plants



B. Distribution of Patents



Notes: Panel A illustrates the distribution of plants in the R&D Survey as of 2007 across West German municipalities. Larger circles indicate more R&D active plants in a given municipality. Panel B plots the spatial distribution of patenting across West Germany. Larger circles indicate that more patents were filed in a given municipality throughout our observation period from 1995–2007. Thick gray lines indicate federal state borders. *Maps: © GeoBasis-DE / BKG 2015* and *OpenStreetMap* contributors.

et al., 2008). However, the search for and coordination of external contractors and collaborations also comes with sizable (transaction) costs that may prevent some firms from engaging in external R&D activities (Berchicci, 2013). In our baseline sample, we observe that half of the covered plants outsource parts of their R&D activity at least once during the sampling period (see Appendix Table B.1). On average, external R&D accounts for around 9% of plants' total R&D expenditures, and 20% if we consider plants with non-zero external spending only. In addition, we are also able to distinguish internal R&D spending on personnel from non-personnel expenses (i.e., for materials and larger investments). On average, two-thirds of plants' internal expenses accrue to its scientific staff, who account for around 7% of plants' total workforce.

Plant-Level Patent Data. To measure innovation output, we link administrative information from the European Patent Office (EPO) on plants' patenting activities to the R&D Survey data (see Appendix A for a detailed description of the matching procedure). Between 1995 and 2007, the surveyed plants filed 151,862 patents, which accounts for around 60% of all patents filed by German

applicants at the EPO during this period.[12] Panel B of Figure 3 shows the spatial distribution of patent activity. Overall, the pattern is in line with the regional prevalence of R&D plants. One quarter of all plants in the R&D Survey filed at least one patent during our sampling period.

The simple count of plants' number of filed patents may only imperfectly capture the true value of innovation output if patent quality varies (Scherer, 1965, Hall et al., 2005). To capture varying patent quality, we construct a second measure of plant-level innovation that weights each patent by the number of citations it receives from patents filed at the United States Patent and Trademark Office (USPTO) within five years of its first registration. Previous evidence has shown that such citation-weighted measures of patent counts correlate well with firms' private returns to innovation (e.g., Harhoff et al., 2003, Kogan et al., 2017, Moser et al., 2018).

Following Danzer et al. (2020), we further use detailed textual information from the patent application files to distinguish product from process innovations. Product innovations generally relate to new or substantially-altered products that may lead to high social returns. However, these innovations can be easily appropriated by rivaling firms and face high market uncertainty, which renders private returns uncertain (Hellmann and Perotti, 2011). In contrast, process innovations, i.e., improvements of a given production process, are commonly considered as the more incremental ones that yield lower social returns but also bear lower risk (Klepper, 1996). We will test below whether an increase in tax rates affects both types of innovations differently.

Panel B of Appendix Table B.1 provides the corresponding descriptive statistics on plants' patenting activities. The average plant files 0.84 patents per year, which receives 1.7 citations over the following five years. Around 60% of all patents in our sample can be categorized as product innovations.

Financial Information. Whereas the R&D Survey offers detailed information on plants' R&D activities, little information is given on plants' financial situation. However, as hypothesized in Section 2.2, we expect credit-constrained plants to react stronger to a given tax increase because the costs of debt financing can be deducted from the tax base while those of equity financing cannot. To proxy plants' financial situation, we therefore add information from Bureau van Dijk's *Amadeus* and *Orbis* databases to the plant-level survey (in particular, information on plants' age and non-current liabilities). The two datasets offer a variety of financial information at the firm level, i.e., we assign the firm-level financial information to plants that are part of a multi-plant firm. As these datasets predominantly cover larger and oftentimes stock-listed plants, we can only supplement around 40% of the surveyed R&D plants with additional information from this data source.

Administrative Regional Data. Last, we complement the plant-level data with annual data on local business tax rates as well as other regional, i.e., municipality- and county-level information. This includes data on municipalities' annual public expenditures, population figures, unemployment rate, and county-level GDP. We will use these variables to test whether local business cycles simultaneously determine municipalities' tax setting and plant activities. Panel C of Appendix Table B.1 provides

[12] By definition, we do not capture patents filed by the government, public universities, or individual inventors. Moreover, not all plants that file a patent during the observation period are covered in the R&D Survey and our baseline sample, respectively. This is especially true for plants with very little or infrequent patent activity.

the corresponding descriptive statistics. We see that innovation predominantly occurs in urban, industrialized regions with relatively little unemployment.

4 Empirical Strategy

To estimate the causal effect of changes in the LBT on plant-level R&D expenses and innovation, we exploit all available changes in the tax rate within a municipality over time in a dynamic generalized difference-in-differences framework with staggered treatment (Suárez Serrato and Zidar, 2016, Fuest et al., 2018, Akcigit et al., 2021). In Section 4.1, we describe the empirical implementation. We discuss the identification of causal effects in our model in Section 4.2.

4.1 Event Study Design

We base our analysis on an event study setup that treats each tax change as an independent event. This allows us to exploit all available variation in local tax rates within municipalities over time. More precisely, we regress a given outcome Y_{it} of plant i in year t belonging to sector s (manufacturing, services, and other) located in a municipality m and commuting zone z on leads and lags of the treatment variable T^k_{mt}. Treatment is either defined as an indicator for a tax change, or a tax change dummy interacted with the size of the change (see below for more details). The corresponding regression model is given by:

$$Y_{it} = \sum_k \beta_k T^k_{mt} + \mu_i + \theta_{zt} + \zeta_{st} + \varepsilon_{it}. \tag{7}$$

We transform outcomes—R&D spending, the number of patents, and various subcategories of the two—using the inverse hyperbolic sine (IHS) transformation.[13] Plant fixed effects (μ_i) account for unobserved time-invariant confounders at the plant level.[14] Moreover, state $\times$ year and commuting zone $\times$ year fixed effects, both included in term θ_{zt}, as well sector $\times$ year fixed effects, ζ_{st}, control for regional and sectoral time-varying confounders, respectively. We calculate cluster-robust standard errors that account for potential correlations across plants, years, and sectors within municipalities.

We adjust this generic event study outline in three dimension to fit our empirical setting. First, we account for the biennial structure of the R&D Survey and base our analysis on the subset of odd years $t = 1995, 1997, \ldots, 2007$ to harmonize samples across outcomes. Leads and lags of the treatment variable, T^k_{mt}, thus sum tax changes in two consecutive years to account for tax reforms in even-numbered years as well. In our preferred specification, we restrict the effect window to six years before and eights year after a tax reform, i.e., three leads and four lags in the given two-year structure of the data, $k \in [-6, -4, \ldots, 8]$. Moreover, we normalize the last pre-treatment coefficient, β_{-2}, to zero, i.e., all effects are relative to two years before treatment.

[13] For any outcome $\tilde{y}$, the inverse hyperbolic sine transformation is defined as: $y = \ln(\tilde{y} + \sqrt{\tilde{y}^2 + 1})$. This transformation comes with the advantage of being well-defined for zero values. This is particularly relevant for the plant-level patent outcomes in the context of this study. For larger values, the IHS transformation is almost identical to the canonical log transformation. We show below that the transformation of outcome variables does not drive the estimates.

[14] We exclude plants that move throughout the observation period in our baseline specification. When adding these plants in robustness checks, we add municpality fixed effects (λ_m) to the regression model.

Second, we account for multiple tax changes per municipality by binning the endpoints of the effect window (McCrary, 2007). Hence, the first lead of the treatment variable, T_{mt}^{-6}, and the last lag, T_{mt}^{8}, take into account all tax reforms that will happen six or more years into the future from period t onward or happened eight or more years in the past, respectively. The underlying assumption is that (pre)-treatment effects are constant beyond these endpoints (Schmidheiny and Siegloch, 2020). Formally, we define the leads and lags of the treatment variable in the following way:

$$T_{mt}^{k} = \begin{cases} \sum_{j=-\infty}^{-6} D_{m,t-j} & \text{if } k = -6 \\ D_{m,t-k} & \text{if } -6 < k < 8 \\ \sum_{j=8}^{\infty} D_{m,t-j} & \text{if } k = 8, \end{cases} \tag{8}$$

where $D_{m,t}$ is the actual tax reform indicator denoting treatment in year t.

Third, we use two alternative definitions of treatment. On the one hand, we use a simple dummy variable specification adjusted to our biennial data. Treatment is the sum of two dummy variables indicating a tax increase from $t-1$ to t and $t-2$ to $t-1$ (Equation (9a)). On the other hand, we take into account the full variation in the local business tax rate by scaling the dummy variable with the size of the tax change (Equation (9b)):

$$D_{m,t}^{inc} = \mathbb{1}(\tau_{mt} > \tau_{m,t-1}) + \mathbb{1}(\tau_{m,t-1} > \tau_{m,t-2}) \tag{9a}$$

$$D_{m,t}^{cha} = \mathbb{1}(\tau_{mt} > \tau_{m,t-1}) \cdot (\tau_{mt} - \tau_{m,t-1}) + \mathbb{1}(\tau_{m,t-1} > \tau_{m,t-2}) \cdot (\tau_{m,t-1} - \tau_{m,t-2}). \tag{9b}$$

Implied Elasticities. While event study estimates inform about dynamics of treatment effects, it is useful to derive one central take-away elasticity. Our baseline summary measure is the elasticity as implied by the estimates of the last lag in the event study regressions, $\widehat{\beta}_8$, which measures the long-run effect more than seven years after the tax reform. We compare these implied long-run elasticities to medium-run elasticities and simple difference-in-differences estimates in Section 5.

4.2 Identification

To estimate causal effects, we relate within-plant changes in R&D activities to changes in LBT rates while absorbing common, time-varying shocks to federal states, commuting zones, and economic sectors. To interpret estimates $\widehat{\beta}_k$ as causal effects, we have to assume that tax changes are not systematically correlated with (trends in) local factors within the same federal state, commuting zone, and economic sector that also affect plants' R&D expenses or innovative activities. Small and insignificant pre-treatment coefficients in the event study estimates would support this assumption, as most confounding effects that violate the identifying assumption would show up as diverging pre-trends. Similarly, if reverse causality was an issue—i.e., if local policy-makers would adjust LBT rates because of changes in plants' innovative activities—we should observe diverging trends in R&D investments *before* treatment. As shown in Section 5, pre-trends across the different event study specifications are indeed flat for the set of plant-level outcomes under investigation.

Another concern for identification are confounding shocks that coincide with the tax change, but have no visible effect before treatment. Whether such shocks are able to impede identification

depends on the geographical level at which they arise. Our preferred specification includes state-by-year fixed effects, which control for any change in state policies or varying electoral cycles. To control for shocks below the state level, we also account for time-varying economic or political shocks at the level of the 204 West German commuting zones (*Arbeitsmarktregionen*, henceforth CZ) in our baseline specification.[15] However, to test the sensitivity of our results with respect to the potential presence of regional shocks at varying geographical levels, we deviate from this baseline model below and replace the CZ-by-year fixed effects with coarser and finer regional controls, respectively. If systematic local shocks were to violate our identifying assumption, we would expect results to differ alongside these changes in the empirical specification. Precisely, we absorb common shocks at the level of the 28 administrative districts (*Regierungsbezirke*, NUTS II), the level of the 71 statistical planning regions (*Raumordnungsregionen*, ROR), or the 272 counties (*Kreise und kreisfreie Städte*) in alternative specifications, respectively. Appendix Figure B.2 illustrates these different jurisdictions for the federal state of Bavaria. We find hardly any difference in pre-treatment effects when moving between specifications. Post treatment, treatment effects become somewhat larger (in absolute terms) when controlling for shocks at finer regional levels.

We also take these findings as evidence in favor of the validity of the stable unit treatment value assumption (SUTVA) with respect to local innovation spillovers. Recent work by Matray (2021) shows that these spillovers are generally positive, which would suggest that we might underestimate the true effect of a tax increase on R&D activity. This potential bias should become larger the finer we control for regional trends because the respective control group becomes more and more restricted to municipalities in close proximity to the treated one. The fact that our estimates become even larger when including finer regional time controls suggests that the SUTVA is not violated.

Finally, economic or political shocks may also occur at the municipality level and, potentially, coincide with the timing and size of the tax change. In this regard, one might particularly worry that local economic developments at the municipal level simultaneously determine municipal tax setting and plant activities. We address this concern twofold. First, we show that socioeconomic indicators at the municipality level (population, the share of unemployed among the population, public expenditures, and public revenues) do not display any systematic pre- or post-trend when used as dependent variables in the event study model as set up in Equation (7); a finding in line with previous studies (Fuest et al., 2018, Blesse et al., 2019).[16] Second, we sacrifice some econometric rigor and include lagged socioeconomic indicators in our baseline event study model. While these additional variables may constitute "bad controls", estimated effect patterns remain unaffected.

Heterogeneous Treatment Effects. Recent contributions by Sun and Abraham (2020), Callaway and Sant'Anna (2020), de Chaisemartin and D'Haultfœuille (2020), and Borusyak et al. (2021) have shown that standard two-way fixed effects models with staggered treatment may deliver biased estimates in case of treatment effect heterogeneity across cohorts, i.e., in case the impact of a given treatment varies with the year of its implementation. In our setting with multiple treatments per unit over time, we address this concern by restricting the sample to those municipalities that experienced either no

[15] On average, there are eight municipalities and 25 plants per commuting zone in our baseline sample.

[16] In Section 6.2, we also investigate the effect of LBT increases on local GDP. Besides establishing flat pre-trends, we show that local GDP declines in response to a tax rate increase (see Panel A of Figure 8).

or just one tax increase throughout the observation period. We show that the standard event study estimates for this sub-sample are close to the ones from our baseline sample. We then apply the estimators proposed by Sun and Abraham (2020) and Borusyak et al. (2021) to the sub-sample and show that our results remain unchanged when explicitly accounting for the potential presence of hetereogeneous treatment effects across cohorts (see Section 5.1 for detailed results).

5 Empirical Results

We next present our empirical results. We first investigate whether an increase in the local business tax rate reduces plants' total R&D spending (*Hypothesis 1*)[17] and extensively test the robustness of our key effect of interest. We then analyze whether the tax-induced reductions are indeed particularly strong for R&D investment types with flat marginal adjustments (*Hypothesis 2*) and for credit-constrained plants (*Hypothesis 3*). Last, we investigate the corresponding effects of an increase in the LBT on innovation output as measured by the (citation-weighted) number of filed patents.

5.1 Effects on Overall R&D Spending

Figure 4 presents the estimated dynamic effect of an increase in the local business tax rate on plants' total R&D spending from the two different event study models as defined in Equations (7)–(9b). First, we notice that pre-trends are relatively flat and statistically insignificant. In line with *Hypothesis 1*, we further detect that an increase in the LBT rate exerts a substantially negative and statistically significant effect on plants' total R&D spending post treatment. The effect builds up over the first two years and levels off thereafter. Effects are similar when using the simple tax increase dummy variable or the actual size of the tax rate increase as treatment.[18] The binary event study model as given by Equation (9a) will serve as our baseline specification throughout the rest of the paper.

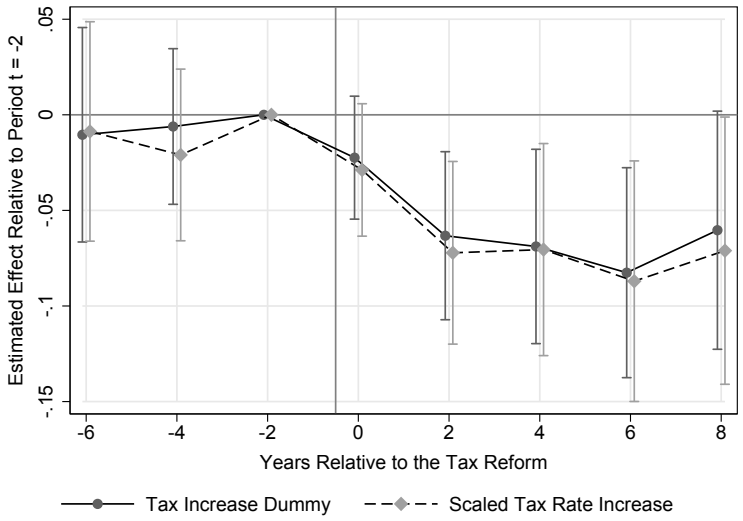
In terms of magnitudes, our long-run estimate ($\widehat{\beta}_8$) implies that a one percentage point increase in the local business tax leads to a decrease in R&D spending of 6–7% after eight years. This semi-elasticity translates into a long-run elasticity of around -1.25. In Appendix Figure C.2, we report alternative summary estimates of the effect size and their respective confidence intervals using different lags of the classic event study model or when estimating a simple difference-in-differences model with the same set of fixed effects.

We conduct several sensitivity checks to test whether our baseline estimates are robust to varying specifications of the event study model.

Time-Varying Confounders. Departing from our baseline model with state-by-year and commuting zone-by-year fixed effects, we first re-estimate the event study model with less or more fine-grained region-by-year fixed effects (at the NUTS-II, ROR, or county level). Changes in the event study coefficients are informative about the importance of local shocks as a potential source of bias. Panel A of Appendix Figure C.3 shows that effects are statistically significant at conventional levels

[17] As shown in Appendix Figure C.1, we find no corresponding positive effect of a tax decrease on R&D spending. However, in light of the relatively few tax decreases in our sample, we shy away from emphasizing this particular result.
[18] The average tax rate increase is almost equal to one percentage point (cf. Section 3).

Figure 4: The Effect of a Business Tax Increase on Total R&D Spending



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study models as defined in equations (7)–(9b). The dependent variable is a plant's annual (inverse hyperbolic sine transformed) total R&D spending. For the treatment group, the business tax change occurred in year $t = 0$ or $t = -1$. The regressions include plant, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are clustered at the municipal level.

irrespective of the type of region-by-year fixed effects, but become slightly larger when controlling for local shocks at a finer geographical level—suggesting that local shocks bias coefficients towards zero. These results also address concerns about a potential violation of stable unit treatment value assumption (SUTVA) due to possible positive R&D spillovers across space (Matray, 2021).

We further explicitly test for the impact of time-varying confounders at the municipality or county level. Sacrificing some econometric rigor, we include time-lagged variables on municipalities' annual level of population, counties' unemployment rate, and county-level GDP per capita as controls in the event study model. Point estimates remain literately unaffected by the inclusion of these observable confounders (see Panel B of Appendix Figure C.3).[19] The same is true when using different transformations of the outcome variable, see Panel C of the corresponding figure. Estimates also remain statistically significant when allowing for correlation in the error term at broader regional levels than the municipality (see Panel D of Appendix Figure C.3). Last, we show that the use of different event window specifications does not render the observed effect pattern (see Panel E). Pre-trends remain flat when extending the number of leads. Post treatment, estimated effects on R&D spending level off around four years after treatment irrespective of the number of lags.

Heterogeneous Treatment Effects. We further test whether our baseline results remain unchanged when explicitly accounting for the potential presence of heterogeneous treatment effects across cohorts (see, e.g., Sun and Abraham, 2020, Callaway and Sant'Anna, 2020, de Chaisemartin and D'Haultfœuille, 2020, Borusyak et al., 2021). Our baseline estimation strategy may deliver biased

[19] In addition, we follow Oster (2019) and more explicitly assess potential biases due to unobserved time-varying confounders by systematically assessing coefficient stability in specifications with and without local lagged controls. In Appendix Figure C.2, we plot the bias-corrected long-term elasticity, which is close to our baseline estimate.

estimates in case the dynamic impact of a given tax increase varies with the year of its implementation. While the given contributions offer alternative estimators to derive unbiased coefficients, none is fully applicable to settings with multiple treatment effects for the same unit over time. To this end, we limit our sample to those municipalities that experienced either no or just one tax increase throughout a given effect window. In light of the fact that many municipalities increased their local business tax rate more than once throughout our baseline window (using tax rates 1986–2013) and in order to arrive at a sufficiently large estimation sample, we yet shorten the effect window, restricting the sample to plants in municipalities that saw no or just one tax increase between 1990 and 2011. Overall, this leaves us with roughly 16% of the baseline estimation sample.[20] Appendix Figure C.4 presents the corresponding effects. Two key findings become evident. First, we see that our baseline event study design yields very similar effects when limiting the sample to municipalities with either zero or one tax increase within the given effect window. Second, the two different estimators by Sun and Abraham (2020) and Borusyak et al. (2021) deliver very similar results compared to our standard event study model. We conclude that baseline results from Figure 4 are unlikely driven by the presence of heterogeneous treatment effects across cohorts.

Single vs. Multi-Plant Firms. We also test for heterogeneous responses by single- versus multi-plant firms. If the tax-induced reductions in total R&D spending as implied by Figure 4 were only due to changes in the R&D spending behavior of multi-plant firms, estimated effects might be due to the spatial reallocation of R&D activities across plants within a given firm and not necessarily point to an absolute reduction in innovative activity due to higher taxation. As displayed in Appendix Figure C.5, we yet find that both multi-plant and single-plant firms similarly reduce their R&D spending in response to an increase in the local business tax rate.[21] Hence, higher taxation leads to a real reduction in R&D spending.

Firm Mobility. Last, we can assess to what extent firm mobility drives our effects. Given that moving costs of R&D-intensive plants should be quite substantial, it appears unlikely that changes in local business taxes induce substantial relocation responses of plants. One plausible margin of adjustment is to relocate the R&D unit of one plant to another plant within the same firm. As shown above this does not seem to drive results. Nevertheless, marginal single-plant firms may respond to local tax incentives at the extensive margin, too. In our sample, we observe 283 plants that change their municipality of residence during the observation period. We exclude those in the baseline model. However, as shown in Appendix Figure C.6, we find almost identical effects when including these plants and assigning them the corresponding tax rates of the first observed municipality (intent-to-treat). Overall, it seems that mobility responses are not very relevant in the context of the German local business tax—a finding consistent with Fuest et al. (2018).

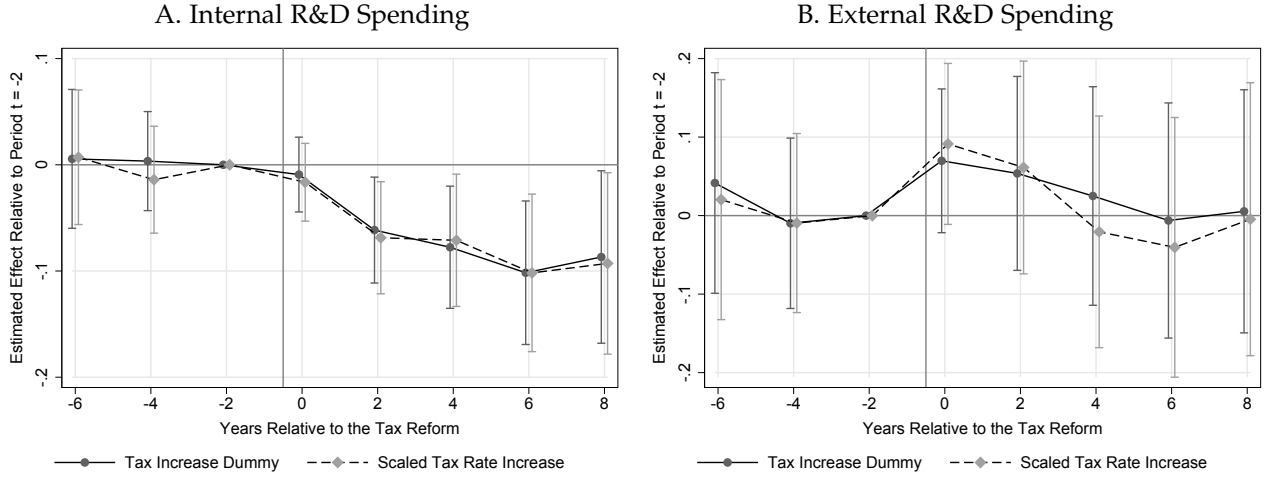
[20] In detail, we base estimation on around 1,700 plants located in 542 municipalities in this exercise. We therefore choose to control for time-varying local shocks at a coarser level than in our baseline, the level of the NUTS-II regions.

[21] Standard errors are considerably larger for multi-plant firms because they account for less than one-third of our estimation sample only. Unfortunately, the data does not allow identifying those plants that belong to the same firm. This deters us from investigating whether multi-plant firms indeed shift R&D activity across locations.

5.2 Heterogeneous Effects

Effects by R&D Expenditure Types. In correspondence with *Hypothesis 2*, we expect tax-induced reductions to be stronger for R&D investments types with flat marginal adjustment costs. We investigate this hypothesis by separately estimating the effect of a tax increase on plants' internal versus external R&D expenses. By subcontracting R&D to external corporations or entrepreneurs, plants generally commit to longer-term partnerships and contractual arrangements, which should in turn limit plants' ability to reduce their marginal external R&D expenditures in response to an increase in the LBT. Figure 5 presents the corresponding event study estimates for both expenditure types. We first note that pre-trends become even flatter when dis-aggregating plants' total R&D expenditures along this margin. Moreover, and in line with expectations, we find the effect on total R&D spending to be entirely driven by reductions in plants' internal R&D expenditures (Panel A). Spending on external R&D remains unchanged in response to an LBT increase (Panel B). The corresponding long-term elasticity for plants' internal R&D spending, based on the last coefficient ($\widehat{\beta}_8$) of the classic tax increase event study model, amounts to -1.77. While this finding is in support of our hypothesis, it appears to be at odds with recent evidence by Agrawal et al. (2020). Analyzing the effects of targeted R&D tax incentives for a subset of Canadian firms, they find stronger responses on externally-conducted R&D. However, the firms under investigation in their paper are much smaller than ours—such that marginal internal adjustment costs may be steeper in their case.

Figure 5: The Effect of a Business Tax Increase on Internal and External R&D Spending



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study models as defined in equations (7)–(9b). The dependent variable is plants' annual internal R&D spending in Panel A, and plants' annual external R&D spending in Panel B. Both outcomes are inverse hyperbolic sine transformed. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are clustered at the municipal level.

We further test for heterogeneous effects on plants' *internal* expenditures on R&D personnel and non-personnel R&D expenses. As displayed in Appendix Figure C.7, we find quite similar effects for both outcomes. This suggests that the two production factors are complements; a finding in line with recent evidence by Curtis et al. (2021). In Appendix Figure C.8, we show that the reduction

in spending on R&D personnel can be (partly) explained by a reduction in R&D headcount: plants lower the stock of employees that conduct R&D in response to a tax increase; the respective long-term elasticity amounting to -0.94.

Effects by Plant Characteristics. Next, we investigate whether particular types of plants react differently to an increase in the local business tax rate. To this end, we focus on plants' total R&D spending as outcome variable of interest and use the binary specification of the event study design.

First, we test whether tax-induced reductions in R&D investments are indeed larger among credit-constrained than non-constrained plants (*Hypothesis 3*). Absent detailed financial information in the R&D Survey, we use two different variables to proxy plants' financial situation: age and the non-current liabilities to sales ratio. Firm age has been used as a proxy for firms' financial situation in many related studies (see, e.g., Guceri and Liu, 2019, Moon, 2020). Younger firms are generally assumed to have less collateral and to face larger information asymmetries with potential lenders, which should in turn increase the cost of debt financing. Likewise, plants with greater non-current liabilities relative to their sales should face higher interest rates and, correspondingly, finance a smaller share of their R&D expenditures via debt.[22]

Panels A and B of Figure 6 present the corresponding effects.[23] We find that the overall negative effect of a tax increase on R&D spending appears to be primarily driven by financially-constrained plants. While overlapping confidence intervals warrant caution when interpreting the visual effect differences between financially-constrained and unconstrained plants, we take the effect pattern as suggestive evidence in line with our hypothesis. The results are also in line with earlier studies by, e.g., Zwick and Mahon (2017), Guceri and Liu (2019), and Moon (2020), who detect particularly strong (R&D) investment responses to tax incentives by financially-constrained firms.

Second, we test for effect heterogeneity across the plant size distribution. Many countries offer more generous targeted R&D tax incentives (such as tax credits or deduction possibilities) to smaller plants, partly because of policy makers' beliefs that SME's have less access to debt and should (hence) benefit more from R&D tax incentives than larger plants (Gonzales-Cabral et al., 2018). In Panels C and D of Figure 6, we test this hypothesis by allowing for heterogeneous effects by plants' sales volume and stock of employees, respectively. We find very similar effects for smaller and larger plants, irrespective of the exact definition of size. Pre-trends are flat and statistically insignificant. Post treatment, smaller and larger plants reduce their R&D spending by equal amounts.

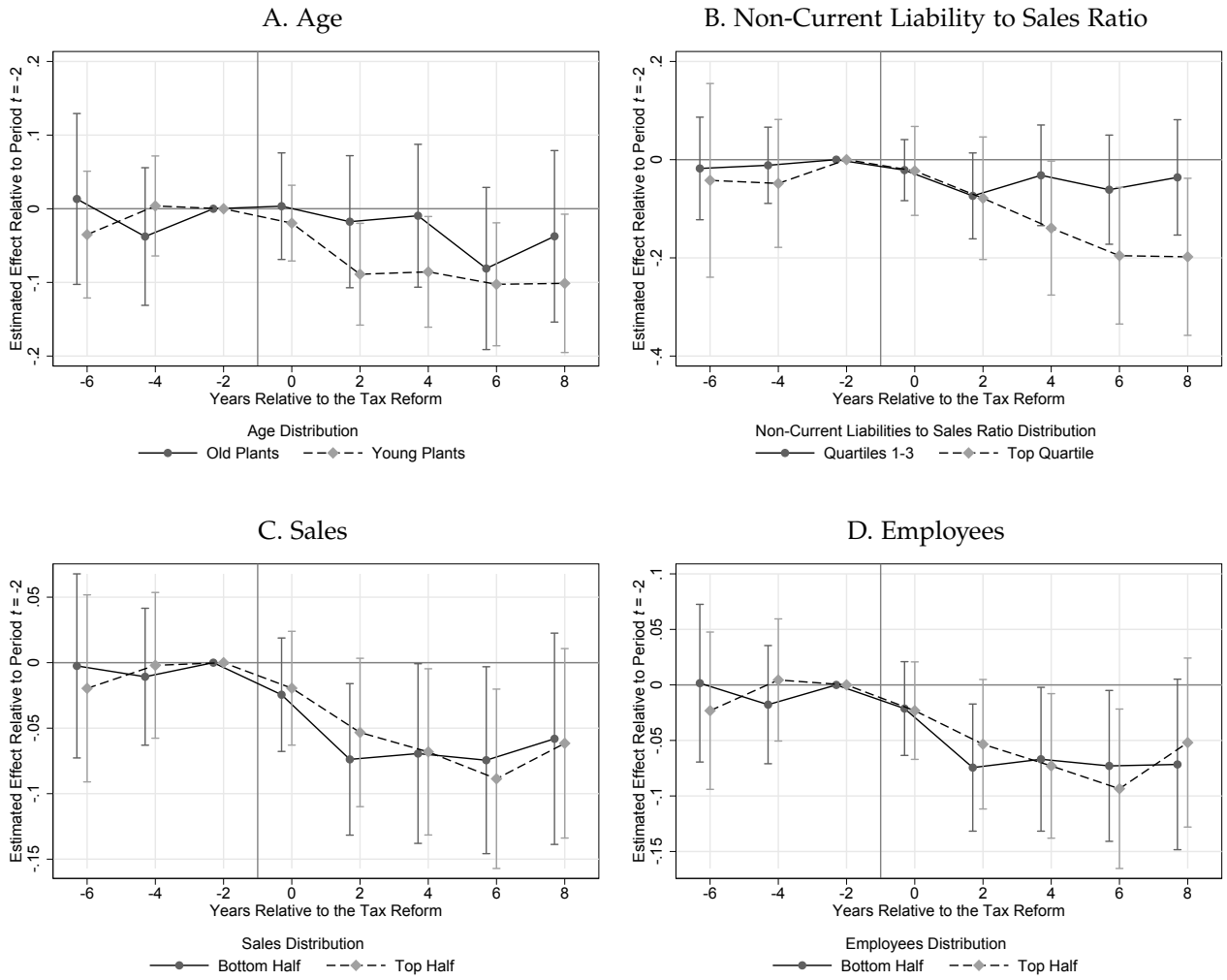
5.3 Effects on Innovation

Last, we investigate whether the tax-induced reductions in R&D spending result in less innovation as measured by plants' (citation-weighted number) of filed patents. Panel A of Figure 7 displays the corresponding event study estimates when looking at the raw patent count. We first note that pre-trends are flat and statistically insignificant. Post treatment, we detect a negative and statistically

[22] Information on both variables are taken from the Bureau van Dijk's *Amadeus* datasets (see Section 3 for details). As the dataset primarily covers larger plants, our analysis is limited to a relatively small subset of the baseline estimation sample. In Appendix Figure C.9, we show that baseline effects remain unaffected when using the smaller samples.

[23] We construct two groups (using each plant's first observed value) and interact these with all leads and lags of the treatment variable. We also include group $\times$ year fixed effects to allow for flexible differential trends across groups.

Figure 6: LBT Increases and Total R&D Spending – Heterogeneous Effects by Size and Liquidity

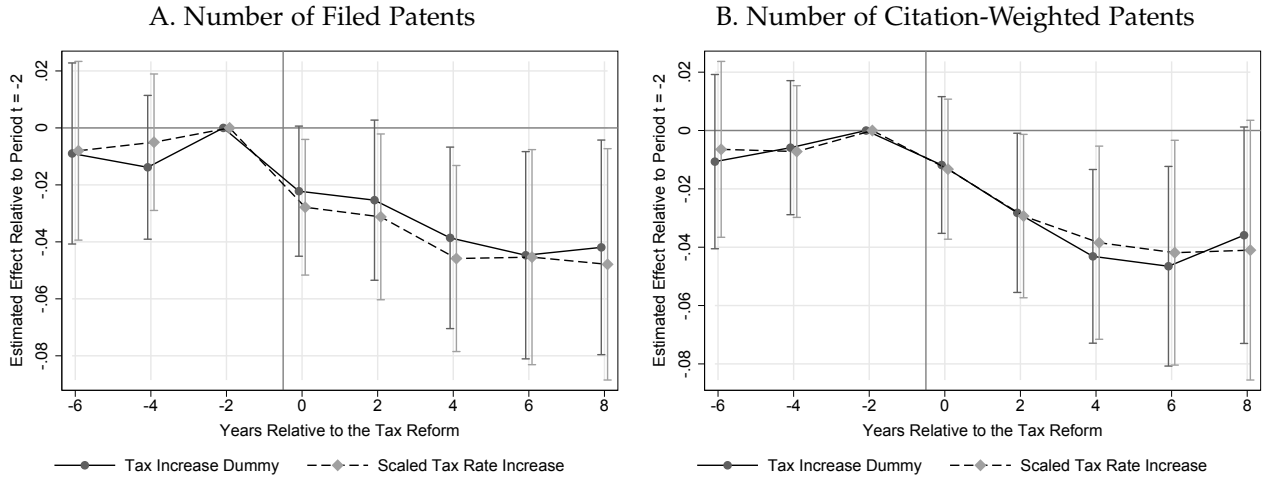


Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study models as defined in equations (7)–(9a) when allowing for heterogeneous treatment effects by plant characteristics. In Panels A and B, we test for differential effects among plants with and without credit constraints. In Panel A, we follow the literature and proxy credit constraints by plant age. In Panel B, we use plants' non-current liability to sales ratio as a proxy. In Panels C and D, we allow for different responses along the plant size distribution in terms of sales and employment, respectively. The regressions include plant, state $\times$ year, commuting zone $\times$ year, sector $\times$ year, as well as group $\times$ year fixed effects. We exclude all municipalities that experience a tax decrease during the observation period. Standard errors are clustered at the municipal level.

significant effect that materializes around four years after the tax increase. The implied long-run elasticity is -0.87, an estimate close to the ones reported in Akcigit et al. (2021).

In Panel B, we plot the corresponding effects on the citation-weighted number of filed patents. This outcome accounts for the fact that the economic value of patents varies substantially (Scherer, 1965, Hall et al., 2005), such that the simple count may not measure the true value of innovation output in a correct way. Moreover, if plants only abandon marginal R&D projects in response to a tax increase, we might see a reduction in the plants' quantity of patents but no effect on innovation quality. However, we detect very similar effects when looking at the citation-adjusted number of filed patents; the long-term elasticity amounting to -0.94.

Figure 7: The Effect of a Business Tax Increase on Patents



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study models as defined in equations (7)–(9b). In Panel A, the dependent variable is the plant-level annual number of filed patents. In Panel B, the dependent variable is the number of citation-weighted patents. Both outcomes are inverse hyperbolic sine transformed. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are clustered at the municipal level.

In Appendix Figure C.10, we show that estimates remain unaffected when using different transformations of the outcome, i.e., when using a $\ln(Y + 1)$-transformation or specifying the outcome in levels. We further show that estimated effects on the citation-weighted number of patent applications are very similar for single- and multi-plant firms (see Appendix Figure C.11). We consider this result as particularly relevant in light of previous work by, e.g., Dischinger and Riedel (2011), Karkinsky and Riedel (2012), and Griffith et al. (2014), who show that multi-national firms respond to changes in corporate taxation by transferring intangible assets, such as patents, to affiliates in low-tax countries to reduce their overall tax burden. While estimated effects for multi-plant firms may indeed be driven by this tax avoidance strategy, the estimated effects for single-plant firms show that an increase in the business tax rate leads to a real reduction in plants' overall innovation output, not just the spatial re-location of intellectual property.

Finally, we use detailed information from each patent's claims text to distinguish effects on product versus process innovations. While R&D on new products is associated with substantial risks that may in turn open up or revolutionize a market, process innovations are generally considered as the more incremental ones with limited social returns (Klepper, 1996). As displayed in Appendix Figure C.12, we find no heterogeneous responses along this margin. Effects on process innovations materialize a bit earlier, but overall responses are very similar in quantitative terms.

6 Implications for Tax Policy and Growth

We now take the reduced-form evidence from Section 5 to carefully discuss implications for tax policy instruments intended to incentivize R&D investments and stimulate economic growth.

6.1 Targeted vs. Non-Targeted R&D Tax Policies

We start by assessing the role of general business taxes as an instrument to spur firm-level R&D investments. Recall that we find a long-run elasticity of R&D spending with respect to the business tax rate of -1.25. To better compare the magnitude of this elasticity with estimates for other types of R&D tax policies, we translate our estimate into an elasticity with respect to the user cost of capital (ε_{CoC}^{Inv}). Following Yagan (2015), we calculate this elasticity as: $\varepsilon_{CoC}^{Inv} = \varepsilon_{\tau}^{Inv} / \varepsilon_{\tau}^{CoC}$, where the numerator ε_{τ}^{Inv} refers to the spending elasticity with respect to the business tax rate. The denominator ε_{τ}^{CoC} is defined as the elasticity of the cost of capital with respect to the tax rate. In the German setting, the user cost of capital are given by $CoC = r/(1-\tau)$. We assume a pre-tax rate of return $r = 0.07$ and set the total profit tax rate to $\tau = 0.32$, using the average local business tax and the federal corporate tax rate during the period under investigation. It follows that $\varepsilon_{\tau}^{CoC} = 0.47$. The implied elasticity with respect to the user cost of capital thus amounts to around -2.66.

This estimate is well in line with elasticities from recent other studies investigating the effects of targeted R&D tax policy instruments such as tax credits, special deduction possibilities or subsidies. Guceri and Liu (2019) and Dechezleprêtre et al. (2016) report user cost elasticities ranging from -1.59 to -3.0 for a UK reform that eased R&D deduction possibilities for medium-sized firms. Agrawal et al. (2020) estimate a user-cost elasticity in the range between -0.71 to -4.57 for Canadian firms that got access to a tax credit. Chen et al. (2021) analyze the impact of tax cuts for R&D intensive firms in China and report a user cost elasticity of -2.0; although they show that more than one-third of the overall response is due to the relabeling of general to R&D expenditures.[24] Overall, we therefore conclude that firm-level R&D expenditures seem to respond strongly and relatively homogeneously to different tax policies.

In a second step, we assess the cost effectiveness of general business taxes as a tool to boost R&D investments. To this end, we first add up the reported total R&D expenses of all surveyed plants in 2007, the last year of our observation period. Using our derived elasticity of plants' R&D expenditures with regard to the local business tax rate, we then simulate the change in total local R&D expenses in response to a tax increase and scale it by the mechanical increase in local tax revenues. Our estimates imply that R&D expenses decrease by around 40,000 EUR per R&D-active plant for a one percentage point increase in the local business tax rate. Assuming symmetry, this implies that governments can raise R&D investment by 0.34 EUR when giving up 1 EUR of local business tax revenue.

This cost-effectiveness estimate is rather low compared to estimates for targeted R&D tax incentives as, for example, implemented in the UK. Dechezleprêtre et al. (2016) and Guceri and Liu (2019) estimate that the UK R&D tax relief scheme generated around 1.0–1.7 GBP of additional R&D spending for each GBP of lost tax revenue. The higher cost effectiveness of targeted R&D tax incentives seems reasonable as changes in general business taxes also affect non-R&D firms—in the German case only around four percent of all manufacturing plants report R&D activities, for example. Hence, we conclude that targeted R&D tax policy instruments are the preferable tool to promote plant-level innovation.

[24] Compared to estimates on *overall* (rather than R&D) investment, our user cost elasticities is rather high (see, e.g. Yagan, 2015, Zwick and Mahon, 2017, Ohrn, 2019, Maffini et al., 2019, Chen et al., 2020, Moon, 2020).

The results of our analysis, however, question policy makers' common practice to link eligibility criteria for targeted R&D tax incentives to firm size. We find no notable heterogeneous effects across the plant size distribution, neither in terms of sales nor employees. This finding is in line with recent work by Curtis et al. (2021) who study the effect of accelerated depreciation rules on investments and do not find significantly different effects by firm size either. Galaasen and Irarrazabal (2021) show that size-based eligibility thresholds for R&D tax incentives may even cause unintended consequences: by promoting the R&D activities of small firms, size-dependent R&D tax incentives boost the expansion of relatively unproductive firms, which may mitigate firm selection and hinder aggregate economic growth. Thus, we conclude that the use of size-based R&D incentives for firms should be critically reassessed and replaced by more nuanced measures determining eligibility.

6.2 Stimulating Innovation and Growth

In a final step, we widen the scope of our analysis beyond the plant level and explore the broader economic consequences of tax-induced reductions in R&D spending and innovation. Among others, Lentz and Mortensen (2008) and Kogan et al. (2017) highlight the role of firm-level innovation for economic growth by raising average firm productivity. We revisit this mechanism in the context of our study, proceeding in three steps. First, we cautiously assess the impact of innovation on economic growth at the municipal level. In a second step, we estimate the *total* effect of an increase in the local business tax on local GDP, acknowledging that business taxes should affect economic performance through other margins than innovation—e.g., via lower wages (Fuest et al., 2018). Third, we combine the different estimates in a back-of-the-envelope calculation to simulate the tax-induced decline in local economic growth that is due to reduced plant-level innovation.

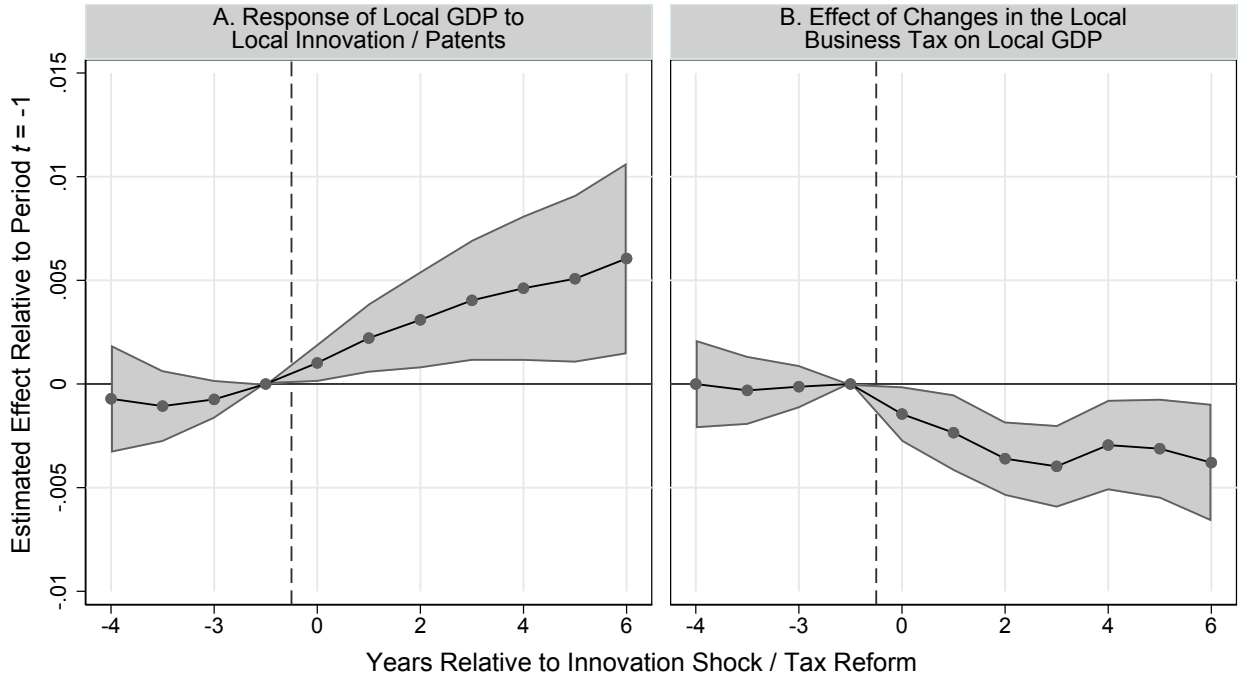
Local Innovation and Growth. To assess the importance of local innovation for economic growth, we adapt the empirical design of Kogan et al. (2017) to our setting and relate changes in local GDP to the citation-weighted number of patents at the municipal level. Specifically, we estimate the following distributed lag model:

$$\ln GDP_{mt} = \sum_{k=-3}^{6} \gamma_k \ln\left(1 + Patents_{m,t-k}\right) + \mu_m + \zeta_{rt} + \varepsilon_{mt}, \tag{10}$$

where $\ln GDP_{mt}$ refers to municipality m's (log) GDP in year t and $Patents_{m,t-k}$ to the citation-weighted number of patents filed in municipality m at time $t-k$. As both the outcome and the regressor of interest are observed on an annual basis, we specify the event time in years and restrict the event window from four years before to six years after a tax reform. Municipality and NUTS-II region $\times$ year fixed effects, denoted by μ_m and ζ_{rt}, account for time-(in)variant confounders.

Panel A of Figure 8 plots the corresponding estimates. The resulting pattern shows a lagged positive GDP response to a local innovation shock, with a flat pre-trend in economic growth in the years before. When interpreting the notable structural break at the point of innovation in the spirit of a Granger causality test, these estimates suggest that innovation precedes higher levels of local economic growth, while economic growth does not trigger local innovation. Quantitatively, a one percent increase in the local number of patents is associated with an increase in local GDP by 0.6%

Figure 8: Business Taxes, Local Innovation, and Economic Growth



Notes: This graph illustrates the relation between business taxes, local innovation, and economic growth. In Panel A, we plot the dynamic correlation between changes in municipalities' number of (citation-weighted) patents and local GDP based on equation (10). In Panel B, we plot the relationship between changes in the local business tax rate and GDP, see equation (11). Depicted effects are based on the cumulated distributed lag estimates. Exemplarily, we back out the effects in Panel A as $\widetilde{\gamma}_k = -\sum_{j=k+1}^{-1} \widehat{\gamma}_j$ for $k \in [-4, -2]$, $\widetilde{\gamma}_{-1} = 0$, and $\widetilde{\gamma}_k = \sum_{j=0}^{k} \widehat{\gamma}_j$ for $k \in [0, 6]$. Gray shaded areas indicate 95% confidence intervals. Standard errors are clustered at the municipal level. See Appendix A for details on all variables.

after eight years. Overall, this pattern at the municipality level is very much in line with evidence by Kogan et al. (2017), who find similar time-series evidence at the national level for the United States.

Local Business Taxes and Growth. In a second step, we estimate the total effect of changes in the local business tax rate on local GDP. Several contributions have analyzed the impact of corporate taxation on growth before, such as Gemmell et al. (2011) for OECD countries and Dahlby and Ferede (2012) for Canadian regions. We adapt this idea to the municipal level and relate local GDP to leads and lags of the local business tax rate τ_{mt}:

$$\ln GDP_{mt} = \sum_{k=-3}^{6} \delta_k \tau_{m,t-k} + \mu_m + \zeta_{rt} + \varepsilon_{mt}. \tag{11}$$

In correspondence with Equation (10), we again condition on municipality (μ_m) and NUTS-II region $\times$ year fixed effects (ζ_{rt}).

The corresponding relationship is summarized in Panel B of Figure 8. Overall, we find a pronounced negative association between business taxes and economic growth. The pattern builds up over time and levels off around two years after the tax increase. Quantitatively, our results imply that an increase in the LBT by one percentage point decreases local GDP by around 0.38% in the medium

to long run. Small and insignificant pre-trends support the causal interpretation of this pattern.

Innovation as a Mediator. Last, we perform a back-of-the-envelope calculation to trace out how much of the overall negative growth effect of local business taxes can be linked to tax-induced reductions in plant-level innovation. To this end, we combine the results shown in Panel A and B of Figure 8 with our patent elasticity from Section 5. Multiplying the GDP-innovation elasticity of 0.006 with the firm-level tax elasticity of patents (0.87) and the average relative tax change (5.3%), we calculate a long-run loss of 0.03% in GDP for each one percentage point increase in the local business tax. This tax-induced reduction in plants' innovation on local growth amounts to around eight percent of the total negative effect of an LBT increase on local GDP (see Figure 8, Panel B), falling in the range of estimates by Kogan et al. (2017). We take this as an additional piece of evidence for the important role of innovation for long-run growth (Solow, 1957, Romer, 1990, Jones and Summers, 2020).

7 Conclusions

In this paper, we study the effect of business taxes on plants' R&D spending and innovation. We exploit the institutional setting of local business taxation in Germany, where municipalities autonomously set their local business tax rate on an annual basis. Using event study techniques, we show that an increase in the local business tax rate has a statistically significant negative effect on plants' R&D expenditures. Quantitatively, we derive a long-term elasticity of R&D spending with respect to the LBT rate of -1.25. In line with our theoretical priors, we further show that effects are less pronounced for R&D expenditure types that face steeper marginal adjustment costs, and stronger among credit-constrained plants. The observed reductions in R&D activities are accompanied by lower innovation output (as measured by patents), the long-term elasticity amounting to around -0.9. Extending the scope of our analysis beyond the plant level, we further provide within-country evidence for the important role of innovation for economic growth and highlight the effects of tax policy in this relationship. Tax-induced reductions in local innovation explain around 8% of the total negative GDP effect associated with an increase in local business taxes.

In future work, it would be worthwhile to take an explicit look at the effects of tax incentives for large firms with different sites in their home country or abroad. Whereas we find quite similar effects for single and multi-plant firms at their sites in treated municipalities, multi-plants might reallocate their R&D activities to shift profits from high-tax to lower-tax regions.

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A Data Appendix

In this section of the Appendix, we provide additional information on each of the different datasets used in the empirical analysis and provide additional descriptive statistics.

The plant-level R&D survey data. The main data source of this analysis is the biennial longitudinal survey dataset *Wissenschaftsstatistik*, collected and administrated by the Sifterverband on behalf of the German Federal Ministry of Education and Research. The survey covers the near-universe of research-active plants in Germany. Those are identified via several distinct channels: (i) federal and European datasets on public R&D funding, (ii) patent applications, (iii) plant or firms' annual accounts or business reports, (iv) commercial company databases, (v) the media, and (vi) membership lists of trade associations with a focus on innovative activity. This continuously-updated register of R&D active plants is compared with information from the *Bureau van Dijk* databases to identify plant closures and changes of plant location. To further ensure the comprehensiveness of the plant register, regular surveys are conducted among plants in R&D active industries (in particular: automotive engineering, mechanical engineering, electrical and chemical engineering), which have not been known for their R&D activity so far. Results of these short surveys indicate that a very high share of all R&D-active plants in Germany is indeed covered by the dataset.

The survey covers detailed information on plants' overall R&D spending, its R&D expenses by subcategories (internally- vs. externally conducted R&D, personnel vs. non-personnel R&D spending) and its R&D staff (by age structure, qualification, education). Moreover, it offers information on plant size (in terms of sales and employees), industry classification and plant's organizational structure (single- vs. multi-plant firms). By special agreement with the Stifterverband, we also gained access to each plant's exact address (postal code and location) in a given year, which allows us to precisely assign the applicable LBT (treatment). Panel A of Appendix Table B.1 provides detailed descriptive statistics. The survey forms the basis of Germany's official reporting of its entrepreneurial R&D activities to EU authorities and the OECD.

Patent data. To assess the impact of profit taxation on innovation output, we link administrative information on plants' patenting activity from the *European Patent Office* (EPO, *PATSTAT* dataset as of 4/2016) to the R&D survey. As plants often register the very same innovation at multiple intellectual property (IP) protection institutions, worldwide patent databases focus on "patent families", i.e., pool those inventions that show the very same content and priority date. The latter refers to the date of the first patent application within a patent family at any institution and determines the start of the IP protection period. The focus on patent families effectively rules out the threat of double-counting the very same patented innovation within and across different IP systems. Within the EPO system, double-counting of patents may still occur in cases of parallel or divisional applications. However, these cases are very rare.

To best match the plant-level survey, we limit ourselves to patent families that were first registered between 1995 and 2007 and identify each patent family's initial applicant(s). This is particularly important in the context of our analysis: we want to identify the plant where the initial invention occurred, not the current IP holder. We next drop all patent applications that have not been (co-)filed

by a plant (as classified by *PATSTAT*), and geocode all remaining patents. In a final step, we use detailed information on the applicants' name(s) and location(s) of residence to merge the number of filed patents to the plant-level survey by means of a fuzzy matching algorithm. In case multiple actors jointly invented a new product or process, we only assign the respective share of a patent to a surveyed plant. Overall, the surveyed plants account for around 60% of all patents filed by a German applicant during the period from 1995 to 2007.

As the value of patents differs substantially (Scherer, 1965, Hall et al., 2005), we create a second measure of innovation output that weights each patent family according to the number of citations it receives from patents filed at the United States Patent and Trademark Office (USTPO) within the first five years after its registration.[25] Citation-adjusted weighted counts are widely used in the literature and have been shown to correlate well with real-world measures of innovation quality such as profitability (see, e.g., Harhoff et al., 2003, Kogan et al., 2017, Moser et al., 2018). Relying on data from Danzer et al. (2020), we further distinguish product from process innovations. To group patents along this margin, information from the highly standardized patents' claims texts is used. Patents are classified as process innovation if the claim text of a patent includes terms such as "method", "process" or "procedure". Panel B of Appendix Table B.1 provides descriptive statistics on each measure of plants' innovation output. Note that some patent applications (12%) do not provide enough information to classify a patent accordingly. Excluding these patents from the baseline regressions does not affect estimates.

Financial plant-level data. While the Stifterverband data provide detailed information on plants' R&D activities, the survey offers only few insights on plants' financial situation. To test for heterogeneous effects among more or less cash-constrained plants, we link additional information from the Bureau van Dijk's (BvD) AMADEUS and ORBIS databases to the surveyed plants.[26] The match between the R&D survey and the BvD data has been established by the Stifterverband as part of the survey's implementation strategy. The two BvD datasets offer a variety of financial information at the *firm* level. We assign the firm-level financial information to plants that are part of a multi-plant firm. As the BvD datasets predominantly cover larger and oftentimes stock-listed plants or firms, we can only match around 40% of the surveyed plants to the BvD data.

To prepare the BvD data for the purposes of our study, we predominantly follow Kalemli-Ozcan et al. (2015) and Gopinath et al. (2017). We first combine multiple vintages of the AMADEUS and ORBIS datasets to increase coverage over time. Ultimately, we use vintages of the AMADEUS database from 2001, 2002, 2007 and 2010, as well as the 2016 ORBIS version. When a given plant appears in more than one vintage, we follow Gopinath et al. (2017) and take those information from the most recent vintages. When multiple financial accounts are available for a given plant in a given year, we always refer to accounts with higher quality. Here, we always prefer those accounts that cover the full twelve months of a given year. Moreover, we prefer accounts in accordance to IFRS guidelines over GAAP accounts or those with unknown reporting standards. Last, we choose unconsolidated

[25] Effects remain unaffected when using citations from patents filed at the EPO. Citations counts are quite different in these two institutions as the USPTO requires patent applicants to list all relevant patents prior art, whereas such a requirement does not exist at the EPO. Citation data is taken from PATSTAT 10/2019 to completely rule out attrition.

[26] The data was kindly made available by the LMU-ifo Economics & Business Data Center (`https://www.ifo.de/EBDC`).

over consolidated accounts. In the empirical analysis, we measure plants' liquidity constraints via the level of non-current liabilities; Panel A of Appendix Table B.1 provides the corresponding descriptive statistics.

Regional admin data. Information on LBT scaling factors (*Realsteuerhebesätze der Gewerbesteuer*) for all West German municipalities were obtained from the Federal Statistical Office and the Statistical Offices of the German States. We construct a balanced panel dataset for the universe of municipalities by combining two different sources. Data for the period from 1987 to 2000 was obtained by filing individual requests to the respective Statistical Offices of the German States. Information for the years from 2001 to 2013 is publicly accessible via annual reports: "*Hebesätze der Realsteuern*", published by the Statistical Offices of the German States.

Information on local GDP per capita is available at the county level only and can be accessed via the Working Group Regional Accounts of the Federal Statistical Office and the Statistical Offices of the German States. The available data cover the period from 1992 to 2014. We use the revision from 2011 and account for inflation by calculating real GDP per capita in 2010 prices using the consumer price index published by the Federal Statistical Office (*Verbraucherpreisindex*). For the analysis conducted in Section 6.2, we impute municipalities' GDP by multiplying the respective county's GDP per capita with a given municipality's population figures.

Data on municipal expenses for all West German municipalities over the period from 1998-2007 were obtained from the Federal Statistical Office and the Statistical Offices of the German States. Since 2001, information on local expenses are publicly available via the annual reports "*Statistik Lokal*", published by the Statistical Offices of the German States. For the period from 1998-2000, we filed a data request to the statistical offices. Again, we account for inflation by using the consumer price index and express expenses in 2010 prices.

Information on population levels are available for the entire effect window (1987-2013) and were taken from the Federal Statistical Office and the Statistical Offices of the German States. We combine two different sources to construct a balanced panel for the universe of West German municipalities. First, data for the period from 1987 to 1999 are based on data requests we filed to the Statistical Offices of the German States. Second, data on population levels from 2000 onwards are publicly available via the annual German municipality register (*Gemeindeverzeichnis*).

Last, we collect information on the number of unemployed individuals per municipality for the period 1998 to 2013 from the annually report *Bestand an Arbeitslosen, Rechtskreise SGB III und SGB II, Insgesamt*, published by the German Federal Employment Agency. In the empirical analysis, we divide this number by the respective municipality's annual population level to proxy local unemployment rates.

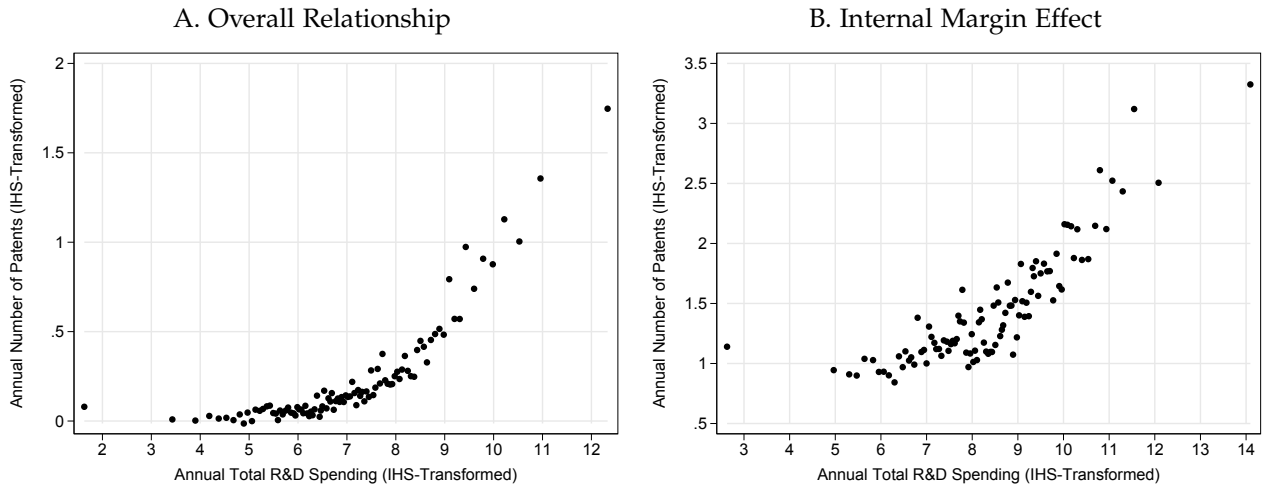
B Descriptive Statistics, Correlations and Definitions

Table B.1: Descriptive Statistics for Baseline Sample

	Mean	SD	*P*5	*P*25	*P*75	*P*95	*N*
A. Plant-Level R&D Survey							
R&D Spending Levels (in Thousand EUR)							
Total R&D Spending	8,109.77	89,163.33	46	208.00	2,089	16,357	31,648
Internal R&D Spending	6,837.73	75,420.28	29	180.00	1,869	14,509	31,648
External R&D Spending	1,272.03	21,521.87	0	0.00	86	1,284	31,648
Internal Spending on R&D Personell	4,152.55	42,238.54	18	118.00	1,210	9,441	31,648
Internal R&D Spending on Non-Personell	2,685.16	35,403.09	3	44.00	594	5,269	31,648
Spending Shares (in %)							
Share of Internal R&D Expenses	90.93	18.36	52	90.00	100	100	31,529
Share of External R&D Expenses	9.07	18.36	0	0.00	10	48	31,529
Share of Internal R&D Expenses for Scientific Staff	67.22	16.54	37	60.00	79	94	31,072
Share of Internal R&D Expenses for Non-Personell	32.78	16.54	6	21.43	40	63	31,072
Other Plant Characteristics							
No. of Employees	536.54	3,035.90	8	37.00	324	1,700	31,623
No. of Employees Conducting R&D	37.70	308.85	1	2.00	14	99	31,541
Sales (in Million EUR)	210.95	1,601.39	1	6.00	82	572	31,445
R&D Spending per Employee (in Thousand EUR)	23.43	347.68	1	2.56	15	60	31,623
Non-Current Liabilities to Sales Ratio	0.53	4.48	0	0.13	0	1	5,879
Manufacturing Sector	0.85	0.36	0	1.00	1	1	31,648
Service Sector	0.10	0.31	0	0.00	0	1	31,648
Other Sector	0.05	0.21	0	0.00	0	0	31,648
B. Patent Data							
Number of Patents	0.84	7.62	0	0.00	0	3	31,648
Citation-Weighted Number of Patents (USPTO)	1.72	19.66	0	0.00	0	5	31,648
Citation-Weighted Number of Patents (EPO)	0.97	9.96	0	0.00	0	3	31,648
Citation-Weighted Number of Process Innovations	0.39	4.96	0	0.00	0	1	31,648
Citation-Weighted Number of Product Innovations	0.48	4.53	0	0.00	0	2	31,648
C. Local Characteristics							
Population	27,432.37	76,806.31	1,917	5,601.00	23,485	81,584	11,403
Log Unemployment Per Capita	3.44	1.34	2	2.46	4	6	8,179
Local Public Expenditures (in Thousand EUR)	745.85	3,773.02	29	91.02	422	2,018	8,181
GDP Per Capita	28,280.75	10,380.23	18,835	22,714.64	30,547	44,369	10,917

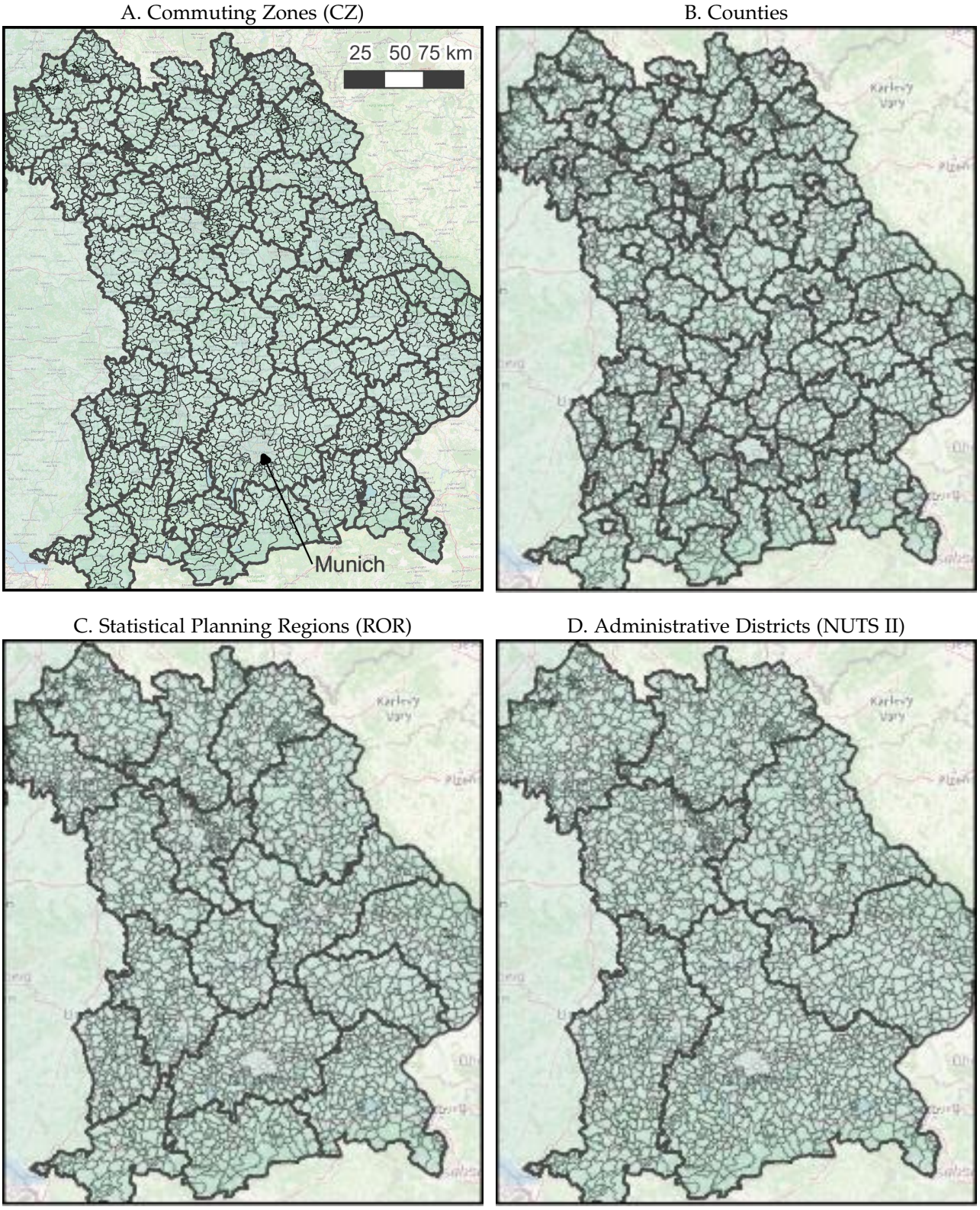
Notes: This table shows descriptive statistics for our baseline sample. Panel A provides insights on covered plants' R&D spending in levels, spending shares, and other plant characteristics. Panel B offers information on plants' patenting activities. Last, in Panel C information on municipality- and district-level characteristics are given. See the Data Appendix A for more information on the respective data sources.

Figure B.1: Assessing the Link between R&D Spending and Patenting



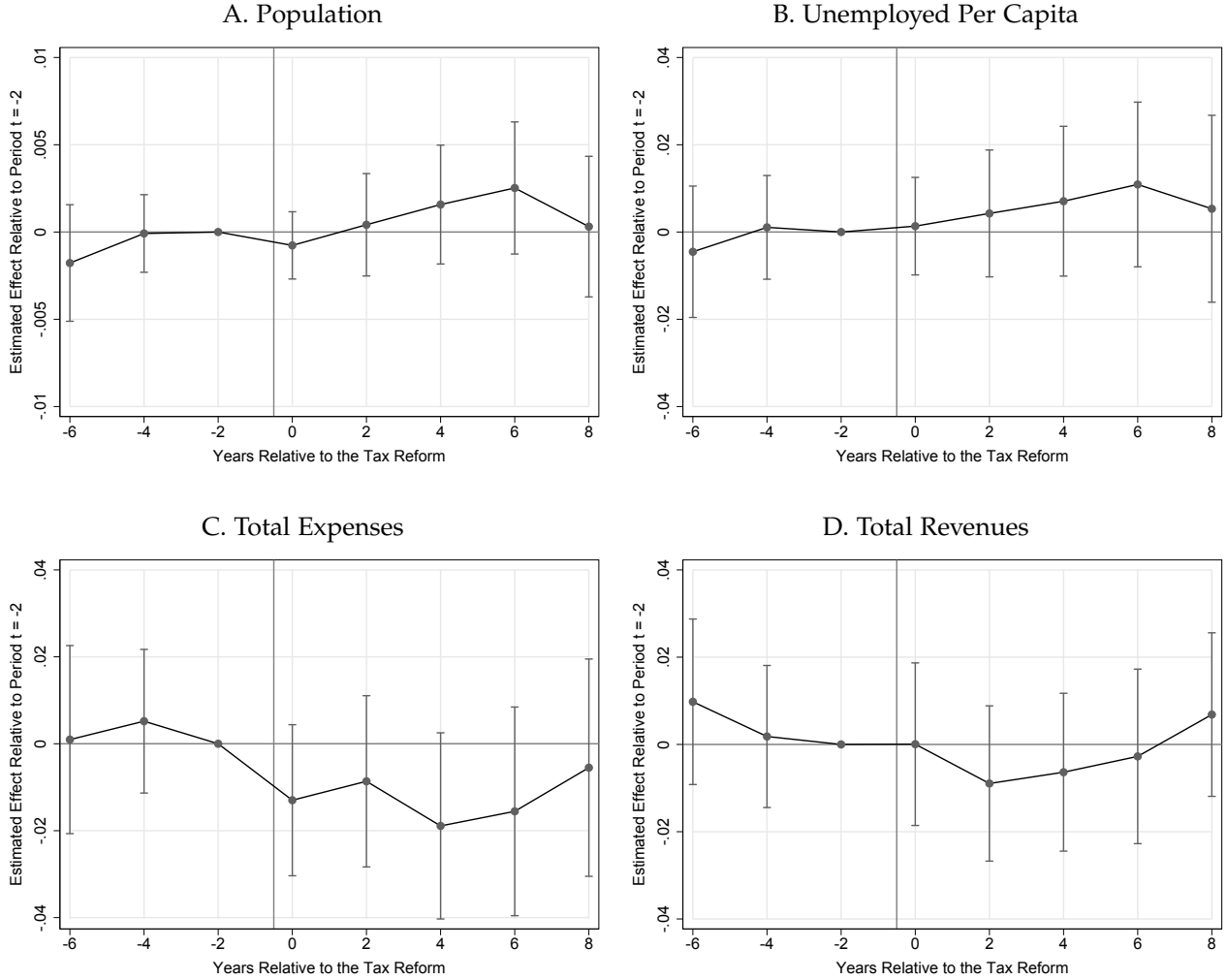
Notes: This binscatter plot illustrates the relationship between plants' annual R&D spending and their respective number of filed patents in our baseline sample. An inverse hyperbolic sine transformation is applied to both variables. Year and industry fixed effects are accounted for. We plot the overall relationship in Panel A, and the intensive margin effect in Panel B. Information on plants' R&D spending stems from the *Wissenschaftsstatistik*. Information on plants' patenting activities is taken from the European Patent Office. See Data Appendix A for more information on both datasets.

Figure B.2: Regional Classifications of Municipalities in the Free State of Bavaria



Notes: This figure illustrates different regional subdivisions used to control for region-times-year fixed effects in our study, focusing on the 2,056 municipalities in the Free State of Bavaria for the purpose of illustration (thin black lines indicate municipality borders as of December 31, 2010). Panel A plots municipalities along with the 56 commuting zones in Bavaria (thick black lines), which corresponds to our baseline specification. Panel B shows instead the 96 counties and city counties (*kreisfreie Städte*) in Bavaria (nested in commuting zones). Panels C and D show the 18 statistical planning regions (*Raumordnungsregionen*, ROR) and seven administrative districts (*Regierungsbezirke*, NUTS II), respectively, which are geographical aggregations of commuting zones. *Maps:* © GeoBasis-DE / BKG 2015, OpenStreetMap contributors.

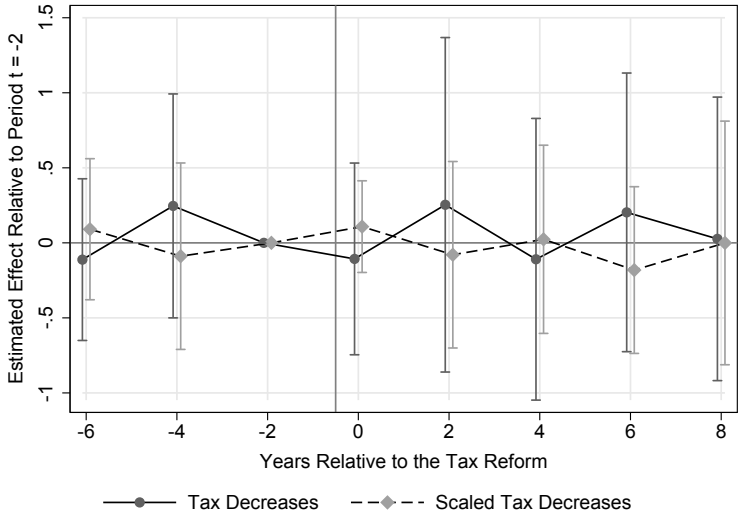
Figure B.3: The Effect of a Tax Rate Increase on Municipality-Level Outcomes



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in equations (7)–(9a). The dependent variable is a municipality's annual population in Panel A, it annual share of unemployed per capita in Panel B, its total annual expenditures in Panel C, and its total annual revenues in Panel D. All outcomes are in logs. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include municipality, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are clustered at the municipal level.

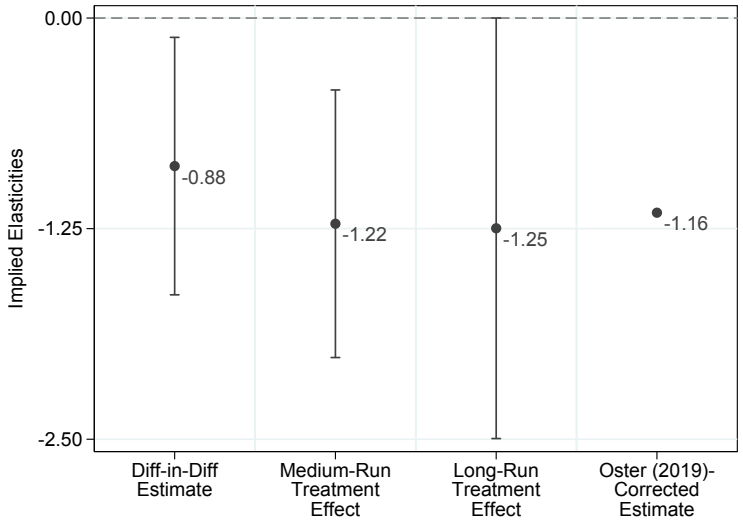
C Additional Estimates

Figure C.1: The Effect of a Tax Decrease on Total R&D Spending



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in equations (7)–(9b). For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax increase during the event window period are excluded. Standard errors are clustered at the municipal level.

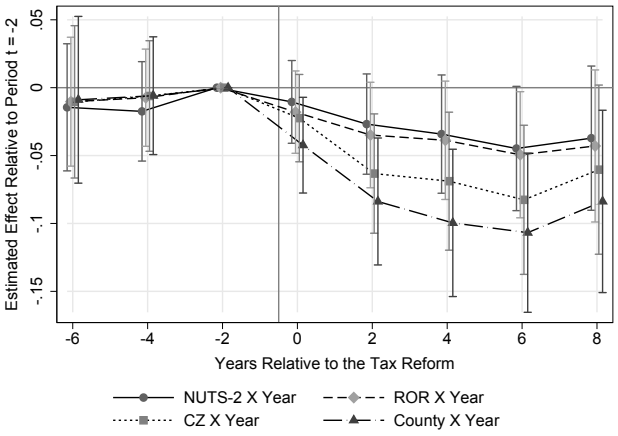
Figure C.2: Implied Elasticities – Total R&D Spending



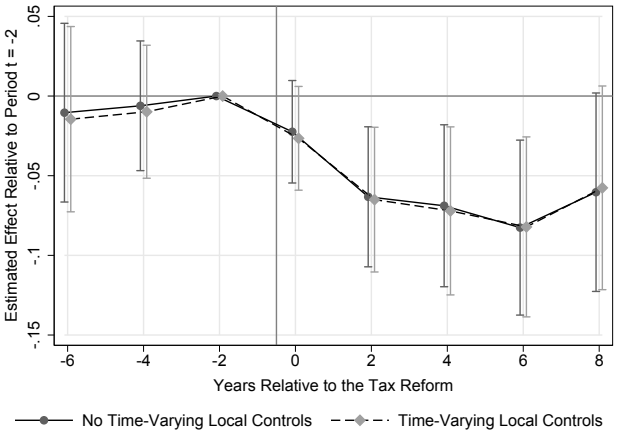
Notes: This graph displays different implied elasticities of plants' total R&D spending in response to an increase in the local business tax rate. We calculate elasticities (i) using a simple difference-in-differences model with the log local business tax rate as the explanatory variable, (ii) taking the mean over treatment effects $\widehat{\beta_0}$–$\widehat{\beta_6}$ from the classic event study specification as defined in equation (9a) and shown in Figure 4, and (iii) taking the last treatment effect ($\widehat{\beta_8}$) of the same event study specification. Last, we take this long-term elasticity and calculate bounds in the spirit of Oster (2019), assessing coefficient stability in specifications with and without time-lagged variables on local business cycle conditions. The first two elasticities indicate average treatment effects, the latter ones capture the long-run effect of a tax increase.

Figure C.3: The Effect of a Tax Increase on Total R&D Spending – Robustness Checks

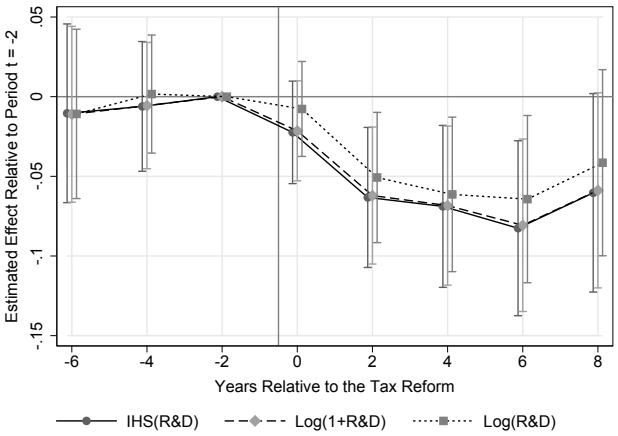
A. Region X Year Fixed Effects



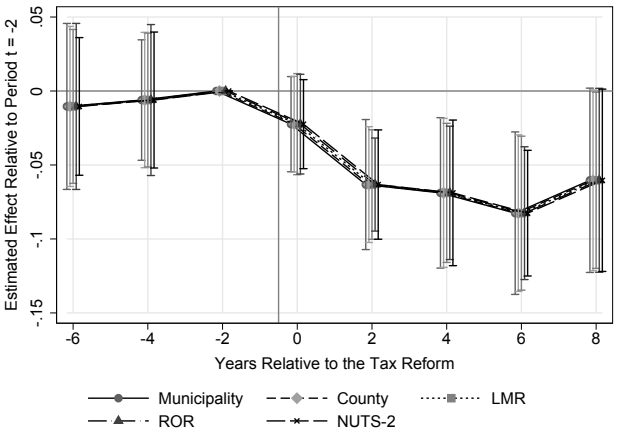
B. Local Time-Varying Confounders



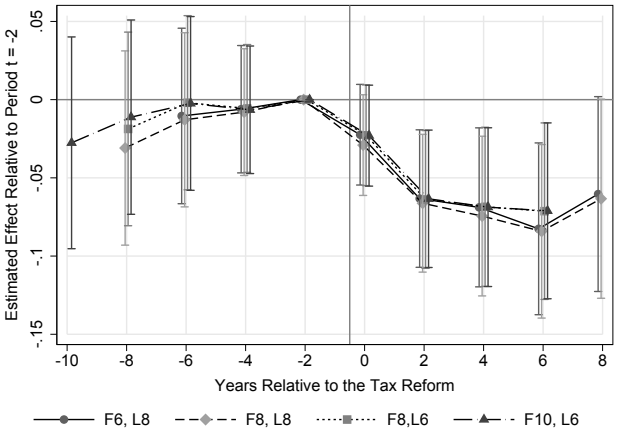
C. Different Outcome Transformations



D. Alternative Inference

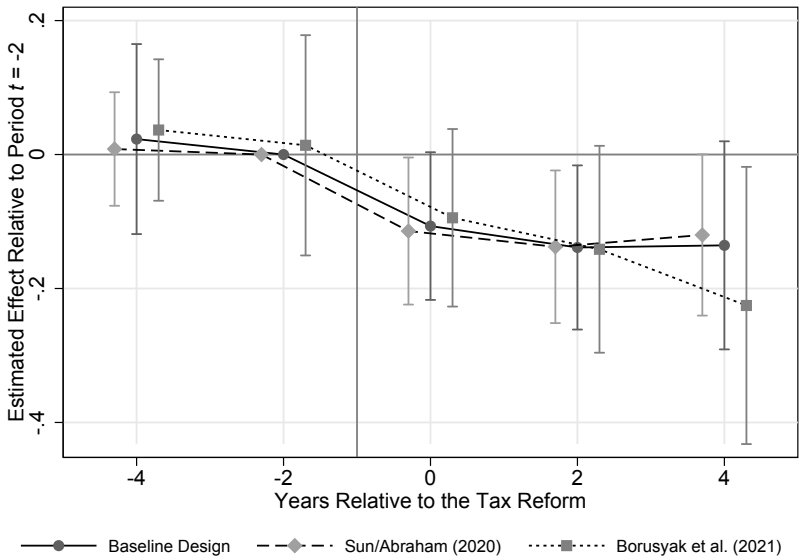


E. Different Event Windows



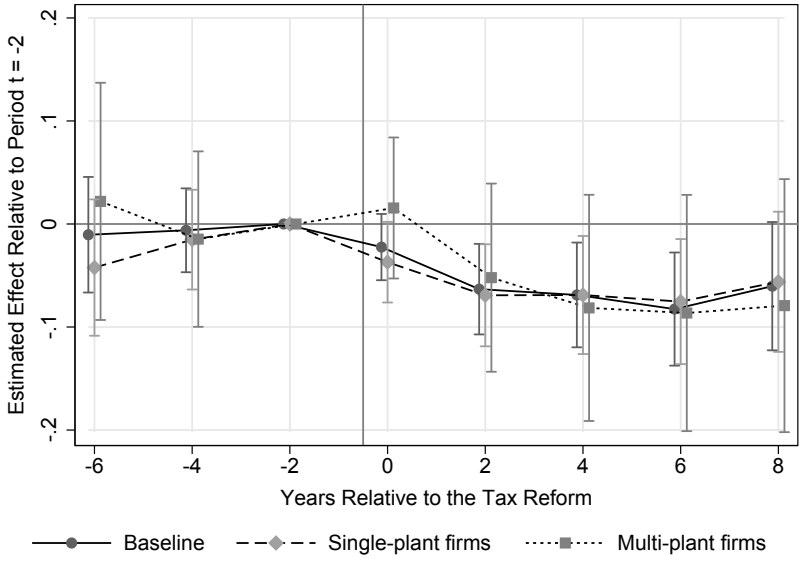
Notes: This graph illustrates the robustness of our baseline effects on total R&D spending with regard to various alternative specifications of the classic tax increase event study model as defined in equation (9a). In Panel A, we plot the corresponding point estimates and 95% confidence intervals when using varying regional $\times$ year fixed effects. In Panel B, we add lagged local business cycle controls to our preferred specification. In Panel C, we use different transformations of the outcome variable. In Panel D, we test the robustness of our baseline results when using alternative ways of drawing inference. Last, in Panel E we allow for differently sized event windows. In Panels B–E, all regressions include plant, state $\times$ year, commuting zone $\times$ year as well as sector $\times$ year fixed effects. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. Municipalities that experienced a tax decrease during the event window period are excluded. In Panels A, B, C and E, standard errors are clustered at the municipality level.

Figure C.4: The Effect of a Tax Increase on Total R&D Spending – Heterogeneous Effects By Cohorts



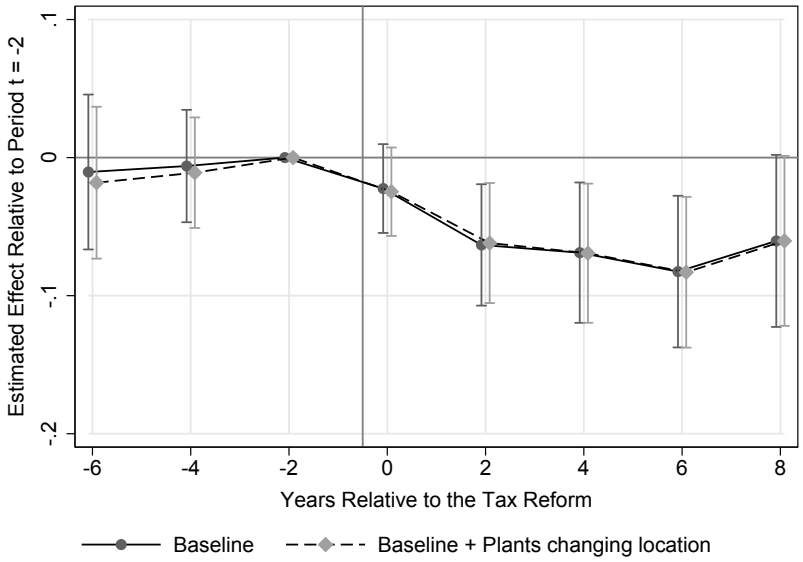
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-4, \ldots, 4]$), and corresponding 95% confidence intervals when (i) using our baseline event study model as defined in equations (7)–(9a) and (ii) explicitly correcting for possible cohort-specific treatment effects as suggested by Sun and Abraham (2020) and Borusyak et al. (2021), respectively. The analysis is limited to plants in municipalities that experienced either no or just one tax increase during the period 1990–2011. Municipalities that experienced a tax decrease during this period are excluded. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, NUTS-2 $\times$ year, as well as sector $\times$ year fixed effects. Standard errors are clustered at the municipal level.

Figure C.5: The Effect of a Tax Increase on Total R&D Spending – Single- vs. Multi-Plant Firms



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in equations (7)–(9a) when allowing for heterogeneous effects for single- vs. multi-plant firms. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax increase during the event window period are excluded. Standard errors are clustered at the municipal level.

Figure C.6: The Effect of a Tax Increase on Total R&D Spending – Including Plants Changing Locations

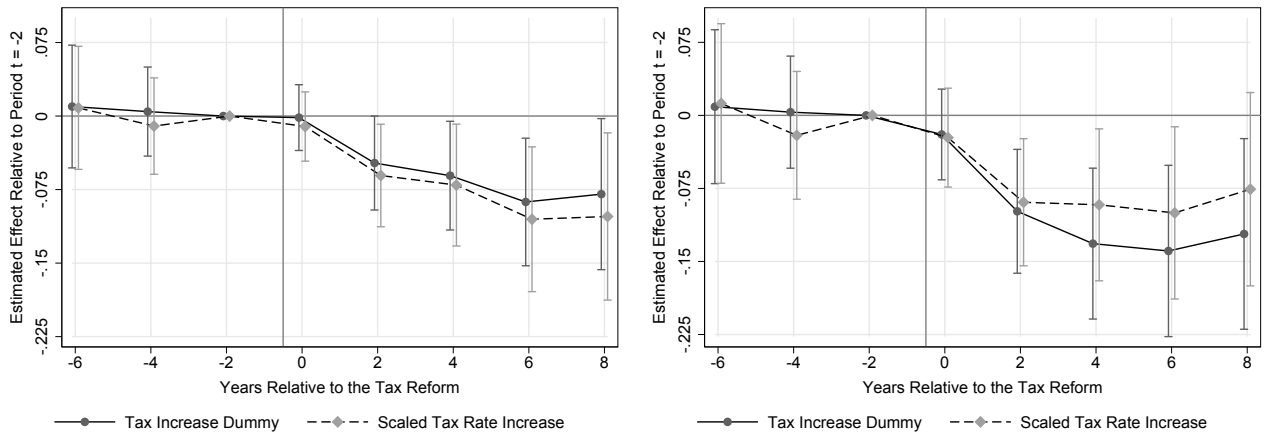


Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in equations (7)–(9a) for (i) the baseline sample and (ii) when including those plants that change municipalities during the observation period and assigning them the local tax rates of the first observed municipality (intent-to-treat). For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax increase during the event window period are excluded. Standard errors are clustered at the municipal level.

Figure C.7: The Effect of a Business Tax Increase on Personnel vs. Non-Personnel R&D Spending

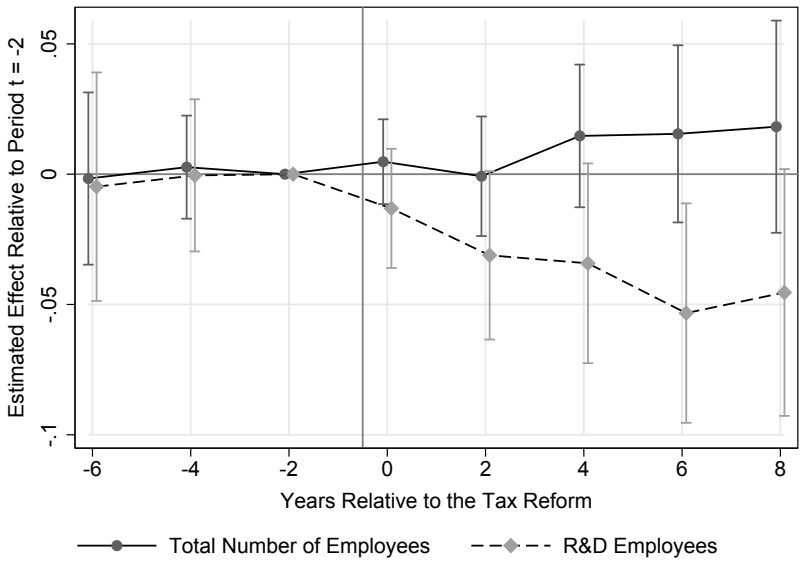
A. Internal Spending on R&D Personnel

B. Internal Non-Personnel R&D Spending



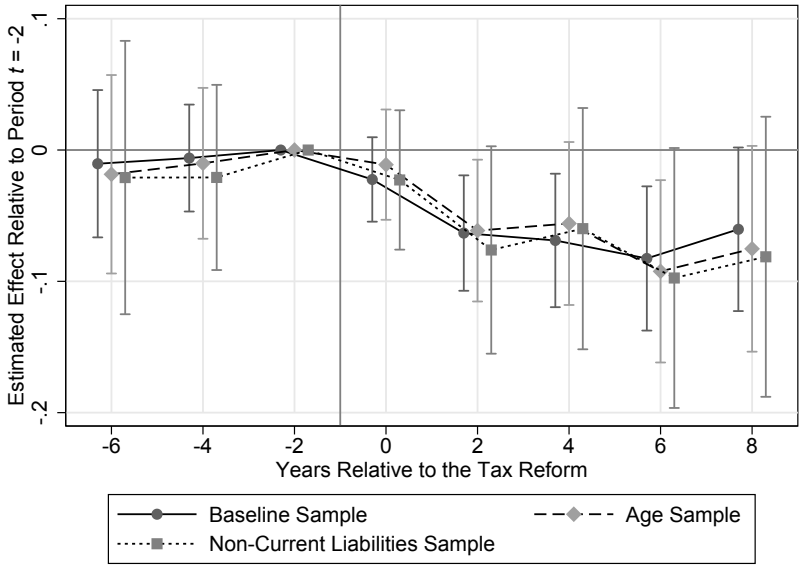
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study models as defined in equations (7)–(9b). The dependent variable is plants' annual internal spending on R&D personnel in Panel A, and a plants' annual internal non-personnel R&D spending in Panel B. Both outcomes are inverse-hyperbolic-sine transformed. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are clustered at the municipal level.

Figure C.8: The Effect of a Tax Increase on Plants' Total and R&D Staff



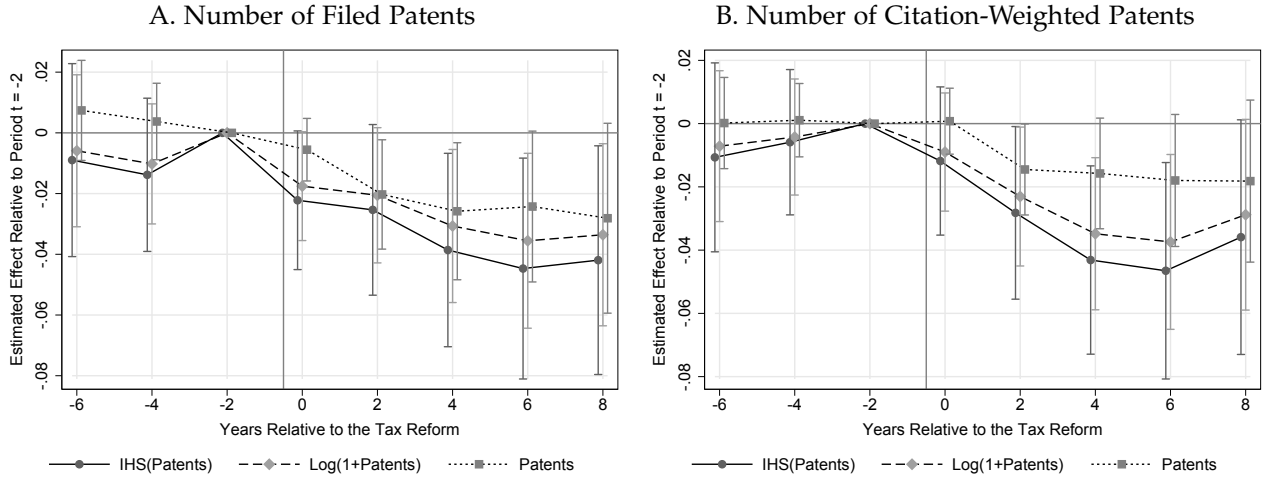
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in equations (7)–(9a). The dependent variable refers to plants' total annual number of employees and the stock of workers conducting R&D, respectively. For the treatment group, the business tax change occurred in year $t = 0$ or $t = -1$. The regressions include plant, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are clustered at the municipal level.

Figure C.9: The Effect of a Tax Increase on Plants' Total R&D Spending – Different Samples



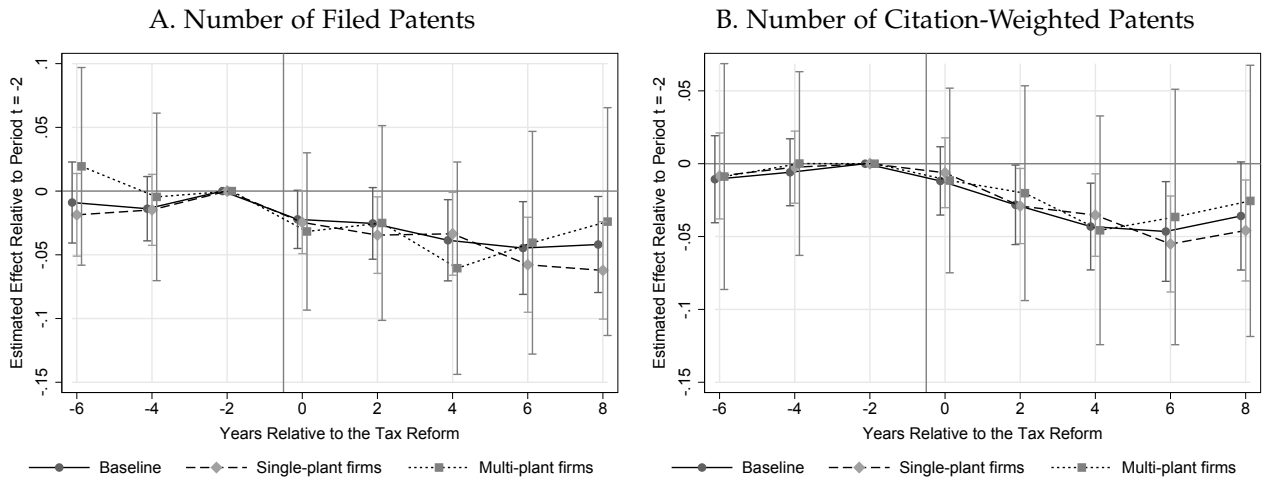
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in equations (7)–(9a) when using different samples. The dependent variable refers to plants' total annual R&D spending. For the treatment group, the business tax change occurred in year $t = 0$ or $t = -1$. The regressions include plant, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are clustered at the municipal level.

Figure C.10: The Effect of a Business Tax Increase on Patents – Different Outcome Transformations



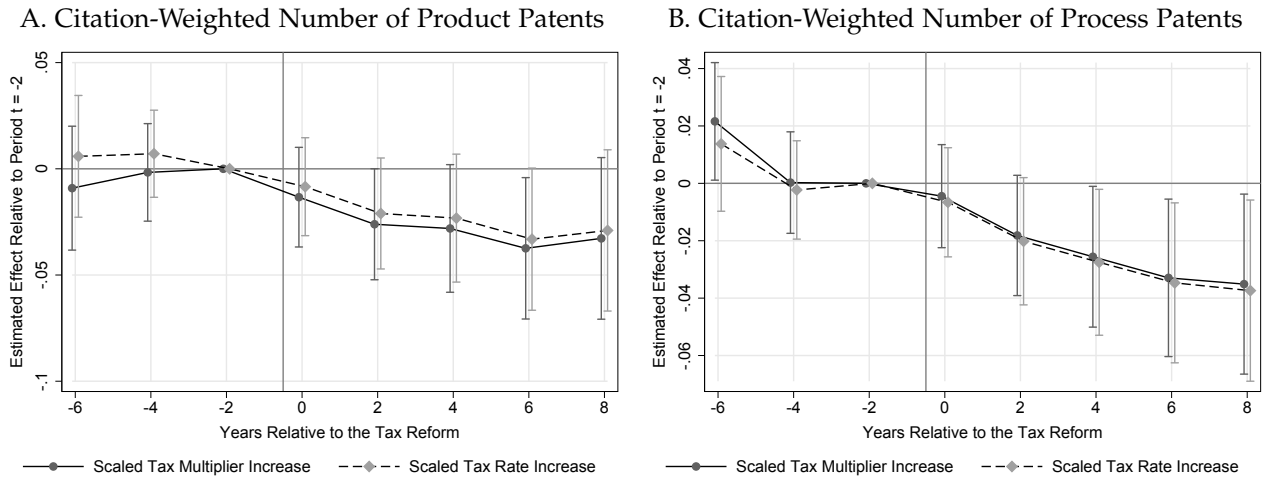
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study models as defined in equations (7)–(9b) when using different transformations of the outcome variable. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are clustered at the municipal level.

Figure C.11: The Effect of a Business Tax Increase on Patents – Single- vs. Multi-Plant Firms



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study models as defined in equations (7)–(9b) when (i) using the baseline estimation sample, (ii) focusing on single-plants firms, and (iii) restricting the sample to multi-plant firms. In Panel A, the dependent variable is the plant-level annual number of filed patents. In Panel B, the dependent variable is the number of citation-weighted patents. Both outcomes are inverse hyperbolic sine transformed. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are clustered at the municipal level.

Figure C.12: The Effect of a Tax Rate Increase on Product and Process Innovations



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study models as defined in equations (7)–(9b). In Panel A, the dependent variable refers to a plants' annual number of patents referring to product innovations. In Panel B, the dependent variable is the number of citation-weighted patents referring to process innovations. Both outcomes are inverse hyperbolic sine transformed. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are clustered at the municipal level.

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