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Abstract. We study how business taxes affect establishments' R&D activities. Relying on geocoded panel data targeting the universe of R&D-active establishments in Germany, we exploit around 7,300 changes in the local business tax rate over the period 1987–2013 for identification. Using event study techniques, we find a sizable negative and statistically significant effect of an increase in the local business tax on establishments' total R&D spending and patents filed. Zooming into the process of innovation production, we uncover substantial heterogeneity in the impact of business taxation for various R&D inputs, among establishment characteristics, and for different types of research projects.

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). Firms play a key role in this process, serving as the cradle of most groundbreaking new technologies and products: 90% of all patents in both the U.S. and Germany are filed by firms. Their engagement in research and development (R&D) serves as an instrument to expand in size and 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 by both firms and other innovators are generally considered to be below the social optimum (Jones and Williams, 1998).

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 specific expenses, to spur firm-level engagement in R&D. These policy instruments reduce the burden of business taxes for specific firms conditional on engaging in R&D. A recent and large body of research, discussed in more detail below, shows that targeted R&D tax incentives increase firms' innovative activities. By contrast, there is little systematic evidence of the potential disincentive effects of general business taxes on firms' R&D activities. This lack of evidence is problematic because general profit taxes play an important role in the optimal design of R&D policies (Akcigit et al., 2022b) and exist in almost every country in the world. In addition, there is relatively little evidence about the mechanisms underlying firms' responses to R&D tax policy—mostly due to a lack of suitable data. Knowledge about the channels at play will be particularly helpful for policymakers when designing R&D policies.

In this paper, we aim to fill both gaps by analyzing the impact of profit taxation on firms' R&D spending and innovation output. We make two major contributions. First, we use rich panel data targeting all R&D-active establishments in Germany to zoom in on the process of innovation production at the level of the firm.[1] The dataset allows us decomposing establishments' R&D spending along various input factors in knowledge production, as well as testing for heterogeneous effects by establishment types. We further enrich the dataset with administrative information on establishments' patenting activities from the European Patent Office to learn about the effects of business taxes on different types of research projects. Second, we contribute to the small literature studying the effect of general business taxes on innovation. We exploit the unique and compelling institutional setting of profit taxation in Germany, where municipalities autonomously set their local business tax each year. Our identification comes from around 7,300 local tax changes over the period 1987–2013. This setting enables us to flexibly account for time-varying unobserved confounders at fine-grained regional levels when estimating the effect of interest.

We set up a simple theoretical model that builds upon earlier contributions by Chetty and Saez (2010) as well as Chen et al. (2021) to guide our empirical analysis. We derive three hypotheses. First, an increase in the local business tax rate reduces establishments' R&D investments because it lowers their after-tax returns. Second, the negative effect of business taxation should be particularly strong for types of R&D expenditures that are easier to adjust at the margin. Third, an increase in

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

the local business tax rate is expected to cause stronger reductions in R&D spending among more credit-constrained firms. The latter hypothesis draws upon the institutional feature that the costs of debt financing can be deducted from an establishment's tax base, its operating profits, whereas the costs of equity financing cannot. This distinct tax treatment distorts establishments' financing decisions towards debt. However, investments in R&D are generally highly uncertain, come with substantial information asymmetries between the innovator and financial backers, and often lack collateral (see, e.g., 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 establishments to respond stronger to a given tax increase than unconstrained ones.

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, we show that an increase in the local business tax rate has a negative and statistically significant effect on establishments' total R&D expenditures. We derive a baseline elasticity of R&D expenditures with respect to the business tax rate of -1.15, which translates into an elasticity with respect to the user cost of capital of -2.45. This effect size is similar to estimates reported in the context of targeted R&D tax credits or subsidies in other settings. The tax-induced decline in plant-level R&D spending is accompanied by a reduction in innovation output, the corresponding elasticity for the number of patents with respect to the tax rate amounting to -0.65; an estimate close to recent evidence from the U.S. by Akcigit et al. (2022a).

Having documented the negative effect of business taxes on establishments' R&D spending and its robustness to varying specifications of the event study model,[2] we show that this effect is entirely driven by reductions in internal R&D spending. The scale of establishments' R&D activities conducted by outside parties on their behalf remains unaffected. We argue that this finding is consistent with steeper marginal adjustment costs for external R&D spending, e.g., because of long-term contractual arrangements that are more costly to alter at the margin. We further show that the effect on internal R&D spending comes from reductions in both personnel and capital expenses, suggesting that the two inputs are complements in R&D production. Around 75% of the effect on personnel spending can be linked to a reduced R&D headcount, the remaining 25% to lower wages. In terms of R&D employment, we find a strong effect on unskilled R&D staff, whereas the level of scientists and engineers engaged in R&D remains unaffected. We rationalize this latter finding with varying adjustment costs for the two types of workers, e.g., due to different firm-specific human capital.

We further show that reductions in R&D spending are stronger for credit-constrained establishments as approximated by establishments' non-current liabilities to sales ratio. In contrast, we do not observe heterogeneity by establishment size (in terms of employees). Targeted R&D policy instruments typically target smaller firms, partly because of policymakers' implicit beliefs that they are more liquidity-constrained. Thus, smaller firms should benefit more from any given level of support (Gonzales-Cabral et al., 2018). Our results hence indicate that size may serve as a poor proxy for firms' financial situation. We also show that those plants predominantly using R&D spending to obtain new knowledge and technologies, i.e., investing a higher share of their budget in basic research, respond less than those spending more on applied R&D.

[2] Among others, effects are insensitive to the inclusion of varying regional trends and not driven by local economic shocks. Moreover, effects remain unchanged when accounting for possible heterogeneous treatment effects across cohorts in the spirit of de Chaisemartin and D'Haultfœuille (2020), Sun and Abraham (2021) and Borusyak et al. (2021).

In a final exercise, we discuss the implications of our results for R&D tax policy. To this end, we assess the efficiency of general business taxes as a tool to stimulate firm-level innovation compared to targeted R&D tax policies. Assuming symmetric effects of tax increases and decreases, 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 R&D tax incentives appear to be the more efficient policy instrument altogether. Our results yet question policy makers' common practice to link firms' eligibility to targeted R&D tax incentives to their size—a result that lines up with recent empirical and theoretical evidence. Curtis et al. (2021) study the effect of accelerated depreciation rules on firm-level investment in the context of the U.S. and find no differential effects by firm size either. Galaasen and Irarrazabal (2021) use a Schumpeterian growth model and data from Norway to show that size-based eligibility thresholds for R&D tax incentives 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. In this context, Akcigit et al. (2022b) theoretically show that optimal R&D tax policies shall rather target the most R&D-productive firms.

Related Literature. Our paper contributes to two strands of the literature. We first add to the small and recent literature that exploits variation in sub-national tax policy settings to analyze the effect of (business) taxation on innovation. Moretti and Wilson (2017) study inventor mobility in the U.S. and show that "star scientists" respond to changes in personal and corporate state-level tax incentives by relocating to lower-tax states. Most related to our study is work by Akcigit et al. (2022a), who use U.S. state-level panel data on corporate and personal income tax rates as well as patents over the entire 20th century to study the effect of changes in tax policy on innovation. The authors find that higher taxes reduce the quantity and quality of patents as well as affect the geographic spread of innovative activities.[3]

Relative to these studies, we open the black box of R&D production at the establishment level. The available data and setting allow us estimating the effect of business taxation on plants' R&D spending and innovation output in a joint framework as well as identifying the underlying mechanisms at play. In detail, we test which types of (i) R&D inputs, (ii) establishments, and (iii) research projects are affected the most by an increase in business taxation. We further provide evidence for the detrimental effect of business taxes on innovation in a setting outside the United States. While being interesting in its own, the focus on Germany as a laboratory also offers major advantages in terms of identifying variation. In our empirical analysis, we exploit changes in the local business tax (*Gewerbesteuer*), a substantial tax on profits with an average tax rate of 17% that is levied at the level of the municipality. The institutional set-up enables us to exploit around 7,300 tax changes of sizable magnitude with an average tax change of around one percentage point and to account for possible confounding regional shocks at very fine geographical levels. Moreover, the local tax applies to both corporate and non-corporate firms as well as to entrepreneurs such that a tax change affects all firm types in a

[3] An earlier study by Mukherjee et al. (2017) offers similar results.

similar way. Last, Germany did not provide any other direct or indirect tax incentives for firms' R&D activities during the sample period (until January 1, 2020). To this end, we do not need to account for other, potentially confounding tax policies in our empirical design.

The paper also 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. Rao (2016) studies the impact of the U.S. R&D tax credit during the period 1981 to 1991 and finds that a reduction in the user cost of R&D led to a sizable increase in the research intensity of firms. 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.

We contribute to this strand of the literature in two ways. First, we consider a general business tax, a policy instrument that usually complements targeted tax incentives. Second, the local business tax applies to corporate and non-corporate firms as well as single entrepreneurs, which allows estimating effects along the full distribution of R&D-active establishments. Hence, we are able to identify average treatment effects but also to test for heterogeneous responses along various establishment characteristics. We can further rule out misreporting as documented in Chen et al. (2021), given that our policy instrument does not specifically target establishments' R&D spending.

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 discusses how an increase in the (local) business tax rate should affect firm-level R&D spending from a theoretical perspective. Section 3 describes the establishment-level survey data, as well as the matching of patent information and additional financial variables to the set of covered establishments. In Section 4, we set up our empirical research design and discuss the plausibility of the design's underlying identifying assumptions. Section 5 presents the empirical results. Section 6 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, the country's lowest level of government administration.[4] In the empirical analysis presented below, we will exploit within-municipality variation in local business tax (LBT) rates for identification.

The local business tax serves as municipalities' most important source of revenue. The tax base is operating profits, with limited loss carryforward and no loss carryback. Taxable profits of firms with establishments in more than one municipality are divided between municipalities according to formula apportionment that is based on each establishment's respective payroll share. Rules for the tax base are defined at the national level and cannot be altered by state or municipal governments. As the LBT applies to almost all firm types, profits of corporations are subject to the same tax as profits of individual inventors who are typically organized as sole proprietorships or partnerships. The tax rate is derived as the product of the basic federal tax rate (*Steuermesszahl*) and a local scaling factor (*Hebesatz*) that 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' sole 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. The basic tax rate is set at the national level and applies to all municipalities.

Figure 1 illustrates the spatial and temporal variation in local tax rates across West Germany.[5] 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%). The average and median tax rates in our sample amount to 17%. 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 × 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.[6] 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.

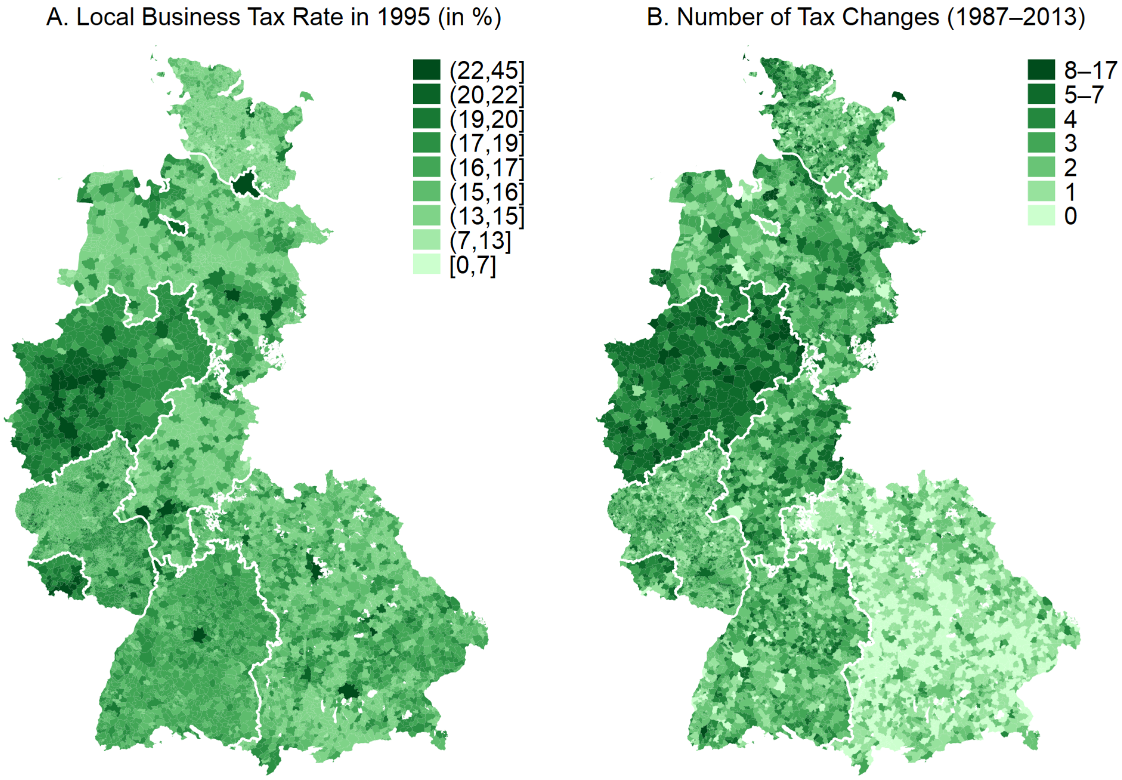
Figure 2 provides additional information about the underlying variation in tax changes over time. Panel A shows that more than 90% of all tax changes during the covered period were tax increases; the majority of them being rather small. The average tax increase amounted to around one percentage point,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

[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] 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 establishments account for less than five percent of the country's total R&D expenditures during the period under investigation.

[6] 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

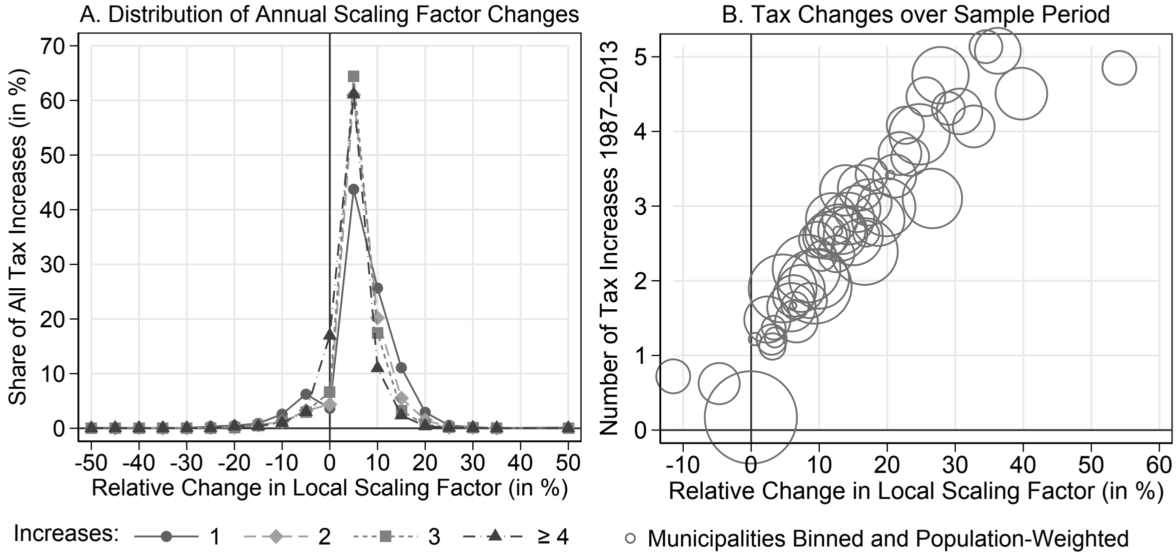


Notes: This figure illustrates the spatial and temporal variation in the local business tax rate across West German municipalities. In Panel A, the 1995 local business tax rate is plotted for each municipality. Darker colors indicate higher levels of the LBT. In Panel B, the number of total LBT changes 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.*

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). 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 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% (own calculations based on the OECD's Main Science and Technology Indicators database).

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.

2.2 Modeling Business Tax Incentives and R&D Investments

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

In period one, the establishment decides on the level of investment I. The establishment uses financial resources P to finance investments. We start by assuming that the establishment 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 establishment 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 establishment has to pay a per-unit investment cost c. Following Chen et al. (2021), the establishment 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 interest r^D. Abstracting from discounting, the establishment'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 establishment'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 establishment-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 establishments 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 establishment-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 various R&D inputs. Assuming the marginal returns for all types of investments to be identical in equilibrium, we expect that an increase in local business taxes leads to more pronounced reductions in, e.g., 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, establishments generally commit to longer-term partnerships and contractual arrangements, which should limit establishments' ability to alter its marginal external R&D expenditures to a large extent in response to an increase in the local business tax. Similarly, we hypothesize that firms rather reduce non-specialized, low-skilled R&D employment than their scientific staff, e.g., because of differences in firm-specific human capital. We also investigate effects on various other input factors in Section 5.2.

Up to this point, we assumed that the establishment finances its R&D expenses in period one by raising debt. In the context of R&D, credit constraints may be particularly relevant for establishments. 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 establishments.

Hence, we also explore the effect of taxes on R&D investments when the establishment 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 establishments that have to finance marginal R&D projects to a larger extent via equity, for instance, when more binding credit constraints limit debt financing.*

The heterogeneity in the effect by financing type is driven by the deductibility of financing costs. If equity financing costs were to some extent deductible from the business tax base, differences between debt and equity financing were less pronounced. 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

Establishment-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 establishments 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.[7] The survey contains detailed information on establishments' 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 establishments' size, industry classification, and organizational structure. Detailed data on each establishment's location of residence further allows the exact assignment of the applicable LBT in each year covered by the dataset. See Appendix A for more detailed information on this dataset.

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 establishments (6% of the total sample) that report R&D activities not only for their own establishment 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 establishments only. Second, we drop 283 establishments which moved during the survey period to exclude variation that is due to plants' potentially endogenous mobility decisions.[8]

Ultimately, our baseline sample contains 31,648 establishment-year observations in 2,442 municipalities. In total, these establishments spent around 37 billion EUR per year on R&D, which accounts for around 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 establishments 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 establishment. 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 sample, establishments' 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

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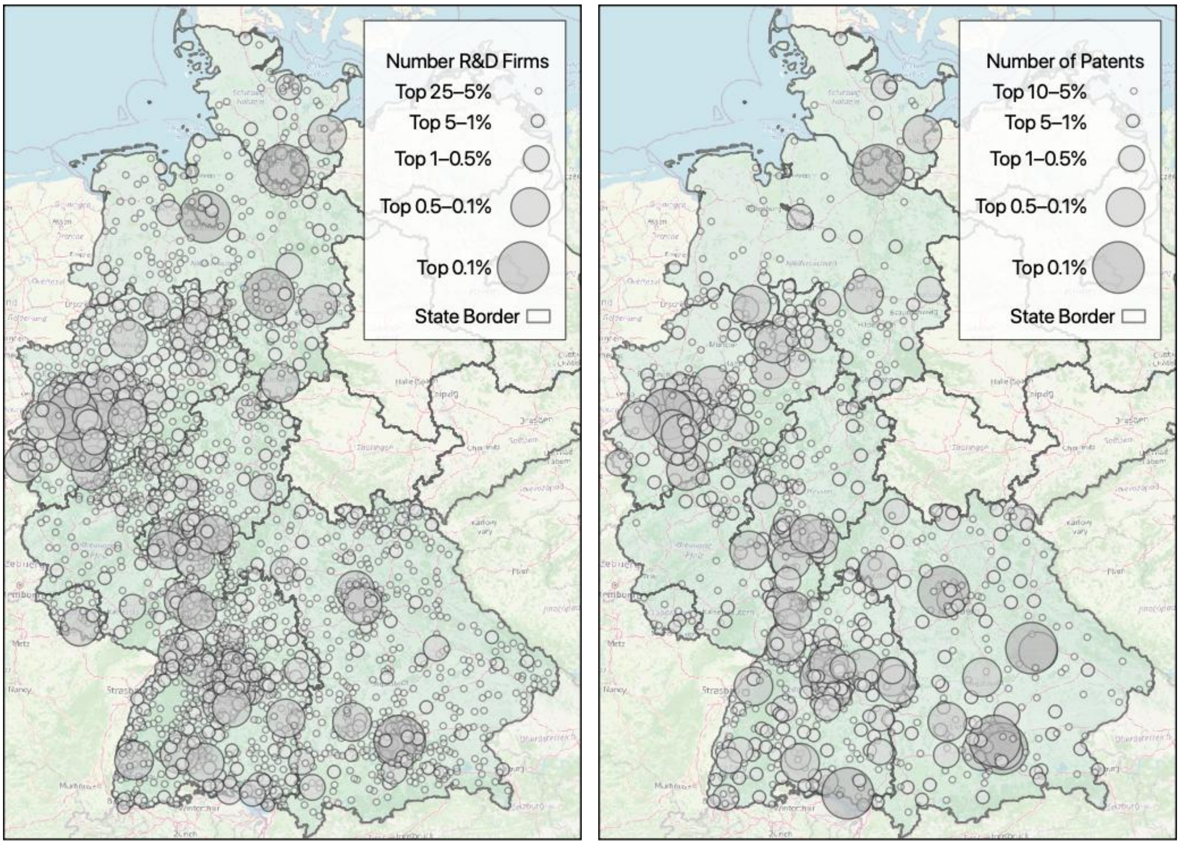
[7] The survey also acts as one source of the OECD's Analytical Business Enterprise Research and Development database (*ANBERD*), which has been used in related research (see, e.g., Bloom et al., 2002).

[8] We find very similar effects when including these establishments 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 establishments that change their location of residence actually relocate to municipalities with higher local business tax rates. The baseline estimation sample is unbalanced. We show below that results remain unaffected when using a balanced estimation sample.

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

A. Distribution of R&D Establishments B. Distribution of Patents



Notes: Panel A illustrates the distribution of establishments in the R&D Survey as of 2007 across West German municipalities. Larger circles indicate more R&D active establishments 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.

establishments' total R&D expenses along several margins. First, information on establishments' 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 by 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 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). Half of the establishments covered in our baseline sample outsourced parts of their R&D activities at least once during the sampling period (see Appendix Table B.1). On average, external R&D accounts for 9% of establishments' total R&D expenditures, and 20% if we consider establishments with non-zero external spending only. 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 establishments' internal expenses accrue to their scientific staff, who account for around 7% of establishments' total workforce.

Establishment-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 establishments filed 151,862 patents, which accounts for around 60% of all patents filed by German applicants at the EPO during this period.[9] 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 establishments.

However, the simple count of establishments' 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 establishment-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).

Using detailed patent data, we further distinguish different types of innovations. First, we identify those patents that are closely connected to science by focusing on patents that reference scientific publications. Such innovations are generally based on longer-term investments and more fundamental research. References to scientific publications are identified via front-page citation lists. Second, we distinguish product from process innovations using information from each patent's claim text following the procedure of Danzer et al. (2020).

Panel B of Appendix Table B.1 provides the corresponding descriptive statistics on establishments' patenting activities. The average establishment files 0.84 patents per year, which receives 1.7 citations over the following five years. However, the distribution of patents is notably skewed: the plant at the 75th percentile files zero patents per year and only around one quarter of all plants in the R&D Survey filed at least one patent during the sampling period. Of all patents in the baseline sample, 4% refer to science-based innovations and 54% are product innovations, respectively.

Financial Information. As hypothesized in Section 2.2, we expect credit-constrained establishments 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. Previous evidence by, e.g., Zwick and Mahon (2017), Guceri and Liu (2019), and Moon (2020) has indeed detected particularly strong (R&D) investment responses to tax incentives by financially-constrained firms. To investigate this link in our setting, we add information from Bureau van Dijk's *Amadeus* and *Orbis* databases to the survey. The two datasets offer a variety of financial information at the firm level, i.e., we assign firm-level financial information—foremost on the non-current liabilities to sales ratio—to establishments that are part of a multi-establishment firm. As both datasets predominantly cover larger and oftentimes stock-listed establishments, we can only supplement a subset of the surveyed R&D plants with additional information from this data source (see Panel C of Appendix Table B.1 for descriptive statistics). We show below that our baseline results are very similar when estimating the event study model separately for plants with and without this additional financial information.

[9] By definition, we do not capture patents filed by the government, public universities, or individual inventors. Moreover, not all establishments 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 establishments with very little or infrequent patent activity.

Administrative Regional Data. Last, we complement the establishment-level data with annual information on local business tax rates as well as other regional, i.e., municipality- and county-level characteristics. 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 establishment activities. Panel D of Appendix Table B.1 provides the corresponding descriptive statistics. We see that innovation predominantly occurs in urban, industrialized regions with relatively little unemployment.

4 Empirical Strategy

We derive the causal effect of business taxes on establishment-level R&D expenses and innovation output using a generalized event study design with staggered treatment (see, e.g., Suárez Serrato and Zidar, 2016, Fuest et al., 2018, Akcigit et al., 2022a). In Section 4.1, we detail the empirical implementation of this framework in light of the given institutional setting. In Section 4.2, we discuss the identification of causal effects using within-municipality variation in tax rates over time.

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 use all available variation in local tax rates within municipalities over time. Intuitively, we regress an outcome $Y_{i,t}$ of establishment i in year t belonging to sector s (manufacturing, services, and others) located in a municipality m and commuting zone z on leads and lags k of a treatment variable ($\underline{k} \leq k \leq \bar{k}$). In our baseline specification, the treatment variable $T^k_{m,t}$ is defined as follows:

$$T^k_{m,t} = \begin{cases} \tau_{m,2013} - \tau_{m,t-\underline{k}} & \text{if} \quad k = \underline{k} \\ \tau_{m,t-k} - \tau_{m,t-k-2} & \text{if} \quad \underline{k} < k < \bar{k} \\ \tau_{m,t-\bar{k}} - \tau_{m,1987} & \text{if} \quad k = \bar{k}, \end{cases} \tag{7}$$

where $\tau_{m,t}$ is the local business tax rate in municipality m and year t. Three things are noteworthy when looking at Equation (7). First, we need to account for the biennial structure of the R&D data. Hence, leads and lags of the treatment variable, $T^k_{m,t}$, relate to the total change in tax rates over two consecutive years. We observe outcomes in odd years ($t = 1995, 1997, \ldots, 2007$). Estimates are similar when using annual tax changes as explanatory variables. Second, treatment is defined as a dummy variable for a tax increase scaled by the size of the tax rate change.[10] Hence, we extend the standard event study design—relying on event dummy variables only—to exploit all available variation in local tax rates. This generalized event study model rests on the assumption that the effect of reforms is linear with respect to the size of the tax rate change. As a robustness check, we also estimate standard event study models using tax increase indicators only. As shown in Section 5.1, results are very similar. Third, at the endpoints of the effect window (for $k = \underline{k}$ and $k = \bar{k}$), the treatment

[10] As only 10% of all changes in the LBT during the relevant effect window were tax decreases, we exclude municipalities with tax cuts from the baseline analysis and focus on the effect of tax increases in the empirical analysis. Nevertheless, we also investigate the small subset of establishments in municipalities with tax decreases in separate analyses below.

variable takes into account all observable tax changes happening outside of the effect window—this practice, which is often referred to as binning, is particularly advisable in settings with multiple events per unit (McCrary, 2007, Schmidheiny and Siegloch, 2022).[11] The resulting event study model is given by:

$$Y_{i,t} = \sum_{k=\underline{k}}^{\bar{k}} \beta_k T_{m,t}^k + \mu_i + \theta_{z,t} + \zeta_{s,t} + \varepsilon_{i,t}. \tag{8}$$

We transform outcomes—R&D spending, the number of patents, and various subcategories of the two—using the inverse hyperbolic sine (IHS) transformation.[12] Establishment fixed effects (μ_i) account for unobserved time-invariant confounders at the establishment level. In our baseline specification, we exclude establishments that move during the observation period. When adding these establishments in a robustness check, we include municipality fixed effects in the regression model. 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 establishments, years, and sectors within municipalities.

In our preferred specification, we restrict the effect window to six years before and eights year after a tax reform, i.e., to three leads and four lags in the given two-year structure of the data, $k \in [-6, -4, \dots, 8]$. We normalize the last pre-treatment coefficient, β_{-2}, to zero such that all effects are relative to two years before treatment.

Heterogeneous Treatment Effects. Recent contributions by de Chaisemartin and D'Haultfœuille (2020), Callaway and Sant'Anna (2020), Sun and Abraham (2021), 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 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 de Chaisemartin and D'Haultfœuille (2020), Sun and Abraham (2021) and Borusyak et al. (2021) to this sub-sample and show that results remain similar when explicitly accounting for the potential presence of heterogeneous treatment effects across cohorts (see Section 5.1 for detailed results).

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 average over all post-reform treatment effect estimates, a measure of the medium-run

[11] The alternative would be to drop observations, which are outside of the effect window.

[12] 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 establishment-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.

effect. In Section 5, we compare these implied elasticities to alternative summary measures from the event study model as well as a simple two-way fixed effects regression model.

4.2 Identification

To estimate causal effects, we relate changes in establishments' R&D activities to changes in municipalities' 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 establishments' R&D expenses or innovation output. Small and insignificant pre-treatment coefficients in the event study 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 establishments' R&D activities—we should observe diverging trends in R&D investments *before* treatment. As shown below, pre-trends across the different event study specifications are indeed flat for the set of establishment-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. On average, there are eight municipalities and 25 establishments per commuting zone in our baseline sample. 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 2), the 71 statistical planning regions (*Raumordnungsregionen*, ROR), and the 272 counties (*Kreise und kreisfreie Städte*) in alternative specifications, respectively. Appendix Figure B.1 illustrates these different jurisdictions for the federal state of Bavaria for illustration. We find hardly any difference in pre-treatment effects when moving between specifications. Post-treatment estimates 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 establishment 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 (8) (see Appendix Figure B.2); a finding in line with previous studies (Fuest et al., 2018, Blesse et al., 2019). 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.

5 Empirical Results

In the following, we present our empirical results. In Section 5.1, we first investigate the overall effect of an increase in the local business tax rate on plants' overall R&D activity and test its sensitivity with regard to varying specification choices; providing the empirical test for *Hypothesis 1* as derived in Section 2.2. After having established our main effect of interest, we use the detailed establishment-level data to open the black box and analyze the underlying channels at work (Section 5.2). In particular, we assess which types of (i) R&D inputs, (ii) establishments, and (iii) research projects are affected the most by an increase in the LBT, relating our empirical findings to *Hypotheses 2 and 3*. Last, we discuss the implications of our findings for R&D tax policy design in Section 5.3.

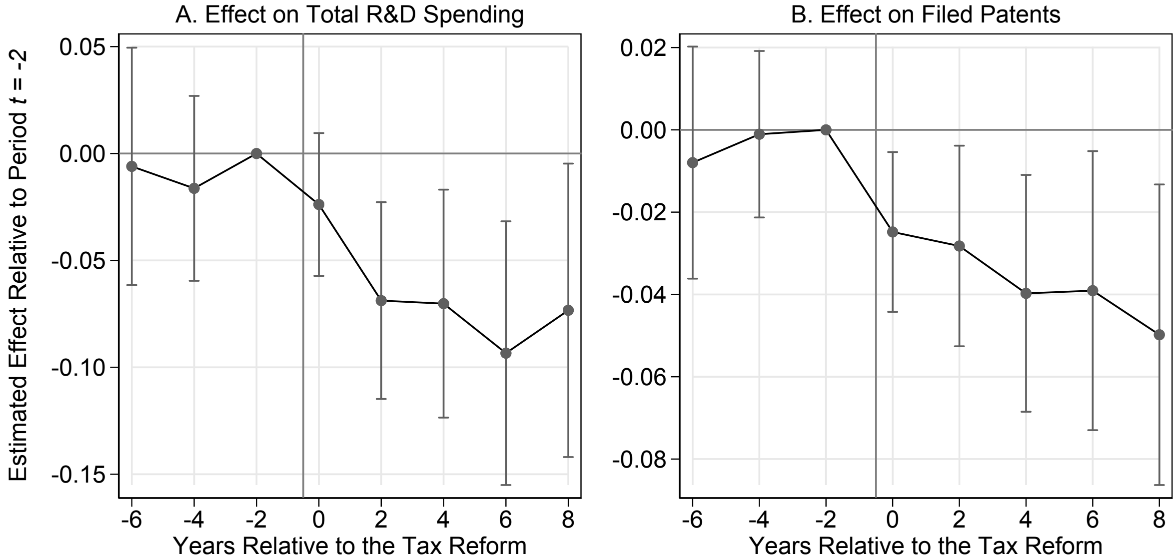
5.1 Main Effect on Overall R&D Activity

Panel A of Figure 4 presents the dynamic effect of an increase in the local business tax rate on establishments' total R&D spending based on the event study model as defined in Equation (8). Pre-trends are flat and statistically insignificant. Post-treatment, an increase in the local business tax rate exerts a substantially negative and statistically significant effect on establishments' total R&D spending in line with *Hypothesis 1*. The effect builds up over the first two years and remains constant thereafter. Quantitatively, a one percentage point increase in the local business tax rate leads to a decrease in R&D spending by around 8%, translating into a medium-run elasticity of -1.15 (standard error 0.41). In Appendix Figure C.1, we report alternative summary elasticity estimates of the effect size based on different lags of the event study model and by estimating a simple two-way fixed effects model using the (log) local business tax rate as the explanatory variable of interest.[13]

To better compare the magnitude of our baseline elasticity with estimates for other types of (R&D) tax policies, we further translate our estimate into an elasticity with respect to the user cost of capital. Following Yagan (2015), we calculate this elasticity as the ratio of the derived spending elasticity with respect to the business tax rate and the elasticity of the user cost of capital with respect to the tax rate.[14] The corresponding implied elasticity amounts to around -2.45; an estimate well in line

[13] As shown in Appendix Figure C.2, 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.
[14] Formally, the elasticity with respect to the user cost of capital ε_{CoC}^{Inv} is defined as: $\varepsilon_{CoC}^{Inv} = \varepsilon_{\tau}^{Inv} / \varepsilon_{\tau}^{CoC}$. The numerator ε_{τ}^{Inv}

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



Notes: Panel A plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$) and corresponding 95% confidence intervals of an increase in the local business tax rate on establishments' annual total R&D spending using the event study model as defined in Equation (8). Panel B plots the corresponding effects on establishments' annual number of filed patents. Both outcome variables are inverse-hyperbolic-sine transformed. For the treatment group, the business tax increase occurred in year $t = 0$ or $t = -1$. The regressions include establishment, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. Municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are clustered at the municipal level. The corresponding regression coefficients are provided in Appendix Table C.1.

with elasticities from recent studies analyzing the effects of targeted R&D tax policy instruments. For example, 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 an R&D 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.

In the following, we conduct several sensitivity checks to test whether baseline estimates reflect real firm-level behavior and are not driven by modeling choices.

Outcome-Based Measure: Effect on Patents. First, we validate our input-based measure of innovative activity with an outcome-based measure, obtained from an independent data source. Panel B of Figure 4 displays the effect of an increase in the LBT on establishments' number of filed patents. Pre-trends are flat and statistically insignificant. Post-treatment, we find negative and statistically significant effects, building up in the first four years after the tax increase and leveling off thereafter. The medium-run elasticity of the patent count with regard to the tax rate is -0.65 (0.22), an estimate close to the ones reported in Akcigit et al. (2022a). In Section 5.2, we make use

refers to the spending elasticity with respect to the business tax rate. The denominator ε_{τ}^{CoC} refers to the elasticity of the cost of capital with respect to the tax rate. In the German setting, the user cost of capital is given by $CoC = r/(1-\tau)$. We assume the pre-tax rate of return to amount to $r = 0.07$ 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 $\varepsilon_{\tau}^{CoC} = 0.47$.

of detailed information about each patent to analyze whether establishments selectively abandon specific types of R&D projects.

Standard Event Study Model. In our baseline specification, we leverage differences in the size of the tax rate increase for identification. A standard event study model relies on dummy variables indicating leads and lags of a tax increase instead of the two-year difference in the LBT rate. We compare both specifications to assess the sensitivity of our results. Estimates are very similar both in terms of treatment dynamics and effect size (see Appendix Figure C.3).

Time-Varying Confounders. We further re-estimate our baseline model with region-by-year fixed effects at different geographical levels (at the NUTS 2, 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.4 shows that effects are statistically significant irrespective of the level of region-by-year fixed effects. However, the estimates become slightly larger when controlling for local shocks at a finer geographical level. This suggests that local shocks bias coefficients towards zero. The observed stability of coefficients also addresses concerns about a potential violation of the 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 unaffected by the inclusion of these observable confounders (see Panel B of Appendix Figure C.4). In addition, we follow Oster (2019) and investigate potential biases due to unobserved time-varying confounders by systematically assessing coefficient stability in specifications with and without local lagged controls. The bias-corrected elasticity is close to our baseline summary estimate (see Appendix Figure C.1).

Functional Form of the Outcome, Inference, Event Window and Sample. We further note that point estimates remain unaffected by transformations of the outcome variable (see Panel C of Appendix Figure C.4). Moreover, allowing for correlation in the error term at broader regional levels than the municipality does not affect the significance of estimates (see Panel D of the corresponding figure). Effect patterns also remain unaffected when using different event window specifications (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. Last, we show in Panel F of Appendix Figure C.4 that estimates remain similar when using a balanced estimation sample or exploiting one-year instead of two-year tax rate differences (cf. Section 4.1).

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., de Chaisemartin and D'Haultfœuille, 2020, Sun and Abraham, 2021, Callaway and Sant'Anna, 2020, Borusyak et al., 2021). Our baseline estimation strategy may deliver biased estimates in case the dynamic impact of a given tax increase varies with the year of its implementation. Unfortunately, these alternative estimators do not allow for settings with multiple staggered treatments and dynamic 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 variation in tax rates from 1987–2013) and in order to arrive at a sufficiently large estimation sample, we thus shorten the effect window, restricting the sample to establishments 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.[15] Appendix Figure C.5 presents the corresponding effects. Two key findings become evident. First, we see that our baseline event study design yields similar effects when limiting the sample to municipalities with either zero or one tax increase within the given effect window. Second, the proposed estimators deliver notably similar results compared to our standard event study model. We conclude that our baseline results are unlikely biased by the presence of heterogeneous effects across treatment cohorts.

Firm Mobility. Last, we assess to what extent firm mobility drives our effects. Given that moving costs of R&D-intensive establishments should be quite substantial, it appears unlikely that changes in local business taxes induce substantial relocation responses of establishments. One plausible margin of adjustment is to relocate the R&D unit of one establishment to another establishment within the same firm. We investigate the presence of heterogeneous effects for multi- versus single-plant firms in detail in Section 5.2. In our sample, there are 283 establishments 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 establishments 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).

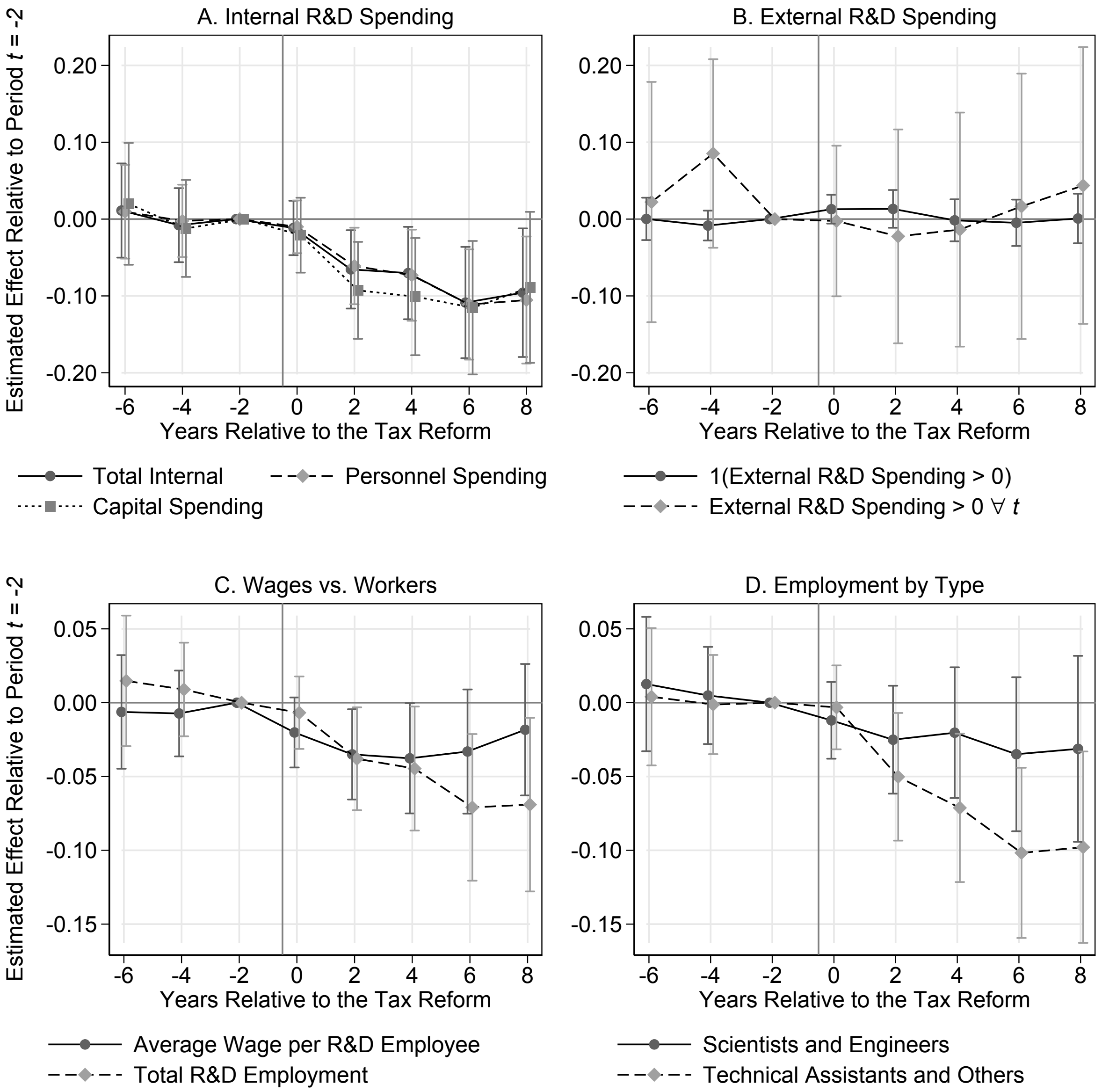
5.2 Firm-Level Mechanisms

Next, we explore the mechanisms behind the overall negative effect of an increase in the LBT on plants' R&D activity. In particular, we assess which types of (i) R&D input factors, (ii) establishments, and (iii) research projects are affected the most by an increase in the local business tax.

Effects by R&D Inputs. We first analyze the effect of an increase in the LBT on different R&D input factors. Following up on *Hypothesis 2*, we expect input factors to react more strongly to tax rate changes if they are easier to adjust at the margin. To this end, we first examine possible heterogeneous effects on internal versus external R&D spending. Firms can pursue R&D projects either at their own facilities or by contracting external institutions or companies. As detailed in Section 3, previous evidence has shown that external R&D is typically used as a strategy to acquire missing knowledge or exploit economies of scale but is also linked to sizable (transaction) costs and associated with long-term contractual arrangements, which may limit plants' responses to changes

[15] In detail, we base estimation on around 1,700 establishments 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 2 regions.

Figure 5: The Effect of a Business Tax Increase on Various R&D Inputs



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 Equation (8). In Panel A, the dependent variable refers to establishments' annual internal R&D spending as well as their internal personnel and capital/material expenditures, respectively. All outcomes are IHS-transformed. In Panel B, the dependent variable indicates whether establishments engage in external R&D as well as refers to the actual (IHS-transformed) external R&D expenditures for plants regularly contracting outside partners, respectively. In Panel C, we analyze the effect of an LBT increase on average annual wages per R&D employee and the stock of workers engaged in R&D. In Panel D, we test for heterogeneous effects across the R&D workforce and separately analyze effects on scientific and non-scientific R&D employees, respectively. All outcomes in Panels C and D are IHS-transformed. For the treatment group, the business tax change occurred in year $t = 0$ or $t = -1$. The regressions include establishment, state × year, commuting zone × year, as well as sector × 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. The corresponding regression coefficients are provided in Appendix Table C.1.

in the local business tax. In our baseline sample, around half of all plants display positive external R&D spending at some point. However, only around 20% of all plants covered contract external partners on a regular basis.

Panels A and B of Figure 5 present the corresponding event study estimates. From Panel A we infer that an increase in the LBT leads to sizable reductions in plants' internal R&D expenditures, the corresponding medium-run elasticity amounting to -1.25 (0.46). By contrast, we find no effect on the probability of contracting external firms or institutions (extensive margin). The same is true when looking at the level of external R&D spending for the subset of plants that regularly contract outside parties to conduct R&D on their behalf (intensive margin). We take these findings as suggestive evidence in line with *Hypothesis 2*.[16]

Panel A further highlights that the estimated reduction in internal R&D spending is due to cuts in expenditures for both personnel and capital/materials; a finding in line with evidence by Curtis et al. (2021). To better assess the relative contribution of the adjustment margins, we evaluate the effects for the median plant in our dataset: The median plant would cut R&D expenditures by around EUR 40,000 in response to a one percentage point increase in the LBT rate. Roughly 70% of this cut comes from reductions in personnel costs, the remaining 30% from reductions in capital expenses. As the median plant has hardly any external R&D expenses, we ignore this margin here.

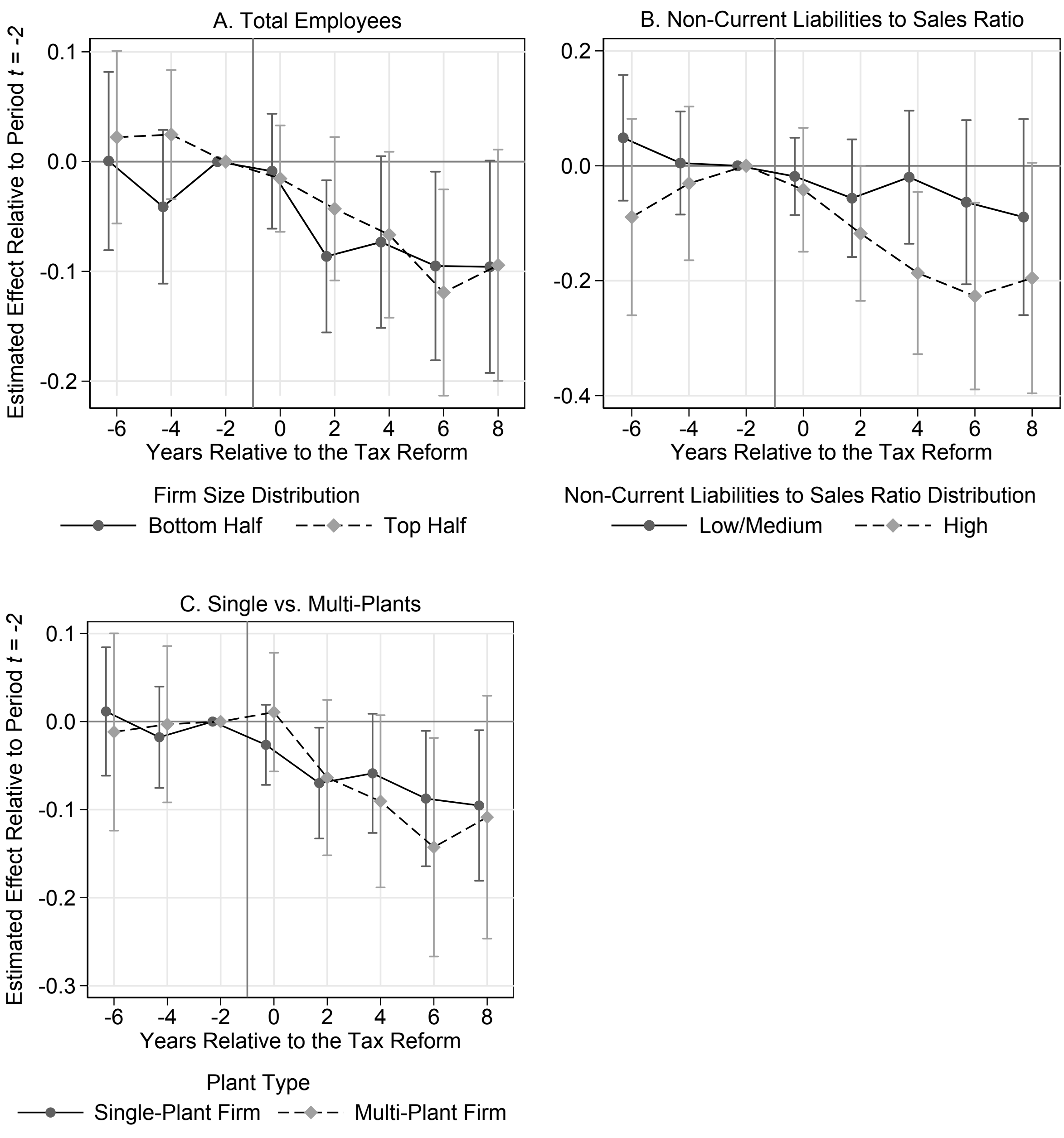
Panel C of Figure 5 further demonstrates that both employment and wages react to an increase in the local business tax rate. We find a negative effect on the average wage for employees conducting R&D—a finding line with evidence by Fuest et al. (2018)—as well as an effect on establishments' R&D staff. The latter one is quantitatively more important: around 75% of the overall reduction in spending on R&D personnel can be linked to reduced employment, the remaining 25% to lower wages. In Panel D of the respective figure, we investigate which types of workers engaged in R&D are most affected by a tax increase. Our estimates suggest that establishments cut back on technical and support staff rather than scientists and engineers, for whom we find only small and statistically insignificant effects. We take this finding as suggestive evidence that an increase in the LBT leads to a change in the composition of plants' R&D workforce—a finding that may also explain the slightly reverting effect on average wages in Panel C. The differential effect of a tax increase on plants' workforce may in turn be linked to varying adjustment costs for low- and high-skilled employees in line with *Hypothesis 2*, e.g., because of differential firm-specific human capital for both types of workers that make layoffs of scientists and engineers more costly.

Effects by Plant Characteristics. We next test for heterogeneous effects among different types of establishments. In light of the evidence presented above, we focus on plants' internal R&D spending as outcome variable of interest.

In many countries, eligibility for targeted R&D tax incentives is linked to firm size; partly because of policy makers' beliefs that small- and medium-sized firms are more responsive to a given level of support, e.g., due to more severe financing constraints (Gonzales-Cabral et al., 2018). The

[16] At first sight, these findings appear at odds with recent evidence by Agrawal et al. (2020), who analyze the effects of targeted R&D tax incentives for a subset of Canadian firms and find stronger responses on externally-conducted R&D. However, the firms under investigation are substantially smaller than ours (factor 200 in terms of average sales)—such that the costs of starting a new R&D project in-house may be larger than contracting external partners to conduct R&D.

Figure 6: LBT Increases and Total R&D Spending – Effects by Plant Characteristics



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 defined in Equation (8) when allowing for heterogeneous treatment effects by establishment characteristics. In Panel A, we allow for different responses along the establishments' size distribution in terms of employment. In Panel B, we test for differential effects among establishments with and without credit constraints using establishments' non-current liability to sales ratio as a proxy. In Panel C, we allow for heterogeneous treatment effects for single- vs. multi-plant firms. All regressions include establishment, state × year, commuting zone × year, sector × year, as well as group × year fixed effects. We exclude all municipalities that experience a tax decrease during the observation period. Standard errors are clustered at the municipal level. The corresponding regression coefficients are provided in Appendix Table C.2.

underlying idea is as follows: as the costs of equity financing cannot be deducted from the tax base, establishments' financing decisions are distorted towards debt (see *Hypothesis 3*). If smaller firms are, on average, more prone to financing constraints, they might indeed benefit more from targeted R&D tax policies. In Panel A of Figure 6, we therefore investigate whether effects of an increase in the LBT affect smaller and larger plants to a different extent. To implement this test, we cut the sample at the median number of employees and test for heterogeneous effects by plant size.[17] We find no evidence for heterogeneous responses among smaller versus larger plants. Both types of establishments react notably similar to an increase in the LBT, the respective medium-run elasticities amounting to -1.26 (0.59) and -1.19 (0.58), respectively.

However, firm size may be a poor proxy for plants' financial constraints. We thus make use of a more explicit measure of plants' financial situation: their non-current liabilities to sales ratio. Establishments exhibiting a higher non-current liabilities to sales ratio should face higher interest rates and, correspondingly, finance a smaller share of their R&D expenditures via debt. This should, in turn, lead to stronger spending responses in case of an increase in the LBT. Panel B of Figure 6 presents the corresponding effects. We find supportive evidence that those plants that are particularly credit-constrained indeed react significantly stronger than financially-unconstrained ones, especially in the medium- to long-run. The respective estimated treatment effects for both groups four to eight years after the tax reform are jointly statistically significantly different from each other (p-value 0.08). We take this result as suggestive evidence in line with *Hypothesis 3* and 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. In Appendix Figure C.7, we show that the same conclusions arise when using the raw patent count instead of internal R&D spending as the outcome variable: treatment effects for smaller and larger plants are similar, whereas financially-constrained plants react stronger than unconstrained ones.[18]

Last, we test for heterogeneous responses by single- versus multi-establishment firms. If the tax-induced reductions in R&D activities were merely driven by multi-establishment firms, estimated effects might be due to the spatial reallocation of R&D activities across establishments within a given firm instead of an absolute reduction in innovative activity. Among others, Dischinger and Riedel (2011), Karkinsky and Riedel (2012), and Griffith et al. (2014) provide evidence for profit shifting via intangible assets such as patents in the context of cross-country differences in corporate tax rates. As displayed in Panel C of Figure 6, we find that both multi-establishment and single-establishment firms exhibit a similar reduction in their internal R&D expenditures in response to an increase in the local business tax rate. Treatment effects for both groups are not statistically significantly different from each other. As displayed in Panel C of Appendix Figure C.7, the same pattern holds true for the raw patent count. Hence, higher taxation leads to a real reduction in plants' R&D activity. As less than one-third of plants in our estimation sample are part of multi-establishment firms, we abstain from testing for heterogeneous effects for domestically- versus internationally-owned

[17] Technically, we interact the size group dummy with all leads and lags of treatment and add group $\times$ year fixed effects to account for differential trends across the two types of plants.

[18] Note that information on plants' non-current liabilities is taken from the Bureau van Dijk *Amadeus* and *Orbis* datasets and only available for a subset of surveyed plants. To test for potential selection effects, we re-estimate the event study model separately for establishments with and without data on plants' financial situation. We find very similar baseline effects when restricting the sample accordingly (see Appendix Figure C.8).

multi-plant firms due to the lack of statistical power. The survey also does not allow identifying those establishments that belong to the same firm, which deters us from investigating whether multi-establishment firms shift R&D activity across locations.

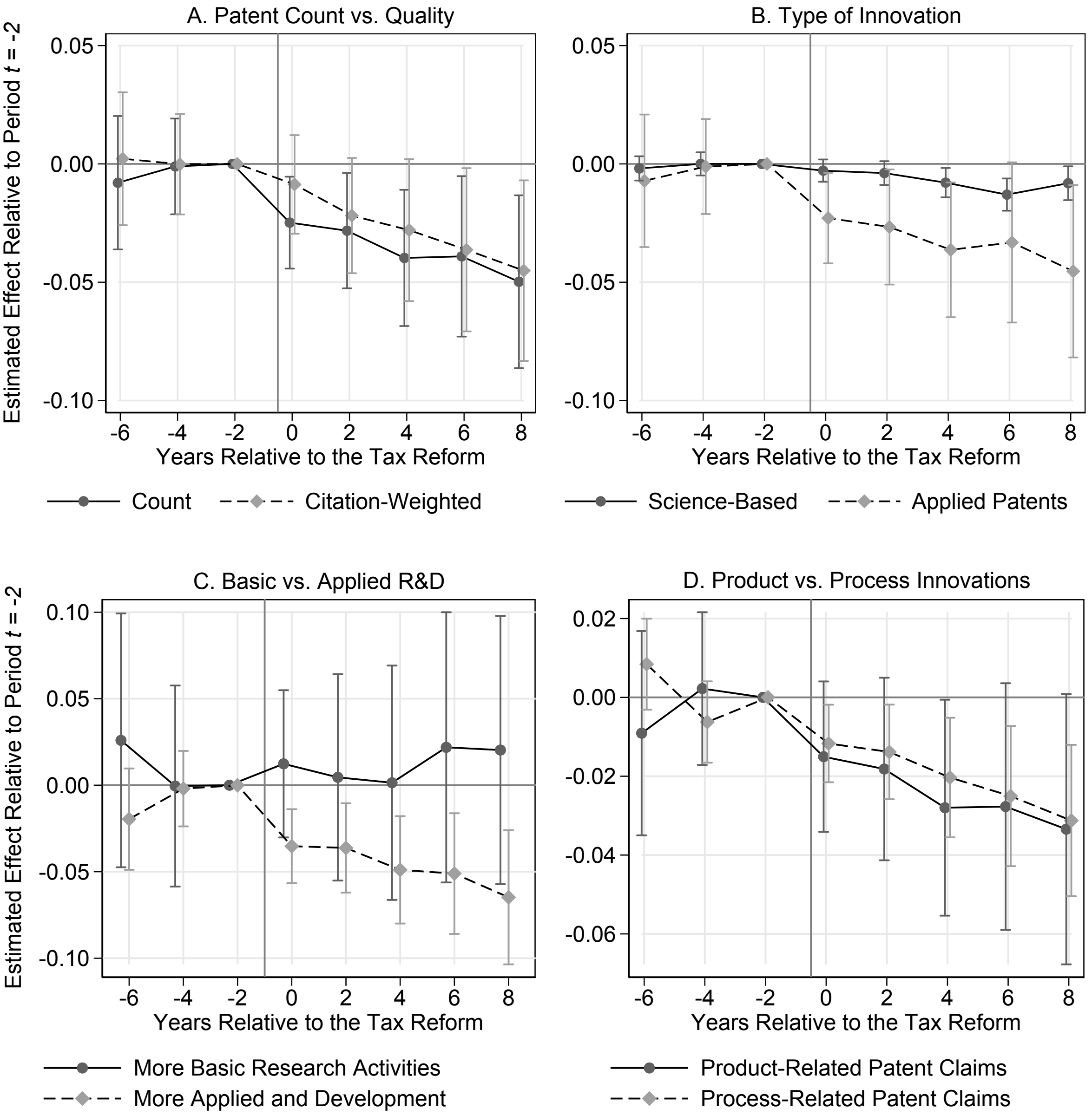
Effects by Project Types. In a final step, we analyze whether plants abandon specific types of R&D projects in response to an increase in the LBT. To do so, we use rich patent data and test for heterogeneous effects among different kinds of innovations. We first compare the estimated treatment effects on the raw patent count to the citation-weighted number of filed patents because the economic value of patents varies substantially (see, e.g., Scherer, 1965, Hall et al., 2005). If plants only abandon marginal R&D projects in response to a tax increase, we might see a reduction in plants' quantity of patents but no effect on innovation quality. By contrast, if plants actually cancel high-risk projects with potentially high returns, we might find even stronger negative effects on the number of citation-weighted patents. As shown in Panel A of Figure 7, we detect little heterogeneity along this margin. Effects are similar in terms of effect pattern and size. If anything, the effect on the citation-weighted number of patents comes with some temporal lag.

In the next step, we investigate the impact of an increase in the LBT on patents that are closely connected to scientific research, identified as patents that cite scientific publications as prior art. These patents represent innovations that are generally based upon longer-term investments and more fundamental research. Such science-based patents are, on average, more valuable than patents of more applied nature, both in technological and commercial terms (Ahmadpoor and Jones, 2017). As displayed in Panel B of Figure 7, we only find a very small reduction in the number of science-based patents (compared to the corresponding effect on applied patents). Thus, plants appear to selectively cut back on innovation projects with relatively minor scientific advances, whereas long-term and more fundamental innovations are less prone to expense reductions.

Panel C of Figure 7 corroborates this finding. Focusing on plants' total number of filed patents, we allow for heterogeneous effects among establishments with a varying focus of their R&D department. The overall treatment effect on patents appears to be driven by those establishments relatively more engaged in applied research and development. By contrast, plants that have a particularly strong focus on basic research—those in the upper quartile of the respective distribution—see little reduction in the filed number of patents. Estimated treatment effects four to eight years after treatment are statistically significantly different for both groups (p-value 0.07). Results presented in Panels B and C can be reconciled with *Hypothesis 2*: longer-run, more involved projects, which are more costly to adjust at the margin, are less responsive to business tax increases.

Last, we find little heterogeneity when distinguishing process from product innovations (see Panel D). 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 the more incremental ones with limited social returns (Klepper, 1996). Overall, we take these findings as tentative evidence that the explicit purpose of R&D appears to drive plants' innovation response to a given tax increase. Plants that predominantly use R&D expenditures as an investment to obtain new knowledge and technologies, i.e., invest more in basic research, appear to respond less than those that invest more in applied R&D.

Figure 7: The Effect of a Business Tax Increase on Different Project Types



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 Equation (8). In Panel A, the dependent variable refers to plants' total annual number of filed patents measured as the simple count as well as the count weighted by each patent's number of citations, respectively. In Panel B, we separately estimate the effects on the annual number of science-based and applied patents. In Panel C, we focus on the simple patent count as an outcome but allow for heterogeneous effects among firms with a weaker/stronger focus on basic R&D by interacting all treatment variables with the respective dummy variable (and adding group $\times$ year fixed effects). Last, in Panel D, the dependent variable refers to plants' annual number of product and process innovations, respectively. All outcomes are inverse-hyperbolic-sine transformed. For the treatment group, the business tax change occurred in year $t = 0$ or $t = -1$. All regressions include establishment, 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. The corresponding regression coefficients are provided in Appendix Tables C.2 and C.3.

5.3 Implications for R&D Tax Policy

In this section, we discuss the implications of our empirical results for R&D tax policy. The set of derived results may be interpreted in favor of business tax cuts as a policy tool to foster firm-level innovation. However, it remains to be assessed whether such a reform would be efficient relative to other policy instruments, such as targeted R&D (tax) policies. Our estimates imply that R&D expenses decrease by around 40,000 EUR for the median R&D-active establishment in case of a one percentage point increase in the local business tax rate. Assuming symmetric effects of tax increases and decreases, this implies that governments might raise firm-level R&D expenditures by 0.34 EUR when giving up 1 EUR of local business tax revenue. This cost efficiency estimate is rather low compared to estimates for targeted 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 appears plausible as changes in general business taxes also affect those firms not engaged in R&D. In Germany, only around four percent of all manufacturing plants report R&D activities. Consequently, boosting plant-level innovation via reduced business taxes might be relatively costly. Moreover, the assumption of symmetric responses to tax increases and decreases might be too strong because the reduction of R&D activities might be easier than their expansion, which makes our cost efficiency measure an upper bound.

Our results thus support the common practice of combining a general profit tax to raise revenues with targeted tax policies to stimulate R&D. To date, these targeted R&D policy instruments typically target smaller firms (in terms of sales or employment), partly because of policy makers' implicit beliefs that credit market frictions make smaller firms more liquidity-constrained and hence more sensitive to tax policy (Gonzales-Cabral et al., 2018). The results of our study, however, suggest that size might be a poor proxy for plants' financial constraints. We find no differential response to an increase in the LBT rate for smaller vs. larger plants. By contrast, when using a more precise proxy of plants' financial situation—the non-current liabilities to sales ratio—we find the expected pattern: plants subject to liquidity constraints reduce their R&D expenses and patenting activities more strongly than unconstrained ones.

Our conclusion that (R&D) tax policies should not be based on firm size lines up with recent empirical and theoretical evidence in related contexts. Curtis et al. (2021) study the effect of accelerated depreciation rules on firms' investment decisions and do not find significantly different effects by firm size either. Using a Schumpeterian growth model and data from Norway, 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. Akcigit et al. (2022b) rather show that, from a theoretical perspective, optimal targeted R&D policies shall be directed towards the most R&D-productive firms.

6 Conclusion

In this paper, we study the effect of business taxes on establishments' R&D spending and innovation. We exploit the institutional setting of local business taxation in Germany, where municipalities autonomously set the applicable tax rate on an annual basis. Using survey data targeting the universe of R&D-active establishments in Germany and more than 7,300 changes in the local tax rate, we employ event study techniques for the identification of causal effects.

We show that an increase in the local business tax rate has a statistically significant negative effect on establishments' overall R&D expenditures. Estimates are quantitatively sizable, the medium-run elasticity amounting to -1.15. Drawing upon the rich nature of the survey, we further decompose the overall effect along various margins. We show that it is almost entirely driven by reductions in plants' internal R&D expenditures. We do not find any effect on plants' external R&D expenditures. In turn, tax-induced reductions in internal R&D expenses stem from cuts in spending on both R&D personnel and materials/capital, suggesting that both inputs are complements in R&D production. The effect on personnel spending is predominantly driven by a reduction in plants' R&D headcount rather than wages. In terms of R&D employment, plants predominantly reduce the stock of their technical and support staff. The stock of employed scientists and engineers remains unaffected. We further uncover varying effects for different types of establishments. Whereas estimated treatment effects are very similar for smaller and larger establishments, we find that financially-constrained plants react stronger to an increase in the local business tax rate than unconstrained ones.

The tax-induced reduction in R&D spending is accompanied by a (lagged) decline in innovation output as measured the number of filed patents. We derive a medium-run elasticity with respect to the applicable local business tax rate of -0.65. Drawing upon rich patent data, we further provide suggestive evidence that the explicit purpose of plants' R&D activities appears to drive plants' innovation response to a given tax increase. Plants that predominantly use R&D expenditures as an investment to obtain new knowledge and technologies, i.e., invest relatively much in basic research, appear to respond less than those that invest more in applied R&D.

The results of this study have implications for tax policy setting. We show that business taxes affect plants' R&D spending and innovation. However, the derived cost effectiveness of lowering business taxes to boost firm-level R&D is relatively modest compared to specific R&D tax policies such as tax credits or deductions. Hence, our results support the common practice of combining a profit tax to raise revenues with specific tax policies to stimulate firm-level R&D. Yet, our findings question the common feature of using firm size as a characteristic to target liquidity-constrained firms.

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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 descriptive statistics.

Establishment-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. It forms the basis of Germany's official reporting of its entrepreneurial R&D activities to EU authorities and the OECD.

The Stifterverband maintains a continuously updated register targeting the universe of all research-active establishments in Germany. In general, the survey's methodology follows the methodological recommendations for the collection and interpretation of R&D data of the OECD.[19] The target population contains all researching and developing firms as well as institutions for collaborative research based in Germany with at least one employee. A firm is included in the survey if it is known that it was or is R&D-active or if this can be assumed with a certain degree of probability. To this end, new firms are added to the inventory if there is a reasonable chance that they are conducting R&D. Candidates are identified through the regular screening of different sources: federal funding data, the CORDIS database of the European Commission, firm information (financial statements and annual reports), media information, commercial business databases (e.g., the Markus database), patent applications and member lists of business associations with an innovative focus of business activity.[20] To further ensure the comprehensiveness of the firm register, regular surveys are conducted among firms 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 firms in Germany is indeed covered by the survey dataset.

The sketched methodology results in an unbalanced, representative dataset covering the majority of German R&D-active firms. During the period of our analysis, spanning from 1995 to 2007, the establishments covered in our baseline sample accounted for around three-quarters of Germany's total R&D spending. The survey itself is conducted at the establishment level and covers detailed information on establishments' 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 establishment size (in terms of sales and employees), industry classification, and the establishment's organizational structure (single- vs. multi-establishment firms). By special agreement with the Stifterverband, we also gained access to each establishment's exact address (postal code and location), which allows us to assign the applicable local business tax rate (treatment) in a precise way. Panel A of Appendix Table B.1 provides detailed descriptive statistics for each variable used in the analysis.

[19] Frascati Manual, `https://ec.europa.eu/eurostat/ramon/statmanuals/files/Frascati_Manual_2015_de.pdf`

[20] Among others, these associations encompass the *Bundesverband der Energie- und Wasserwirtschaft e.V*, *Forschungskreis der Ernährungsindustrie e.V.*, *Verband der Chemischen Industrie e.V.*, *Verband der Automobilindustrie e. V.*, *Verband Deutscher Maschinen- und Anlagenbau e.V.*, *Verband Forschender Arzneimittelhersteller e.V.*, *Zentralverband der Elektrotechnik und Elektroindustrie e. V.*, *Hauptverband der Deutschen Bauindustrie e.V.*, and *BITKOM e.V.*.

Patent Data. To assess the impact of profit taxation on innovation output, we link administrative information on establishments' patenting activity from the *European Patent Office* (EPO, *PATSTAT* dataset as of 4/2016) to the R&D survey. As establishments 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 establishment-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 establishment where the initial invention occurred, not the current IP holder. We next drop all patent applications that have not been (co-)filed by an establishment (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 establishment-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 establishment. Overall, the surveyed establishments account for around 60% of all patents filed by a German applicant during the covered period.

As the value of patents differs substantially (Scherer, 1965, Hall et al., 2005), we focus on both the simple patent count and an outcome measure 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.[21] 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). We further identify science-based patents using data from Poege et al. (2019), who match information on references to prior art other than patents to the Web of Science, a database of scientific publications. Science-based patents are those that reference at least one scientific publication on the patents' front page. Relying on data from Danzer et al. (2020), we also 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 establishments' innovation output. Note that some patent applications (12%) do not provide enough information to classify a patent as a product or process innovation. Excluding these patents from the baseline regressions does not affect estimates.

Financial Establishment-Level Data. While the Stifterverband data provide detailed information on establishments' R&D activities, the survey offers few insights on establishments' financial situation.

[21] 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.

To test for heterogeneous effects among more or less credit-constrained establishments, we link additional information from the Bureau van Dijk's (BvD) *Amadeus* and *Orbis* databases to the surveyed establishments.[22] 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, i.e., we assign the firm-level financial information to establishments that are part of a multi-establishment firm. As the BvD datasets predominantly cover larger and oftentimes stock-listed establishments or firms, we cannot match all surveyed establishments to the BvD data.

To prepare the BvD data for the purposes of our study, we follow Kalemli-Özcan 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 establishment appears in more than one vintage, we follow Gopinath et al. (2017) and take those information from the most recent vintage. When multiple financial accounts are available for a given establishment in a given year, we always refer to accounts with higher quality. Here, we prefer those accounts that cover the full twelve months of a given year. Moreover, we prefer accounts in accordance with IFRS guidelines over GAAP accounts or those with unknown reporting standards. Last, we choose unconsolidated over consolidated accounts. In the empirical analysis, we measure establishments' liquidity constraints via their non-current liabilities to sales ratio; Panel A of Appendix Table B.1 provides the corresponding descriptive statistics.

Regional Admin Data. Information on local business tax scaling factors (*Realsteuerhebesätze der Gewerbesteuer*) for all West German municipalities was collected 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.

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 is available for the entire effect window (1987-2013) and was 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. Data for the period from 1987 to 1999 are based on data requests we filed to the Statistical Offices of the German States. Data on population levels from 2000 onward are publicly available via the annual German municipality register (*Gemeindeverzeichnis*).

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

Last, we collect information on the number of unemployed individuals per municipality for the period 1998 to 2013 from the annual 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.

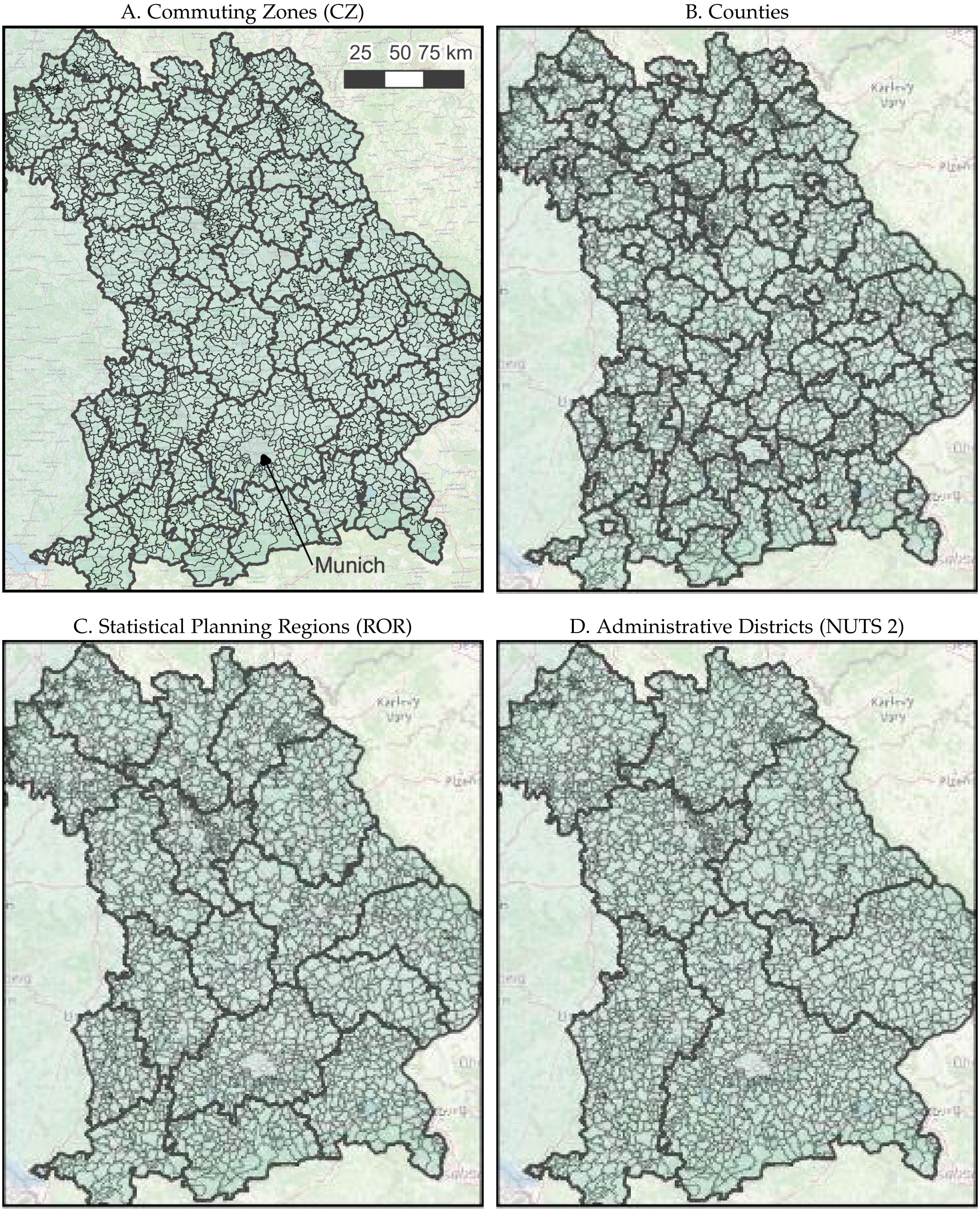
B Descriptive Statistics, Correlations and Definitions

Table B.1: Descriptive Statistics for Baseline Sample

	Mean	Std Deviation	5% Pctl.	25% Pctl.	75% Pctl.	95% Pctl.	Observations
A. Plant-Level R&D Survey							
R&D Spending Levels (in Thousand EUR)							
Total R&D Spending	9,292.7	103,105.2	47.0	208.0	2,205.5	18,630.0	32,600
Internal R&D Spending	7,782.1	85,423.6	30.0	183.0	1,977.0	16,492.5	32,600
External R&D Spending	1,510.6	26,107.9	0.0	0.0	88.0	1,406.5	32,600
Internal Spending on R&D Personell	4,658.6	46,356.8	18.0	119.0	1,262.5	10,451.5	32,600
Internal R&D Spending on Non-Personell	3,123.5	42,008.8	4.0	45.0	624.0	5,963.0	32,600
Spending Shares (in %)							
Share of Internal R&D Expenses	91.0	18.3	52.7	90.0	100.0	100.0	32,481
Share of External R&D Expenses	9.0	18.3	0.0	0.0	10.0	47.3	32,481
Share of Internal R&D Expenses for Scientific Staff	67.1	16.6	36.9	60.0	78.4	93.4	32,013
Share of Internal R&D Expenses for Non-Personell	32.9	16.6	6.6	21.6	40.0	63.1	32,013
Share of Basic R&D Expenses	4.2	9.1	0.0	0.0	5.1	20.0	32,280
Other Plant Characteristics							
No. of Employees	573.2	3,186.8	8.0	38.0	340.0	1,800.0	32,575
No. of Employees Conducting R&D	41.4	333.5	1.0	2.0	15.0	107.0	32,493
No. of Scientists and Engineers	19.6	161.7	0.0	1.0	7.0	47.9	32,531
No. of Technical Assistants and Others	21.7	190.4	0.0	1.0	7.6	57.0	32,531
Sales (in Million EUR)	233.2	1,806.4	1.0	7.0	86.0	627.0	32,394
R&D Spending per Employee (in Thousand EUR)	23.9	349.6	0.5	2.6	15.5	60.7	32,575
Manufacturing Sector	0.8	0.4	0.0	1.0	1.0	1.0	36,829
Service Sector	0.1	0.3	0.0	0.0	0.0	1.0	36,829
Other Sector	0.1	0.2	0.0	0.0	0.0	1.0	36,829
B. Plant-Level Bureau van Dijk Data							
Firm Age Since Year of Establishment	29.8	28.0	5.0	13.0	34.0	90.0	21,103
Non-Current Liabilities to Sales Ratio	0.6	9.1	0.0	0.1	0.4	0.9	6,112
C. Plant-Level EPO Patent Data							
Number of Patents	0.78	7.34	0.0	0.0	0.0	3.0	36,829
Citation-Weighted Number of Patents (EPO)	0.91	9.75	0.0	0.0	0.0	3.0	36,829
Number of Science-Based Patents	0.08	1.52	0.0	0.0	0.0	0.0	36,829
Number of Applied Patents	0.70	6.18	0.0	0.0	0.0	2.8	36,829
Number of Process Innovations	0.15	2.08	0.0	0.0	0.0	0.5	36,829
Number of Product Innovations	0.55	4.75	0.0	0.0	0.0	2.0	36,829
D. Local Characteristics							
Local Business Tax Rate	17.3	1.9	15.0	16.0	18.3	21.3	12,543
Δ LBT Rate (If Increased)	0.9	0.5	0.3	0.5	1.0	1.8	2,198
Population	25,760.1	73,464.9	1,788.0	5,327.0	21,878.0	77,596.0	12,543
Log Unemployment Per Capita	3.4	1.3	1.7	2.4	4.2	5.9	9,209
Expenses (in Thousand EUR)	679.5	3,561.3	26.4	85.1	384.6	1,822.1	9,210
GDP per capita	28,094.3	10,167.9	18,827.0	22,597.4	30,404.6	43,619.1	12,011

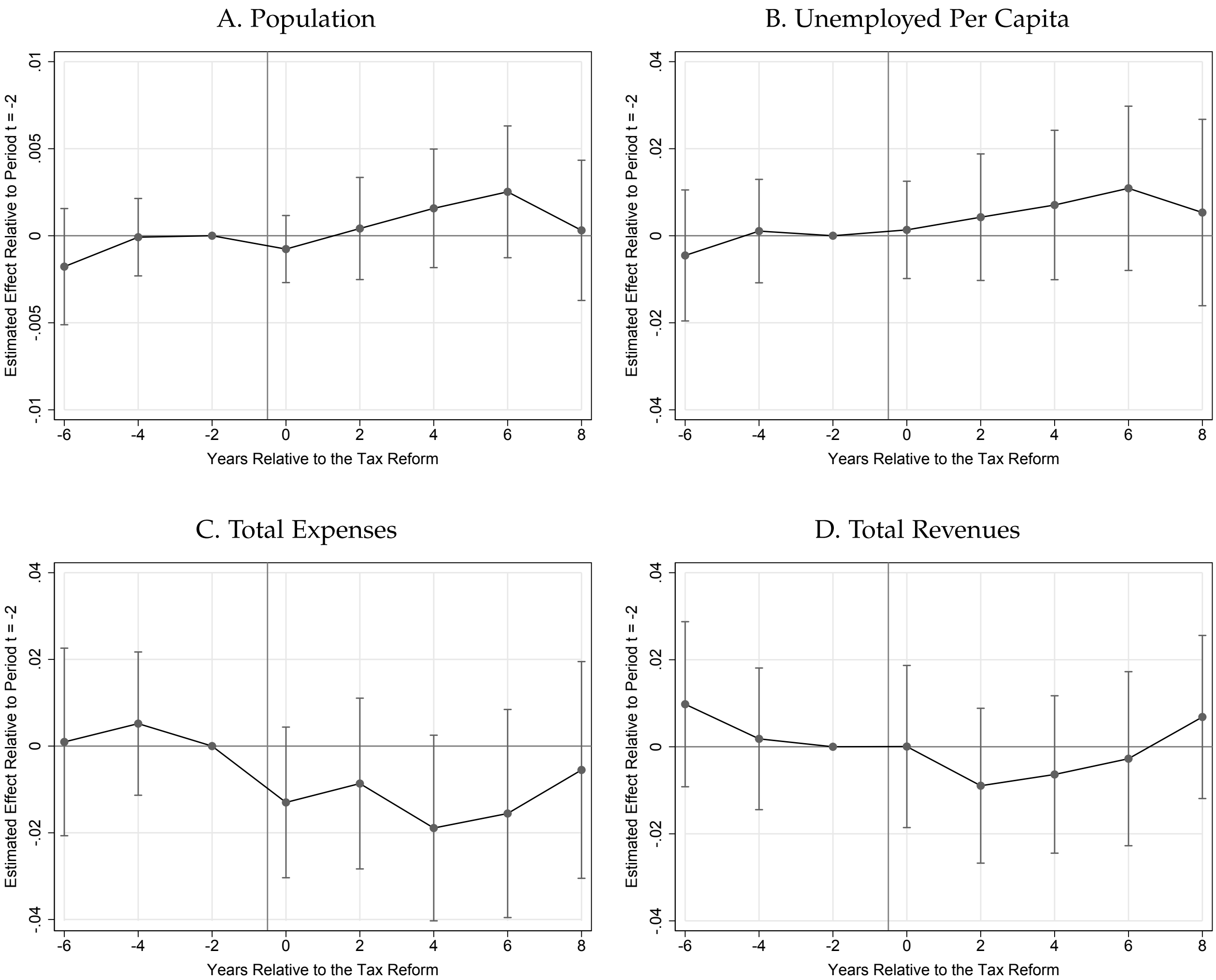
Notes: This table presents descriptive statistics for the variables used in the empirical analysis.

Figure B.1: 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 2), respectively, which are geographical aggregations of commuting zones. *Maps:* © GeoBasis-DE / BKG 2015, OpenStreetMap contributors.

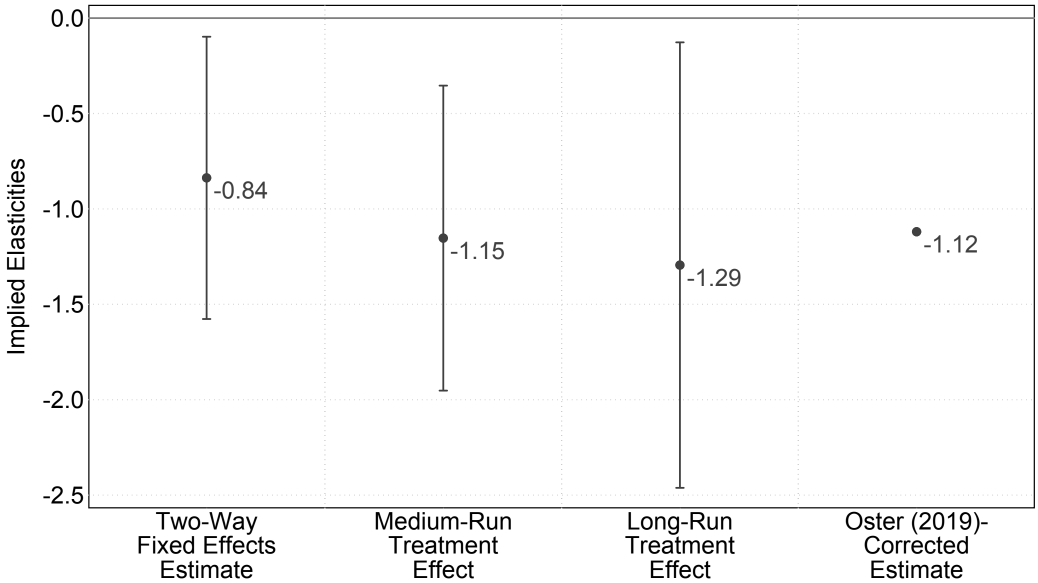
Figure B.2: The Effect of a Tax Rate Increase on Municipality-Level Outcomes



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 equation (8). 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 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.

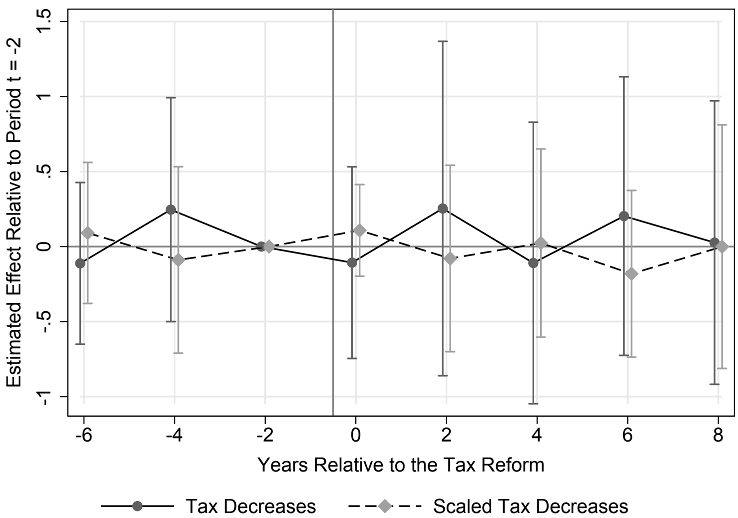
C Additional Estimates

Figure C.1: Implied Elasticities – Total R&D Spending



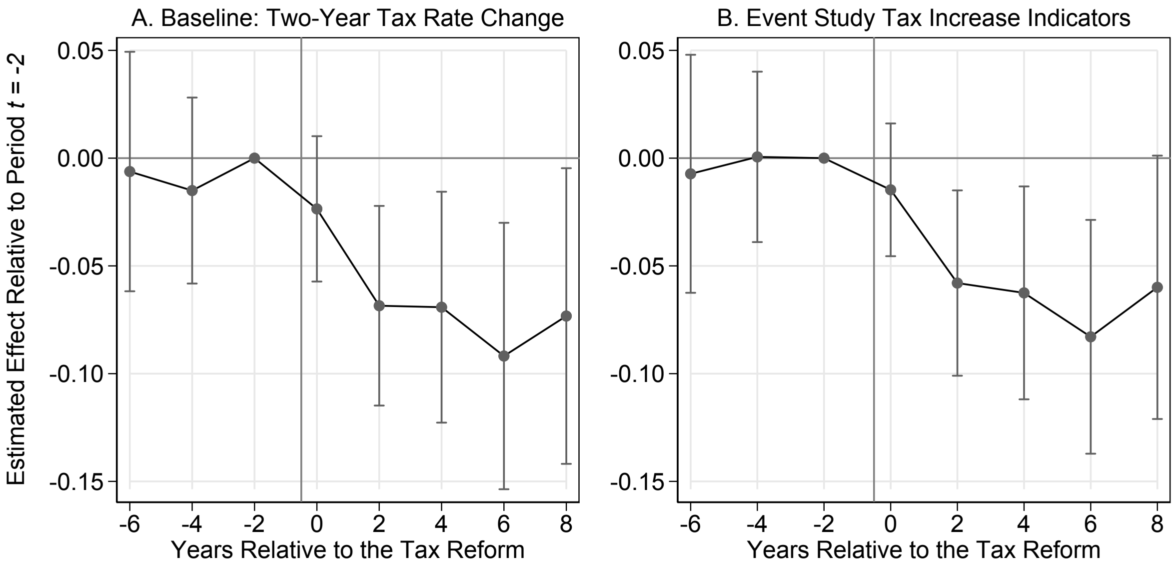
Notes: This graph displays different implied elasticities of establishments' total R&D spending in response to an increase in the local business tax rate. We calculate elasticities (i) using a simple two-way fixed effects model with the log local business tax rate as the explanatory variable, (ii) taking the mean over treatment effects $\widehat{\beta_0}$–$\widehat{\beta_8}$ from the event study specification as defined in Equation (8) and shown in Panel A of 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.

Figure C.2: The Effect of a Tax Decrease on Total R&D Spending



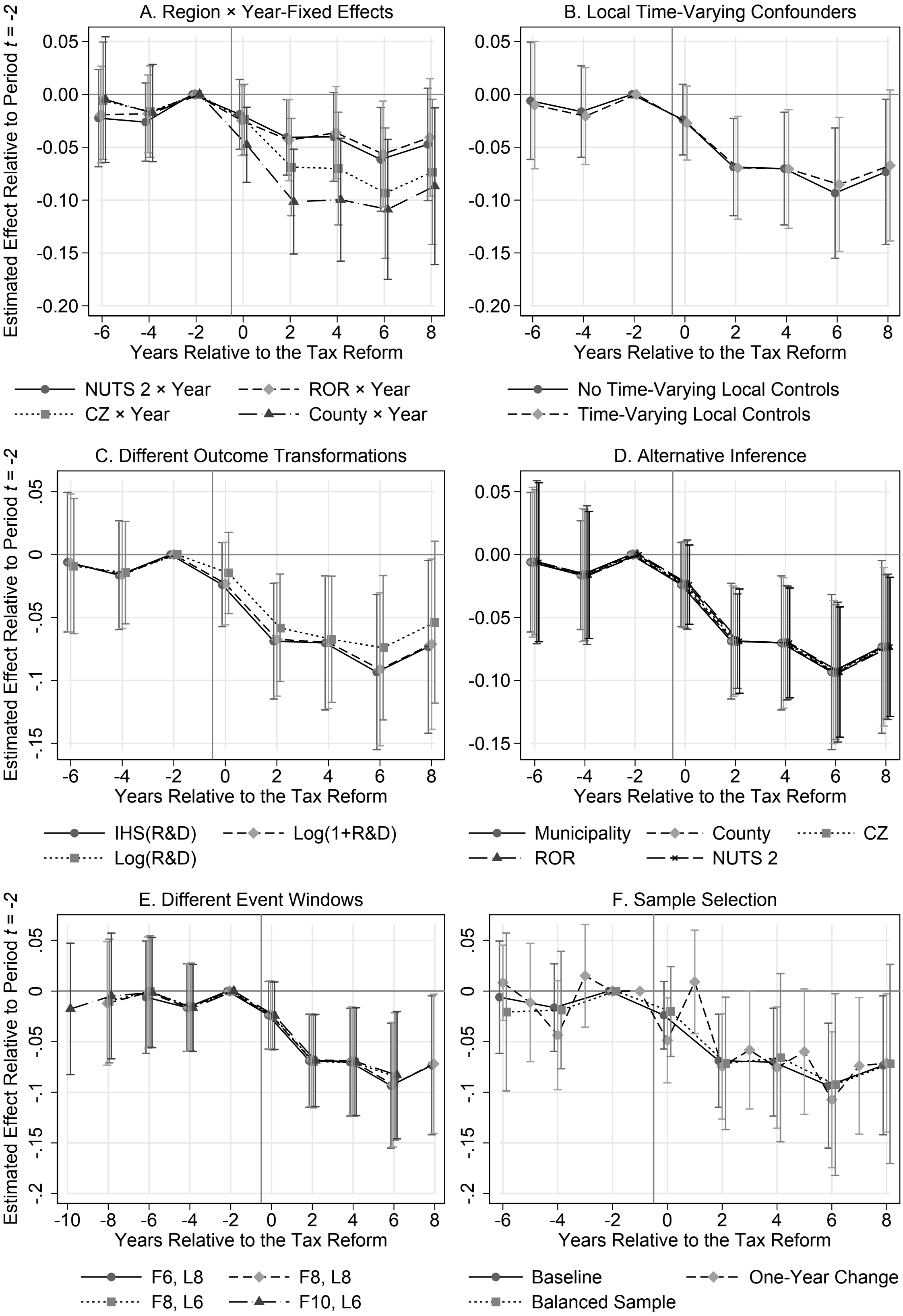
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equation (8). For the treatment group, the business tax decrease occurred in year $t = 0$ or $t = -1$. The regressions include establishment, state × year, commuting zone × year, as well as sector × 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.3: The Effect of a Tax Increase on Total R&D Spending



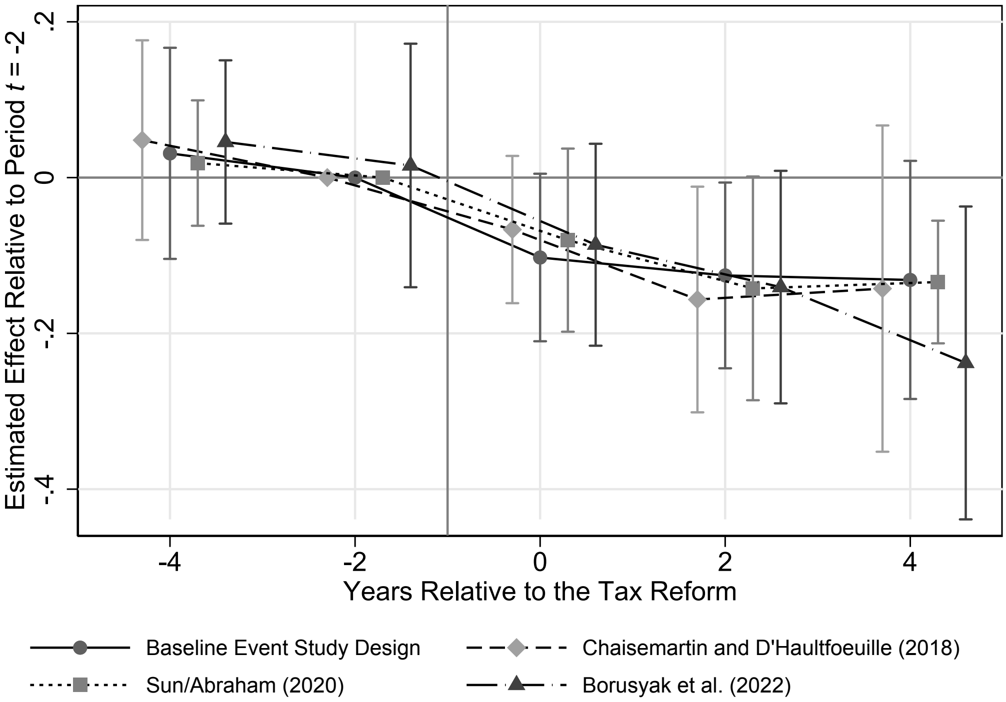
Notes: This graph compares our baseline event study model as depicted in Panel A of Figure 4 (Panel A) to the simple event study specification using event indicators instead of tax rate changes as explanatory variable (Panel B). The two graphs depict point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equation (8). For the treatment group, the business tax increase occurred in year $t = 0$ or $t = -1$. The regressions include establishment, 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.4: The Effect of a Tax Increase on Total R&D Spending – Robustness Checks



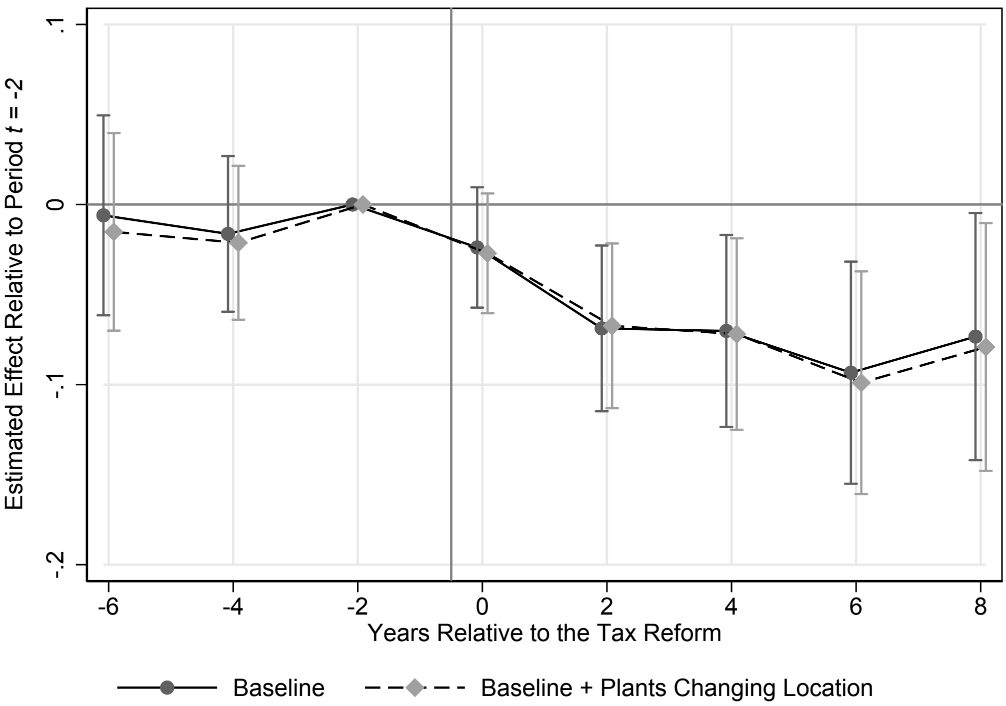
Notes: This graph illustrates the robustness of our baseline effects. In Panel A, we plot the corresponding point estimates and 95% confidence intervals when using varying regional × year fixed effects. In Panel B, we add lagged local business cycle controls to our preferred specification. In Panel C, we use different transformations of the outcome variable. In Panel D, we use alternative ways of drawing inference. In Panel E we allow for different event windows. In Panel F, we plot the corresponding estimates when using one-year tax changes or a balanced estimation sample. In Panels B–F, all regressions include establishment, state × year, commuting zone × year as well as sector × year fixed effects. Municipalities that experienced a tax decrease during the event window period are excluded. In Panels A, B, C, E and F, standard errors are clustered at the municipality level.

Figure C.5: The Effect of a Tax Increase on Total R&D Spending – Heterogeneous Effects By Cohorts



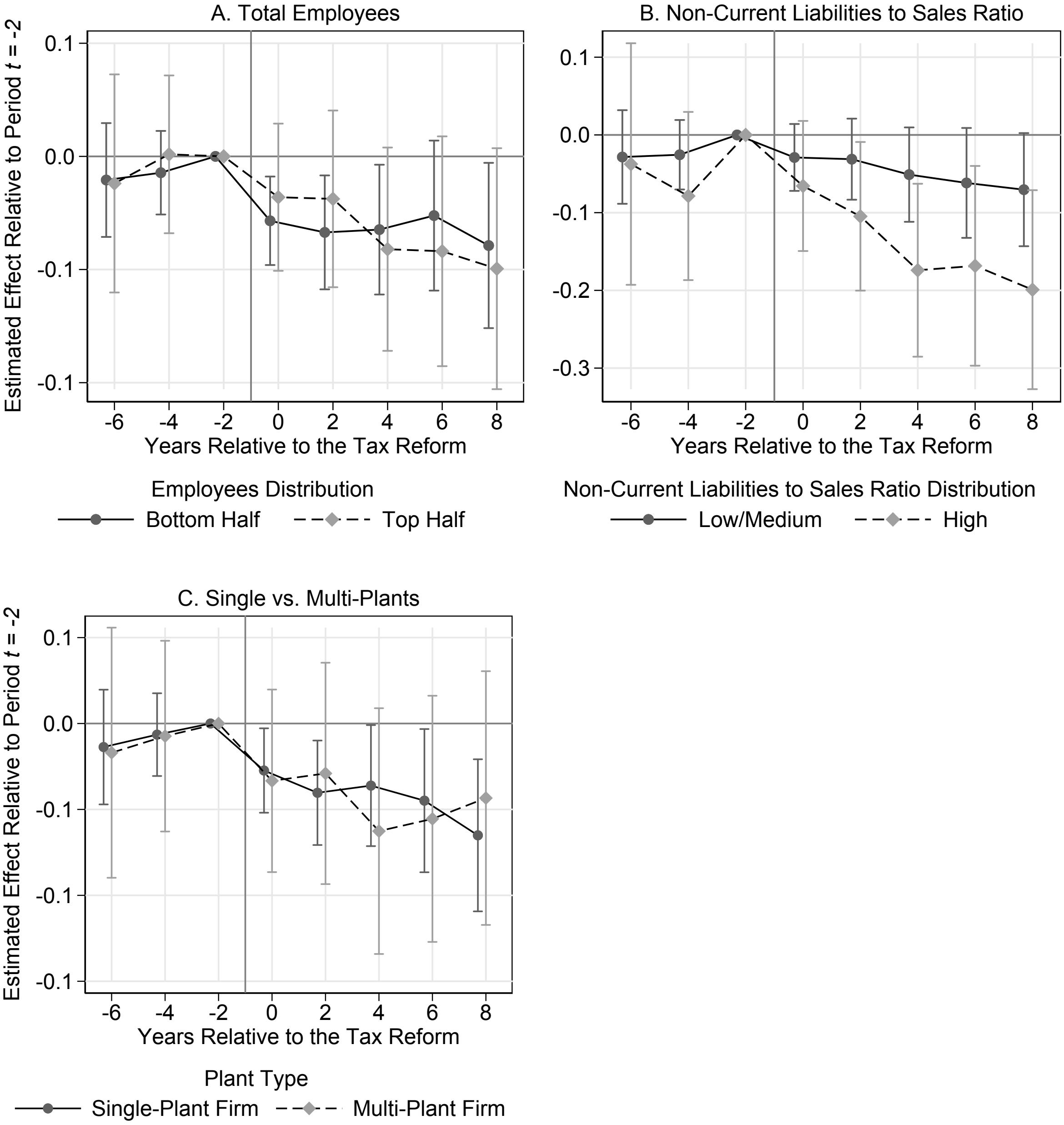
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-4, \ldots, 4]$), and corresponding 95% confidence intervals when (i) using our baseline event study model as defined in Equation (8) and (ii) explicitly correcting for possible cohort-specific treatment effects as suggested by de Chaisemartin and D'Haultfœuille (2020), Sun and Abraham (2021), and Borusyak et al. (2021), respectively. The analysis is limited to establishments 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 in year $t = 0$ or $t = -1$. The regressions include establishment, NUTS 2 $\times$ year, as well as sector $\times$ year fixed effects. Standard errors are clustered at the municipal level.

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



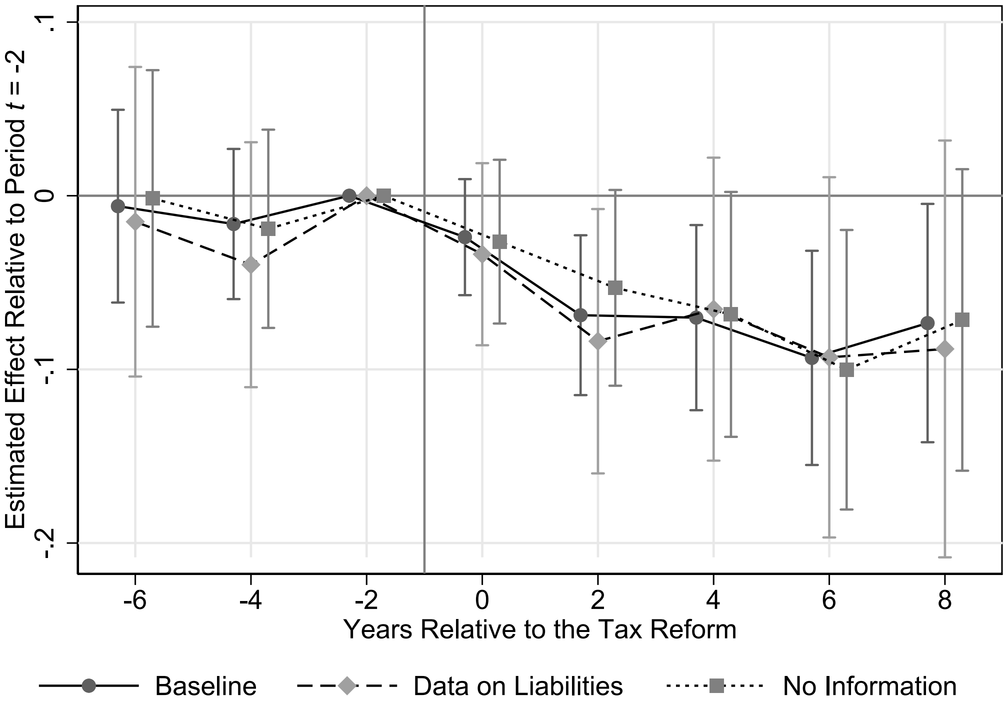
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equation (8) for (i) the baseline sample and (ii) when including those establishments that change municipalities during the 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 in year $t = 0$ or $t = -1$. The regressions include establishment, state × year, commuting zone × year, as well as sector × 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: LBT Increases and Filed Patents – Effects by Plant Characteristics



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 equation (8) when allowing for heterogeneous effects by plant characteristics. Panel A differentiates between smaller and larger establishments as measured by the number of employees. Panel B differentiates establishments according to their non-current liabilities to sales ratio. Panel C allows for heterogeneous effects between single and multi-plant establishments. The dependent variable refers to establishments' total number of filed patents. For the treatment group, the business tax change occurred in year $t = 0$ or $t = -1$. The regressions include establishment, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects and group $\times$ year dummies. 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 Establishments' 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 equation (8) when using different samples. The dependent variable refers to establishments' total annual R&D spending. For the treatment group, the business tax change occurred in year $t = 0$ or $t = -1$. The regressions include establishment, state × year, commuting zone × year, as well as sector × 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.

Table C.1: Estimation Results

Panel A – Total R&D and Internal Spending

	Total R&D			Internal R&D		
	Total Spending (1)	Increase Dummy (2)	Patent Count (3)	Internal Spending (4)	Personell Spending (5)	Materials Spending (6)
$t-6$	-0.006	-0.007	-0.008	0.011	0.010	0.016
	(0.028)	(0.028)	(0.014)	(0.031)	(0.031)	(0.040)
$t-4$	-0.016	-0.001	-0.001	-0.008	-0.003	-0.017
	(0.022)	(0.020)	(0.010)	(0.025)	(0.024)	(0.032)
t	-0.024	-0.016	-0.025**	-0.012	-0.008	-0.019
	(0.017)	(0.016)	(0.010)	(0.018)	(0.018)	(0.025)
$t+2$	-0.069***	-0.059***	-0.028**	-0.065**	-0.058**	-0.091***
	(0.023)	(0.022)	(0.012)	(0.026)	(0.026)	(0.032)
$t+4$	-0.070***	-0.065***	-0.040***	-0.070**	-0.069**	-0.099**
	(0.027)	(0.025)	(0.015)	(0.031)	(0.031)	(0.039)
$t+6$	-0.093***	-0.085***	-0.039**	-0.109***	-0.110***	-0.115***
	(0.031)	(0.027)	(0.017)	(0.037)	(0.037)	(0.044)
$t+8$	-0.073**	-0.061**	-0.050***	-0.096**	-0.104**	-0.086*
	(0.035)	(0.031)	(0.019)	(0.043)	(0.043)	(0.050)
Number of Observations	20,244	20,244	23,330	20,244	20,244	20,244
Adjusted R-squared	0.886	0.886	0.598	0.868	0.874	0.820
Number of Municipalities	1,456	1,456	1,489	1,456	1,456	1,456

Panel B – External Contracting and Personell Spending

	External R&D		Personell Spending			
	Extensive Margin (1)	Intensive Margin (2)	Average Wage (3)	Total Employment (4)	Scientists & Engineers (5)	Assistants & Others (6)
$t-6$	0.001	0.018	-0.004	0.015	0.013	0.005
	(0.014)	(0.080)	(0.020)	(0.022)	(0.023)	(0.024)
$t-4$	-0.008	0.073	-0.006	0.009	0.004	-0.000
	(0.010)	(0.063)	(0.015)	(0.016)	(0.017)	(0.017)
t	0.013	-0.003	-0.019	-0.005	-0.011	-0.000
	(0.010)	(0.049)	(0.012)	(0.013)	(0.013)	(0.015)
$t+2$	0.013	-0.011	-0.034**	-0.036**	-0.024	-0.047**
	(0.013)	(0.069)	(0.016)	(0.018)	(0.019)	(0.022)
$t+4$	-0.002	-0.002	-0.036*	-0.041*	-0.018	-0.067***
	(0.014)	(0.076)	(0.019)	(0.022)	(0.023)	(0.026)
$t+6$	-0.005	0.031	-0.032	-0.068***	-0.032	-0.099***
	(0.015)	(0.085)	(0.021)	(0.026)	(0.027)	(0.030)
$t+8$	0.000	0.063	-0.018	-0.066**	-0.027	-0.094***
	(0.016)	(0.089)	(0.023)	(0.030)	(0.032)	(0.033)
Number of Observations	20,244	4,665	19,879	20,191	20,214	20,214
Adjusted R-squared	0.579	0.855	0.752	0.901	0.871	0.865
Number of Municipalities	1,456	582	1,444	1,452	1,452	1,452

Notes: This table presents estimated coefficients for the event study models presented in Figures 4 and 5.

Table C.2: Heterogeneous Effects by Firm Characteristics

	R&D Spending			Filed Patents			
	Firm Size (1)	Non-Current Liabilities (2)	Single- vs. Multi Plant (3)	Firm Size (4)	Non-Current Liabilities (5)	Single- vs. Multi Plant (6)	Share of Applied R&D (7)
Group 1: $t-6$	0.001	0.049	0.011	-0.018	-0.035	-0.014	0.010
	(0.041)	(0.056)	(0.037)	(0.015)	(0.033)	(0.017)	(0.040)
Group 1: $t-4$	-0.041	0.005	-0.018	-0.008	-0.026	-0.006	-0.011
	(0.036)	(0.046)	(0.029)	(0.012)	(0.025)	(0.012)	(0.033)
Group 1: t	-0.009	-0.018	-0.026	-0.034***	-0.030	-0.027**	-0.003
	(0.027)	(0.034)	(0.023)	(0.012)	(0.024)	(0.013)	(0.025)
Group 1: $t+2$	-0.086**	-0.056	-0.070**	-0.038**	-0.037	-0.040***	-0.008
	(0.035)	(0.052)	(0.032)	(0.015)	(0.030)	(0.015)	(0.034)
Group 1: $t+4$	-0.073*	-0.020	-0.059*	-0.034**	-0.054	-0.036**	-0.012
	(0.040)	(0.059)	(0.035)	(0.017)	(0.034)	(0.018)	(0.037)
Group 1: $t+6$	-0.095**	-0.063	-0.087**	-0.029	-0.074*	-0.045**	0.003
	(0.044)	(0.073)	(0.039)	(0.020)	(0.039)	(0.021)	(0.042)
Group 1: $t+8$	-0.096*	-0.089	-0.095**	-0.041*	-0.080**	-0.065***	0.023
	(0.049)	(0.087)	(0.044)	(0.021)	(0.039)	(0.023)	(0.044)
Group 2: $t-6$	0.022	-0.089	-0.012	-0.014	-0.024	-0.017	-0.022
	(0.040)	(0.087)	(0.057)	(0.027)	(0.078)	(0.037)	(0.017)
Group 2: $t-4$	0.025	-0.031	-0.003	-0.004	-0.056	-0.007	-0.002
	(0.030)	(0.068)	(0.045)	(0.020)	(0.060)	(0.028)	(0.013)
Group 2: t	-0.015	-0.042	0.011	-0.020	-0.077	-0.033	-0.037***
	(0.025)	(0.055)	(0.034)	(0.020)	(0.047)	(0.027)	(0.013)
Group 2: $t+2$	-0.043	-0.118**	-0.064	-0.022	-0.114**	-0.029	-0.039**
	(0.033)	(0.060)	(0.045)	(0.024)	(0.052)	(0.033)	(0.016)
Group 2: $t+4$	-0.067*	-0.187***	-0.091*	-0.044*	-0.173***	-0.063*	-0.050***
	(0.039)	(0.072)	(0.050)	(0.027)	(0.062)	(0.036)	(0.018)
Group 2: $t+6$	-0.119**	-0.227***	-0.143**	-0.048	-0.179***	-0.055	-0.053***
	(0.048)	(0.083)	(0.063)	(0.029)	(0.068)	(0.037)	(0.020)
Group 2: $t+8$	-0.094*	-0.195*	-0.109	-0.051*	-0.193***	-0.043	-0.069***
	(0.054)	(0.102)	(0.070)	(0.031)	(0.069)	(0.038)	(0.023)
Observations	20,244	8,583	18,771	20,244	8,583	18,771	20,212
Adjusted R^2	0.868	0.860	0.869	0.606	0.617	0.607	0.602
Municipalities	1,456	805	1,372	1,456	805	1,372	1,455

Notes: This table presents estimated coefficients for the event study models presented in Figures 6, 7, and C.7.

Table C.3: Estimation Results by Type of Innovation

	Patent Count (1)	Citation-Weighted (2)	Science Based (3)	Non-Science Based (4)	Product Patents (5)	Process Patents (6)
$t-6$	-0.008	0.002	0.005	-0.011	-0.009	0.008
	(0.014)	(0.014)	(0.004)	(0.014)	(0.013)	(0.006)
$t-4$	-0.001	-0.000	-0.001	-0.001	0.002	-0.006
	(0.010)	(0.011)	(0.004)	(0.010)	(0.010)	(0.005)
t	-0.025**	-0.009	-0.004	-0.024**	-0.015	-0.012**
	(0.010)	(0.011)	(0.003)	(0.010)	(0.010)	(0.005)
$t+2$	-0.028**	-0.022*	-0.008**	-0.027**	-0.018	-0.014**
	(0.012)	(0.012)	(0.004)	(0.012)	(0.012)	(0.006)
$t+4$	-0.040***	-0.028*	-0.012**	-0.036**	-0.028**	-0.020***
	(0.015)	(0.015)	(0.005)	(0.014)	(0.014)	(0.008)
$t+6$	-0.039**	-0.036**	-0.016***	-0.032*	-0.028*	-0.025***
	(0.017)	(0.018)	(0.005)	(0.017)	(0.016)	(0.009)
$t+8$	-0.050***	-0.045**	-0.011*	-0.044**	-0.033*	-0.031***
	(0.019)	(0.019)	(0.006)	(0.018)	(0.017)	(0.010)
Number of Observations	23,330	23,330	23,330	23,330	23,330	23,330
Adjusted R-squared	0.598	0.553	0.452	0.592	0.574	0.488
Number of Municipalities	1,489	1,489	1,489	1,489	1,489	1,489

Notes: This table presents estimated coefficients for the event study models presented in Figure 7.