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# Profit Taxation, R&D Spending, and Innovation†

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*We study how profit taxes affect establishments' R&D activities. Relying on detailed panel data of R&D-active firms in Germany over two decades, we exploit identifying variation induced by more than 10,000 municipal changes in the local business tax rate and federal tax reforms with locally varying effects. Using event-study techniques, we find a sizable, negative effect of profit taxes on establishments' total R&D spending and patents filed. Zooming into the innovation production process, we uncover substantial heterogeneity in the impact of profit taxation for various R&D input factors, among firm characteristics, and for different types of research projects.* (*JEL* D22, G32, H25, H32, H71, O31, O34)

Innovation has long been emphasized as a key driver of economic growth (Solow 1957; Romer 1990). Firms constitute a central element in this process by serving as the cradle of most groundbreaking new technologies and products. From the perspective of the firms, their engagement in research and development (R&D) is generally seen as an instrument to increase productivity and expand market shares (Balasubramanian and Sivadasan 2011), assimilate knowledge from competitors (Aghion and Jaravel 2015), as well as to secure long-term growth (Kogan et al. 2017). While private returns to successful R&D projects are generally high, the level of R&D is considered to be below the social optimum due to market failures

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and knowledge spillovers (Jones and Williams 1998). To bring innovation activities closer to socially optimal levels, R&D (tax) policies have been implemented in many countries around the world. While recent studies have shown that firm-level R&D is generally responsive to tax incentives, we still know relatively little about the underlying mechanisms at the firm level. Particularly, we lack evidence on which types of firms respond to tax incentives and what the margins of adjustment are.

In this paper, we intend to open the black box of firm-level R&D production and its responsiveness to tax incentives. We rely on rich survey panel data targeting all R&D-active firms in Germany from 1995 onward. The dataset allows us to zoom in on the process of innovation production at the level of the establishment and decompose R&D expenditures along various dimensions—for example, by distinguishing R&D spending on internally conducted versus outsourced projects or labor versus capital R&D expenditures. We enrich the dataset with administrative information on establishments' patenting activities from the European Patent Office (EPO) to investigate effects on output-based innovation measures and learn about the consequences of business taxation for different types of research projects. To establish a causal link between tax incentives and establishment-level R&D, we exploit the institutional framework of profit taxation in Germany, where municipalities autonomously set the local business tax (LBT) rate in each year and federal-level reforms of the corporate income tax (CIT) and personal income tax (PIT) have locally varying effects because of interactions between the two margins of taxation. Overall, we exploit identifying variation from more than 10,000 changes in the LBT and around 20 tax reforms at the national level. This enables us to flexibly account for time-varying unobserved confounders at fine-grained regional levels when estimating the effects of interest. Moreover, as profit taxes affect all types of businesses, we can study heterogeneous effects across the distribution of R&D-active firms in detail.

To guide our empirical analysis we set up a theoretical model that builds upon earlier contributions by Chetty and Saez (2010) as well as Chen et al. (2021). We derive three hypotheses. First, an increase in the tax on profits should reduce firms' R&D investments because it lowers their after-tax returns. Second, the negative effect of profit taxation should be smaller for R&D expenditures with more convex costs. Third, an increase in the profit tax is expected to cause stronger reductions in R&D spending among more credit-constrained firms, as the costs of debt financing are tax deductible whereas the costs of equity financing are not.

Based on an event-study model with staggered treatment that flexibly controls for fine-grained, time-varying regional trends at the level of the metropolitan statistical areas (MSAs), we show that an increase in the profit tax has a statistically significantly negative effect on establishments' total R&D spending. The treatment effect builds over time and levels off around four years after the change in the tax rate. Our baseline estimates imply an elasticity of R&D spending with respect to the user cost of capital of $-3.8$—an estimate of similar magnitude compared to related studies on the effects of targeted R&D tax policies (see, e.g., Agrawal, Rosell, and Simcoe 2020; and Dechezleprêtre et al. 2023). We detect no evidence for asymmetric effects of tax increases versus decreases. Our results are robust to a variety of identification checks. Among others, we demonstrate that effects remain unchanged when (i) moving toward a border design by controlling for regional shocks at more fine-grained levels than in the baseline, (ii) limiting identification to (large) increases in the LBT rate, (iii) exploiting local differences in the impact of the 2008 federal tax reform by means of dynamic difference-in-difference design, or (iv) accounting for possible heterogeneous treatment effects across cohorts.

We continue our analysis by investigating the mechanisms behind these baseline effects. We show that internal and outsourced R&D spending react similarly in the long run. However, the effect on externally conducted R&D spending materializes later. We cautiously interpret this finding as being consistent with steeper marginal adjustment cost for outsourced R&D spending—for example, because of long-term contractual arrangements that are more costly to alter at the margin. Investigating effects on innovation production within the R&D-active establishments, we find that spending on R&D labor and capital decreases as profit taxes rise—with suggestively stronger responses for capital. For R&D labor, we further show that around 65 percent of the decrease can be attributed to reduced employment and the remaining 35 percent to lower real wages.

Next, we uncover meaningful heterogeneous effects along the distribution of R&D-active firms. In line with our theoretical priors, we show that reductions in R&D spending are statistically significantly stronger for credit-constrained firms (as approximated by their noncurrent liabilities to sales ratio). In contrast, we do not detect heterogeneous effects by firm size. Moreover, we find statistically significantly stronger effects for those firms that can shift R&D within a corporate group. We further show that the tax-induced reductions in establishments' R&D spending are accompanied by reductions in their patenting activity. Based on OLS and count data models, we find that an increase in the profit tax has a sizable negative effect on the number of filed patents. We show that this patent effect is driven by process rather than product innovations.

In terms of cost effectiveness, our estimates imply that the average local government has to forgo €1 of tax revenues in order to raise establishments' R&D expenses by €0.26. Equivalent measures of targeted R&D tax incentives for the United Kingdom are substantially higher—between £1 and £2.3 of increased R&D spending for each pound decrease in revenues (Guceri and Liu 2019; Dechezleprêtre et al. 2023). Thus, targeted R&D tax incentives appear to be the more efficient policy instrument altogether. Our results yet question policymakers' common practice to link firms' eligibility to targeted R&D (tax) incentives to their size as a proxy for liquidity constraints.

*Related Literature*.—The paper contributes to two strands of the literature. Foremost, we speak and add to the small and recent set of papers using subnational variation in the corporate and PIT rates across US states to analyze the effect of (business) taxation on innovation. Moretti and Wilson (2017) studies the mobility of star scientists and shows that they respond to changes in the personal and corporate tax rates by relocating to lower-tax states. Mukherjee, Singh, and Žaldokas (2017) and Akcigit et al. (2022) study innovation activity and show that higher taxes reduce the number of patents and the level of R&D investments. Relative to these studies, we make two key contributions. First, we open up the black box of R&D production at the level of the establishment. The detailed R&D data allow for identifying the underlying mechanisms at play. For instance, we are able to test which types of R&D inputs, firms, and research projects react (the most) to a change in the profit tax rate. These results yield valuable insights for the optimal design of targeted R&D tax policies.

Second, we provide clean empirical evidence on the effect of general business taxes on firms' R&D spending and innovation in a setting outside the United States. Our institutional setup allows for accounting for possible confounding regional shocks at very fine geographical levels. As the period under investigation saw both sizable tax cuts and tax increases with locally varying impacts, we can also explicitly test for asymmetric effects of tax increases and decreases on establishments' R&D activities.

The paper is also related to a recent set of papers analyzing the effects of targeted R&D policies, such as tax credits, deduction possibilities, and subsidies (Bronzini and Iachini 2014; Rao 2016; Guceri and Liu 2019; Agrawal, Rosell, and Simcoe 2020; Chen et al. 2021; Dechezleprêtre et al. 2023). All studies provide clean causal evidence by exploiting policy cutoffs to establish quasiexperimental research designs. Consequently, effects are identified locally. In our setting, all R&D-active firms are subject to profit taxation, which allows for assessing effect heterogeneity along different firm characteristics in more detail. Our results generally justify the implementation of targeted R&D tax policy instruments: they reduce the tax burden for R&D-active firms and thereby lower the distortion of business taxation on socially desired innovation. Our estimates yet suggest that (in light of constrained budgets) policymakers should carefully reconsider which types of R&D firms to support with targeted R&D instruments to increase their overall efficiency.

The remainder of the paper is structured as follows. In Section I, we set up a theoretical framework to derive hypotheses about the (varying) effect of profit taxes on firms' R&D activities that will be empirically tested in the later part of the paper. Section II describes the various data sources combined for the purpose of the empirical analysis as well as the institutional background and local variation of profit taxation in Germany. In Section III, we set up the empirical research design and discuss the plausibility of the design's underlying identifying assumptions. Section IV presents the empirical results. Section V concludes.

## I. Theoretical Framework

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

In period 1, the establishment decides on the level of investment $I$. It uses financial resources $P$ to finance investments. We start by assuming that the establishment collects cash by raising debt $D$—that is, $P = D$. Investment decisions are made in period 1, but yield a return of $R(I)$ in period 2, where $R$ is an increasing and concave function of initial R&D investments ($R' > 0, R'' < 0$). The establishment is subject to a tax on profits in period 2 with a rate of $\tau$. We abstract from taxation in the initial period and from policy uncertainty. To simplify the model, we do not model corporate risk-taking directly but discuss the implications of risky R&D within the existing modeling framework below.

Investments generate cost in both periods. In period 1, the establishment has to pay per-unit investment costs $c$. 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 1 is, thus, given by $P = D = cI + g(I)$. In period 2, debt $D$ has to be repaid, including interest $r^D$. Abstracting from discounting, the establishment's profits over the two periods are then given by

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

Using the budget constraint from period 1, we can simplify equation (1) and set up the establishment's maximization problem:

$$\max_I (1-\tau)\left[R(I) - r^D D\right] - D \quad \text{subject to} \quad cI + g(I) = D.$$

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

$$(1-\tau)R'(I^*) = \left[1 + (1-\tau)\,r^D\right]\left[c + g'(I^*)\right] \quad \text{if} \quad P = D.$$

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

$$\left.\frac{dI^*}{d\tau}\right|_{P=D} = \frac{R'(I^*) - r^D\left[c + g'(I^*)\right]}{(1-\tau)\,R''(I^*) - \left[1 + (1-\tau)\,r^D\right]g''(I^*)} < 0. \tag{2}$$

From equation (2), we derive the following first hypothesis.

HYPOTHESIS 1 (Investment Effect): *An increase in the profit tax rate $\tau$ lowers the after-tax return on investments and leads to lower establishment-level R&D investments $I^*$. Conversely, a decrease in the tax rate raises establishments' R&D investments.*

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

Next, we investigate the role of adjustment costs. The negative investment effect derived above decreases in $g''(I^*)$. In other words, the larger $g''(I^*)$, the weaker the negative effect of an increase in the tax rate $\tau$ on R&D investment. A simple example for such an adjustment cost function would be the quadratic function $g(I) = bI^2$ with scaling parameter $b > 0$—see, for example, 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 Cost Effect): *An increase in the profit tax rate $\tau$ leads to relatively smaller reductions in establishment-level R&D investments, the stronger the marginal adjustment costs increase in the level of I*—that is, *for investments where $g''(I)$ is relatively high.*

Below, we will test this hypothesis by analyzing the effect of profit tax changes on various R&D inputs. While the theoretical mechanism is unambiguous, assessing the validity of this hypothesis empirically needs additional assumptions on the convexity of different types of adjustment costs. In Section IVB, we carefully discuss four different types of adjustments in light of Hypothesis 2: costs for adjusting (i) internal versus outsourced R&D activities, (ii) R&D input factors labor versus capital, (iii) the stock of R&D employment versus R&D employees' wages, and (iv) R&D expenses in establishments (not) belonging to a corporate group with other R&D-active firms.

Up to this point, we assumed that the establishment finances its R&D expenses in period 1 by raising debt. In the context of R&D, however, credit constraints may be particularly relevant. 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, Fazzari, and Petersen 2009; Hall and Lerner 2010; Bakker 2013). Hence, R&D investments are typically riskier than other capital investments, which makes access to debt more expensive and maybe even prohibitively expensive for some establishments. Hence, we also explore the effect of profit taxes on R&D activities in case the establishment relies upon equity financing, the alternative financing channel. Abstracting from adjustment cost, the maximization problem under pure equity financing ($P = E$) is then given by

$$\max_I (1-\tau)R(I) - \left(1 + r^E\right)E \quad \text{subject to} \quad cI = E.$$

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

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

Totally differentiating and rearranging terms yields the following prediction:

$$\text{(3)} \qquad \left.\frac{dI^*}{d\tau}\right|_{P=E} = \frac{R'(I^*)}{(1-\tau)\,R''(I^*)} < 0.$$

Recall from equation (2) that the corresponding effect under debt financing is given by

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

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

HYPOTHESIS 3 (Financing Effect): *An increase in the profit tax rate $\tau$ leads to relatively stronger reductions in R&D investments for establishments that have to finance the marginal R&D project to a larger extent via equity, for instance, in case more binding credit constraints limit debt financing.*

The predicted heterogeneity in the effect by financing type is driven by the deductibility of financing costs. If the costs of equity financing were to some extent (or fully) deductible from the profit tax base, differences between debt and equity financing would be less pronounced (absent). Empirically, we test Hypothesis 3 by analyzing whether the negative effect of profit taxes on R&D investments is more pronounced for firms that are relatively more credit constrained, and therefore limited in their capacity to finance investments via debt. Another empirical test is to compare our results on R&D spending with the corresponding effects on other capital investments, which should be relatively more debt financed.

## II. Data and Institutional Background

In this section, we introduce the data sources underlying the analysis, as well as the institutional background of German profit taxation. Section IIA describes the establishment-level survey data, which serves as the primary source of information, as well as the matching of patent information and additional financial variables to the surveyed establishments. Section IIB describes the framework of profit taxation in Germany and depicts the policy variation we exploit for identification.

### A. *Data Sources*

*R&D Survey*.—Our main data source is the biennial longitudinal dataset *Survey on Research and Development of the German Business Enterprise Sector*—henceforth, *R&D Survey* (Stifterverband 2021). The dataset is collected and administrated by the Stifterverband on behalf of the German Federal Ministry of Education and Research. The survey targets all firms located in Germany that are engaged in R&D—from a small single proprietorship to a large (German) multinational. It forms the basis for the country's official reporting on its entrepreneurial R&D activities to EU authorities and the OECD.[1] The survey methodology follows the OECD's recommendations

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

for the collection and interpretation of R&D data. To be sampled, a firm has to make R&D expenses at least once during the sample period or be assumed to perform R&D with a certain degree of probability. New R&D firms are identified through the regular screening of a variety of sources, including, for example, funding databases of the European Union, media information, and commercial business datasets.[2]

For each research-active firm, the survey targets the establishments within the firm that conduct R&D. Around 96 percent of the surveyed firms have 1 research-active establishment—either because it is a single-establishment firm or because it is a multiestablishment firm with only one R&D site. We drop the 4 percent of firms that report multiple R&D-active establishments from the baseline sample because R&D expenditures are reported jointly at the firm level and we cannot allocate them to the respective establishments. Thus, our sample consists of research-active establishments that conduct all R&D of a firm. The surveyed R&D sites additionally report some firm-level information: total (= R&D and non-R&D) employees, sales, industry classification, as well as organizational structure. We do not observe if other non-R&D establishments exist. Overall, the survey covers 92 percent of Germany's business sector R&D expenditures.

The *R&D Survey* was initiated in 1995 and has been conducted in every odd year ever since. As we use an event-study model with eight leads and lags in the profit tax rate in our preferred empirical specification (see Section IIIA for details), the baseline R&D sample covers the years 1995–2013 as well as information on tax changes from 1987 to 2021.[3] Therefore, we restrict the analysis to West Germany.[4] We further discard firms from the sample that engage in R&D very infrequently or that report implausible growth rates in R&D. For our baseline sample, we also exclude firms that move during the survey period. In a robustness check, we reintroduce these firms in the sample to take a closer look at shifting behavior or tax-induced mobility decisions of firms. Our baseline sample contains 47,644 establishment-year observations in 2,681 municipalities and covers around 35 percent of Germany's business sector R&D.[5] The baseline estimation sample is unbalanced. We show below that results remain unaffected when using a balanced estimation sample. Panel A of Supplemental Appendix Figure A.1 illustrates 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

[2] Hence, we do not observe some R&D activities in the lower tail of the distribution since newly R&D-active firms will not be sampled in their very early years, and the survey does not capture irregular or one-off R&D activities. See Supplemental Appendix B for more details on the dataset.

[3] The observation window of the outcome is determined by the length of the effect window of the event-study model and the data availability of the treatment variable. Using eight lags, we need to observe tax rate changes from 1987 onward, eight years before the first survey wave 1995. With eight leads, the latest usable survey wave is 2013 due to the biennial structure of the *R&D Survey*. In order to use a longer outcome window, we have to cut back on the number of prereform estimates. We demonstrate the robustness of our findings when using fewer leads and all R&D data up until 2017 in robustness checks presented in Section IVA.

[4] The current system of business taxation was not applicable in East Germany before reunification. Moreover, many municipal borders in East Germany were redrawn during the 1990s and 2000s, which prevents the exact assignment of the LBT rate to affected establishments. East German establishments account for less than 5 percent of the country's total R&D expenditures during the period under investigation.

[5] The firms with multiple R&D sites excluded from the baseline sample account for around 55 percent of total R&D expenditures. Estimates remain unaffected when including these firms (see specification (7) of Supplemental Appendix Table D.2).

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, annual total spending on R&D varies from around €36,000 (fifth percentile) to around €8.5 million (ninety-fifth percentile); see panel A of Supplemental Appendix Table A.1 for detailed descriptive statistics. The *R&D Survey* allows for decomposing total R&D expenses along several margins. First, we can differentiate between internal and outsourced R&D spending.[6] Outsourced R&D is typically used as a strategy to acquire missing knowledge, either by engaging in licensing 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, Belderbos, and Carree 2008). However, the search for and coordination of external contractors and collaborations also comes with sizable transaction cost 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 Supplemental Appendix Table A.1). On average, outsourced R&D accounts for 9 percent of establishments' total R&D expenditures, and 21 percent if we consider establishments with nonzero outsourcing only. The dataset further provides information on various inputs in innovation production, such as internal R&D spending for labor and capital, the number of in-house R&D employees (scientists versus engineers versus others), and their average annual wage bill. On average, establishments' spending on R&D personnel (capital) accounts for around 68 percent (32 percent) of total internal R&D spending, and scientists and engineers make up for around 82 percent of the R&D staff.

*Patent Data*.—To measure innovation output, we link administrative information from the EPO (2024) on establishments' patenting activities to the *R&D Survey* (see Supplemental Appendix B for a detailed description of the matching procedure). Between 1995 and 2013, the surveyed establishments filed 228,366 patents, which accounts for around 60 percent of all patents filed by German applicants at the EPO during this period.[7] Panel C of Supplemental Appendix Figure A.1 shows the spatial distribution of patent activity. Overall, the pattern is in line with the regional prevalence of R&D establishments. Accordingly, Supplemental Appendix Figure A.2 shows that establishment-level R&D spending and patenting are positively correlated—both in terms of levels and changes.

Because the simple count of establishments' number of filed patents may only imperfectly capture the true value of innovation output (Scherer 1965; Hall, Jaffe, and Trajtenberg 2005), we look at both the simple and citation-weighted number of filed patents as outcomes in the empirical analysis. We construct the latter by weighting each patent by the number of citations it receives from other patents filed

[6] Outsourced R&D should be reported as internal R&D by the subcontractor in case the subcontractor is located in Germany. Our R&D measures are accurate at the level of the firm. Double counting only becomes an issue when aggregating R&D expenditures across firms.

[7] We do not capture patents filed by the government, 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.

at the EPO 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 (see, e.g., Harhoff, Scherer, and Vopel 2003; Kogan et al. 2017; Moser, Ohmstedt, and Rhode 2018). Using information from each patent's claim text and following Danzer, Feuerbaum, and Gaessler (2024) and Gaessler (2024), we further distinguish product from process innovations.

Panel C of Supplemental Appendix Table A.1 provides the corresponding descriptive statistics on establishments' patenting activities. The average establishment files 0.7 patents per year, which receive on average 1.2 citations over the following five years. However, the distribution of patents is notably skewed: the establishment at the seventy-fifth percentile files zero patents per year and only around one-fifth of all establishments in the *R&D Survey* filed at least one patent during the sampling period. Of all patents filed in our baseline sample, 56 percent are product innovations.

*Bureau van Dijk* (*BvD*) *Data*.—To measure firms' financial situation and non-R&D outcomes in more detail, we further link information from the BvD's *Amadeus* and *Orbis* databases (BvD 2024a, b) to the surveyed establishments (see Supplemental Appendix B for more information on the preparation and link of the BvD data). As the BvD datasets predominantly cover larger and oftentimes stock-listed firms, we cannot match all surveyed establishments to the BvD data. We show below that baseline effects remain unchanged when restricting the sample to establishments with information from the BvD datasets. In detail, we measure firms' financial situation via their noncurrent liabilities to sales ratio and their profitability via earnings before interest, taxes, depreciation, and amortization (EBITDA). We also use information about firms' total investments and their global ultimate owner. We use the latter to determine membership in a corporate group (see panel B of Supplemental Appendix Table A.1 for the respective descriptive statistics).

*Administrative Regional Data*.—Last, using information on each surveyed establishment's address, we complement the establishment-level data with annual information on the applicable LBT (see Section IIB for details).[8] We also add other regional characteristics at the municipal and county level. These include data on municipalities' annual public expenditures and revenues, population figures, stock of unemployed, and county-level GDP. We will use these variables to test whether local business cycles simultaneously determine municipalities' tax setting behavior and establishments' R&D activities. Panel D of Supplemental Appendix Table A.1 provides the corresponding descriptive statistics.

## B. *Institutional Background*

Business profits are taxed at two different administrative levels in Germany: the federal and the municipal level. For all business taxes, the tax base is operating profits, with limited loss carryforward and no loss carryback.

[8] The data mentioned in this paragraph combine Destatis (2024a, b), StABL (2024a, b), BA (2024), and BBSR (2024a, b).

*Business Taxes at the Federal Level.*—Corporations are subject to the CIT (*Körperschaftsteuer*). The respective tax rate, denoted by $\tau^{CIT}$, is flat and decreased from 46 percent in the late 1980s to 25 percent in the early 2000s and finally to 15 percent in 2008 (see Supplemental Appendix Table C.1 for an overview of all relevant tax rates and their evolution over time). Profits of noncorporate firms, such as sole proprietorships or partnerships, are subject to the PIT (*Einkommensteuer*). The PIT is progressive, with marginal tax rates increasing until a threshold. Above this threshold, the marginal tax rate is flat. Most noncorporate firms face the top marginal tax rate, which we denote $\tau^{PIT}_{top}$. It decreased from 56 percent in the late 1980s to 42–45 percent in the late 2010s. In addition, there is a tax surcharge $\tau^{SUR}$ (*Solidaritätszuschlag*) imposed on the both $\tau^{CIT}$ and $\tau^{PIT}$. The surcharge on CIT and PIT was introduced to finance the process of Germany's reunification and amounted to 5.5 percent for most years of our sample.

*The LBT.*—At the municipality level, both corporate and noncorporate firms are subject to the LBT (*Gewerbesteuer*).[9] The LBT serves as municipalities' most important source of revenue. Rules for the tax base are defined at the national level and cannot be altered by state or municipal governments. 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.

Similarly to the CIT, firms face a flat LBT rate. The LBT rate, $\tau^{LBT}$, is the product of the basic federal tax rate (*Steuermesszahl*) and a local scaling factor (*Hebesatz*) that acts as a municipality-specific multiplier: $\tau^{LBT} = \phi^{LBT}_{fed} \cdot \sigma^{LBT}_{mun}$. The scaling factor, $\sigma^{LBT}_{mun}$, 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 federal tax rate, $\phi^{LBT}_{fed}$, is set at the national level and applies to all municipalities. It was set at 5 percent until 2007, and at 3.5 percent thereafter. Local scaling factors, $\sigma^{LBT}_{mun}$, vary between 2.3 and 5.2 during our sample period, resulting in LBT rates from 8 percent to 26 percent.

*Statutory Total Tax Rate* (STTR).—Firm decisions should be based on total business tax rates. Corporations, for instance, should consider the joint tax burden arising from the CIT and the LBT. As a result, we use the statutory total (marginal) tax rate as our main policy measure. We first consider the STTR for corporate firms, which account for around 80 percent of the firms in our sample. Until 2007, firms were allowed to deduct the LBT payments as business expenses from the CIT/LBT tax base. As of 2008, this deduction possibility was eliminated. In sum, the statutory total marginal tax rate of a corporate firm, $\tau_{corp}$, is therefore defined as

$$(4)\quad \tau_{corp} = \begin{cases} \left[\tau^{CIT} \cdot \left(1 + \tau^{SUR}\right) + \phi^{LBT}_{fed} \cdot \sigma^{LBT}_{mun}\right] \Big/ \left(1 + \phi^{LBT}_{fed} \cdot \sigma^{LBT}_{mun}\right), & \text{if } t \leq 2007 \\ \left[\tau^{CIT} \cdot \left(1 + \tau^{SUR}\right) + \phi^{LBT}_{fed} \cdot \sigma^{LBT}_{mun}\right], & \text{if } t \geq 2008 \end{cases}.$$

[9] Only a few types of firms are exempt from the LBT: agricultural firms, nonprofit organizations, and self-employed individuals in liberal professions (such as accountants, journalists, or architects).

Nonincorporated firms are subject to the PIT instead of the CIT. Their statutory total marginal tax rate, $\tau_{noncorp}$, is defined in a similar way as the one for corporations, but the formula is slightly more complicated as some share of the LBT payments can additionally be credited against the PIT payments (see Supplemental Appendix C and Supplemental Appendix equation (C.2) for details).

*Tax Variation*.—In our baseline empirical specification, we use the STTR variation as given by equation (4) and Supplemental Appendix equation (C.2). In the empirical analysis, we exploit tax changes from 1987 to 2021 given a baseline R&D sample from 1995 to 2013 (see Section IIA) and an event-study model with eight posttreatment and eight pretreatment periods (see Section IIIA). Figure 1 illustrates the spatial and long-run temporal variation in the STTR for corporations, $\tau_{corp}$, across West Germany. Panel A plots the STTR for each municipality in 2007. We observe substantial differences across the country, with marginal tax rates varying between 33 percent and 50 percent (first percentile: 35 percent; ninety-ninth percentile: 40 percent). The average and median STTRs in our sample amount to 37 percent, respectively. In addition, we see that tax rates are spatially correlated at the level of the federal states, which is driven by higher LBT rates in some states. This can be reconciled with varying fiscal equalization schemes across states, a feature we account for by including MSA-by-year fixed effects in the estimations (see Section IIIA for details).

Panel B highlights the long-run variation in total rates within municipalities over our sample period. As federal-level taxes have been decreasing strongly since the late 1980s, we observe decreasing values of the STTR for corporate firms across Germany. However, the panel also highlights substantial regional variation in the extent of total tax decreases, ranging from 15 to 30 percentage points. These striking differences are driven by variation in municipal scaling factors, which lead to differentiated LBT rates. Panel A of Figure 2 shows the corresponding variation in the STTR that is due to municipal scaling factor changes over the period from 1987 to 2021. Roughly 90 percent of scaling factor changes during our observation window are tax increases. The average scaling factor increase amounted to around 0.16, or 5 percent relative to the mean.

In addition to the direct variation via changes in the municipal scaling factors, equation (4) shows that municipal variation in $\tau_{corp}$ can also be induced indirectly through federal tax reforms. Most importantly, in 2008, the federal basic rate, $\phi_{fed}^{LBT}$, was lowered from 5 percent to 3.5 percent. Moreover, the deductibility of LBT payments from the tax base was eliminated. For a given scaling factor, the reform implied that corporations in municipalities with higher scaling factors experienced relatively smaller decreases in $\tau_{corp}$. Panel B of Figure 2 depicts the resulting variation in the STTR. Similarly, other federal-level tax reforms of $\tau^{CIT}$ and $\tau^{PIT}$ before 2007 also induced indirect variation due to the deduction possibility of the LBT. However, this variation is small compared to the 2008 reform (see Supplemental Appendix C for details). In our baseline empirical specification, we control for federal-level changes in tax rates of equal amounts by including year fixed effects as well as municipal fixed effects. Hence, we effectively exploit direct and indirect variation in $\tau_{corp}$ and $\tau_{noncorp}$ to identify policy effects.

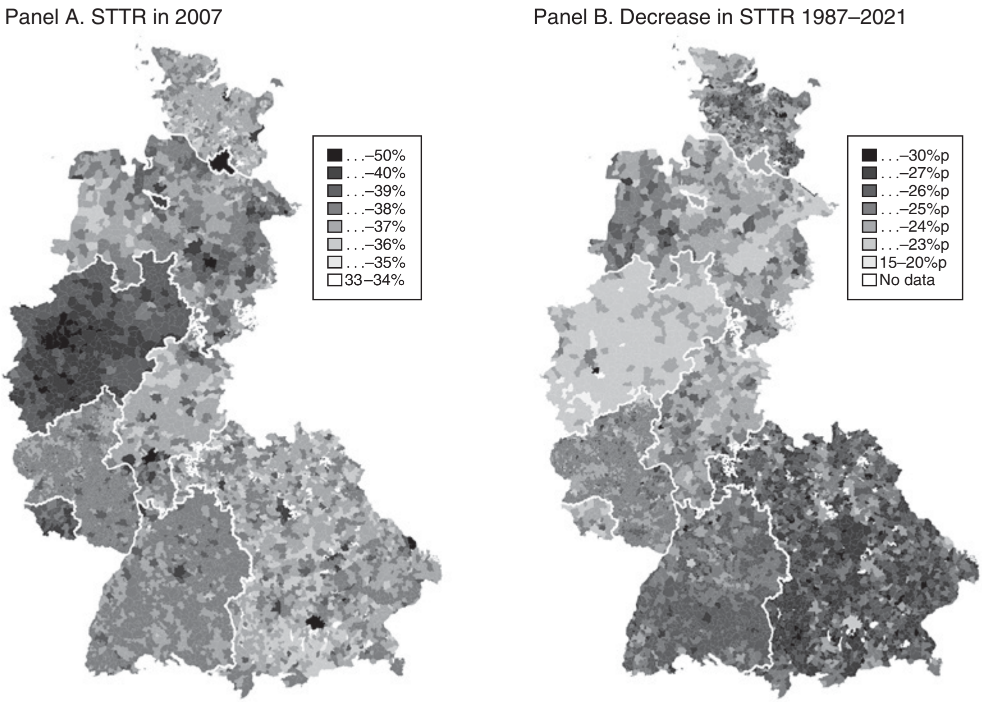


Figure 1. Spatial and Temporal Variation in STTR

*Notes:* This figure illustrates the spatial and temporal variation in STTR for corporate firms across West German municipalities. In panel A, the 2007 STTR is plotted for each municipality. Darker shades indicate higher levels of the tax. In panel B, total STTR changes over the period 1987–2021 are plotted for each municipality. Darker shades indicate more pronounced tax decreases in a given municipality. Thick white lines indicate federal state borders.

*Maps:* © GeoBasis-DE/BKG 2015.

*No Other R&D Tax Incentives*.—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 2013, the country's total R&D expenditures amounted to around 2.48 percent of its GDP on average, which is close to the US level of 2.59 percent and considerably higher than the 27 EU countries' average of 1.73 percent (own calculations based on the OECD (2023)).

## III. Empirical Strategy

We derive the causal effect of profit 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, Peichl, and Siegloch 2018; Akcigit et al. 2022). In Section IIIA, we detail the empirical implementation of

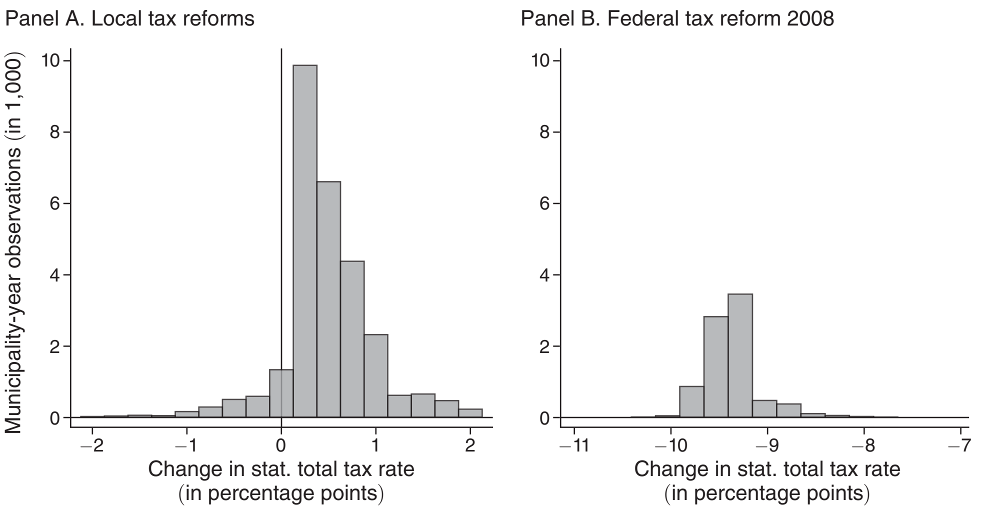


Figure 2. Municipal Variation in the STTR

*Notes:* This figure illustrates the variation in the STTR for corporate firms across all West German municipalities. Panel A illustrates the distribution of STTR changes driven by municipal scaling factor changes during the period 1987–2021. Panel B illustrates the indirect municipality-level variation in the STTR created by the federal-level tax reform in 2008.

this framework in light of the given institutional setting. In Section IIIB, we discuss the identification of causal effects using within-municipality variation in tax rates over time.

## A. *Event-Study Design*

We seek to estimate the causal effect of profit taxes on establishment-level innovation activities. We denote an R&D outcome of interest $Y_{i,t}$, where $t$ indicates the year and $i$ a firm of legal status $l$, belonging to sector $s$ (at the 1 digit level), located in a municipality $m$ and MSA $z$. In the baseline model, profit taxes are measured by the STTR, according to equation (4) for corporate firms and according to Supplemental Appendix equation (C.2) for noncorporate ones.[10]

Our baseline specification is an event-study model with an effect window of eight leads and eight lags. Formally, the model is given by

$$(5) \qquad Y_{i,t} = \sum_{k=-8}^{8} \beta_k T_{ml(i),t}^{k} + \mu_i + \theta_{zl(i),t} + \zeta_{sl(i),t} + \varepsilon_{i,t},$$

[10] While effective tax rates are relevant for firm decisions, we use variation in statutory tax rates driven by policy changes for identification of causal effects (see Suárez Serrato and Zidar 2016; Fuest, Peichl, and Siegloch 2018, for similar approaches).

where $T_{ml,t}^{k}$ is the treatment variable that captures leads and lags of changes in the STTR $\tau_{ml(i)}$—that is, tax reform events—faced by establishment $i$ as follows:

$$(6) \qquad T_{ml,t}^{k} = \begin{cases} \tau_{ml,2021} - \tau_{ml,t-k-2}, & \text{if} \quad k = -8 \\ \tau_{ml,t-k} - \tau_{ml,t-k-2}, & \text{if} \ -8 < k < 8 \\ \tau_{ml,t-k} - \tau_{ml,1985}, & \text{if} \quad k = 8. \end{cases}$$

Three aspects are noteworthy when looking at the definition of the treatment variable as denoted in equation (6). First, we account for the biennial structure of the *R&D Survey*, in which we observe outcomes in odd years only ($t = 1995, 1997, \ldots$). Hence, leads and lags of the treatment variable, $T_{ml,t}^{k}$, relate to the total change in tax rates over two consecutive years (and thus $k = -8, -6, \ldots, 8$). We normalize the last pretreatment coefficient, $\hat{\beta}_{-2}$, to zero such that all effects are relative to two years before treatment. Estimates are similar when using annual tax changes as explanatory variables. Second, treatment can be interpreted as a dummy variable for an event (tax increase/decrease) scaled by the size of the respective tax rate change. Hence, we extend the standard event-study design—relying on event dummy variables only—to exploit all available variation in tax rates. This generalized event-study model rests on the assumption that treatment effects are linear in the size of the tax rate change (Schmidheiny and Siegloch 2023). In additional analyses, we test for effect symmetry between tax increases and decreases, and we estimate standard event-study models using tax increase indicators only. Third, at the endpoints of the effect window (for $k = -8$ and $k = 8$), the treatment variable takes into account all observable tax changes happening outside the 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 2023). It rests on the assumption that treatment effects are constant beyond the endpoints of the effect window.

Equation (5) includes establishment fixed effects ($\mu_i$), which account for unobserved time-invariant confounders at the establishment level. In our baseline specification, we exclude establishments that relocate during the observation period. When adding these in a robustness check, we include municipality fixed effects in the regression model. The baseline model further absorbs secular time trends in a detailed way by including legal-form specific MSA-by-year ($\theta_{zl,t}$) and sector-by-year fixed effects ($\zeta_{sl,t}$). Absorbing these flexible time trends limits the identifying STTR variation to (i) direct changes in the municipal scaling factors and (ii) differential effects of federal-level tax reforms at the local level, as discussed in Section IIB. We calculate cluster-robust standard errors that account for potential correlations across establishments, years, and sectors within municipalities.

### B. *Identification*

To estimate causal effects, we relate changes in establishments' R&D activities to changes in municipalities' STTRs while absorbing legal-type specific time-varying shocks at the level of the MSAs and the 1 digit industries. To interpret estimates $\hat{\beta}_k$

as causal effects, we have to assume that tax changes are not systematically correlated with remaining trends in local factors that also affect establishments' R&D expenses or innovation output. Small and insignificant pretreatment 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—for example, if local policymakers would adjust LBT rates because of changes in establishments' R&D activities—we should observe diverging trends in R&D activities before treatment. Our empirical results (to be presented in Section IV) show no significant pretreatment differences for the set of 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 MSA-by-year fixed effects, which account for time-varying economic or political shocks at the level of the 74 West German MSAs (*Raumordnungsregionen*). MSAs are nested within states, which implies that we also control for changes in state policies and varying electoral cycles. On average, there are 37 municipalities and 165 establishments per MSA in our baseline sample.

To test the sensitivity of our results with regard to specification choices, we conduct four additional identification tests that we detail in Section IVA: (i) we move toward a border design by including more fine-grained region-by-year fixed effects, (ii) we test for the importance of observable local confounders as outcomes and controls, (iii) we merely rely on indirect local variation induced by the 2008 federal tax reform, and (iv) we only consider large local tax changes.

*Heterogeneous Treatment Effects*.—Recent contributions by de Chaisemartin and D'Haultfœuille (2020); Callaway and Sant'Anna (2021); Goodman-Bacon (2021); Sun and Abraham (2021); and Borusyak, Jaravel, and Spiess (2024) have shown that standard two-way fixed effects models with staggered treatment may deliver biased estimates in case of treatment effect heterogeneity across cohorts—that is, in case the impact of a given treatment varies with the year of its implementation. Given that we are facing an institutional setting with multiple treatments of different intensities per unit over time, we apply the estimator by de Chaisemartin et al. (2023) to test for heterogeneous treatment effects across cohorts, which yields similar results as our baseline estimates (see Section IVA).

## IV. Empirical Results

In the following, we present the results of our empirical analysis. In Section IVA, we first investigate the overall effect of changes in the STTR on establishments' R&D spending and, thus, provide the empirical test for Hypothesis 1 as derived in Section I. We also test the robustness of this result with regard to alternative specification choices and identification strategies. In Section IVB, we then use the detailed establishment-level data to open the black box of innovation production and analyze the underlying channels at work. In particular, we assess which types of (i) R&D inputs, (ii) firms, and (iii) research projects react the most to changes in profit taxes, 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 IVC.

### A. *Effect on Total R&D Spending*

Panel A of Figure 3 presents the dynamic effect of a change in the STTR on establishments' total R&D spending (in logs) based on the event-study model as defined in equation (5). Pre-trends are flat and statistically insignificant. Posttreatment, an increase in the STTR 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 four years and remains constant thereafter. Quantitatively, a 1 percentage point increase in the STTR is associated with a long-run decrease in establishments' R&D spending of around 8 percent. In terms of elasticities, the average treatment effect implies an R&D spending response with regard to the STTR of $-2.4$ (SE 0.59).

To make our tax elasticity comparable beyond the German setting, we translate it into an elasticity of R&D spending with respect to the user cost of capital. Following Yagan (2015), we calculate this elasticity as the ratio of the derived R&D spending elasticity with respect to the STTR, $\varepsilon_{\tau}^{RD}$, and the elasticity of the user cost of capital with respect to the tax rate, denoted by $\varepsilon_{\tau}^{CoC}$: $\varepsilon_{CoC}^{RD} = \varepsilon_{\tau}^{RD}/\varepsilon_{\tau}^{CoC}$. In the German setting, the user cost of capital is given by $CoC = r/(1-\tau)$, with $r$ referring to the pretax rate of return and $\tau$ to the STTR. Using the average STTR during the period under investigation ($\tau = 39.2\%$), the user cost elasticity with respect to the tax rate is $\varepsilon_{\tau}^{CoC} = -0.65$. The corresponding implied elasticity of R&D spending with regard to the user cost of capital then amounts to around $-3.7$. This elasticity is similar to a recent estimate by Dechezleprêtre et al. (2023), which reports a user cost elasticity with regard to R&D spending of $-4.1$ in the context of a reform in the United Kingdom that eased R&D deduction possibilities for medium-sized firms.

*Tax Increases versus Decreases*.—Next, we explicitly test for asymmetric effects of increases versus decreases in the STTR on establishments' (log) total R&D spending. Recall from Section IIB that we face an environment in which the STTR for both corporate and noncorporate firms decreased over time. This is particularly due to meaningful tax reforms in the CIT and PIT rates at the national level. However, the evolution of the LBT rate, with many municipalities implementing tax increases of different size at varying times, slowed down this overall decrease in the STTR to a varying extent across German municipalities. To test for asymmetry in the effect of profit tax rates on R&D spending, we adjust equation (5) and include two sets of event-study treatment variables: one set where we scale treatment indicators for tax increases with the size of the tax increase, and another set where we scale indicators for tax decreases with the actual size of the cut. Panel B of Figure 3 presents the corresponding effects. We detect very small and statistically insignificant pre-trends for both tax increases and decreases. Posttreatment, estimated effects are very similar both in terms of size and dynamics. As a consequence, we cannot reject symmetric effects for tax increases and decreases. As most decreases in the STTR are induced

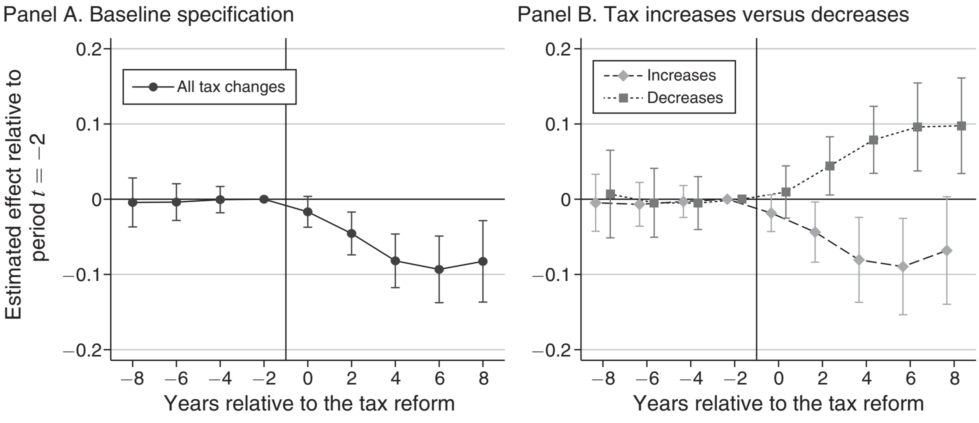


Figure 3. The Effect of Profit Taxes on R&D Spending

*Notes:* This figure plots the point estimates, $\hat{\beta}_k$ $\left(k \in [-8, -6, \ldots, 8]\right)$, and corresponding 95 percent confidence intervals for a 1 percentage point increase in the STTR on establishments' annual total R&D spending (in logs) using the event-study model as defined in equation (5). The model includes establishment fixed effects as well as legal-form specific MSA-by-year and sector-by-year fixed effects. Panel B is based on the same specification of the event-study model but allows for effect asymmetry of tax increases versus decreases by using two distinct sets of treatment variables. In both panels, the STTR changed in year $t = 0$ or $t = -1$ for the treated establishments. Standard errors are clustered at the municipality level. The corresponding regression coefficients are provided in Supplemental Appendix Table D.1.

by federal-level tax cuts and most increases are driven by municipality-level tax hikes, we cannot reject symmetric effects for federal versus local tax changes, either.

*Identification Test: Toward a Border Design*.—To test the sensitivity of our estimates with respect to the potential presence of regional shocks at varying geographical levels, we deviate from our baseline model as defined in equation (5) and replace the MSA-by-year fixed effects with finer region-by-year fixed effects. Precisely, we absorb common shocks at the level of the 204 West German commuting zones (*Arbeitsmarktregionen*) and the 325 counties (*Kreise und kreisfreie Städte*), respectively.[11] For comparison, we also provide estimates that account for shocks at more aggregate levels: at the 28 administrative districts (*Regierungsbezirke*, in the second subdivision level of Eurostat's Nomenclature of Territorial Units for Statistics) and the ten West German federal states. If estimates are stable across specifications that account for local shocks at smaller administrative levels, we would interpret this as supporting evidence against the presence of systematic local shocks threatening identification. In an additional test, we further absorb time-varying shocks at the level of the 558 West German three-digit zip code areas (*Postleitzahl*). In many instances, zip codes cut through municipalities or counties, which allows for comparisons of establishments in two neighboring municipalities within the same zip code—that is, the comparison of establishments in close geographic proximity—that are exposed to different changes in the STTR. The specification, hence, accounts for fine-grained local

[11] Supplemental Appendix Figure C.3 depicts these different regional classifications for the federal state of Bavaria for illustration.

shocks that are unrelated to possible policy setting at administrative levels. In general, by including fine-grained region-by-year fixed effects, we move toward conventional border designs (see, e.g., Dell 2010; Ehrlich and Seidel 2018) as we compare municipalities and establishments in close geographic proximity.[12]

Supplemental Appendix Figure A.3 shows the corresponding results. Overall, we detect very similar posttreatment effects when including finer region-by-year fixed effects. Pretreatment coefficients remain flat and statistically indistinguishable from zero, too. While we take these effects as evidence in favor of our proposed identification strategy, the results also support 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 change in tax rates 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 remain similar when including finer regional time-varying controls suggests that the SUTVA is not violated.

*Identification Test: Local Confounders as Outcomes and Controls*.—Economic or political shocks may also occur at the municipality level and potentially coincide with the timing and size of a tax change. In this regard, one might particularly worry that local economic developments at the municipal level simultaneously determine the LBT and establishment activities. We address this concern in two ways. First, we show in Supplemental Appendix Figure A.4 that socioeconomic indicators at the municipality level (population, the stock of unemployed, public expenditures, and revenues) do not display any systematic pre-trend when used as dependent variables in the event-study model. We do not detect any confounder that can explain the change in local tax rates. The (imprecise) increase in revenues after a tax increase and the slightly negative effect on expenditures suggests that municipalities use tax increases to balance their budgets—a finding in line with related studies investigating municipal taxes in Germany (Fuest, Peichl, and Siegloch 2018; Blesse, Doerrenberg, and Rauch 2019; Löffler and Siegloch 2021). 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 when including these variables (see Supplemental Appendix Figure A.5).

*Identification Test: Exploiting the 2008 Federal Tax Reform*.—As discussed in Sections IIB and IIIA, variation in the STTR is induced by both changes in the LBT rate at the municipality level and federal tax reforms exerting differential effects across municipalities due to preexisting differences in LBT rates. To further corroborate the robustness of our baseline findings, we next base identification on variation induced by the 2008 federal tax reform only. The reform reduced the basic federal rate of the LBT $\phi_{fed}^{LBT}$ from 5 percent to 3.5 percent and eliminated the deductibility

[12] Standard spatial regression discontinuity designs are difficult to implement in our setting because we have many borders and many tax rate changes.

of LBT payments from the profit tax base. Overall, the reform led to a tax cut in all municipalities. However, tax cuts were smaller in municipalities with higher local scaling factors $\sigma_{mun}^{LBT}$ prior to the reform. We exploit this fact by implementing a dynamic difference-in-difference design that compares the evolution of establishments' log R&D spending in municipalities that were relatively less affected by the reform to establishments' R&D spending in municipalities that were more affected:

$$(7) \quad Y_{i,t} = \sum_{k=2003}^{2017} \gamma_k SmallerTaxCut_{m(i)} \mathbf{1}\{k = t\} + \mu_i + \theta_{z(i),t} + \zeta_{s(i),t} + \varepsilon_{i,t}.$$

We set the cutoff determining treatment and control group municipalities at the prereform scaling factor of 4.3 to yield substantial variation in the reform-induced average tax cut between the two groups. The average tax cut for municipalities with a local scaling factor below 4.3 amounted to 25 percent ($-9$ percentage points), and to 20 percent ($-8$ percentage points) for municipalities with a local scaling factor equal or above 4.3.

Panel A of Supplemental Appendix Figure A.6 shows the evolution of average STTRs for treatment and control corporations around the reform. It illustrates that average tax rates in the treatment and control group evolved in parallel before 2008. We consider those municipalities as treated that saw smaller tax cuts (25 percent of the covered municipalities)—we thus expect to find negative estimates $\hat{\gamma}_k$. As in our baseline specification, we include establishment fixed effects $\mu_i$, and account for MSA-by-year as well as sector-by-year fixed effects, denoted by $\theta_{z,t}$ and $\zeta_{s,t}$, respectively. Because of the establishment fixed effects, we set one treatment coefficient, $\gamma_{2007}$, to zero. Estimates of $\gamma_{k \neq 2007}$ capture the causal effect of the 2008 reform on establishments' total R&D spending under standard identifying assumption. Note that we estimate an intent-to-treat effect as municipalities may have reacted to the federal tax reform by adjusting local scaling factors in turn.

Panel B of Supplemental Appendix Figure A.6 depicts the corresponding estimates $\hat{\gamma}_k$. While pre-trends are more noisy compared to the baseline model, they are not statistically significantly different from zero. Posttreatment effects are negative and show a pattern that is in line with our baseline results presented in panel A of Figure 3. The effect builds up over time, becomes statistically significant four years after the tax reform, and levels off thereafter. As the difference in the average STTR cut for treatment and control group amounts to approximately 1 percentage point, estimates are also comparable to the baseline results in terms of magnitude.

*Identification Test: Large Increases in the LBT.*—Mirroring the idea of the previous sensitivity check, we also limit identification to variation in the municipality-specific scaling factor of the LBT. As shown in Section IIB, most changes in the STTR are attributable to municipalities' adjustments of the local scaling factor.[13] Estimated effects are very similar compared to our baseline specification when restricting identification to variation in $\sigma_{LBT}^{mun}$—see Supplemental Appendix Figure A.7. The same

[13] Compare Section IIB for details. In earlier versions of this paper, we merely relied on this source of variation for identification.

conclusion arises when limiting identifying variation in the local scaling factor to tax increases only and specifying treatment via indicator variables. Increases make up for around 90 percent of all changes in the LBT. In a last step, we follow common practice (see, e.g., Fuest, Peichl, and Siegloch 2018 and Akcigit et al. 2022) and limit identification to large local tax reforms, defined as those above the median tax increase. The average tax increase in the corresponding estimation sample amounts to 1.1 percentage points, which is twice the average increase. Despite these changes in the specification of the event-study model, estimated effects remain qualitatively unaffected (see Supplemental Appendix Figure A.7).

*Further Robustness Checks*.—We provide additional sensitivity checks to ensure that our results are not driven by modeling assumptions. First, we apply the estimator by de Chaisemartin et al. (2023) that is robust to the presence of heterogeneous treatment effects across cohorts in environments with multiple treatments. To ensure a sufficiently high number of switchers, we reduce the number of estimated leads from eight to six and use state-by-year instead of MSA-by-year fixed effects to control for regional time-varying shocks. Second, we use different effect windows—a modeling choice that also affects the number of survey years covered in the estimations. Third, we cluster standard errors at different geographical levels. Fourth, we run the analysis on a balanced sample and assess whether firm mobility affects the results. Fifth, we exploit one-year instead of two-year tax rate differences. Sixth, we compare our baseline results to estimates based on various subsamples. Supplemental Appendix Figure A.8 shows the corresponding results. Our findings survive all these additional sensitivity checks.

## B. *Mechanisms and Heterogeneity*

Next, we explore the mechanisms behind the effect of profit tax changes on establishments' R&D activity. In particular, we assess (i) which R&D input factors are adjusted, (ii) which types of firms respond, (iii) what kind of research projects are affected the most, and (iv) if and how other (non-R&D) firm outcomes are affected by a change in the profit tax rate.

*Effects by R&D Inputs*.—We first analyze the effect of STTR changes on different R&D input factors. We start by investigating potentially heterogeneous effects for internal versus outsourced 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 IIA, 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 firms' responses to changes in the business tax at the margin.

Panels A and B of Figure 4 present the corresponding event-study estimates for internal versus outsourced R&D spending, respectively. From panel A we infer that an increase in the STTR leads to a sizable reduction in establishments' internal R&D expenditures. Panel B investigates the corresponding effect on external R&D.

In our baseline sample, around half of all establishments display positive external R&D spending at some point. However, only roughly 20 percent of the covered establishments contract external partners on a regular basis. Using a linear probability model, we first test for effects at the extensive margin. We find no evidence that profit taxes affect the probability of (un)contracting external firms or institutions. When looking at the intensive margin of external R&D expenditures (conditional on outsourcing), we find a long-run effect that is of similar magnitude as the effect on internal R&D spending—yet less precisely estimated. The effect on external R&D, however, materializes with a temporal lag. We cautiously interpret the difference in effect dynamics as suggestive evidence in favor of Hypothesis 2: whereas internal resources devoted to a given R&D project can be scaled up or down relatively easily by assigning more or less staff and materials to other non-R&D projects, existing contractual arrangements with external partners are more difficult to adjust in the short run.

With regard to R&D input factors, panel A further reveals that the estimated reduction in establishments' internal R&D spending in response to an increase in the STTR is due to adjustments in both labor and capital inputs, which suggests that firms scale down overall R&D production as a response to higher tax burdens (similar to in Curtis et al. 2021). Capital is usually seen to be more mobile and, hence, expected to respond more strongly according to Hypothesis 2. We find that capital indeed responds somewhat more strongly than labor, with average posttreatment effects amounting to $-0.076$ and $-0.059$, respectively. However, we cannot reject that capital and labor responses are proportional.

In panel C of Figure 4, we decompose the effect on the R&D input labor into an employment and a wage margin. We find a negative effect of an increase in the STTR on the average wage of R&D employees—a finding in line with evidence by Fuest, Peichl, and Siegloch (2018)—as well as an effect on establishments' R&D staff. The latter one is more precisely estimated and quantitatively more important; around 65 percent of the overall reduction in spending on R&D personnel can be linked to reduced employment, the remaining 35 percent to lower average wages.

In panel D of Figure 4, we last investigate whether specific R&D workers are more affected by an increase in the profit tax. According to Hypothesis 2, we might expect effects on high-skilled R&D staff to be more attenuated (due to different adjustment costs). The respective estimates, however, suggest that establishments reduce their scientific and support staff by equal proportions.

*Effects by Firm Characteristics*.—We next test for heterogeneous effects among different types of firms. In detail, we focus on heterogeneity by R&D financing, size, and corporate structure.[14]

Imperfect financial markets might lead to socially suboptimal levels of innovation. As R&D projects are relatively risky and often lack collateral, they are predominantly financed via equity. In other words, it is hard to raise external debt for R&D projects

[14] To account for endogeneity concerns, we use each firms' first available information on the respective variables in the dataset when testing for heterogeneity along these margins.

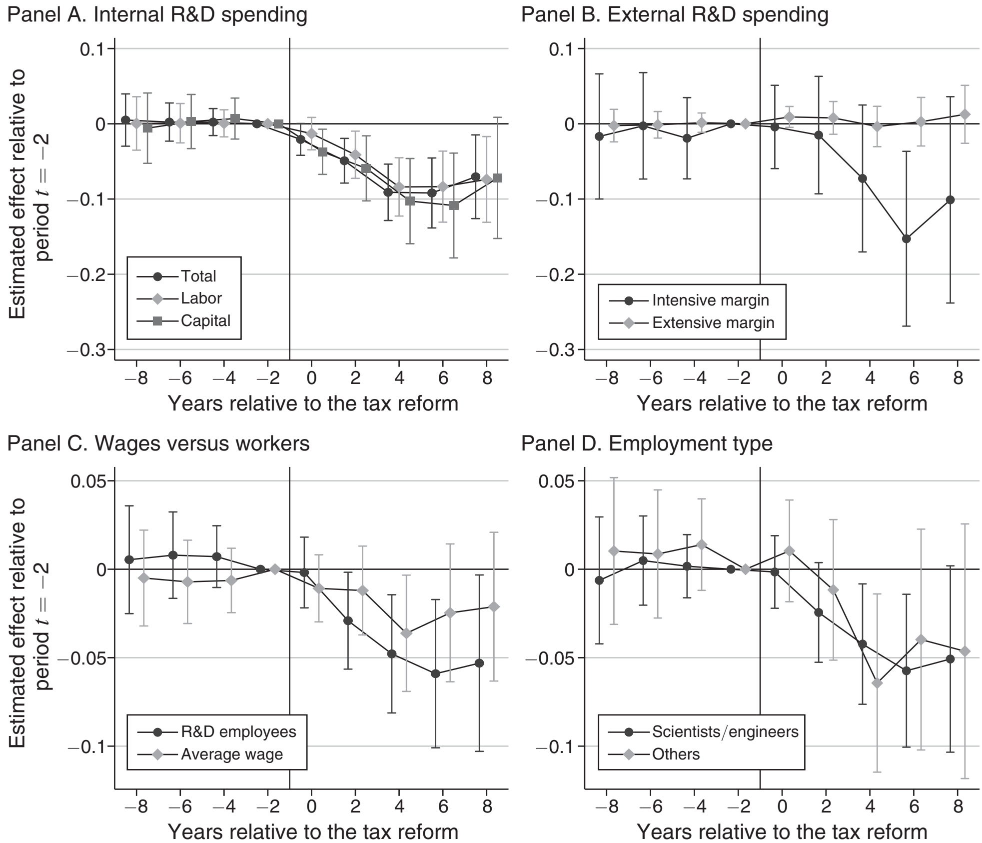


Figure 4. The Effect of Profit Taxes on Various R&D Inputs

*Notes:* This figure plots the point estimates, $\hat{\beta}_k$ $\left(k \in [-8,-6, \ldots ,8]\right)$, and corresponding 95 percent confidence intervals for a 1 percentage point increase in the STTR on various R&D outcomes using the event-study model as defined in equation (5). In panel A, the dependent variable refers to establishments' annual total internal R&D spending as well as their annual internal R&D spending on labor and capital/materials, respectively. All outcomes are in logs. In panel B, the dependent variable refers to (i) a dummy variable indicating whether an establishment engages in external R&D in a given year as well as (ii) the annual amount of external R&D expenditures (in logs). In panel C, we analyze effects on annual average wages for R&D employees and the stock of R&D staff. In panel D, we assess effects on the number of employed scientists/engineers and nonscientific R&D staff, respectively. All outcomes in panels C and D are in logs. The regressions include establishment fixed effects as well as legal-form specific MSA-by-year and sector-by-year fixed effects. In all panels, the STTR changed in year $t = 0$ or $t = -1$ for the treated establishments. Standard errors are clustered at the municipality level. The corresponding regression coefficients are provided in Supplemental Appendix Table D.1.

and certain firms might face credit constraints impeding their innovation production. Hypothesis 3 shows that an increase in the profit tax should have stronger negative effects on R&D spending if firms rely on equity- rather than debt-financing because these financing costs cannot be used as a tax shield. To test this hypothesis we use firm-level information on the noncurrent liabilities to sales ratio as a proxy for credit constraints, assuming that firms with a higher ratio face higher interest rates and are, thus, more credit constrained. Panel A of Figure 5 shows that more credit-constrained firms indeed react stronger than financially unconstrained ones

($p$-value 0.09). We take this result as suggestive evidence in line with Hypothesis 3 and earlier studies by, for example, Zwick and Mahon (2017); Guceri and Liu (2019); and Moon (2022), who all detect similar effect patterns in other contexts.[15]

In many countries, policymakers try to address this market imperfection by providing R&D tax incentives for credit-constrained firms. As it is difficult to observe the financial situation of firms, many targeted R&D policies use firm size as a proxy. The hypothesis is that small- and medium-sized firms are more responsive to a given level of support due to more severe financing constraints (Gonzales-Cabral, Appelt, and Galindo-Rueda 2018). In panel B of Figure 5, we investigate whether an increase in the STTR indeed affects smaller and larger firms (as measured by the number of employees) to a different extent. We find no evidence for statistically significant differences between smaller and larger firms ($p$-value 0.55). Panel C corroborates this result. Long-run treatment effects are similar across quartiles of the firm size distribution, too. Moreover, the pattern remains unaffected when size is proxied by firms' total sales rather than their overall number of employees.

Recall that Hypothesis 2 predicts tax effects to be larger in case marginal adjustment costs are lower. This might imply that firms, which have the option to shift R&D activities to other sites within the same firm or corporate group, face lower costs and, hence, respond more strongly to a given tax change. Among others, Dischinger and Riedel (2011); Karkinsky and Riedel (2012); and Griffith, Miller, and O'Connell (2014) provide evidence for profit shifting via intangible assets in the context of cross-country differences in corporate tax rates. While we cannot isolate shifting responses within a firm (cf. Section IIA), we can investigate shifting within a corporate group—which has been the key focus of the literature on shifting within multinationals. Using the BvD data, we can observe whether different firms in the *R&D Survey* have the same ultimate owner. Belonging to a corporate group with another R&D-active firm should make it less costly to adjust to a tax increase via shifting behavior and should lead to larger decreases in R&D spending, in turn (cf. Hypothesis 2). Panel D of Figure 5 confirms this prediction, as we find statistically significantly larger responses in R&D spending for firms that are part of a corporate group with multiple R&D sites ($p$-value 0.05). Note that in light of the results presented in panels B and C of the figure, this pattern is unlikely to be driven by differences in firm size.

*Effects by Project Types*.—We next analyze which types of R&D projects are mostly affected by changes in the STTR. We pursue this exercise by looking at patents as an output-based measure of innovation (cf. Section IIA). Unlike R&D spending, patenting is a rare event. In our baseline sample, only around 25 percent of the R&D-active establishments file at least 1 patent throughout the sampling period, and just 4 percent of the covered establishments file at least 1 patent in each year.

Hence, we first look at extensive-margin responses. Using a linear probability model, we estimate the effect of an increase in the STTR on establishments'

[15] Information on firms' noncurrent liabilities is taken from the BvD data and is only available for a subset of surveyed establishments. To test for selection effects, we reestimate the event-study model separately for establishments with financial data. We find very similar baseline effects when restricting the sample accordingly (see panel F of Supplemental Appendix Figure A.8).

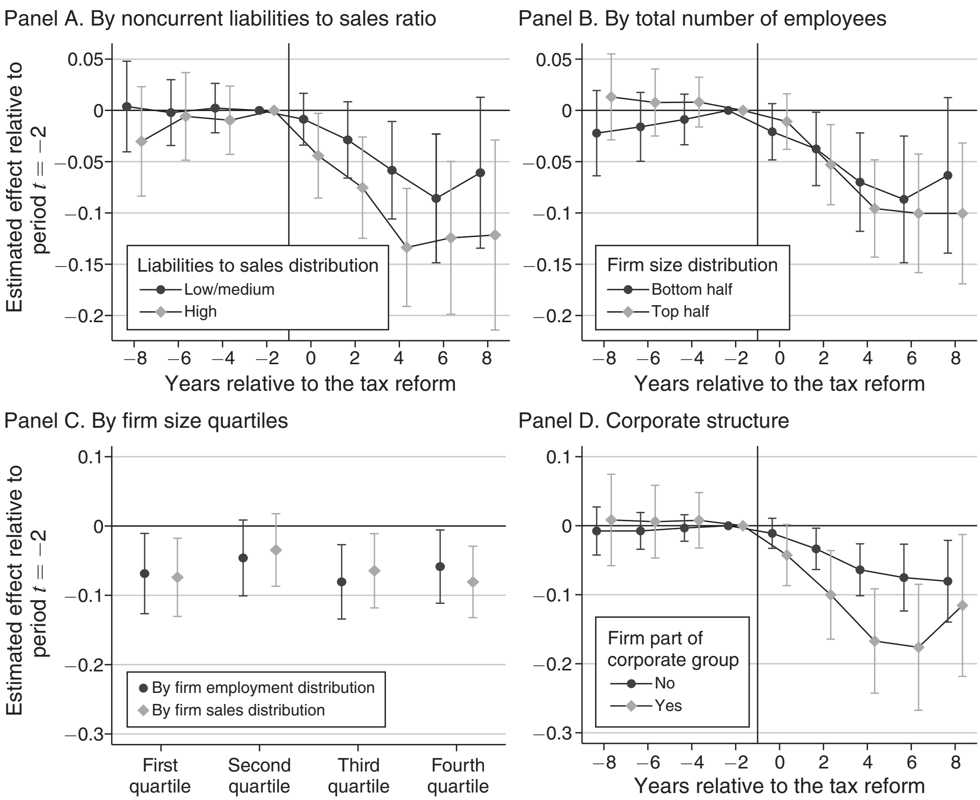


Figure 5. The Effect of Profit Taxes on R&D Spending—Heterogeneity by Firm Characteristics

*Notes:* This figure plots the point estimates, $\hat{\beta}_k$ $(k \in [-8, -6, \ \dots \ ,8])$, and corresponding 95 percent confidence intervals for a 1 percentage point increase in the STTR on establishments' annual total R&D spending (in logs) using the event-study model as defined in equation (5). In panel A, we allow for heterogeneous effects for capital-constrained versus unconstrained firms as approximated by their noncurrent liabilities to sales ratio. In panels B and C, we test for differential effects among firms of different size. In panel B, we plot the corresponding event-study estimates when allowing for heterogeneous effects for firms below and above the median of the firm size distribution (in terms of employees). In panel C, we plot the corresponding average treatment effects when allowing for heterogeneous effects among firms of different size in more detailed ways, using quartiles of the firm size distribution in terms of employment and sales, respectively. Last, in panel D we test for different effects among firms that belong to a corporate group with another R&D-active firm versus those who do not. All regressions include establishment, as well as legal-form specific MSA-by-year, sector-by-year, and group-by-year fixed effects. In all panels, the STTR changed in year $t = 0$ or $t = -1$ for the treated establishments. Standard errors are clustered at the municipality level. The corresponding regression coefficients are provided in Supplemental Appendix Table D.2.

probability of filing a patent in a given year. Panel A of Figure 6 displays the corresponding estimates. The probability of filing a patent seems to decrease in response to an increase in the STTR—with the average treatment effect amounting to around $-7$ percent relative to the variable's mean—but effects are somewhat imprecisely estimated. We find similar extensive-margin effects when only looking at cited patents.

We next investigate the corresponding effects on establishments' annual (citation-weighted) number of filed patents. In our baseline model, we follow

Dechezleprêtre et al. (2023) by specifying the outcome variables in levels and estimating effects via OLS.[16] Panel B depicts the corresponding results. Treatment effects are similar for both outcomes but more pronounced (and statistically significant) when accounting for the varying economic value of patents. Quantitatively, a 1 percentage point increase in the STTR is associated with a decrease in establishments' citation-weighted number of patents of around 9.5 percent, which yields a patent elasticity with regard to the STTR of $-3.6$.

In panel C, we test for heterogeneous effects among different types of R&D projects by looking at the effect of an increase in the STTR on the citation-weighted number of process and product innovations, respectively. We find stronger effects on process rather than product innovations. Whereas product innovations are generally associated with the commencement of new market activities, process innovations typically concern internal production processes and workflows (Klepper 1996). Our results suggest that these internal optimization activities are scaled back when profit tax rates increase.

*Other Firm Outcomes*.—Last, we use our combined data to assess the effects of changes in the STTR on non-R&D outcomes (cf. Section IIA). In particular, we study how (i) total and non-R&D investments, (ii) the level of non-R&D employment, as well as (iii) the labor productivity and profitability of R&D-active firms change in response to an increase in the profit tax rate. These additional outcomes are either observed in the *R&D Survey* (employment, productivity) or obtained through the survey's link to the BvD data (total investment, profitability). Irrespective of the data source, results of this part of the analysis warrant careful interpretation as measures of non-R&D outcomes are not necessarily confined to the R&D-active establishment. In case a given R&D establishment belongs to a multiestablishment firm, the non-R&D outcomes comprise information from all establishments of this firm, which are possibly located in different municipalities with varying tax rates. Hence, if the observed R&D site was small relative to the rest of the firm we might expect small effects. In case of perfect shifting to non-R&D sites within a given firm, we should not see any effects on non-R&D outcomes either.

Panel A of Supplemental Appendix Figure A.10 first contrasts the estimated effect on establishments' R&D capital spending (see also panel A of Figure 4) with the effect on firms' total and non-R&D investments. We see that firms' total investments also decrease in response to an increase in the STTR. The estimated effect is of similar size as the one on R&D capital spending but less precisely estimated. For non-R&D investments only, we see no clear effect pattern. Point estimates are small and imprecisely estimated. We cautiously interpret this finding in line with Hypothesis 3; because of lacking collateral and the projects' higher risk, R&D investments are generally more heavily financed via equity than non-R&D investments and, thus, react more strongly to an increase in the profit tax.

[16] We find qualitatively very similar results when employing a count data model using Poisson-pseudo maximum likelihood instead of the OLS regressions (see Supplemental Appendix Figure A.9).

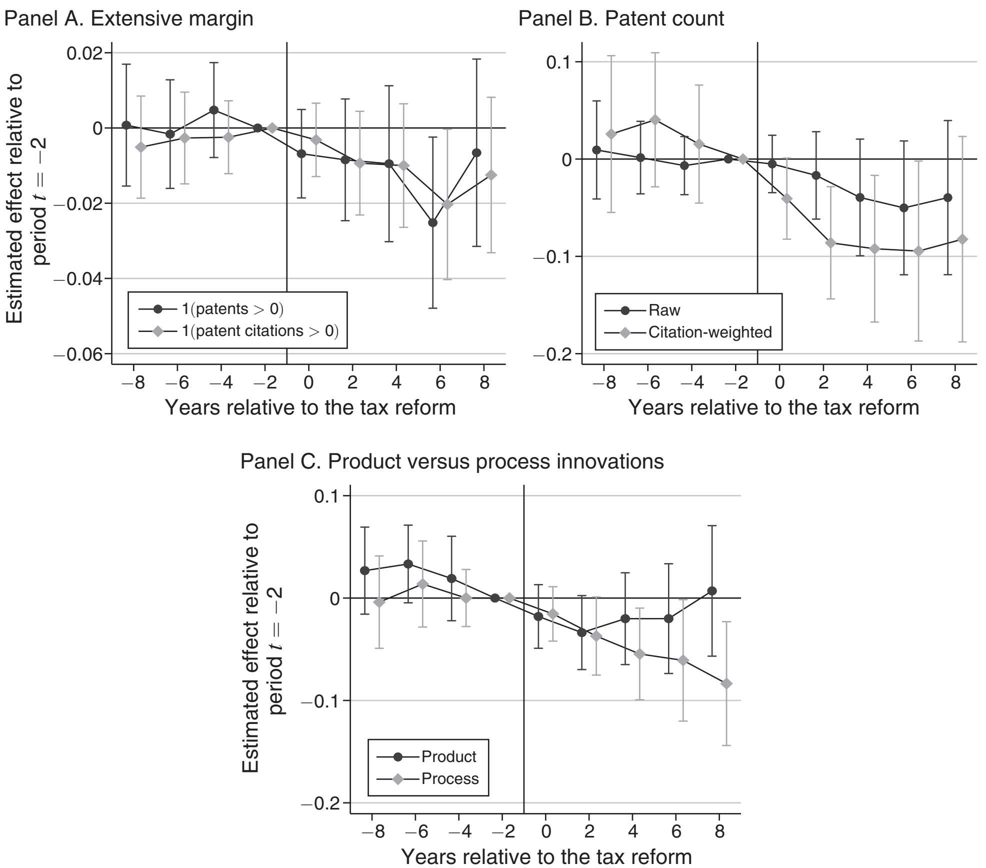


Figure 6. The Effect of Profit Taxes on Patenting Activity

*Notes:* This figure plots the point estimates, $\hat{\beta}_k$ $(k \in [-8, -6, \ldots, 8])$, and corresponding 95 percent confidence intervals for a 1 percentage point increase in the STTR on establishments' patenting activities using the event-study model as defined in equation (5). The dependent variable refers to an establishment's probability of filing a patent (that is cited by another patent within the first five years of its registration) in a given year in panel A, to the (citation-weighted) number of patents filed in panel B, as well as the citation-weighted number of product and process patents in panel C, respectively. Outcomes in panels B and C are specified in levels. All regressions include establishment fixed effects as well as legal-form specific MSA-by-year and sector-by-year fixed effects, and are estimated using OLS. In all panels, the STTR changed in year $t = 0$ or $t = -1$ for the treated establishments. Standard errors are clustered at the municipality level. The corresponding regression coefficients are provided in Supplemental Appendix Table D.1.

In panel B of Supplemental Appendix Figure A.10, we next compare the effect on R&D employment to the one on firms' non-R&D employment. Whereas the number of R&D employees declines in response to an increase in the profit tax rate, firm-level non-R&D employment remains unaffected. Panel C of Supplemental Appendix Figure A.10 further shows that firms' total sales per employee are not affected by changes in the STTR either. Firms scale down innovation production by reducing R&D inputs (cf. panel A of Figure 4), but overall labor productivity does not decrease. In contrast, we find some suggestive evidence that long-run profitability seems to decrease as innovative activity is scaled down (same panel). Effects are imprecisely estimated but suggest that firms' earnings EBITDA decline in the long run.

### C. *Discussion of Results and Policy Implications*

Overall, our results suggest that firms respond to a tax change by scaling down innovation production. While such reductions do not impact labor productivity, they tend to manifest in the long-term profitability through the loss of potentially valuable R&D activities that particularly affect innovations of internal production processes and workflows. In this section, we discuss the implications of these results for R&D tax policy.

*Cost Effectiveness.*—At first sight, our main results might be interpreted in favor of profit tax cuts to foster firm-level innovation. However, it is unclear whether such a reform is the most effective policy option. In the following, we assess the cost effectiveness of profit tax cuts compared to targeted R&D (tax) policies. Our estimates displayed in panel B of Figure 3 imply that R&D expenses increase by around €22,800 for the median R&D-active establishment in case of a 1 percentage point decrease in the STTR at the local level. Assuming no other behavioral changes, the average local government thus has to forgo €1 of tax revenue in order to raise establishments' R&D expenditures by €0.26.[17] This cost efficiency estimate is rather low compared to estimates for targeted tax incentives as, for example, implemented in the United Kingdom. Guceri and Liu (2019) and Dechezleprêtre et al. (2023) estimate that the UK R&D tax relief scheme generated around £1.0–£2.3 of additional R&D spending for each Great Britain pound of lost tax revenue. The higher cost-effectiveness of targeted R&D tax incentives appears plausible because changes in profit tax rates also affect those firms not engaged in R&D. In Germany, only around 4 percent of all manufacturing establishments report R&D activities. Consequently, boosting establishment-level innovation via reduced profit taxes might be relatively ineffective compared to targeted (tax) policy instruments.

*Implications for Targeted R&D Incentives.*—Our results thus support the common practice of combining a general business tax to raise revenues with targeted R&D (tax) policy instruments to stimulate R&D. To date, these instruments typically target smaller firms (in terms of sales or employment), in particular because of policymakers' implicit beliefs that credit market frictions make smaller firms more liquidity-constrained and, hence, more sensitive to tax policy (Gonzales-Cabral, Appelt, and Galindo-Rueda 2018). Our results suggest that size might be a poor proxy for firms' financial constraints. We find no differential responses in R&D spending with regard to an increase in the STTR across the firm size distribution.

[17] We arrive at this measure of cost effectiveness by totaling establishments' simulated increases in R&D spending for a 1 percentage point decrease in the STTR at the municipality level and dividing this number by the mechanical loss in municipal tax revenues. We then assess the median cost efficiency across municipalities with at least one R&D-active establishment.

By contrast, when using a more precise proxy of firms' financial situation—the noncurrent liabilities to sales ratio—we do find the expected pattern; firms subject to liquidity constraints reduce their R&D spending more strongly than unconstrained ones. We take this finding as evidence that firm size might not be the ideal characteristic to identify credit-constrained firms. This conclusion lines up with recent empirical and theoretical evidence in related contexts. Curtis et al. (2021) studies the effect of accelerated depreciation rules on firms' investment decisions and does not find significantly different effects by firm size either. Using a Schumpeterian growth model and data from Norway, Galaasen and Irarrazabal (2021) shows 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, Hanley, and Stantcheva (2022) further shows that, from a theoretical perspective, optimal targeted R&D policies shall be directed toward the most R&D-productive firms.

## V. Conclusion

In this paper, we study the effect of profit taxes on establishments' R&D spending and innovation. Using unique panel data targeting all R&D-active firms in Germany, we shed light on the production of innovation at the level of the establishment and its responsiveness to tax incentives. To identify causal effects, we exploit variation from more than 10,000 changes in the LBT as well as around 20 federal tax reforms with locally varying impact using event-study models with staggered treatment.

We find that an increase in the STTR has a statistically significantly negative effect on establishments' overall R&D expenditures. Estimates are quantitatively sizable, the elasticity of R&D spending with respect to the user cost of capital amounting to $-3.8$. We further find that establishments respond symmetrically to tax increases and decreases. Looking deeper inside the process of innovation production, we show that internal and outsourced R&D respond similarly to tax changes in the long run. However, the effect on external R&D takes longer to unfold—potentially because of higher marginal adjustment cost. We further show that establishments scale down overall innovation production in response to a tax increase, reducing both labor and capital R&D-inputs.

In terms of firm heterogeneity, we show that—in line with theoretical priors—credit-constrained firms respond statistically significantly more strongly to tax changes than unconstrained ones. In contrast, we detect no evidence for heterogeneous effects across the firm size distribution. Finally, we show that patents, as an outcome-based measure of innovation, also respond to changes in the STTR. The number of patented process innovations declines more strongly than the number of product innovations.

The results of our study have important implications for tax policy setting. We show that profit taxes affect establishments' R&D spending and innovation output. However, the effectiveness of reduced profit taxes to boost firm-level R&D is relatively low compared to targeted 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 targeted tax policy instruments to stimulate firm-level R&D. Our findings, however, question the common feature of using firm size as a characteristic to identify liquidity-constrained firms.

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