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Do Higher Corporate Taxes Reduce Wages? Micro Evidence from Germany[†]

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This paper estimates the incidence of corporate taxes on wages using a 20-year panel of German municipalities exploiting 6,800 tax changes for identification. Using event study designs and difference-in-differences models, we find that workers bear about one-half of the total tax burden. Administrative linked employer-employee data allow us to estimate heterogeneous firm and worker effects. Our findings highlight the importance of labor market institutions and profit-shifting opportunities for the incidence of corporate taxes on wages. Moreover, we show that low-skilled, young, and female employees bear a larger share of the tax burden. This has important distributive implications. (*JEL* H25, H31, H71, J16, J24, J31)

The incidence of corporate taxation is a key issue in tax policy debates. The distribution of the tax burden between labor and capital has important implications for the progressivity of the tax system. According to surveys, most people think that capital owners bear the burden of corporate taxation.[1] Business lobbyists, in contrast, argue that the tax reduces investment so that labor productivity and wages decline, which means that workers bear the tax burden. Most economists take a middle ground and think that the tax burden is shared between labor and capital. Yet, even among researchers in the field, there is substantial disagreement about how

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[1] See, e.g., Sheffrin (1994) and various Gallup polls (http://www.gallup.com/poll/1714/taxes.aspx).

much of the burden is shifted to workers.[2] The main reason is that credible empirical evidence on the causal effect of corporate taxes on wages is scarce. In this paper, we revisit the question of how corporate taxes affect wages.

We exploit the specific institutional setting of the German local business tax (LBT) to identify the corporate tax incidence on wages. The German setting is well suited for several reasons. First, there is substantial tax variation at the local level. From 1993 to 2012, on average about 10 percent of all municipalities adjusted their LBT rates annually, resulting in 17,999 tax changes in 10,001 municipalities between 1993 and 2012. Second, municipalities can only change the LBT *rate*. The tax base definition and rules about which types of firms are liable to the tax are determined at the federal level.[3] Moreover, municipal autonomy in setting tax rates allows us to treat municipalities as many small open economies within the highly integrated German national economy, with high mobility of capital, labor, and goods across municipal borders. In this setting, general equilibrium effects on interest rates or consumer prices, which may complicate measuring the incidence of the tax on workers, are likely to be of minor importance.

Our analysis combines administrative panel data on the universe of German municipalities with administrative linked employer-employee micro data from social security records. In these data, we observe firms in 3,522 municipalities, leaving us with 6,802 tax changes for identification. We use nonparametric event study designs to show that wages decrease significantly after tax increases. At the same time, they do not react in the periods prior to a tax reform. In addition, we use the event study setup to show that tax reforms are not driven by local business cycles. These flat pre-trends support our identifying assumption and the causal interpretation of our estimates.

We then estimate difference-in-differences (DiD) models to quantify the magnitude of the wage response. Averaging over all firms liable to the LBT, we find that workers bear approximately 51 percent of the total tax burden.[4] Our findings are robust to the inclusion of a comprehensive set of flexible nonparametric local controls at different aggregation levels, suggesting that potentially relevant omitted variables such as local shocks are not driving the results. Finally, we estimate various heterogeneous firm and worker effects and discuss the results with regard to different (labor market) theories.

We contribute to the literature in several ways. We provide new estimates of the corporate tax incidence on wages by exploiting the compelling German institutional setting. So far, credible empirical evidence on the incidence of corporate taxes has been scarce because sufficient and exogenous variation in corporate tax rates is lacking in previous studies. While cross-country research designs (such as Hassett and Mathur 2006; Felix 2007; Desai, Foley, and Hines 2007; Clausing 2013; Azémar and Hubbard 2015) need to defend their (implicit or explicit) common trend

[2] For example, public economists surveyed by Fuchs, Krueger, and Poterba (1998) respond on average that 40 percent of the corporate tax incidence is on capital, leaving a substantial share of the burden for labor (and landowners or consumers). However, one-quarter of the surveyed economists believed that the capital share is below 20 percent, while another one-quarter believed the share to be 65 percent or higher.

[3] Kawano and Slemrod (2016) compare a large number of reforms of nationwide corporate taxes and show that tax rate changes are usually combined with changes in the tax base as well.

[4] We observe only very few nominal wage decreases in the data but rather smaller wage increases leading to lower future wage levels in the treated municipalities.

assumptions, single-country designs can establish a valid control group more easily. Most existing single-country studies (see, e.g., Dwenger, Rattenhuber, and Steiner 2011; Arulampalam, Devereux, and Maffini 2012; Liu and Altshuler 2013), however, have to rely on variation in the tax burden that is not solely driven by policy reforms but also by firms' choices. For instance, differences in tax burdens across industries or across regions due to formula apportionment may depend directly on sales and investment activities, which may be endogenous to tax rates. The contribution of our paper is to exploit substantial within-country variation in statutory municipal tax rates. In a recent contribution, Suárez Serrato and Zidar (2016) estimate the incidence of US state-level corporate taxes using a spatial equilibrium framework exploiting regional variation in tax rates and apportionment rules.[5] The German setting has the advantage to offer substantially larger variation in terms of both number and size of tax rate changes.

Furthermore, we go beyond a cleanly identified average effect of corporate taxes on wages and analyze the economic factors driving these changes. We estimate heterogeneous firm effects and discuss the results in light of different labor market theories and tax incidence mechanisms. The German labor market, with its variety of wage-setting institutions, is particularly useful for this exercise. Exploiting the rich administrative linked employer-employee data, we find that labor market institutions matter for the incidence of corporate taxes on wages. In particular and in line with Felix and Hines (2009) and Arulampalam, Devereux, and Maffini (2012), collective bargaining agreements play a key role: if wages are set via collective bargaining at the firm level, wage responses are larger than in cases where wages are set at the sector level or without collective bargaining. Overall, our results suggest that the higher the rents to be shared between firms and workers, the higher the pass-through on wages. For instance, wages are more sensitive to tax changes in more profitable firms. However, we find that wage effects are close to zero for very large firms, foreign-owned firms, and for firms that operate in multiple jurisdictions. This can be explained by better profit-shifting capabilities of these firms. In general, the interaction of labor market institutions, avoidance opportunities, and tax rates has received little attention in the literature on the incidence of corporate taxes, both theoretically and empirically. Our heterogeneous firm effects show that such interactions affect wage responses. This has implications beyond the German setting. While labor market institutions differ internationally, most countries exhibit a mixture of unionized and non-unionized firms or sectors, so that the heterogeneous effects we find in our setting are likely to be relevant in many other countries as well. This is also true for the differences between firm types, in particular the finding that higher taxes do not seem to reduce wages in firms with profit-shifting opportunities.

Last, we add to the distributional debate about the burden of corporate taxation. By estimating the tax incidence for heterogeneous worker groups, we show that higher taxes reduce wages most for the low-skilled, women, and young workers. Both the average pass-through on wages of 51 percent and the heterogeneous worker

[5] Felix and Hines (2009) also use US state tax variation but rely on cross-sectional data. Bauer, Kasten, and Siemers (2012) also investigate the German LBT but without using linked employer-employee data. Moreover, as in an earlier version of this paper (Fuest, Peichl, and Siegloch 2011), they have to average tax rates at the county level (consisting of 28 municipalities on average) which leads to biased results.

effects are important for tax policy because they qualify the widespread view that the corporate income tax is highly progressive. In a back-of-the-envelope calculation based on Piketty and Saez (2007), we show that the estimated progressivity of the overall tax systems in both Germany and the United States would decrease by 25–40 percent if we account for our incidence estimates.

Our analysis focuses on the corporate tax incidence on workers and therefore on the causal *wage* response to corporate tax changes. We do not investigate the impact on input factors, production levels, firm entry or exit. Studying these other margins is important to understand the overall efficiency costs of corporate taxes. Such an analysis would, however, be complicated by data (linkage) limitations and is beyond the scope of this paper.

The rest of this paper is structured as follows. In Section I, we describe the institutional setting of business taxation in Germany and introduce the datasets used in the empirical analysis. The empirical model is presented in Section II. In Section III, we present our main estimates of the corporate tax incidence on wages. Section IV provides evidence on heterogeneous worker and firm effects which we discuss with respect to different theoretical models and mechanisms. Section V concludes.

I. Institutional Background and Data

We estimate the incidence of corporate taxes on wages by exploiting the particular features of the German business tax system. We describe this system in Subsection IA, with a special emphasis on the local business tax (LBT, *Gewerbesteuer*). In Subsection IB, we document the cross-sectional and time variation of the LBT. In Subsection IC, we introduce the administrative linked employer-employee dataset, while Subsection ID contains the definition of our estimation sample and descriptive statistics.

A. *Business Taxation in Germany*

There are three taxes on business profits in Germany: the municipal LBT, as well as the corporate income tax (CIT, *Körperschaftsteuer*) and the personal income tax (PIT, *Einkommensteuer*), which are both set by the federal government. In the following, we describe the LBT, while the CIT and PIT are described in online Appendix B.1.

The LBT applies to both corporate and noncorporate firms, but most firms in the agricultural and public sector are not liable.[6] The tax base of the LBT is basically operating profits. The cost of debt financing is deductible, with some limitations,[7] and the cost of equity financing is not. Taxable profits of firms with establishments in more than one municipality are divided between municipalities according to formula apportionment based on the payroll share. Importantly, the local government

[6] To be precise, paragraphs 2 and 3 of the LBT law (*Gewerbesteuergesetz*) regulate which firms are exempt from the LBT. The main criteria are interactions of legal form and industry. Moreover, certain professions such as accountants, lawyers, journalists, or physicians are exempt.

[7] A special feature of the LBT is that 25 percent of interest costs are added to the tax base. Another peculiarity is that, until 2007, the LBT itself was deductible as an expense.

can change the tax rate but neither the tax base nor the liability criteria. Both are set at the federal level.

The tax rate, τ_{LBT}, consists of two components: the basic rate (*Steuermesszahl*), t^{fed}_{LBT}, which is set at the federal level, and a local scaling factor (*Hebesatz*), θ^{mun}_{LBT}, which is set at the municipal level. Each year, the municipal council votes on next year's θ^{mun}_{LBT}, even if it remains unchanged. The total LBT rate is given by $\tau_{LBT} = t^{fed}_{LBT} \cdot \theta^{mun}_{LBT}$. From 1993 to 2007, t^{fed}_{LBT} was 5.0 percent and decreased to 3.5 percent in 2008. For example, for the median θ^{mun}_{LBT} of 3.9, τ_{LBT} was 19.5 percent before 2008. In the empirical analysis, we rely on variation in τ_{LBT} induced by changes in θ^{mun}_{LBT} (described next).

B. *Municipal Data and Tax Rate Variation*

We use administrative statistics provided by the Statistical Offices of the 16 German federal states (*Statistische Landesaemter*) on the fiscal situation of all 11,441 municipalities. Most important, the dataset contains information on θ^{mun}_{LBT}, but also on population, municipal spending, and revenues. In addition, we observe county (*Kreis*) level GDP as well as unemployment rates compiled by the German federal employment agency.

We combined and harmonized the annual state-specific datasets and constructed a panel on the universe of all municipalities from 1993 to 2012. In the administrative wage data (see Section IC), we can identify municipalities according to their boundaries as of 2010. Due to mergers, various municipal borders predominantly in East Germany changed prior to 2010. As we cannot assign the exact LBT rate for affected jurisdictions, we exclude all municipalities that underwent a municipal merger between 1993 and 2010 from our baseline sample. This concerns 47 percent of East German and 0.6 percent of West German municipalities.[8] Overall, there are 10,001 non-merged municipalities in Germany.

Figure 1 visualizes the substantial cross-sectional and time variation in LBT rates. Panel A shows the cross-sectional variation in τ_{LBT} for the year 2003, the midyear of our sample.[9] Online Appendix Table C.1 provides measures of the distribution of θ^{mun}_{LBT} over time. Panel B illustrates this time variation by showing the number of changes in θ^{mun}_{LBT} per municipality during the period 1993–2012 (online Appendix Table C.2 shows the corresponding numbers). Overall, 19 percent of the non-merged municipalities did not change θ^{mun}_{LBT} during the 20-year period. More than one-half of the jurisdictions changed it once or twice, and only 7.5 percent experienced 4 or more changes. In total, we observe 17,999 tax rate changes in 10,001 non-merged municipalities.

[8] East German municipalities were rather small after reunification in 1990 and were subsequently merged (sometimes several times) to bigger jurisdictions. As a sensitivity check, we impute tax rates for merged municipalities by using weighted averages. See online Appendix C for a more detailed discussion of the jurisdictional changes and Figure C.1 showing the tax rate variation including merged municipalities.

[9] The cross-sectional variation reveals some regional clustering: for instance, scaling factors are higher in the state of North Rhine Westphalia. This is partly due to particularities of that state's fiscal equalization scheme. Empirically, we account for such differences by including "state × year" fixed effects.

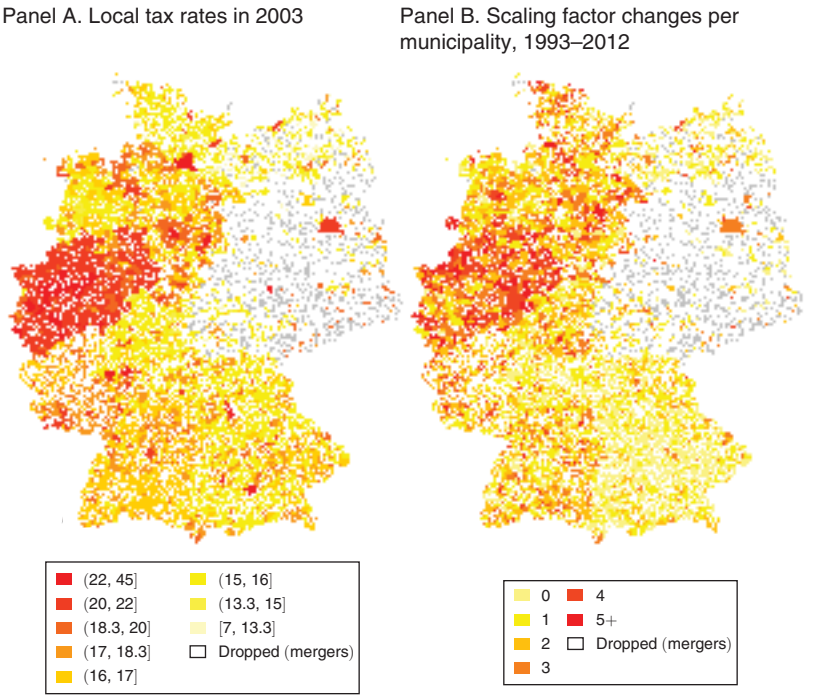

Figure 1. Cross-Sectional and Time Variation in Local Tax Rates

Notes: This figure shows the cross-sectional and time variation in municipal scaling factors of the German LBT. Panel A plots the cross-sectional variation in LBT rates (in percent) induced by different scaling factors for 2003 (the mid-year of our sample). Panel B indicates the number of scaling factor changes per municipality between 1993 and 2012. White areas are municipalities that underwent a change of boundaries due to a merger; which are dropped from the baseline sample (see online Appendix Figure C.1 for the same graphs including the dropped municipalities). Jurisdictional boundaries are as of December 31, 2010.

Source: Author's calculations based on data from Statistical Offices of the Laender, Maps: GeoBasis-DE/BKG 2015

C. *Linked Employer-Employee Data*

We combine the municipal data presented in the previous subsection with linked employer-employee data (LIAB) provided by the Institute of Employment Research (IAB). The LIAB combines administrative worker data with firm-level data (Alda, Bender, and Gartner 2005).

The firm component of the LIAB is the IAB Establishment Panel (Kölling 2000), which is a 1 percent stratified random sample of all German establishments. The term establishment refers to the fact that the observational unit is the individual plant, not the firm. The employer data cover establishments with at least one worker subject to social insurance contributions and contains about 15,000 establishments. We extract the following variables: number of employees, industry, union status (sector- or firm-level wage bargaining or no collective agreement), self-rated profitability,[10] firm structure (single versus multi-plant firms), and residence (domestic or foreign) of the owner.

[10] The survey question asks for a self-assessment of the profit situation. We construct a three-point scale (high, medium, low) for profitability with well-balanced support over the three categories.

In addition to the establishment-level information, the dataset contains information on all employees in the sampled establishments. This includes between 1.6 and 2.0 million workers (corresponding to about 6 percent of all workers) per year. The employee data are taken from the administrative employment register of the German Federal Employment Agency (*Bundesagentur für Arbeit*) covering all employees paying social security contributions (Bender, Haas, and Klose 2000). While civil servants, self-employed individuals, and students are not observed in the social security data, the dataset covers more than 80 percent of all employed persons in Germany. The employee information is recorded on June 30 of each year and includes information on wages, age, gender, occupation, employment type (full-time or part-time employment), and skill.

Importantly, wages are right-censored at the ceiling for social security contributions (63,400 euros in 2008 for Western Germany). Up to 13 percent of the observations are censored (see online Appendix Table C.4 for the distribution of censored workers across firms). Note that the censoring does not affect our baseline results at the firm-level since we use the median wage in the establishment as our left-hand-side variable. At the individual level, we opt for a conservative approach and assign censored individuals the cap, leading to an underestimation of the wage effect.

D. *Sample Definition and Descriptive Statistics*

We select a ten-year panel of the administrative wage data spanning the years 1999 to 2008 for our analysis. This choice yields a sufficient number of years before and after tax changes, which are necessary to set up the event study design with a window running from four years prior to five years after the reform, implying that we need tax data from 1993 (the first year available to us) until 2012 (see Section II for details on the empirical model). Furthermore, ending in 2008 avoids potential wage effects of the Great Recession.

As discussed in Section IB, we focus on the 10,001 municipalities that did not change jurisdictional borders between 1993 and 2012. In the LIAB data, we observe firms in 3,522 of those non-merged municipalities. This leaves us with 6,802 tax changes to identify the effect of corporate taxes on wages. Figure 2 shows the distribution of these changes. Panel A shows all non-merged municipalities, while the panel on the right-hand side is based on the non-merged municipalities represented in our estimation sample. The figure shows that tax rate variation in both samples is very similar. In both samples, 93 percent of the tax changes are increases (see, also, online Appendix Tables C.2 and C.3, for more details on the (similarity of the) tax rate variation).[11] The mean increase of τ_{LBT} is 0.9 percentage points (or 5 percent) and the seventy-fifth percentile of the tax increase distribution is equal to

[11] Given the international trend toward lower corporate tax rates this seems surprising. Yet, both the federal CIT rate and the top PIT rate decreased in Germany over the period 1993–2012 so that the overall business tax rate declined as well (see online Appendix B.1 for more details). Thus, a rise in the LBT rates in a municipality over time has to be seen as leading to a *slower decrease* in the overall tax burden for firms in these municipalities compared to firms in jurisdictions with constant local tax rates.

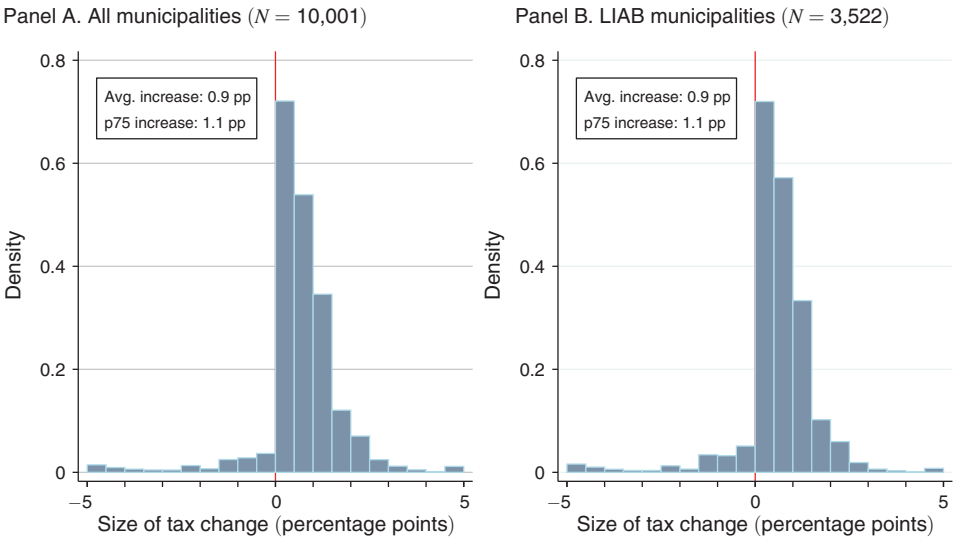

FIGURE 2. DISTRIBUTION OF LOCAL BUSINESS TAX CHANGES

Notes: The histogram shows the distribution of changes in the LBT rate induced by changes of the municipal scaling factor from 1993 to 2012 in non-merged municipalities. In panel A, the sample consists of all 17,999 tax rate changes in the 10,001 non-merged municipalities, while in panel B it is constrained to the 6,802 tax changes in those 3,522 non-merged municipalities represented in the linked employer-employee data (LIAB). In both histograms, we omit 0.1 percent of the observations with absolute changes larger than 5 percentage points for illustrative purposes. The average LBT rate in the full (LIAB) sample is 16.0 percent (18.7 percent).

Source: Author's calculations based on data from Statistical Offices of the Laender

1.1 percentage points (6 percent). We are therefore able to exploit many and fairly large tax reforms for identification.[12]

Our estimation sample consists of all firms in non-merged municipalities observed in the LIAB data and their corresponding workers. We exclude the few firms that changed their incorporation status during the observation period from the baseline since such a change simultaneously affects the LBT tax base, the applicable business tax at the federal level (see online Appendix B.1), and potentially other firm characteristics such as firm scale or collective bargaining agreements. We also focus on firms with more than three workers to be able to calculate meaningful and reliable wage measures at the firm level. We check the sensitivity of our results with respect to these sample selection choices below. Online Appendix Tables C.5 and C.6 present descriptive statistics of our establishment and worker level sample in non-merged municipalities.[13] Table C.5 shows that the average median firm wage is 2,733 euros per month. The average θ_{LBT}^{mun} is 3.85, and the average τ_{LBT} is 18.7 percent. The average (median) establishment has 265 (53) employees. Sixty-four percent of the establishments are liable to the LBT. Our baseline estimates presented below

[12] For instance, Suárez Serrato and Zidar (2016) exploit about 100 corporate tax changes of US states with an average change (over 10 years) of 1 percent (and about 20 percent of changes larger than 2 percent). Part of their variation stems from tax base differences for example due to different apportionment rules. Suárez Serrato and Zidar (2017) document that tax base rules explain more of the US state corporate tax variation than tax rates do.

[13] In the baseline, we only consider full-time workers. We also looked at the effects on part-time wages but found no significant differences (see below).

will be based on the sample of liable firms, while we use the sample of non-liable firms for a sensitivity check.

Moreover, the descriptive statistics reveal that 62 percent of the establishments are single-plant firms. More than one-half of the firms have sector-level bargaining agreements in place, while about one-third have no collective bargaining agreement. The descriptive statistics of the individual worker sample (see Table C.6) place greater weight on larger firms with more employees. As larger firms pay higher wages, we see that the median wage in the individual level sample increases to 3,363 euros per month. In terms of individual characteristics, the table shows that the average worker in our sample is 41 years old. The share of males is 72 percent. Fourteen percent of the individuals are high-skilled, while about as many are low-skilled.[14] Eighty-one percent of the individuals have never earned a wage higher than the social security contribution ceiling in our sample.

II. Empirical Strategy

A. *Research Design and Identification*

We use different empirical models to estimate the causal effect of LBT changes on wages. Our baseline outcome variable is the log median real full-time wage in firm f, located in municipality m, which is part of commuting zone (CZ) c and state s, in year t, $w^{p50}_{f(m,c,s),t}$.[15] We choose the median as the baseline on the firm level to account for the top-coding of wages at the ceiling for social security contributions (see the discussion in Section IC).

We start our analysis using an event study design, which formally reads:

$$\ln w^{p50}_{f,t} = \sum_{j=-4}^{5} \gamma_j D^j_{m,t} + \mu_f + \mu_m + \psi_{s,t} + \varepsilon_{f,t}. \tag{1}$$

The independent variables of interest are a set of dummies $D^j_{m,t}$ indicating an event happening j periods away. Following Simon (2016), we estimate different specifications, where events are either (i) any LBT increase, (ii) large tax increases, or (iii) tax decreases. Large increases are defined as any tax hike greater than or equal to the seventy-fifth percentile of the tax increase distribution. There are two potential advantages of focusing on large increases. First, wages might not respond to small tax rate changes, e.g., due to adjustment costs. Second, we limit the number of events per firm and reduce the likelihood that other tax events happened within the event window (Simon 2016). As an additional sensitivity check, we estimate the model on a restricted sample of tax changes that have no other changes in the event window. We set a baseline event window, running from four years prior to a tax change to five years after.[16] In addition, we include firm (μ_f) and municipal (μ_m)

[14] We differentiate between three skill groups: high-skilled workers who have obtained a college/university degree; medium-skilled who have completed either vocational training or the highest high school diploma (*Abitur*); low-skilled who have completed neither of the two.

[15] In order to ease notation, we only include the index of the lowest geographical level in the following.

[16] We experimented with different leads and lags, but results are robust to the event window definition. As commonly done, we bin up event dummies at the endpoints of the event window (i.e., $j = -4$ and $j = 5$). Hence, the dummy $D^5_{m,t}$ accounts for all reforms occurring five or more years ago (McCrary 2007). This is necessary as we

fixed effects.[17] To account for regional shocks, our baseline specification includes "state × year" fixed effects ($\psi_{s,t}$). The error term is denoted by $\varepsilon_{f,t}$.

The event study specification uses dummy variables to capture tax rate changes. In order to account for different magnitudes of tax changes, we follow Suárez Serrato and Zidar (2016) and estimate the following distributed lag model:

$$(2)\quad \ln w_{f,t}^{p50} - \ln w_{f,t-1}^{p50} = \sum_{j=-4}^{5} \beta_j [\ln(1 - \tau_{m,t-j}) - \ln(1 - \tau_{m,t-1-j})] + \psi_{s,t} + \varepsilon_{f,t}.$$

We regress the annual change in log wages on the change in the log net-of-business-tax rate. The estimated coefficients $\hat{\beta}_j$ measure the effect of leads and lags of a tax rate change on the annual real wage growth. Time invariant factors are differenced out. We use the estimates of the model to calculate the cumulative effect of a tax change.

In both models (1) and (2), identification is achieved within firms and municipalities over time, and we thus estimate variants of a DiD model with fixed effects. Identification of causal effects in such models requires common trends pre-treatment: that is, no statistically significant wage responses preceding a tax reform. While we use specifications (1) and (2) mainly to establish flat pre-trends, we use the following generalized DiD model to estimate the average effect of a change in the LBT rate on wages relative to the pre-treatment period, which we then use to calculate the tax incidence:

$$(3)\quad \ln w_{f,t}^{p50} = \delta \ln(1 - \tau_{m,t}) + \mu_f + \mu_m + \psi_{s,t} + \varepsilon_{f,t},$$

where δ measures the percent change in wages induced by a one percent increase in the net-of-tax rate.

Given flat pre-trends, our research design would still be invalid if local shocks systematically affected tax rates and wages. We provide three further checks to assess whether such potential local shocks are likely to bias our estimates. First, we run event study designs as specified in equation (1) using GDP, unemployment, as well as municipal revenues and spending as outcome variables. Significant pre-treatment trends for these outcomes would hint at local shocks and cast doubt on our identifying assumption. As will be shown in Section III, there are no local shocks to the business cycle prior to a tax change. Second, we further test the sensitivity of the empirical models with respect to local shocks. While our baseline specifications include "state × year" fixed effects, which nonparametrically account for local shocks at the state level, we can control for shocks at different levels of aggregation. We estimate a simpler model using only year fixed effects and a more complex model with "commuting zone × year" fixed effects (there are 258 commuting zones (CZ) in Germany). If confounding local labor market shocks were important, estimates should vary across different specifications since they should be picked up at least partly by "CZ × year" fixed effects. Third, besides these nonpara-

have a balanced panel in terms of years (1993–2012), but reform years differ across municipalities, which yields an unbalanced panel in event time. Because of this, we do not plot the endpoint estimates in the event study graphs.

[17] Firm and municipal fixed effects are highly collinear as only very few firms move between municipalities in the data.

metric specifications, we directly account for local time-varying confounders by additionally controlling for (lagged) GDP, unemployment, population, and municipal spending. As will be shown below, our results are robust to these tests for omitted confounders.

Heterogeneous Effects.—In order to test for heterogeneous effects, we interact the local tax rates in the DiD models with firm or worker characteristics. Some of these characteristics such as wage setting institutions are potentially endogenous to the tax rate. For this reason, we fix the characteristics to the values of 1997, i.e., two years prior to our first panel observation. Heterogeneous firm effects are estimated at the firm level, and worker effects at the individual level. In terms of controls, the models include municipal, firm, "state $\times$ year" fixed effects (cf. model (3)) and additionally "firm/worker type $\times$ year" fixed effects. On the worker level, the outcome variable is the log individual wage, and we additionally include worker fixed effects.

Inference.—In our baseline approach, we cluster standard errors at the municipal level, i.e., the level of our identifying variation. Given the well-known problems of biased standard errors in difference-in-differences models (Bertrand, Duflo, and Mullainathan 2004), we conduct two tests to assess the sensitivity of our estimates. First, we aggregate the data to the municipal level, finding similar results. Second, we follow the suggestions by Angrist and Pischke (2009) to "pass the buck up one level" and cluster standard errors on a higher level of aggregation, which in our case is the county or the commuting zone. As will be shown below, standard errors of estimates are hardly affected.

B. *Measuring the Tax Incidence*

The DiD estimate from equation (3) measures the elasticity of the wage rate with respect to the net-of-business tax rate, $\hat{\delta} = \frac{dw}{d(1-\tau)}\frac{(1-\tau)}{w}$. We can use this estimate to calculate the incidence of corporate taxes on wages as the share of the total business tax burden falling on workers. We do so by relating the welfare change of workers induced by a marginal change in the net-of-tax rate to the sum of the welfare changes of workers and firm owners (see Suárez Serrato and Zidar 2016).

Assume that worker i in municipality c maximizes utility $U(C, L)$ over consumption C and leisure L, subject to the budget constraint $C = w(1-t)L$, where t is the personal income tax rate and L the quantity of labor.[18] The indirect utility function can be written as $V((1-t)w)$ and the change in worker utility induced by a change in the wage rate is given by $dV = L(1-t)\,dw$. A representative firm j faces a corporate tax rate τ and maximizes profits, $\Pi = (1-\tau)[F(K,L) - wL] - (1-\alpha\tau)rK$, over capital K and labor L. The tax base T is given by $T = F(K,L) - wL - \alpha rK$, where α is the share of deductible capital costs. By the envelope theorem, the change in welfare for firm owners is given by $d\Pi = -d\tau T - dwL(1-\tau)$. The share of workers in the overall burden of a marginal change in the corporate tax rate is given

[18] We omit indices for readability. For notational simplicity, we assume quasilinear preferences and normalize the marginal utility of income to unity.

by $I^w = \frac{dV}{dV + d\Pi}$. Plugging in our estimate of $\hat{\delta}$ and rearranging, the share of workers in the tax burden can be written as

$$I^w = \frac{wL\hat{\delta}(1 - t)}{(1 - \tau)T - wL\hat{\delta}(t - \tau)}. \tag{4}$$

Equation (4) measures the incidence of corporate taxation on wages. As in Suárez Serrato and Zidar (2016), the wage elasticity with respect to the net of tax rate is a sufficient statistic to calculate marginal welfare changes of both workers and firms.[19] It would also be interesting to look at responses in input factors or output. This would allow us to calculate the excess burden of the corporate income tax. However, given that the necessary information is either incomplete (output) or not available (capital) in our administrative wage data (see Section IC) and given that linking another dataset to our data is not possible, addressing these questions is beyond the scope of this paper.

III. Baseline Results

We start our analysis of the wage effects of the LBT by plotting the event study estimates from equation (1) in panel A of Figure 3 for three different specifications: any increase, large increases, and any decrease. Given the 0-1-event dummy definition, we exclude tax decreases (increases) from the sample used to estimate the effect of tax increases (decreases).[20] We find a negative and significant effect of business tax increases on real wages. We hardly observe any decline in nominal wages in our data but find slower wage growth in affected firms over time, leading to lower levels in the future. Reassuringly, wage effects become stronger when focusing on the 25 percent largest tax increases. Estimates for tax decreases (which are relatively rare: cf. Figure 2) are noisy and inconclusive. The point estimates hint at a slight yet insignificant pre-trend. In the sensitivity checks below, we show that municipalities with tax decreases are not driving our results.

In order to exploit the different sizes of tax changes, we plot the cumulative effects of the distributed lag model (equation (2)) in panel B of Figure 3. The higher the *net-of-tax rate* increase, the higher the wage growth. Hence, the results of the event study are confirmed as a tax increase implies a decrease in the net-of-tax rate. Including four leads of the change in the log net-of-tax rate, we again find a flat pre-trend. The model plotted in panel B is estimated on the same sample of municipalities to allow for comparisons to panel A. Online Appendix Figure D.3 shows a similar pattern when including all municipalities.

A remaining concern in our setting is that tax rates might respond to local business-cycle shocks, which could also affect wages. We can test directly for violations of the identifying assumptions by using local economic outcomes as left-hand-side

[19] This approach relies on simplifying assumptions. In particular, all agents are price takers: that is, imperfect competition in input and output markets is not taken into account, and the measure abstracts from heterogeneity of firms and workers.

[20] We also estimated the model on other sample definitions: (i) no restriction, (ii) zero decreases and only one increase in event window, (iii) zero decreases and only one increase in the sample. Online Appendix Figure D.1 shows that those kinds of sample restrictions are not driving our results.

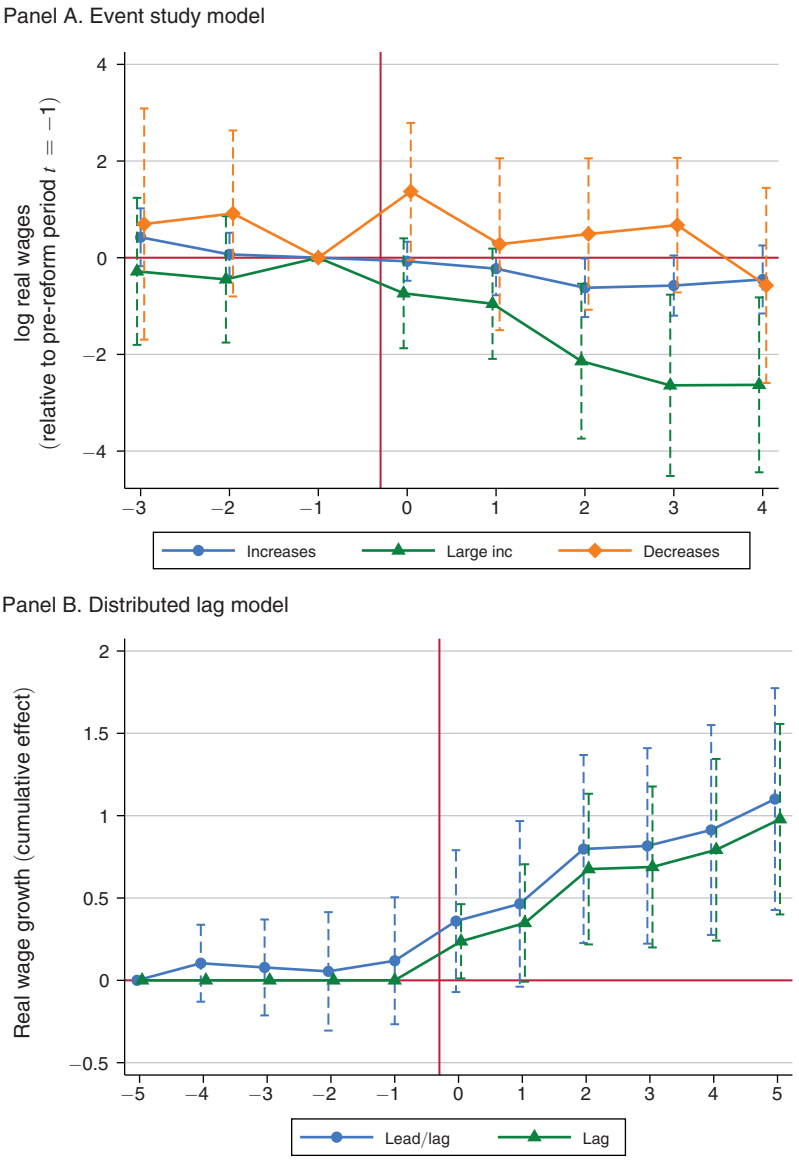

FIGURE 3. BASELINE WAGE EFFECTS

Notes: Panel A plots event study estimates ($\hat{\gamma}_j, j \in [-3, 4]$) and corresponding 95 percent confidence bands of different specifications of equation (1). Dependent variable is the log median firm wage (observed on June 30 for each year). Event variables are dummies equal to 1 for a tax increase, a large tax increase (greater than or equal to the 75th percentile of the tax increase distribution), or a tax decrease (see legend). The estimation sample comprises all establishments liable to the LBT in non-merged municipalities. In specifications with tax increase (decrease) dummies, we exclude all municipalities that experienced a tax decrease (increase) during the observation period. Panel B plots distributed lag model estimates ($\hat{\beta}_j, j \in [-4, 5]$) and corresponding 95 percent confidence bands of different specifications of equation (2). Dependent variable is the yearly change in the log median firm wage. Depending on the specification, main regressors are lags or leads and leads of the yearly change in the net-of-local-business-tax rate (see legend). Note that a tax increase in the event study design in panel A implies a decrease in the net-of-tax rate in panel B. The estimation sample comprises all establishments liable to the LBT in non-merged municipalities that did not experience a tax decrease during the observation period. In both panels, the tax change occurred for the treatment group on January 1 in event year $t = 0$, as indicated by the vertical line. All regression models include municipal, firm and "state × year" fixed effects. Standard errors are clustered at the municipal level. Estimates are reported in online Appendix Tables D.10 and D.13.

Source: Author's calculations based on data from LIAB and Statistical Offices of the Laender

variables in the event study design. Figure 4 shows the results for GDP and unemployment. Investigating the pre-treatment periods, we find flat pre-trends for our specifications using (large) tax increases.[21] We find similar patterns when looking at municipal revenues and spending (see online Appendix Figure D.5). For tax decreases, we find again pre-trends for GDP but not unemployment (see online Appendix Figure D.4).

While we use the graphical representation of the event study and distributed lag specifications mainly to establish flat pre-trends, we use the DiD model given by equation (3) to estimate the average effect of a change in the LBT on wages. The baseline elasticity for liable firms is provided in column 1 of Table 1. A 1 percent decline in the net-of-tax rate (reflecting an increase in the tax rate) reduces wages by 0.39 percent. Applying formula (4), we can calculate the share of the tax burden borne by workers as a measure of tax incidence. We find that 51 percent of the corporate tax burden is passed onto workers.

Sensitivity Checks.—We run a set of sensitivity checks testing whether our estimates are driven by modeling choices. We start with further tests of the robustness of our estimates with respect to unobserved local shocks. The baseline specification includes "state $\times$ year" fixed effects to nonparametrically account for shocks at the state level. We estimate various specifications where we vary the set of control variables, replacing "state $\times$ year" with more aggregated year fixed effects or more disaggregated "commuting zone (CZ) $\times$ year" fixed effects. We also estimate specifications where we add local controls (GDP, unemployment, spending, population) and firm controls (employment) to the model, capturing the local business cycle.[22] If local shocks were important, estimates should vary across different specifications. Yet, the results reported in Table 1 are robust. In particular, estimates are unchanged when moving from the baseline to the very rich specification with CZ-year fixed effects.

While our baseline results are estimated at the firm level, we also estimated the DiD model at the municipal and individual level (cf. online Appendix Table D.1). While estimates are a bit noisier on the municipal level due to smaller numbers of observations, point estimates are reassuringly similar at all three levels of aggregation.

In our baseline specification, the dependent variable is the median wage in the firm. We chose this measure to account for the right-censoring of the data, which would bias our estimates toward zero.[23] Nevertheless, we conduct several additional checks to assess the implications of this choice. First, we check that results are not

[21] This is in line with previous evidence for the German LBT (Foremny and Riedel 2014) as well as for income tax reforms in Europe (Castanheira, Nicodème, and Profeta 2012), which suggests that tax changes are typically triggered by political factors, not shocks on economic variables.

[22] All control variables are in logs and lagged by two periods to reduce endogeneity issues; results are similar when using contemporaneous variables.

[23] If all workers earned above the contribution ceiling, we would not be able to observe any wage change in the data and hence estimate a zero wage effect. However, in our data, the median worker in almost all firms earns a wage below the cap for social security contributions (see Table C.4). We also estimated model (3) using different wage measures as left-hand-side variables (cf. online Appendix Table D.4). When using the mean wage on the firm level (instead of the median), we find smaller yet still significant wage effects. Moreover, we find that wages for the top 25 percent of workers across firms respond less. We discuss potential distributional implications in more detail below.

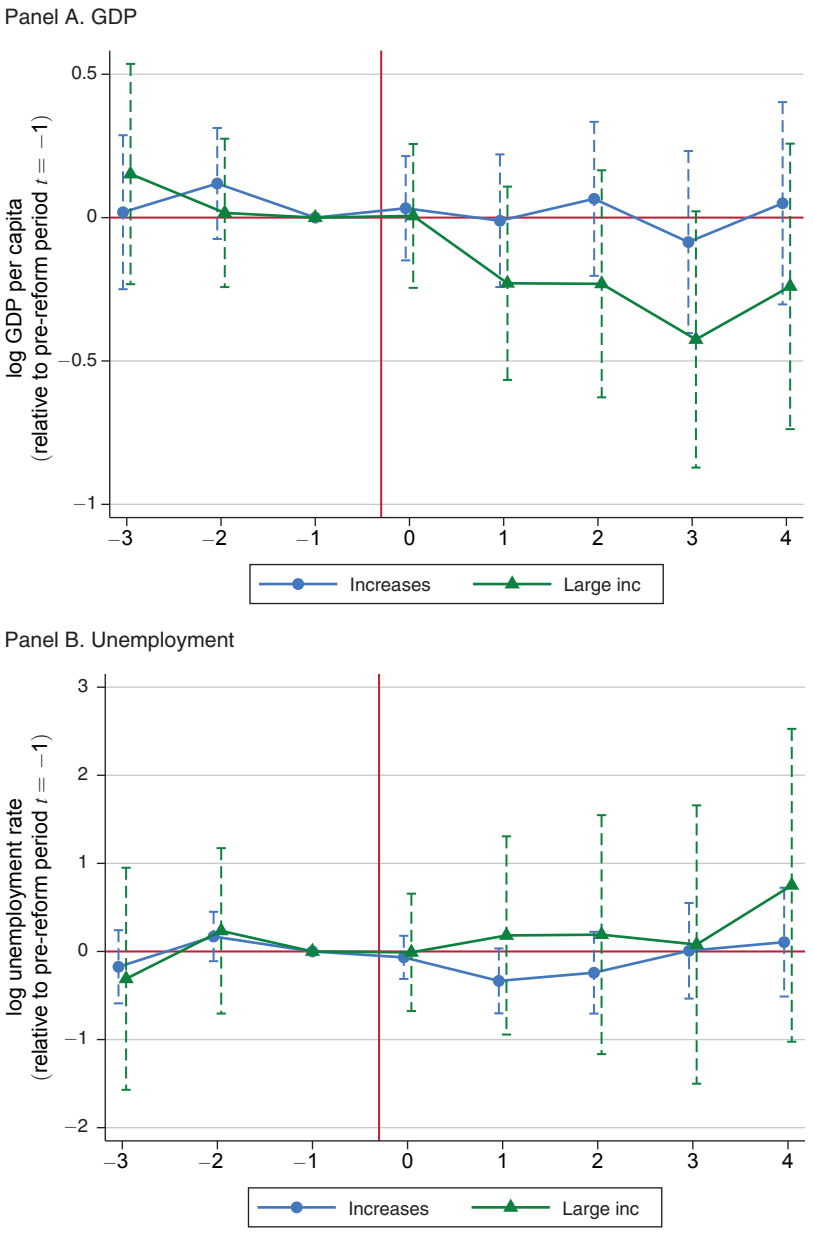

FIGURE 4. EVENT STUDY GRAPHS: LOCAL BUSINESS CYCLE EFFECTS

Notes: The graph plots event study estimates ($\hat{\gamma}_j, j \in [-3, 4]$) and corresponding 95 percent confidence bands of different specifications of equation (1). Dependent variables are log county GDP per capita (panel A) and unemployment rate (panel B). Event variables are dummies equal to 1 for a tax increase or a large tax increase (greater than or equal to the seventy-fifth percentile of the tax increase distribution, see legend). The tax change occurred for the treatment group on January 1 in event year $t = 0$, as indicated by the vertical line. All regression models include municipal and "state × year" fixed effects. The estimation sample comprises all non-merged municipalities from the LIAB data that did not experience a tax decrease during the observation period. Standard errors are clustered at the municipal level. For corresponding event study graphs including tax decrease specifications, see online Appendix Figure D.4. Estimates are reported in online Appendix Tables D.15 and D.16, respectively.

Source: Author's calculations based on data from Statistical Offices of the Laender

Table 1—Difference-in-Differences Estimates: Baseline Wage Effects

	(1)	(2)	(3)	(4)	(5)	(6)
log net-of-LBT rate	0.388	0.229	0.386	0.396	0.343	0.399
	(0.127)	(0.110)	(0.127)	(0.128)	(0.164)	(0.118)
Incidence (I^w)	0.505	0.288	0.502	0.516	0.442	0.520
	(0.170)	(0.140)	(0.170)	(0.172)	(0.217)	(0.159)
"State × year" fixed effects	✓			✓	✓	✓
Year fixed effects		✓				
CZ × year fixed effects			✓			
Municipal controls $t-2$				✓		
Firm controls $t-2$					✓	
Worker shares						✓
Observations	44,654	44,654	44,654	44,654	25,241	44,654

Notes: This table presents the DiD estimates, $\hat{\delta}$, of regression model (3) at the firm level. Coefficients measure the wage elasticity with respect to the net-of-local-business-tax rate. The incidence effect I^w is measured according to formula (4) as the share of the total tax burden borne by workers. All regression models include municipal and firm fixed effects. Additional control variables and fixed effects (year, "state × year," or "commuting zone (CZ) × year") vary depending on the specification (as indicated at the bottom of the table). The estimation sample is restricted to all establishments liable to the LBT in non-merged municipalities. Standard errors are clustered at the municipal level. Corresponding standard errors for the incidence measure are obtained using the Delta method. Our preferred (baseline) specification is shown in column 1.

Source: Author's calculations based on data from LIAB and Statistical Offices of the Laender

driven by the composition of the workforce and hence by a change in the median worker. Specification (6) in Table 1 shows that estimates are hardly affected when controlling for various worker shares (age, gender, skill, occupation, and employment type) at the firm level. This is confirmed by a second test where we estimate the DiD model using the different worker shares as dependent variables. Results are shown in online Appendix Table D.2 and reveal that the worker composition does not react to changes in the tax rate. Estimates are insignificant and/or very small. Third, and in line with the predicted bias toward zero, specification (3) of online Appendix Table D.3 shows that the wage effect increases when controlling for the share of never-censored workers at the firm level. Similarly, we find that wage effects are stronger for firms with fewer censored workers. Last, we estimate the DiD model at the worker level and exclude all individuals who at least once earned a wage above the contribution ceiling during the observation period. Again, we find that estimates increase when excluding censored workers (see column 7 of online Appendix Table D.6 as well as Table D.8).[24]

We also test the sensitivity with respect to sample restrictions (cf. online Appendix Table D.5). Our DiD baseline sample comprises firms liable to the LBT in municipalities that never merged with other municipalities and never experienced a tax decrease during the observation period. We find smaller but still significant effects

[24] Imputing censored wages would be another option used in the literature (Dustmann, Ludsteck, and Schönberg 2009; Card, Heining, and Kline 2013). While this is sensible when analyzing wage inequality, it is problematic in our setting since the LBT rate would have to be included in both the selection equation and the second-stage equation.

when adding tax-exempt firms to the baseline sample (see also the discussion in Section IVB). Likewise, estimates decrease when adding the merged municipalities to the sample. In column 4 of online Appendix Table D.5, we restrict the sample to municipalities without a tax decrease during the observation period to rule out that those decreases are driving the results. DiD estimates increase slightly, which suggests that potential endogeneity would bias our estimates downward. In columns 5 and 6, we add firms that switched their incorporation status and firms with less than four workers to the estimation sample (cf. Section ID). In both cases, the wage elasticity increases a bit. In 2008, the basic federal rate of the LBT was reduced from 5 to 3.5 percent and deductibility of the tax payment itself was abolished (see Section IA). Results are robust to dropping this year from the sample (column 7).

Finally, we show that standard errors hardly change when clustering at higher aggregation levels than municipalities such as counties or commuting zones (see online Appendix Table D.7). These findings are in line with the results that we get when estimating the model on different levels of aggregation (online Appendix Table D.1).

IV. Theoretical Mechanisms and Heterogeneous Effects

We have established that workers on average bear one-half of the corporate tax burden. In this section, we analyze the economic forces driving this effect. We start with a brief discussion of different theoretical models of corporate tax incidence and the wage effects they predict (see online Appendix A for the full theoretical analysis). In a second step, we exploit the rich firm and worker level information in our data to investigate the predictions of the different theories.

A. *Theoretical Predictions*

In his seminal paper on corporate tax incidence, Harberger (1962) considers a closed economy with a corporate and a noncorporate sector. In his setting, the burden of corporate taxes is borne entirely by capital. The subsequent literature has emphasized the importance of international capital mobility. In open economies, higher corporate taxes reduce domestic investment, and wages decline.[25]

In this paper, we study the effects of a local business tax. In this setting, labor is arguably more mobile across jurisdictional borders than internationally. In the polar case of perfect worker mobility, local corporate tax changes should not affect wages because they are determined in the national labor market.[26] Yet, even at the local level, mobility is likely to be imperfect, and it may differ across workers.

Even with perfect worker mobility, models in the spirit of Tiebout (1956) would predict negative wage responses to local corporate tax increases because migration decisions may depend on local public services. If the additional revenue raised is spent on local public services, workers may accept lower local wages. An implication

[25] See, e.g., Bradford (1978); Kotlikoff and Summers (1987). In these models, the share of the (source-based) corporate tax burden borne by domestic immobile factors increases as the size of the economy relative to the rest of the world decreases. See Auerbach (2006), Harberger (2006), and Gravelle (2013) for surveys of the literature.

[26] Along the same lines, a standard assumption is that output prices are determined in national markets for goods and services so that the tax burden cannot be shifted onto consumers.

of the Tiebout model is that wages would also decline in tax-exempt firms if the local tax rate increases.

The models discussed so far are based on the assumption of competitive product and labor markets. If products (or consumers) are costlessly mobile across jurisdictions, firms cannot shift the burden onto their customers. This implies that other shifting channels must be more relevant. Given that we look at a local tax, we expect the pass-through on consumer prices to be of second order. Nevertheless, the incidence on wages might be higher for industries that produce more tradable goods.

Relaxing the assumption of perfectly competitive labor markets, we show in online Appendix A that most models with labor market frictions also predict that higher corporate taxes reduce wages. The mechanisms at work are, however, different, and the magnitude of the effects depends on wage setting institutions. We will briefly discuss the key insights from these models in the following paragraphs.

In collective bargaining models, workers receive a share of the surplus generated by the firm. If higher corporate taxes reduce this surplus, workers bear part of the burden.[27] The level at which employers and unions bargain over wages is important. Local taxes can be expected to have the strongest impact on wages if bargaining is at the firm level, and the firm operates in one jurisdiction only. If wages are set at the sector level, the impact of a tax change in one jurisdiction will decrease with the number of jurisdictions where the sector is present. Similarly, if a firm operates plants in multiple jurisdictions, a tax change in one may not matter much, even if wages are set at the firm level.

Fair wage models (Akerlof and Yellen 1990; Amiti and Davis 2012) also imply that higher corporate taxes reduce wages. In some variants, wages are directly related to after tax profits. In other efficiency wage models, such as shirking models (Solow 1979; Shapiro and Stiglitz 1984), the optimal wage trades off higher output against the cost of higher wages.[28] If wage setting in tax-exempt firms considers wages in taxable firms as a reference for fairness, the prediction would be that wages in tax-exempt firms are also affected by tax changes.

In monopsonistic labor markets where firms have wage setting power, higher corporate taxes also reduce wages. The magnitude of the effect depends on the degree of market power. Firms with a lot of market power will pay lower wages. This implies little room for wages to fall in response to higher corporate taxes and consequently smaller wage effects in firms dominating the local labor market.

Another factor that may affect the incidence of corporate taxes is income shifting to avoid taxes. Large, multi-plant, and in particular foreign-owned firms can avoid taxes by shifting profits across jurisdictions or even abroad. If this is relevant, we should observe smaller effects of tax changes for these firms.

In Table 2, we summarize the different theoretical mechanisms and the predicted wage effects. We can shed light on the relevance of these theories by testing their

[27] The rent accruing to the workers declines, but how this is translated into changes in employment and wages is theoretically ambiguous. If employment is constant or increases, wages decline unambiguously. However, it is theoretically possible that employment declines by so much that wages increase although the overall rent accruing to workers falls.

[28] Here, higher corporate taxes decrease investment and therefore reduce the marginal productivity gain from a wage increase. Consequently, wages fall when corporate taxes increase. A similar mechanism is at play in directed search models, where higher wages affected productivity through better worker-firm matches (Acemoglu and Shimer 1999).

Table 2—Wage Effects of a Local Corporate Tax under Different Theoretical Models

Model	Main mechanism	Predicted wage effect	Empirical findings
Harberger-type model w/ open economy	Mobility of production factors determines incidence.	Larger wage effect for less mobile workers.	✓
Tiebout sorting	Tax revenues increase public good quality, which leads to compensating wage differential.	Wage effects smaller conditional on future municipal spending. Wages in non-liable firms should decline.	(−) (−)
Additional pass-through opportunities	If alternative pass-through opportunities exist, wage channel becomes less important.	Wage effects higher in sectors that produce more tradable goods.	✓
Collective bargaining	Tax reduces rent to be split between firms and workers, reducing wages c.p. Overall effect depends on employment response. Sector-level bargaining dilutes rent effect of *local* tax if sector present in many jurisdictions.	Negative wage effect for plants with CBA. Smaller wage effect for plants with sector-level CBA compared to plants with firm-level CBA.	✓ ✓
Fair/efficiency wages	Wage depends on profits and/or reference wages.	Stronger wage decline in more profitable firms. Wages in non-liable firms should decline.	✓ (−)
Monopsony power	Firms with market power pay lower wages given little room for shifting of corporate tax burden.	Effects smaller in firms with higher regional labor market power.	✓
Income shifting	Firms may shift profits to different jurisdiction or abroad.	Smaller effect for multi-establishment firms. Smaller effect for foreign-owned firms.	✓ ✓

main assumptions and mechanisms using the rich linked employer-employee data. The last column of Table 2 provides a preview of our empirical findings.

B. *Empirical Tests*

In this subsection, we investigate the empirical relevance of the different theories discussed in the preceding section. As different mechanisms may be at play simultaneously, it is difficult (if not impossible) to single out specific channels empirically.[29] Nonetheless, the rich linked employer-employee data allow us to zoom in on central implications of the different theories and test their relevance, assuming that other characteristics are given. We test the different theoretical predictions by interacting the net-of-tax rate from the DiD model (3) with predetermined indicators for specific firm or worker types.

[29] For instance, a large multi-plant firm might be more profitable than others. Consequently, it may be able to shift income abroad. At the same time, wages may be set via collective bargaining at the firm level. In order to isolate and test a specific theory, e.g., union bargaining, we would need exogenous (and exclusive) variation in the bargaining status of the firm.

Table 3—Difference-in-Differences Estimates: Wage Effects by Firm Type

Stratified by	Effect of log net-of-LBT rate by firm type				Observations
Liability	Liable	Non-liable			69,249
	0.388	−0.178			
	(0.127)	(0.154)			
Sector	Manuf.	Const.	Trade	Serv.	44,654
	0.556	0.452	0.151	0.383	
	(0.155)	(0.248)	(0.276)	(0.253)	
CBA	Firm	Sector	None		44,654
	0.731	0.418	0.292		
	(0.351)	(0.127)	(0.239)		
Profitability	High	Medium	Low		43,622
	0.565	0.330	0.210		
	(0.214)	(0.187)	(0.200)		
Firm size (# workers)	Below 10	10 to 99	100 to 499	Above 500	44,654
	1.241	0.311	0.064	−0.212	
	(0.520)	(0.157)	(0.159)	(0.210)	
Size rel. to local labor market (market power)	Small	Medium	Large		44,654
	0.652	0.481	0.456		
	(0.310)	(0.206)	(0.169)		
Firm structure	Single-plant	Multi-plant			44,226
	0.426	0.223			
	(0.160)	(0.162)			
Ownership	German	Foreign			44,654
	0.449	−0.293			
	(0.141)	(0.298)			

Notes: This table presents the DiD estimates $\hat{\delta}$ of regression model (3) for different types of firms as indicated in the table. The heterogeneous effects are estimated by interacting the LBT rate with dummy variables for different firms types. Coefficients measure the wage elasticity with respect to the net-of-local-business-tax rate. All specifications include firm and municipal fixed effects, as well as "state × year" and "firm type × year" fixed effects. The estimation sample comprises all establishments liable to the LBT in non-merged municipalities.

Source: Author's calculations based on data from LIAB and Statistical Offices of the Laender

Firm-Level Heterogeneity.—The firm-level results are presented in Table 3. We start by testing whether tax-exempt firms also respond to tax rate changes.[30] We find a negative but insignificant point estimate for tax-exempt firms.[31] This result suggests that Tiebout sorting mechanisms do not play a major role in the German context. In line with this assertion, we find that estimates do not change when we include current and future municipal spending as additional control variables (see column 3 of online Appendix Table D.3). Instead, the negative point estimate suggests that the higher tax burden on other firms might give tax-exempt firms a competitive advantage, boosting their wages.

Next, we test for differences by industry. Empirically, we find larger and significant effects only for manufacturing and construction sector firms. One explanation

[30] In the absence of any spillovers, we could estimate a triple-difference model. The resulting treatment effect, which would equal the difference between the two DiD estimates for liable and non-liable firms, would be larger.

[31] When considering all firms, column (2) of online Appendix Table D.3 shows that the average worker in Germany bears 22 percent of the LBT instead of 51 percent in liable firms. This is confirmed when estimating the event study design for liable versus non-liable firms (see online Appendix Figure D.2).

for the difference to trade and service sector firms could be that the latter are able to shift part of the burden to their customers as their products and services are on average less tradable than manufacturing goods.

Next, we investigate the interaction of tax rates and different wage-setting institutions.[32] We start by estimating heterogeneous effects by collective bargaining agreement (CBA) of the firm. We group firms into three categories: firms with (i) a sector-level CBA; (ii) a firm-level CBA; (iii) no CBA. Overall, we find larger wage effects for firms under collective bargaining. In line with the theoretical predictions, we find that the incidence effects for firm-level bargaining are stronger than for sector-level CBA. We also find wage responses for firms without CBA but they are smaller and not significant. Another striking empirical pattern is that effects are increasing in firm profitability. This is in line with collective bargaining models, but also many other labor market theories, where rents are split between firms and workers, for instance fair wage models.

When stratifying the results by firm size, we find that the wage effect is driven by small- and medium-sized firms, which account for more than 95 percent of all firms in Germany (and employ about two-thirds of the workers). Taking a closer look, we also find significant wage effects for larger and profitable manufacturing firms with up to 500 employees. These firms (the so-called *"Mittelstand"*) are often considered to be the backbone of the German economy, with many "hidden champions" (Simon 2009). Our results suggest that workers in these companies are more affected by local corporate tax changes than employees of very large firms. One reason for this finding may be local wage setting power of larger firms, as suggested by monopsony models. When interacting the LBT rate with a dummy indicating the size of the firm relative to the local labor market, we indeed find that wages in relatively small firms react more strongly.

Other potential explanations for the insignificant wage effect in large firms include more tax avoidance opportunities or a presence in multiple jurisdictions. Table 3 shows significant wage effects only for single-plant firms, while establishments in multi-plant firms show no wage response. For those firms, tax changes in one jurisdiction might not be relevant enough to influence wages. Another explanation is that multi-plant firms can shift profits to other jurisdictions (nationally and/or internationally). In line with this reasoning, we also find a zero (to be precise, a negative but insignificant) wage effect if a plant has a foreign owner.[33] This supports the theoretical prediction that profit-shifting opportunities dampen effects of local tax changes on wages.

Worker Heterogeneity.—We test for worker heterogeneity by estimating model (3) at the individual level. Baseline estimates are similar to results at the firm level and robust to including various control sets (cf. online Appendix Table D.6).[34] Heterogeneous worker effects are summarized in Table 4.

[32] See online Appendix B.2 for a brief discussion of labor market institutions in Germany.

[33] Neither the effect for single-plant firms nor for German-owned firms is driven by firm size.

[34] Unlike the analysis at the firm-level, for which we used the median wage as our left-hand-side variable, the observed wage at the individual level might be censored as discussed above. We address this issue by estimating each interaction model for the full sample of all workers and for a subsample excluding individuals who have been

Table 4—Difference-in-Differences Estimates: Wage Effects by Worker Type

Stratified by	Effect of log net-of-LBT rate by worker type			Observations
Skill	High	Medium	Low	9,295,488
	0.013	0.357	0.377	
	(0.120)	(0.115)	(0.168)	
Gender	Female	Male		9,295,488
	0.530	0.325		
	(0.129)	(0.119)		
Occupation	Blue-collar	White-collar		9,295,442
	0.363	0.250		
	(0.132)	(0.104)		
Age	Young	Medium	Old	9,295,488
	0.507	0.317	0.329	
	(0.127)	(0.111)	(0.106)	

Notes: This table presents the DiD estimates $\hat{\delta}$ of regression model (3) with the log individual wage as dependent variables for different worker types as indicated in the table. The heterogeneous effects are estimated by interacting the LBT rate with dummy variables for different firms types. Coefficients measure the wage elasticity with respect to the net-of-local-business-tax rate. All specifications include worker, firm, and municipal fixed effects, as well as "state × year" and "worker type × year" fixed effects. The estimation sample comprises all establishments liable to the LBT in non-merged municipalities. Standard errors are clustered at the municipal level.

Source: Author's calculations based on data from LIAB and Statistical Offices of the Laender

In our first test, we look at the effect by skill. While effects are similar for medium- and low-skilled workers, we find no wage effect for high-skilled individuals, even if we exclude workers affected by censoring (see online Appendix Table D.8). A potential reason for this difference is that high-skilled workers are usually more mobile than low-skilled individuals in Germany (Haas 2000). An alternative explanation would be that the wage setting process differs across skill levels.[35]

Mobility effects are also a potential explanation for our heterogeneous effects by gender, where we find larger wage effects for women. In Germany, women are often the secondary earner in a couple. This reduces their mobility. We check that gender effects are not driven by differences in industry, occupation, or different work contracts in terms of working hours. In general, wage effects do not change when including part-time workers; see column 8 of online Appendix Table D.6. When differentiating by broad occupation group, we find a stronger effect for blue-collar workers, in line with the results by industry shown above. Similarly, when stratifying by age, the effect is significantly higher for younger workers.

Our results for heterogeneous types of workers are particularly important for the distributional implications of corporate taxation. We confirm other empirical studies that corporate taxes are not entirely borne by capital, finding that one-half of the burden is shifted onto wage earners. In addition, more vulnerable worker groups are

above the contribution ceiling at least once. As above, we find that wage effects increase when restricting the sample to never censored workers (see online Appendix Table D.8).

[35] More bargaining power of skilled workers is not a sufficient explanation for the observation that wages of this group do not fall in response to higher taxes. Groups with high bargaining power can be expected to capture a high share of the firm's profit ex ante, so that they should suffer larger losses than groups with less bargaining power if corporate taxes increase.

affected more strongly by changes in corporate tax rates. Both findings reduce the progressivity of business taxes and consequently of the overall tax system.

We assess the implications of our findings for tax progressivity in a back-of-the-envelope calculation. Our starting point is the study on the progressivity of the US tax system by Piketty and Saez (2007). They calculate effective average (personal plus corporate) income tax rates across the income distribution, and measure the progressivity of the tax system by comparing the average tax rate of the top 10 percent or top 1 percent to the average tax rate of the bottom 90 percent. Importantly, they assume that corporate taxes fall entirely on capital income. We take their data and estimates as a benchmark for the United States and use comparable data compiled by Bach, Beznoska, and Steiner (2016) for Germany. We then compute two counterfactuals where 50 percent (or 100 percent) of corporate taxes fall on wages. Calculations are reported in online Appendix Table D.9.[36] The ratio between the total effective average tax rate of the top 1 percent and the bottom 90 percent decreases substantially from 3.9 to 3.2 if one-half of the corporate tax burden is borne by labor, or to 2.9 if the full corporate tax burden is shifted onto wages. We find similar relative changes of progressivity for the German tax system (decreasing from 6.1 to 4.6 and 3.7). Overall, our calculations imply that the progressivity of the overall tax system in both countries would decline by between 25 and 40 percent if we account for our incidence estimates.

V. Conclusions

In this paper, we exploit the compelling institutional setting of the German local business tax to analyze the incidence of corporate taxes on wages. We combine administrative information from 1993 to 2012 on the universe of municipalities with administrative linked employer-employee data to estimate the causal effect of corporate taxation on wages. Averaging over firms liable to the LBT, workers bear about 51 percent of the total tax burden. This finding is similar to other studies analyzing the corporate tax incidence on wages (Arulampalam, Devereux, and Maffini 2012; Liu and Altshuler 2013; Suárez Serrato and Zidar 2016).

Our results thus confirm the view that labor bears a substantial share of the corporate tax burden. Importantly, our results are obtained by exploiting variation at the local level. Corporate taxes levied at the subnational level exist in many countries, and our results are likely to be relevant in these countries as well. At the same time, it is important to discuss how our findings are related to settings with state-level or national corporate taxes. Two differences are important. On the one hand, labor is likely to be more mobile at the local level, which attenuates the incidence on wages. On the other hand, focusing on tax changes at the municipal level implies that changes of prices other than wages, in particular output prices and prices of intermediate goods, are probably much smaller than in the case of national corporate tax changes. This would imply that wage effects of local tax changes are larger.

Going beyond the average wage effect, our analysis shows that incidence estimates differ considerably across firms and individuals. First, we do not find effects

[36] Further details are explained in the notes to online Appendix Table D.9.

for firms that are not liable to the LBT. Second, our findings suggest that labor market institutions play a key role for the incidence of corporate taxes on wages. If there is rent sharing in the labor market, due to collective bargaining, for instance, wage responses are larger. Third, wage effects are close to zero for firms that operate in multiple jurisdictions, large firms, and foreign-owned firms. This may be explained by profit-shifting opportunities available to these firms. Clearly, the heterogeneous results are correlations and should be seen as a first step toward understanding the underlying mechanisms of the incidence of corporate taxation on wages. For a more rigorous test of competing theories, additional exogenous variation in labor market institutions and other firm characteristics would be necessary.

The heterogeneous worker analysis reveals stronger wage effects for low-skilled workers, women, and young workers. High-skilled employees are not affected at all. This challenges the widespread view that the corporate income tax is highly progressive. In fact, our estimates imply that the shifting of part of the corporate tax burden onto wages reduces the overall progressivity of the tax systems both in Germany and the United States by 25 to 40 percent compared to a hypothetical situation where no shifting occurs.

An important limitation of our analysis is that we focus on wage effects and do not investigate the impact of tax changes on quantities of input factors, on output, or on entry and exit of firms. These potential responses are important for the efficiency costs of taxes. Another limitation is that we do not consider the impact on land rents. These are issues for future research.

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