Empirical Essays in Public Economics

by

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To my parents, Bettina and Michael,
my brothers, Julius and Steffen,
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ABSTRACT

This dissertation contains three empirical essays that study how government policies impact the economy. Chapter 1 measures how much higher income taxes on U.S. top 1% earners "trickle down" and reduce other workers' wages via geographically concentrated spillovers. While trickle-down effects feature prominently in tax policy debates, relatively little is known about their magnitude. The paper uses an exposure design that combines time-series variation in the federal marginal tax rate for top 1% earners with cross-sectional variation in the top 1% income share across local labor markets. Intuitively, the design asks whether workers in local labor markets where top 1% earners account for a larger share of economic activity are more adversely impacted by a large tax increase for top 1% earners. The results provide very little evidence of local trickle-down effects. The point estimates imply zero local trickle-down effects. At conventional levels of confidence, the estimates are statistically inconsistent with a one percentage point increase in the top tax rate reducing worker wages by more than -0.08%. These results undermine claims that trickle-down effects should be an important consideration in setting top tax rates.

Chapter 2 empirically studies the effect of local business taxes on business entry. Business entry has been linked to productivity growth, job creation and the magnitude of business cycles, highlighting the need for evidence on the impact of business taxes on business entry. The paper combines 5,111 local (municipal) business tax rate changes with administrative data on the universe of business (corporate employer establishment) entrants in Germany between 2004-2012. Using a dynamic difference-in-differences approach, the paper estimates that a one-percentage point increase in the local business tax rate reduces business entry by -4% over the medium term. Cumulated over a six-year period, the loss in business entry amounts to -18% of an entry cohort. The drop in entry is driven primarily by single-establishment firms with less than three employees.

Chapter 3 theoretically and empirically studies the role of firms in transmitting worker-level policies (e.g. mandated benefits, payroll taxes) into wages. A growing body of evidence suggests that firms have labor market power and contribute considerably to wage inequality. Less is known about the role of firms in determining how worker-level policies impact wages. When firms have labor market power, the wage impact of worker-level

policies on a given type of worker can vary across firms. Using a static wage-posting model, I theoretically characterize the aspects of firms that determine how worker-level policies impact wages. Using administrative linked employer-employee data to study a German payroll tax reform, I find that similar workers experienced differential wage changes depending on the reform's impact on their employer's labor costs, one of the firm dimensions highlighted by the model.

CHAPTER I

Do Higher Income Taxes on Top Earners Trickle Down? A Local Labor Markets Approach

1.1 Introduction

Proposals to increase income tax rates for top earners often prompt discussions about whether such policies ultimately harm other workers. When the after-tax return to effort decreases, top earners may work less hard. This change in their effort can impact other workers. If the effort levels of top earners and workers (non-top earners) are complementary, so that a decrease in the effort of top earners decreases workers' productivity, higher income taxes on top earners can "trickle down" and reduce worker wages. Critics argue that such general equilibrium spillovers make higher income taxes on top earners undesirable.[1] Although the policy-relevance of trickle-down effects is theoretically understood (e.g. Sachs et al., 2020), empirical evidence on the magnitude of trickle-down effects is scarce.[2]

In this paper, I develop a local labor markets approach to measuring trickle-down effects from higher income taxes on U.S. top 1% earners. The approach measures the extent to which worker (bottom 99% earner) wages decrease due to geographically concentrated (e.g. within metro area) spillovers from top 1% earner responses to a tax increase. I estimate these local trickle-down effects using an exposure design. The design combines time-series variation in federal tax policy towards top 1% earners with cross-sectional variation in the

[1] For example, discussing former President Obama's proposal to increase taxes on top earners, The Heritage Foundation writes that "tax hikes on high-income earners will hurt the poor" (The Heritage Foundation, Accessed: 11/09/2021).

[2] A number of paper study the impacts of top income taxation on aggregate economic outcomes such as GDP per capita or employment (e.g. Piketty et al., 2014; Zidar, 2019). Two papers that explicitly consider the impacts of top income taxation on the incomes of other workers are Mertens and Montiel Olea (2018) and Risch (2021).

importance of top 1% earners to local labor markets. I measure the importance as the share of local labor market income that is accounted for by top 1% earners (top 1% income share). The paper applies the approach to a large federal tax increase for top 1% earners in 2013. Measured over six years, the point estimates suggest zero local trickle-down effects. At the 95% confidence level, we can be reasonably sure that a one percentage point increase in the top tax rate reduces worker wages by no more than -0.08%. Interpreted through a welfare framework, these results undermine claims that trickle-down effects should be an important consideration when setting top tax rates.

The local labor markets approach this paper develops captures a wide range of general equilibrium spillovers and is identified even if the tax reform is motivated by national economic conditions. Local trickle-down effects capture direct spillovers between top earners and other workers, for example those that occur within or between local firms, as well as indirect spillovers that arise as the direct effects diffuse throughout the local economy. The combination of a national top tax change and subnational variation in the exposure to the tax change results in quasi-exogenous variation of economic shocks across local labor markets. Even if the top tax change is motivated by national economic conditions, which would bias country-level estimates, this paper's approach yields unbiased estimates of local trickle-down effects.

The local trickle-down effects this paper estimates are likely an important component of total trickle-down effects given the nature of U.S. top earners. If top earners were primarily executives in large, nationally operating companies, the impacts of top earner responses to a tax increase would not necessarily be concentrated locally. Such executives, however, are not representative of the top of the U.S. income distribution. As documented by Smith et al. (2019), the top 10,700 executives at S&P 1500 companies account for less than 1% of all top 1% U.S. earners. In contrast, approximately 70% of top 1% earners are the owners and active managers of small to medium-sized, locally or regionally operating businesses.[3] Spillovers from the responses of these owner-managers are arguably concentrated locally.

The exposure design this paper uses to estimate local trickle-down effects is appropriate for two reasons. First, I show that in a model where higher taxes on top earners can trickle down due to the complementarity of top earner and worker labor, trickle-down effects vary across local labor markets in proportion to the top income share. Second, I demonstrate that there is considerable variation in top 1% income shares across U.S. Core-Based Statistical Areas (CBSAs), which I refer to as local labor markets.[4] While taxpayers with an income in

[3]The difference in numbers between owner-managers and executives carries over to income. While the total income of the top 10,700 executives is \$33B, the total income of owner-managers is \$1.5T.

[4]CBSAs are groups of contiguous counties that are centered around an urban core and have strong economic ties as measured by commuting patterns.

the top one percentile of the national income distribution account for less than 5% of local income in some CBSAs, in others they account for more than 25%.

In the model, local spillovers of top earner responses are the product of the top income share and a trickle-down factor. The magnitude of the factor is determined jointly by two components. The first component is the magnitude of real responses to the tax increase, governed by the elasticity of labor supply. The second is the extent to which real responses impact worker wages, governed by the elasticity of substitution between top earner and worker labor. In the literature, there is uncertainty about the value of both parameters, highlighting the necessity for direct evidence on the magnitude of trickle-down effects.

The paper applies the local labor markets approach to a large tax increase for top 1% earners.[5] The American Taxpayer Relief Act and the Affordable Care Act Surtax triggered a large marginal tax rate increase for top 1% earners in 2013 with limited tax-rate changes below the top bracket. The targeted nature of the reforms motivates the focus of this paper on trickle-down effects specifically from top 1% earners. Marginal tax rates for top 1% earners increased across all sources of income and the income-weighted average statutory marginal tax rate increased by 7.5 percentage points from 32.2% to 39.7%.[6]

I estimate the wage effect of a higher exposure to the top 1% tax increase using continuous difference-in-differences regressions that control for a number of other factors influencing CBSA-level wages. Intuitively, I ask whether, following a large top 1% tax increase, worker wages decrease in labor markets with a high top 1% income share relative to wages in labor markets with a low top 1% income share. The identification assumption is that, conditional on controls, any differential wage change between high and low top 1% income CBSAs is attributable to a differential exposure to the reform (Goldsmith-Pinkham et al., 2020; Callaway et al., 2021). The estimated wage effect can be used to test the null hypothesis of zero local trickle-down effects.

The paper combines a variety of publicly available data to construct top 1% income shares and wage measures. I use county-level tabulations of income tax returns to calculate CBSA-level top 1% income shares. These data are based on the population of tax returns (Form 1040) from the IRS Individual Master File system and are collapsed to county by Adjusted Gross Income (total income) size cells. I use the Quarterly Census of Employment and Wages to construct CBSA-level average annual private sector wages (earnings) and

[5] Although I use the local labor markets approach to measure general equilibrium effects of top income tax changes, the approach can also be used to measure general equilibrium effects of tax changes that target other parts of the income distribution. For example, one could study the general equilibrium effects of EITC expansions by exploiting variation in the share of local income accounted for by EITC eligible workers across local labor markets.

[6] As the reform was anticipated during the second half of 2012, I treat 2011 as the last pre-reform year.

the American Community Survey to construct CBSA-level composition-controlled hourly wage measures for the private sector after explicitly excluding top earners.

The estimates reveal zero statistically detectable local trickle-down effects. In support of a causal interpretation of the estimates, annual wages in high and low exposure CBSAs evolved in parallel during the eight years prior to the reform. For six years following a large tax increase for top 1% earners in 2013, annual wages in CBSAs with a high top 1% income share continued to grow at the same rate as wages in CBSAs with a low top income share. Focusing on hourly wages and explicitly removing top earners results in qualitatively identical and quantitatively similar results. The estimated hourly wage effect of a one standard deviation increase in the exposure to the top tax increase (top 1% income share) is 0.08% with a 95% confidence interval of (-0.40%, 0.55%), equivalent to (-0.040, 0.055) standard deviations of log hourly wages across CBSAs.

The point estimates imply that a one percent cut in the top net-of-tax rate has a statistically insignificant positive impact on worker wages of +0.017%. At the 95% confidence level, we can be reasonably sure that worker wage decreases due to local spillovers do not exceed -0.051%.[7] To quantify the magnitude of local trickle-down effects, I interpret the estimated wage effect of a higher exposure to the reform as an estimate of the trickle-down factor in the complementarity model. I measure local trickle-down effects as the average wage change across all workers due to geographically concentrated spillovers from top earner responses to the tax increase, scaled by the size of the reform.

The null result is robust, does not mask heterogeneity across worker types or groups of local labor markets and extends to employment. The estimates are robust to perturbations of the main specification and to exercises that address measurement error concerns. To address the concern that high top income shares may partly reflect a concentration of wealthy retirees or non-human-capital-rich top earners, I repeat the analysis using income shares that exclude capital and pension income or are based solely on labor income (i.e. excluding pass-through income) and find similar results. To address the concern that top earners live and work in different labor markets, I repeat the analysis at the CSA level that merges geographically proximate CBSAs based on commuting patterns, and obtain similar results. The paper tests for, but does not find, evidence in support of heterogeneity across worker types, as defined by education or income, as well heterogeneity across local labor markets, as defined by density, capital intensity, or cyclicality. Lastly, I find no adverse employment effects of a higher exposure to the top tax increase.

The null result is consistent either with a small real response of top earners to the tax

[7] To arrive at the -0.08% bound, multiply the elasticity by 1.57 to take account of the fact that, starting at a tax rate of 35.95%, the effect on the log net-of-tax rate of increasing the rate to 36.95% is -1.57 log points.

increase (small elasticity of labor supply) or a small impact of top earner responses on worker wages (high elasticity of substitution between top earners and workers). I interpret my reduced-form estimates through the lens of the complementarity model to evaluate the consistency of the estimates with various combinations of these two parameters. In the process, I formally discuss how this paper's estimates can be rationalized. The bound on local trickle-down effects implies a lower bound on the ratio of the elasticity of substitution to the elasticity of labor supply of approximately two. Finally, I provide suggestive evidence that the null result is due to a small real response of top earners.

I study the welfare implications of the estimated bound on local trickle-down effects to provide a perspective on its magnitude. Trickle-down effects amount to a regressive pre-tax redistribution of resources. When top earners reduce their effective labor supply, their own marginal product increases and, because of complementarity, the marginal product of workers decreases. This regressive redistribution impacts the social welfare benefit of a higher top tax rate via a revenue and a utility component. The revenue component is positive because income is transferred from workers to top earners, where it is taxed at higher rates. The redistribution lowers worker utility and increases top earner utility, which together comprise the utility component. The sign and magnitude of the utility component depends on the distribution of welfare weights.

The point estimates suggest that local trickle-down effects have zero welfare impact. If welfare weights are inversely proportional to disposable income, the bounds suggest it is unlikely that local trickle-down effects offset the welfare benefit of a higher top marginal tax rate by more than 14% of the mechanical revenue gain – the gain from applying a higher rate to a constant base. The result is robust to alternative welfare weights. Although different welfare weights imply different offsets, more equal welfare weights do not necessarily imply larger offsets. In fact, if welfare weights are constant, the utility gain for top earners fully offsets the loss for workers so that trickle-down effects matter only via tax revenue impacts. The estimates of this paper therefore undermine claims that trickle-down effects should be an important consideration in setting top tax rates.

Connections to Literature on Economic Impacts of Top Income Taxation This paper connects primarily to an empirical literature that examines the aggregate economic impacts and spillover effects of top income taxation.[8] A number of papers use country-level approaches to estimate the relationship between top tax rates and national outcomes such as GDP per capita (Lee and Gordon, 2005; Piketty et al., 2014; Gemmell et al., 2014; Mertens

[8] This literature is related to one that studies the effects of income tax changes on economic growth, surveyed in Gale and Samwick (2017). The distinction lies in the particular focus on *top* income tax policy.

and Montiel Olea, 2018; Hope and Limberg, 2020). Using a state-level approach that is similar in spirit to the local labor markets approach of this paper, Zidar (2019) studies the impact of tax-induced income changes for top 10% earners on aggregate economic activity.[9] Proceeding at the firm level, Risch (2021) asks if top-income owner-managers pass higher tax liabilities on to their workers. This paper instead focusses on whether tax-induced incentive changes for top 1% earners impact the wages of other workers via local labor markets. The local labor markets approach captures a wider range of general equilibrium spillovers than the firm-level approach and is identified under weaker assumptions than the national approach. The estimates of this paper combined with those of Mertens and Montiel Olea (2018) and Risch (2021) offer the only direct evidence of how tax changes for top earners impact the income of other workers.[10] This paper is unique in focussing on wages and interpreting its estimates through a welfare framework.

This paper also connects to a theoretical literature studying the implications of general equilibrium spillovers for optimal income taxation. While the traditional optimal tax framework of Mirrlees (1971) implicitly assumes perfect substitutability across all agents, a number of papers have since studied optimal taxation in models where different worker types are imperfectly substitutable (Feldstein, 1973; Allen, 1982; Stern, 1982; Stiglitz, 1982; Rothschild and Scheuer, 2013; Scheuer, 2014; Sachs et al., 2020).[11] The closest theoretical counterpart to this study, Sachs et al. (2020), characterizes the incidence and welfare implications of tax reforms outside of the optimum. The literature has quantified the normative implications of trickle-down effects by using existing estimates of preference and technology parameters. This paper instead uses direct estimates to quantify the welfare implications of trickle-down effects, thereby sidestepping the uncertainty about the true value of preference and technology parameters, in particular as they pertain to top earners, and relying less on specific functional form assumptions.

Finally, this paper connects to the literature studying behavioral responses of top earners to taxation. One strand of this literature documents large retiming and reclassification responses that are unlikely to systematically affect other workers (Slemrod, 1995; Goolsbee, 2000; Saez et al., 2012b). Another strand, however, documents sizable real

[9] Zidar (2019) asks whether tax changes for bottom 90% or top 10% earners are more effective at stimulating economic activity, as measured primarily by employment and GDP. The paper compares states that, due different income distributions, experienced differential income changes following federal tax reforms between 1950 and 2011.

[10] Mertens and Montiel Olea (2018) use a time-series approach to examine the impacts of tax changes for top 1% earners on the income of bottom 99% earners. Risch (2021) compares workers in similar S-Corporations where the owner was or was not exposed to a top tax increase due other sources of income.

[11] In these models, trickle-down effects can arise due to factor complementarity between top earner and worker labor. Jones (2019) instead considers the case where trickle-down effects can arise because top earners impact total factor productivity.

responses that are more likely to affect other workers (Mertens and Montiel Olea, 2018; Rauh and Shyu, 2019). While the zero trickle-down estimates of this paper are potentially consistent with a large real response of top earners to the reform if one assumes a low impact of top earner responses on wages, the paper provides suggestive evidence that real responses were limited.

The remainder of this paper proceeds as follows. Section 1.2 presents the local labor markets approach, Section 1.3 describes the empirical implementation, Section 1.4 discusses the results and Section 1.5 interprets the results through a welfare framework.

1.2 Local Labor Markets Approach

This section describes the local labor markets approach. I begin by detailing the scope of the approach, and then discuss how the approach proceeds and why it proceeds that way.

1.2.1 Scope: Local Impacts of Top Earner Responses

Top earner responses to a tax-induced decrease in the return to effort can have local and national spillovers. When the manager of a local firm works less hard, this affects workers within the same firm, workers in other local firms that are linked via production networks, and ultimately, via changes in local labor supply and demand, workers throughout the local economy. When the CEO of a nationally operating company works less hard, this affects the local economy where the company is headquartered but also other local economies in which the company operates. When an inventor works less hard, this initially affects the local economy where she is located, because her ideas contribute to local labor demand, but ultimately also the productivity of workers everywhere via total factor productivity.[12]

The total trickle-down effect, the average wage decrease across all workers due to a top tax increase, can therefore be decomposed into a local and a national effect. Consider first the average log wage decrease in a given local labor market k due to an increase in the top marginal tax rate, denoted Δw_b^k. The wage decrease can be separated into a local component, ϕ_k, and a national component, γ,

$$\underbrace{\Delta w_b^k}_{\text{Local avg. wage decrease due top tax increase}} = \underbrace{\phi_k}_{\text{Local component}} + \underbrace{\gamma}_{\text{National component}} \tag{1.2.1}$$

where the local component reflects the local spillovers of local top earner responses to the

[12] Kline et al. (2019a) document that firms that successfully innovate grow faster than comparable firms and Jaffe et al. (1993) document that knowledge spillovers from innovation are concentrated locally.

tax increase and the national component reflects that national spillovers of all top earner responses.[13] Given this separation at the local level, the average log wage decrease across all workers due to trickle-down effects can be decomposed into a local effect and a national effect

$$\underbrace{\Delta w_b}_{\text{Total trickle-down effect}} = \underbrace{\sum_k \omega_k \phi_k}_{\mathcal{L}\text{: Local effect}} + \underbrace{\gamma}_{\mathcal{N}\text{: National effect}} \tag{1.2.2}$$

where ω_k is the share of all workers in k. Note that (1.2.2) corresponds to the worker-weighted average wage decrease across all local economies, so that $\Delta w_b = \sum_k \omega_k \Delta w_b^k$.

This paper focuses on estimating local trickle-down effects, the average wage decrease across all workers due to local spillovers from top earner responses to a tax increase.

As discussed in the introduction, local trickle-down effects are likely an important part of total trickle-down effects given that the majority of U.S. top earners are the owners and active managers of small to medium-sized, locally or regionally operating businesses.[14] Local effects are also potentially the primary component of total trickle-down effects over the short to medium run (1-6 years), which is the focus of this paper.[15] When an inventor produces less patents in response to a tax increase, this can ultimately reduce the pace of technological progress and the productivity of workers everywhere. However, the diffusion of new technology throughout the economy takes time (Gordon, 2016) so that trickle-down effects via total factor productivity are more likely to materialize over the long run. In contrast, changes in top earners' effective labor supply can affect worker wages over a shorter time horizon. For example, Jäger and Heining (2019) show that an unexpected manager death has an immediate adverse impact on the wages of other workers within the same firm.

[13]Note that the decomposition in (1.2.1) implicitly assumes that the national component is constant across local labor markets. In the empirical analysis, I control for a number of factors that are intended to absorb potential differences in the national component across local labor markets.

[14]Note that factor mobility across local labor markets can result in initially local shocks being diffused throughout the economy. If trickle-down effects decrease wages more in one local labor market than another, worker migration can equalize wage decreases. To the extent that worker migration is equalizing wage impacts, one would expect to see initial differences in trickle-down effects across labor markets that disappear over time. The empirical results discussed below do not indicate such a pattern.

[15]Changes in the consumption and savings/investment behavior of top earners in response to a lower post-tax income also potentially affect other workers. However, at the national level, these changes in the behavior of top earners need to be weighed against the changes in government spending and saving/investment due to higher tax revenues.

1.2.2 Approach: Exposure Design

This paper estimates local trickle-down effects using an exposure design. I combine cross-sectional variation in the share of local income earned by residents who are subject to the federal top bracket tax rate – top income share – with time-series variation in the federal top bracket tax rate. Intuitively, I ask whether worker wages decrease in local labor markets with a higher exposure to a federal top tax increase relative to wages in labor markets with a lower exposure, where exposure corresponds to the top income share. The top income share can be interpreted as the marginal product weighted top earner share and captures the economic importance of top earners to a local economy. Formally, I consider the following model for the local component of trickle-down effects in a given local labor market k, ϕ_k,

$$\underbrace{\phi_k}_{\text{Local component}} = \underbrace{S_t^k}_{\text{Top income share}} * \underbrace{\beta}_{\text{Trickle-down factor}} \tag{1.2.3}$$

In the exposure design, wage growth comparisons between high and low exposure local labor markets following a tax increase for top earners reveal the magnitude of the trickle-down factor, and the factor can be used to calculate local trickle-down effects. Consider two arbitrary local labor markets denoted by k and k' and suppose that the top income share in k' is higher, $S_t^{k'} > S_t^k$. The difference in the wage growth between the two labor markets after a tax increase for top earners is given by $\Delta w_b^{k'} - \Delta w_b^k = \beta * \left(S_t^{k'} - S_t^k\right)$, so that the trickle-down factor can be estimated as $\hat{\beta} = \frac{\Delta w_b^{k'} - \Delta w_b^k}{S_t^{k'} - S_t^k}$. The analysis will estimate β by making many such comparisons across many local labor markets with different top income shares. Given an estimate of the trickle-down factor, the local trickle-down effect can then be estimated as

$$\hat{\mathcal{L}} = \sum_k \omega_k \left(\hat{\beta} * S_t^k\right)$$

The exposure design is appropriate for two reasons. First, in a model where higher taxes on top earners can trickle down due to the complementarity of top earner and worker labor, trickle-down effects are more pronounced in local labor markets with higher top income shares. This provides a theoretical foundation for the exposure design and allows a structural interpretation of the trickle-down factor. Second, across U.S. local labor markets there is considerable variation in top income shares. These points are elaborated in the following two subsections.[16]

[16] Exposure designs are commonly used across a wide range of economics. For example, in macroeconomics they are used to study regional multipliers (Chodorow-Reich, 2019), in labor economics

1.2.2.1 Theoretical Foundation of Exposure Design

I begin by describing the economic environment and then present the key result that emerges within the environment. The result provides a theoretical foundation for the exposure design.

Local Labor Markets There is a set of local labor markets $\mathcal{K}$. Each labor market $k \in \mathcal{K}$ is characterized by a set of wages, one for each type of agent.

Agents The population of each local labor market k is composed of different types of agents who differ in their skill, $\theta \in \Theta$. Agents chose how much to work, l, and consume, c. Their preferences are characterized by the quasi-linear and isoelastic utility function $U(c, l) = c - \frac{e}{1+e} l^{\frac{1+e}{e}}$ where e is the elasticity of labor supply.[17] Labor income, $y = wl$, is subject to the tax schedule $T(y)$, so that consumption corresponds to after-tax income, $c = y - T(y)$. Each agent's optimal choice of labor supply is determined by

$$l = w^e \left(1 - T'(wl)\right)^e$$

Tax Schedule Labor income y is subject to the tax schedule $T(y)$. In accordance with the shape of the U.S. income tax schedule, I assume that $T(y)$ is piecewise linear.

Behavioral Responses Given the piecewise linear tax schedule and ignoring kink points, the elasticity of labor supply with respect to the net-of-marginal-tax rate is given by e.[18] Note that the elasticity should be interpreted broadly to capture any changes in effective labor supply, including changes in the effort supplied per hour of work or the amount of time and energy devoted to thinking about work outside of working hours.

they are used to study the impact of immigrants on the wages of natives (Card, 2009), and in public finance to study the impact of federal tax policy (Garrett et al., 2020).

[17] By assumption there are no income effects, so that e corresponds both to the compensated and uncompensated elasticity of labor supply.

[18] In general, with a non-linear tax schedule the elasticities are given by $e(y)_{1-MTR} = \frac{e}{1+e\frac{yT''(y)}{1-T'(y)}}$ and $e(y)_w = \left(1 - \frac{yT''(y)}{1-T'(y)}\right) e(y)_{MTR}$, where the second derivative of the tax schedule captures that a change in the net-of-tax rate (wage) triggers a labor supply adjustment, which triggers an adjustment of the marginal tax rate, which triggers a labor supply adjustment, etc. However, if the tax schedule is piecewise linear and we ignore workers who are located at kink points and those who switch income brackets, the marginal tax rate is constant so that $T''(y) = 0$.

Production Output in each local labor market k is produced according to an aggregate production function that uses labor as an input.[19] Each labor market produces a homogenous output good that serves as the numeraire with a price equal to 1. The production function is constant returns to scale and characterized by a constant elasticity of substitution across worker types.[20] Formally,

$$F^k\left(\left\{L_\theta^k\right\}_{\theta\in\Theta}\right) = A^k\left(\sum_{\theta\in\Theta}\alpha_\theta^k\left(L_\theta^k\right)^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}}$$

where A^k is a productivity parameter, $\{\alpha_\theta^k\}_{\theta\in\Theta}$ are parameters that determine the relative importance of different skill types in the production process, $L_\theta^k = N_\theta^k l_\theta^k$ is the total labor supply of type θ in k, determined for each type as the product of each agent's labor supply l_θ^k and the total number of agents in the labor market N_θ^k, and $\sigma \geq 0$ is the elasticity of substitution. When $\sigma < \infty$, different types of labor are imperfectly substitutable so that increases in the labor supply of one type increase the marginal product of other types. The labor market is competitive, so that wages, denoted w_θ^k, equal marginal products.

Proposition 1. *Consider an increase in the marginal tax rate for top 1% earners, τ_t. Denote the income floor of the top 1 percentile by $P1$. Given the economic environment described above, the log wage change of workers (bottom 99% earners) in labor market k due to a change in the rate for top 1% earners of $\Delta\ln(1-\tau_t) < 0$ is given by*

$$\Delta w_b^k = S_t^k * \left[\frac{e}{\sigma+e} * \Delta\ln(1-\tau_t)\right] \leq 0 \tag{1.2.4}$$

where $S_t^k = \sum_{\{\theta: w_\theta^k l_\theta^k \geq P1\}} S_\theta^k = \sum_{\{\theta: w_\theta^k l_\theta^k \geq P1\}} \frac{w_\theta^k L_\theta^k}{\sum_{\theta\in\Theta} w_\theta^k L_\theta^k}$ is the income share of top 1% earners in k, e is the elasticity of labor supply, and σ is the elasticity of substitution.

Proof. See Appendix A.1.1. □

When the top 1% tax rate increases ($\Delta\ln(1-\tau_t) < 0$), top earners can reduce their effective labor supply and, because of the complementarity of top earner and worker labor,

[19] Abstracting from capital is without loss of generality if the supply of capital is perfectly elastic, the production function is constant returns to scale and depends on capital as well as a labor aggregate.

[20] In Appendix A.1.2, I demonstrate that the key insight of this section — trickle-down effects vary across local labor markets in proportion to top income shares — continues to apply if the elasticity of substitution between top earners and other workers is distinct from the elasticity of substitution among groups of other workers (e.g. high- vs. low-skilled). In that case, the elasticity of substitution in Proposition 1 corresponds to the elasticity of substitution between top earners and other workers.

this can reduce the wages of workers (bottom 99% earners).[21,22] Proposition 1 illustrates that variation across local labor markets in the magnitude of trickle-down effects is driven by variation in top 1% income shares. The proposition provides a theoretical foundation for the functional form of local spillovers in a given labor market in (1.2.3), and a structural interpretation of the trickle-down factor, $\beta = \frac{e}{\sigma+e} * \Delta \ln(1-\tau_t)$. The top 1% income share can be interpreted as the marginal-product-weighted top 1% earner share, and captures the economic importance of top 1% earners to a local economy. As the economic importance of top 1% earners increases, the magnitude of trickle-down effects changes in proportion to the trickle-down factor.

The production function that characterizes local labor markets is an abstract representation of an underlying economy in which the complementarity of top 1% earners and workers can arise in multiple ways. For example, top earners might provide managerial labor that increases the productivity of other workers. Empirical support for such a mechanism comes from Jäger and Heining (2019), who document that unexpected manager deaths reduce the wages of workers within the same firm. Alternatively, as suggested by Smith et al. (2019), top earners can provide entrepreneurial labor which increases the demand for and hence wages of other workers.

Proposition 1 highlights that the magnitude of trickle-down effects is determined jointly by two factors. First, the magnitude of real responses to the tax increase, as governed by the elasticity of labor supply e. Second, the extent to which real responses impact worker wages, as governed by the elasticity of substitution between top earners and other workers σ. To be sure, there is uncertainty about the magnitude of both parameters. For example, while Tortarolo et al. (2020) estimate an elasticity of labor supply of 0.3 for managers, Rauh and Shyu (2019) estimate an elasticity of 2.5-3.5 for high-income earners. While Dustmann et al. (2013) provide a low estimate of the elasticity of substitution of 0.6, Heathcote et al. (2017) suggest a value of 3.1.[23] While some parameter combinations predict small differences in wage changes across local labor markets with different top 1% income shares, others predict considerable differences. This uncertainty highlights the need for direct evidence on the magnitude of trickle-down effects.

[21] Note that complementarity here refers to the notion of q-complements: an increase in the labor supply of top earners increases the marginal product of other workers.

[22] Expression (1.2.4) takes into account that a wage decrease for bottom 99% earners triggers a labor supply decrease, which triggers a wage decrease for top 1% earners and a wage increase for bottom 99% earners, which trigger further labor supply changes, etc. It assumes these effects happen instantaneously.

[23] These elasticities of substitution are not estimated specifically for top earners. I use them as reference values because estimates specifically for top earners are, to the best of my knowledge, not available.

1.2.2.2 Variation in Exposure Across U.S. Local Labor Markets

Figure 1.1 illustrates the variation in top 1% income shares across U.S. local labor markets in 2011, the last year unaffected by the tax changes I investigate below. The concept of a local labor market in this research is the core-based statistical area (CBSA). CBSAs consist of one or more contiguous counties that have strong economic ties as measured by commuting patterns and are centered around an urban core. There are 924 CBSAs and these local labor markets account for approximately 94% of the continental U.S. population. The top 1% income share in a CBSA in 2011 is the share of all CBSA 2011 income that is earned by CBSA residents with an income in the top 1 percentile of the 2011 national income distribution.[24] Formally, let i index taxpayers and k index CBSAs and denote the income of taxpayer i by y_i and the set of taxpayers whose income falls into the top 1% of the national distribution as $\mathcal{T}$. The top 1% income share in CBSA k, denoted S_t^k, is given by

$$S_t^k \equiv \frac{\sum_{\{i:i\in k \wedge i\in \mathcal{T}\}} y_i}{\sum_{\{i:i\in k\}} y_i}$$

There is considerable variation in the top 1% income across U.S. local labor markets. Figure 1.1 Panel (a) depicts variation in top 1% income shares on a map of the continental U.S. and Panel (b) presents a population-weighted histogram. Note that the white areas on the map in Panel (a) are rural regions of the U.S. that are not part of CBSAs. While some CBSAs have top income shares of less than 4%, others have top income shares of more than 20%. The population-weighted mean is 13.5% with a standard deviation of 7.2%. There is variation in top 1% income shares within each state and within similar types of CBSAs. For example, the top 1% income share is 23% in Los Angeles compared to 15% in San Diego, is 26% in Boston compared to 17% in Seattle and 30% in New York City compared to 19% in Chicago. The dispersion of top 1% income shares across U.S. local labor markets mirrors and extends the observations from Gaubert et al. (2021), who document dispersion in top incomes across states.

1.3 Policy Variation, Data and Econometric Methods

This section describes the tax change for top earners that I examine and the data that I use to measure exposure as well as outcomes and the econometric methods.

[24] To be precise, it is the share of income earned by CBSA residents with a total income of more than \$500k in 2011. More details are presented in Section 1.3.2.

1.3.1 Policy Variation

This paper exploits the large 2013 U.S. tax increase for the top 1% highest earners induced by the American Taxpayer Relief Act (ATRA) and the Affordable Care Act (ACA) Surtax. The reforms were implemented in 2013 but could be anticipated during the second half of 2012. Throughout the paper I therefore treat 2011 as the last year unaffected by the reforms. The following description of the policies is informed by Auten et al. (2016) and Saez (2017).

Figure 1.2 summarizes the changes to the income tax system in 2013 and illustrates that the changes were most pronounced among the top 1% highest earners. Panel (a) depicts how the combined federal income and Social Security marginal tax rates changed across the 2011 Adjusted Gross Income (AGI) distribution. In particular, the figure depicts the log change in the net-of-marginal-tax rate for taxpayer earnings. The underlying data are derived from an application of the 2012 and 2013 tax code to the 2011 Internal Revenue Service (IRS) Statistics of Income (SOI) public-use file using the NBER TAXSIM program. The panel reveals that the changes to the 2013 income tax rates were much larger among the top 1% of the 2011 AGI distribution. Taxpayers in the top 1 percentile saw their net-of-tax rate decrease on average by 13.8 log points whereas taxpayers further down the distribution saw their net-of-tax rate decrease by 1.8-2.1 log points.[25] The decrease for the top 1% reflects the combined impact of the ATRA and ACA Surtax, whereas the decrease below the top 1% reflects the expiration of a temporary Social Security tax cut that was introduced as one-year measure in 2011 and extended twice during 2012 before expiring in 2013.[26]

Panel (b) details the effect of the 2013 reforms on the statutory marginal tax rate for various sources of income among top 1% earners. The marginal tax rate on labor income increased by 6.9 percentage points (pp) due to the combined effect of the ATRA additional top tax bracket (+4.6pp), the ATRA phaseout of itemized deductions (+1.4pp) and the ACA Surtax (+0.9pp). The marginal tax rate on active business income increased by 6pp due to the combined effect of the additional top tax bracket and the phaseout of itemized deductions. The marginal tax rate on passive business income increased by 9.8pp due to the combined effect of the additional top bracket, the phaseout of itemized deductions, the ACA surtax and the net investment income tax (+3.8pp). The marginal tax rate on long-term capital gains increased by 10.2pp due to the additional top bracket (+5pp in the case of long-term capital gains), the phaseout of itemized deductions and the net

[25]Figure A.1 depicts the effect on the average tax rate. The average tax rate increased by 1.1-1.6pp below the top 1 percentile and increased by 5.5pp in the top 1 percentile.

[26]In Appendix A.2.7, I discuss how the Social Security tax increase could impact the empirical strategy and provide theoretical and empirical evidence that the empirical strategy is robust to concerns about contamination from the Social Security tax change.

investment income tax. The income-weighted average marginal tax rate increased by 7.5pp from 32.2% to 39.7%.[27]

1.3.2 Data

This paper combines a variety of publicly available data to construct the exposure measure, outcomes and controls. The data are available at the county level and I use 2009 CBSA delineations to aggregate counties to CBSAs.[28]

Exposure Measure — Top 1% Income Share I rely on IRS SOI county-level tabulations of income tax returns to calculate CBSA-level top 1% income shares. The data are based on administrative records of tax returns (Form 1040) from the IRS Individual Master File system and are collapsed to county-by-AGI-size cells. Returns are matched to counties via the ZIP code on the tax return.[29] The data contain information on the number of returns, total Adjusted Gross Income (AGI), totals of other income sources such as salaries & wages, and totals of various deductions such as charitable contributions. Prior to 2010 the data were not reported separately by AGI size. I refer to AGI and total income interchangeably.[30] Because the 2013 tax increase for top 1% earners was anticipated during the second half of 2012, I use data for the 2011 tax year to avoid capturing endogenous responses to the reform.

This paper calculates CBSA top 1% income shares by assuming that the distribution of AGI above $200k within a CBSA is Pareto and demonstrates, using more granular data from California, that this assumption yields highly accurate predictions. As discussed above, the 2013 reform had the largest effect on taxpayers with a taxable income above $400-450k. Because taxable income is approximately 80% of AGI in this income range, I target the share of AGI earned by taxpayers with an AGI above $500k. I refer to the AGI

[27]The marginal tax rate (MTR) increase for the top 1 percentile in Panel (a) is slightly larger than would be expected given the increases for various income sources in Panel (b) due to taxpayers no longer being subject to the alternative minimum tax (AMT) in 2013. Removing AMT taxpayers yields a log change in 1-MTR of approximately -0.125, see Figure A.1 Panel (a). Any remaining differences are due to the fact that marginal rates can differ from statutory rates due to phaseouts or special provisions in the tax code.

[28]Descriptive statistics are presented in Table A.1.

[29]Using tax data to measure geographic location of individuals and local economic outcomes is a common practice: Chetty et al. (2014) link children to commuting zones using the ZIP on the parent's tax return, Chodorow-Reich et al. (2021) use tabulations of tax returns by county to calculate county-level stock-market wealth measures and Agersnap and Zidar (Forthcoming) use tax data to calculate the number of wealthy taxpayers in a state. I recognize that in rare instances the address on the tax return is not the taxpayers residence.

[30]The adjustments in Adjusted Gross Income result in total income being approximately 1-2% higher than AGI.

share above \$500k as the top 1% income share.[31] In the county-level data, the highest AGI group contains information on tax returns with a total AGI above \$200k. To calculate the share of total CBSA income that is earned by taxpayers with an income above \$500k, I first assign counties to CBSAs and then assume that income above \$200k within a CBSA is Pareto distributed. Formally, I assume that conditional on having an income above \$200k, the probability of a taxpayer having an income Y above y in CBSA j is given by $Pr\left(Y > y\right) = \left(\frac{200}{y}\right)^{\alpha_j}$ where $\alpha_j = \frac{E[Y|Y>200]_j}{E[Y|Y>200]_j - 200}$ is the Pareto parameter. I calculate the expected income above \$200k in CBSA j, $E\left[Y|Y > 200\right]_j$, as the average AGI above \$200k in j, $\bar{y}_{a200,j}$, and calculate the Pareto parameter as $\alpha_j = \frac{\bar{y}_{a200,j}}{\bar{y}_{a200,j} - 200}$. The final input required for calculating the top 1% income share is the average income above \$500k. I obtain this quantity by exploiting the fact that, if income above \$200k in CBSA j is Pareto distributed with Pareto parameter α_j, then income above \$500k is Pareto distributed with the same parameter. Therefore, I calculate average income above \$500k as $\bar{y}_{a500,j} = 500 * \frac{\alpha_j}{\alpha_j - 1}$. The top 1% income share in CBSA j, S_j, is then calculated as

$$S_j = \frac{|\left\{i : i \in j \land y_i > 200\right\}| * Pr\left(Y > 500\right) * \bar{y}_{a500,j}}{\sum_{\{i:i\in j\}} y_i}$$

where $|\left\{i : i \in j \land y_i > 200\right\}|$ indicates the number of taxpayers in j with an income above \$200k.

Using more granular data from California, I can show that the Pareto assumption yields highly accurate predictions. While it is known that the top of the national income distribution is well-described by a Pareto distribution (e.g. Diamond and Saez, 2011), it is not ex-ante clear that the same is true at the local level. To assess the appropriateness of the Pareto assumption at the local level, I exploit data available for California. Unlike the IRS data, the highest AGI group in the data published by the California Franchise Tax Board contains information on taxpayers with an AGI above \$1m. I first use the procedure described in the previous paragraph to calculate the share of AGI above \$500k and \$1m and then compare these calculated shares to the actual shares. Figure 1.3 contains scatter plots of the predicted vs. observed income shares above \$500k and \$1m at the county and CBSA level. In each case, the predicted and the observed shares are highly correlated with an R-squared¿0.99.

[31] The IRS SOI public-use microdata file indicates a floor for the top 1 percentile of the AGI distribution in 2011 of ˜\$450k so that the AGI share above \$500k is closer to the top 0.9% income share. For simplicity and without much loss of accuracy, I refer to the AGI share above \$500k as the top 1% income share.

Primary Outcomes — Annual and Hourly Wages I use two complementary data sources to construct CBSA-level measures of annual and hourly wages.

I use the Quarterly Census of Employment and Wages (QCEW) to construct CBSA-level average annual private-sector labor income, which I refer to as annual wages. The data are county-level tabulations of employment and payroll covering more than 95% of U.S. jobs based on administrative reports filed by employers as part of state unemployment insurance programs. The QCEW dataset consists of annual observations for 924 CBSAs from 2003-2018. The QCEW data have both an advantage and a disadvantage. The advantage is that they provide an annual measure of wages at the CBSA level that represents almost the universe of private-sector employment. The disadvantage is that annual wages also react to labor supply changes and that the QCEW data also capture any labor income of top 1% earners.

I use the public-use microdata files from the American Community Survey (ACS) (Ruggles et al., 2021) to construct CBSA-level average annual and hourly wage measures for non-top 1% earners. Using the ACS data, I am able to identify the subset of 361 CBSAs that are Metropolitan Statistical Areas.[32] To increase precision, I pool the samples from 2007-2011 to create a pre-reform mean CBSA wage and 2014-2018 to create post-reform mean CBSA wage.[33] I focus on composition-controlled (CC) annual and hourly wages. The composition-controlled wages account for differential growth across CBSAs due to differential workforce composition and are estimated as the CBSA fixed effect in a Mincerian wage regression of log wages on dummies for education groups (6), sex (2), race (3), ethnicity (2) and a quartic in age. See Appendix A.2.1.2 for more details on the ACS data. The resulting ACS dataset consists of one pre- and one post-reform observation for 361 CBSAs. The advantages of the ACS data are the ability to calculate wage measures, explicitly exclude top 1% earners and composition control. The disadvantages are that the data is not available for all CBSAs and not available at an annual frequency.

Controls and Other Outcomes I draw on a number of different sources of county-level data to construct CBSA-level controls for the regression. I use data on personal income from the Bureau of Economic Analysis to construct measures of CBSA cyclicality, data from the QCEW to construct CBSA Bartik income and employment predictors and data on housing prices from the Federal Housing Finance Agency to construct the CBSA housing

[32]In the data, the most disaggregated geographic identifier is the Public Use Microdata Area (PUMA). A PUMA covers a population of at least 100k. Because a Micropolitan Statistical Area by definition covers a population of less than 100k, I restrict attention to Metropolitan Statistical Areas, resulting in a sample of 361 unique CBSAs. These CBSAs account for 90% of total CBSA population.

[33]Because the ACS is collected continuously throughout the year and the income data refers to the previous 12 months, the estimates for year t are centered around Dec. 15 of year $t-1$.

boom and bust. See Appendix A.2.1.1 and Section 1.3.3 for more details on the data sources and construction of control variables. I also use data from the QCEW on private sector employment and data from the National Cancer Institute's SEER program on CBSA population to examine the effects of a higher exposure to the 2013 tax increase on CBSA employment and population.

1.3.3 Econometric Methods

Following the discussion in Section 1.2, I consider a linear relationship between a CBSA's exposure to the top 1% tax increase, as measured by the top 1% income share, and the impact on the wages of workers (bottom 99% earners). Formally, the data-generating process for log CBSA wage growth relative to 2011 is written as

$$\Delta y_{i,h} = \beta_h S_i + \Gamma_h' X_{i,h} + \epsilon_{i,h} \tag{1.3.1}$$

for $h \in \{2012, \ldots, 2018\}$, where $\Delta y_{i,h} = \ln(y_{i,h}) - \ln(y_{i,2011})$, S_i is the top 1% income share in i in 2011, $X_{i,h}$ is a time-varying vector of controls, and $\epsilon_{i,h}$ is a structural error term that captures unmodeled determinants of $\Delta y_{i,h}$. In (2.3.1), $\beta_h S_i$ is the change in CBSA wages due to local spillovers of top earner responses to the tax increase and $\Gamma_h' X_{i,h} + \epsilon_{i,h}$ is the change due to other factors. I estimate (2.3.1) by OLS on a balanced panel of 924 CBSAs and cluster the standard errors at the CBSA level. For ACS wage measures, (2.3.1) is estimated on the single difference between the post-reform and the pre-reform CBSA wage for 361 CBSAs. Note that the time-differenced outcome measure absorbs a CBSA fixed effect.

The identification assumption necessary for consistent estimation of β_h in (2.3.1) is that, conditional on controls $X_{i,h}$, there would not have been systematic differences in wage growth between high and low exposure CBSAs absent the 2013 tax reform.[34] This assumption is the parallel trends assumption extended to the continuous difference-in-differences setting. Formally, we require $E\left[S_i \epsilon_{i,h} | X_{i,h}\right] = 0$ for $p\lim \hat{\beta}_h = \beta_h$. Note that it is not necessary that top 1% income shares are randomly distributed across labor markets for the assumption to hold. Indeed, the identification assumption does

[34] As discussed by Callaway et al. (2021), a second necessary assumption in the case of continuous difference-in-differences designs is that differences in treatment intensity are not correlated with heterogeneity in the treatment effect at a given treatment intensity. In this setting, the issue could arise in two ways. First, the national component of trickle-down effects could vary across local labor markets and be correlated with the exposure. The Bartik industry shocks and cyclicality controls should absorb such heterogeneity. Second, the workforce composition could vary across local labor markets, and the wage effect of a given exposure to the tax increase could vary across worker types. A heterogeneity analysis that focusses on specific subgroups of workers addresses this issue.

not require the top 1% income shares to be uncorrelated with the level of wages but instead requires them to be uncorrelated with omitted factors that impact the growth of wages (Goldsmith-Pinkham et al., 2020). To assess the plausibility of the parallel trends assumption in the post-reform period, I examine whether it held in the pre-reform period by estimating (2.3.1) for $h \in \{2003, \ldots, 2010\}$.

To bolster the credibility of the identification assumption, the main specification controls for a number of CBSA-level characteristics that are correlated with exposure and explain wage growth. Although the tax reform was plausibly exogenous from the perspective of local labor markets, it is nevertheless possible that CBSAs with different top income shares in general experience different wage growth. The main specification therefore controls for 20 quantiles of CBSA cyclicality, state fixed effects, a Bartik income predictor, as well as the severity of the housing boom and bust. Following Zidar (2019) and Guren et al. (2021), the cyclicality control adjusts for differential exposure to business cycles and is based on the estimated β_1 from a regression for each CBSA over the period 1999-2011 of $\Delta y_{i,t} = \beta_0 + \beta_1 \Delta y_t + e_{i,t}$ where $\Delta y_{i,t}$ is the log change in income-per-capita in i between t and $t-1$ and Δy_t is the national change. The Bartik income predictor at horizon h controls for differences in annual wage growth driven by differential industry composition and is constructed as the change in annual wages that would be expected given the industry composition of labor market i in 2011 and national wage changes within industry. Formally, $b^{inc}_{i,h} = \sum_{j \in NAICS3} \left(\frac{y_{i,j,2011}}{\sum_{j \in NAICS3} y_{i,j,2011}} \right) * \left(\frac{y_{j,h} - y_{j,2011}}{y_{j,2011}} \right)$ where y denotes annual wages, i indexes CBSAs and j indexes 3-digit NAICS industries. The state fixed effects account for differences in state policies and also absorb any state-specific shocks. To avoid dropping singleton cells, I control for Census division fixed effects in the ACS regressions. The housing boom and bust controls account for differential exposure to the housing bubble of the early 2000s, the impacts it had on local economic activity (Mian and Sufi, 2014) and the hysteresis experienced in labor markets that were more adversely affected by the Great Recession (Yagan, 2019 and Hershbein and Stuart, 2021). I weight CBSAs by their 2011 population (winsorized at the 1 percentile). I explore the robustness of the results to these modeling choices.

The estimates $\{\hat{\beta}_h\}_{h>2011}$ capture the wage effect of a higher exposure to the 2013 tax increase and provide a test of the null hypothesis of zero local trickle down. Under the identification assumption $E\left[S_i \epsilon_{i,h} | X_{i,h}\right] = 0$, $\hat{\beta}_h$ is a consistent estimator of β_h and, within the framework of Section 1.2, β_h corresponds to the trickle-down factor that determines how a given top 1% income share translates into a wage impact of local spillovers. Absence of a detectable relationship between CBSA exposure to the reform and CBSA wage growth (unable to reject $H_0 : \beta_h = 0$) therefore implies absence of detectable local trickle-down effects (unable to reject $H_0 : \mathcal{L} = 0$). To facilitate the interpretation of the estimates, I replace S_i in (2.3.1) with $\widetilde{S}_i = S_i/\sigma_{S_i}$, where σ_{S_i} is the population-weighted standard deviation across CBSAs of the top 1% income share in 2011 ($\sigma_{S_i} = 7.2\%$), so that a one unit increase corresponds to a one standard deviation increase in the top 1% income share across CBSAs.

1.4 Empirical Results

This section tests the hypothesis of zero local trickle-down effects, bounds the magnitude of effects statistically consistent with the estimates and concludes with a discussion of how the results compare to the literature and how they can be rationalized.

1.4.1 Zero Statistically Detectable Local Trickle-Down Effects

To allow for a simultaneous evaluation of overall annual wage growth as well as differences in wage growth between high and low exposure CBSAs, Figure 1.4 Panel (a) separately depicts the conditional mean annual wage (conditional on controls) relative to 2011 for CBSAs in the top tercile of the exposure distribution (blue line with square markers) and CBSAs in the bottom two terciles (green line with diamond markers). The difference in the mean top 1% income share between the two groups is 10.2pp. Panel (a) has two implications. First, in support of the identification assumption, wage growth was approximately parallel during the 8 years leading up to 2011. CBSA annual wages in both groups grew by about 21% between 2003 and 2011. Compared to the overall growth experienced over the time period, differences in year-on-year wage growth between the two groups are negligible. Indeed, in any given year, wage growth relative to 2011 is visually indistinguishable between high and low exposure CBSAs. The parallel growth of annual wages prior to 2011 gives credibility to the assumption that wages would have continued to evolve in parallel absent the reform.

Second and more importantly, the estimates indicate zero local trickle-down effects. Following a large tax increase for top 1% earners, annual wages in high and low exposure CBSAs continued to grow at visually indistinguishable rates. Between 2011 and 2018, annual wages in both groups grew by approximately 16%. The absence of a relative wage decrease in high exposure local labor markets suggests that higher income taxes on top earners do not reduce worker wages via geographically concentrated spillovers, an arguably important subset of all spillovers given the nature of U.S. top earners.

To exploit all of the information contained in the exposure variation across CBSAs, to focus on the differences in wage growth relative to 2011 and to depict confidence intervals, Panel (b) plots the estimates $\hat{\beta}_h$ from the main specification (2.3.1) where the standardized top 1% income share enters as a continuous regressor. The implications of the estimates are unchanged compared to Panel (a): the parallel trends assumption was satisfied in the pre-reform period and a higher exposure to the 2013 tax increase for the top 1% did not have statistically detectable adverse effects on CBSA annual wages. Table 1.1 reports the average coefficient over 3 time periods: 2012-2018 ($1/7 * \sum_{h=2012}^{2018} \hat{\beta}_h$), 2012-2014 and 2015-2018. The point estimate for the average wage effect over the post-reform period of a one standard deviation increase in the exposure to the tax increase for top earners is 0.06% with a 95% confidence interval of (-0.50%, 0.63%), equivalent to (-0.02, 0.03) standard deviations of log annual wages across all CBSAs in 2011.

Using composition-controlled annual and hourly wage measures that explicitly exclude any top 1% labor income results in qualitatively identical and quantitatively similar estimates. Column (1) of Table 1.2 reports the estimate using QCEW annual wages for the subset of CBSAs for which ACS data are available. The estimate is similar to although noisier than the estimate for the full sample. Columns (2) and (3) contain the estimates using composition-controlled annual and hourly wage, respectively. Both estimates are quantitatively similar to the annual wage estimate from the full sample. The estimate for the effect of a one standard deviation increase in the exposure to the tax increase for top earners on CC hourly wages in column (3) is 0.08% with a 95% confidence interval of (-0.39%, 0.55%), equivalent to (-0.04, 0.05) standard deviations of log CC hourly wages across CBSAs.

Before proceeding to quantify the magnitude of local trickle-down effects that can be statistically rejected, I address a number of potential concerns about the empirical approach, examine whether the zero average wage response masks heterogeneity across bottom 99% workers or across CBSAs, and whether the zero response carries over to employment.

1.4.1.1 Robustness

Robustness to perturbations of the main specification. Appendix A.2.2 describes several perturbations of inputs to the main specification for QCEW annual wages on the full sample of CBSAs. I consider different versions of the cyclicality (10 quantiles, continuous measure, cyclicality measured 1989-2011) and Bartik controls (based on 2-digit NAICS), adding controls for CBSA size, demographics or capital intensity (as measured by the capital-stock-to-GDP ratio), consider the unweighted regression, examine different geography fixed effects (division, region, nation), two different binary versions of the exposure measure, using the average top 1% income share between 2010-2011, measuring the share of income above $400k instead of $500k as well as using BEA wage & salary income instead of QCEW. The results are qualitatively unchanged and quantitatively similar across all perturbations (see Table 1.3).

Robustness to concerns about exposure measurement. Next, I address three different concerns regarding the crucial measurement of exposure to the top tax rate change (see Table 1.4). The first concern is that the top 1% total income share measures the human capital income share with noise because total income (AGI) includes income such as capital gains that possibly do not reflect the return to human capital.[35] I address this concern by using the top 1% active income share instead of the top 1% AGI share where active income is calculated as AGI less capital gains, interest, dividend, retirement and transfer income. To address concerns that pass-through business income potentially reflects returns to physical capital as well as labor income, that the location of pass-through business income is distinct from the location of pass-through business activity, or that the distribution of pass-through business income is opaque (Cooper et al., 2016), I use the top 1% wage & salary share instead of the total income share. To address the concern that top earners live and work in different CBSAs, I repeat the analysis at the CSA level which merges geographically proximate CBSAs. The estimates in each exercise continue to indicate zero detectable local trickle-down effects and do not support the claim that the zero is due to measurement error in the exposure variable. See Appendix A.2.3 for details.

Evidence on the validity of the exposure measure. To further assuage concerns about the validity of the top 1% income share as an exposure measure, I test whether CBSAs with a higher exposure in 2011 experienced differential changes in their average tax rate, capital gains realizations and charitable contributions post 2011 that would be expected given the nature of the reform and evidence on the responses of top earners documented by Auten et al. (2016) and Saez (2017). Figure 1.5 presents the results. I find that there were no differential changes in the average tax rate in high-exposure CBSAs in 2010 or 2012 but that, consistent with the 2013 tax reform increasing the taxes of the top 1%, high-exposure CBSAs experienced larger average tax rate increases in 2013. I find a spike in capital gains realizations in high-exposure CBSAs in 2012, consistent with time-series evidence in Saez (2017) on accelerated realizations of the top 1% to avoid the higher capital gains tax rate starting in 2013, and a reduction in realizations starting in 2013, consistent with evidence on capital gains responses to taxes from Agersnap and Zidar (Forthcoming). I

[35] Ex ante it is unclear to what extent realized capital gains reflect returns to capital as opposed to shifted returns to human capital, particularly at the top of the distribution (Scheuer and Slemrod, 2020).

find an increase in charitable contributions in high-exposure CBSAs in 2012, consistent with evidence provided by Saez (2017) that top 1% earners increased their charitable contributions in 2012 along with their capital gains realizations, and a further increase post-2012, consistent with evidence of charitable contribution responses to changes in the net-of-tax price (Bakija and Heim, 2011 and Duquette, 2016). See Appendix A.2.5 for details.

These tests support the notion that CBSAs classified as high exposure by my research design were more affected by the 2013 tax increase than low exposure CBSAs. They also illustrate that my approach of comparing the evolution of outcomes across high- and low-exposure CBSAs is capable of detecting top earner responses to the reform.

1.4.1.2 Heterogeneity and Employment Effects

No detectable heterogeneity across worker types. The bottom 99% of the income distribution covers a variety of worker types, raising the question of whether zero mean local trickle-down effects mask heterogeneity across workers types. I assess this by using ACS data and testing for heterogenous effects across high- and low-skilled workers. I classify workers as high-skill if they have a college degree or more and calculate CBSA-level mean wages for both worker types during the pre-reform and post-reform period. I consider both the mean hourly wages and composition-controlled wages.[36] Table 1.5 presents the estimates using the same specification as for the CC mean wage. The estimates imply zero detectable local trickle-down effects for both high- and low-skill workers. The estimated effect of a one standard deviation increase in the exposure to the reform on the CC wage of low skill workers is -0.22% with a 95% confidence interval of (-0.74%, 0.28%), equivalent to (-0.07, 0.03) standard deviations of log CC low-skill wages across CBSAs. The estimated effect of a one standard deviation increase in the exposure to the reform on the CC wage of high skill workers is 0.05% with a 95% confidence interval of (-0.50%, 0.61%), equivalent to (-0.04, 0.05) standard deviations of log CC high-skill wages across CBSAs. The p-value on a test of equality is 0.29. Similar results emerge for non-CC wages.[37]

No detectable heterogeneity across types of CBSAs. Another dimension of heterogeneity is across CBSAs with different industrial composition. If the elasticity

[36] The composition-controlled wages are estimated separately by skill type as the CBSA fixed effects in a Mincerian wage regression. Composition controlling in this setting accounts for the fact that there within the broad skill groups there can still be composition differences across CBSAs that induce differential wage growth.

[37] Appendix A.2.8 presents an additional heterogeneity test that considers different groups of workers based on their location in the within CBSA income distribution. The estimates do not indicate the presence of detectable heterogenous impacts.

of substitution between top earners and workers varies across industries, then differences in industry composition across CBSAs can give rise to heterogeneity in the impact of a higher exposure to the reform on wages. I make three attempts at capturing industry composition. First, I separate CBSAs by population size, as in Autor (2019). Second, I separate CBSAs based on capital intensity as measured by the capital-stock-to-GDP ratio.[38] Third, I separate CBSAs by cyclicality. In each case, I distinguish between above median and below median CBSAs and estimate a version the main specification for QCEW annual wages on the full sample of CBSAs that allows the effect of exposure to vary between the two groups. The estimates are presented in Table 1.6. None of the tests reveals heterogeneity across CBSAs in the impact of a higher exposure to the reform on annual wages.

No detectable adverse impacts on employment. In the presence of wage rigidity or unemployment, additional information is contained in the effect of a higher exposure to the top tax increase on employment. Figure 1.6 depicts the estimates from the main section using the log employment-to-population ratio as an outcome and using a Bartik employment as opposed to income predictor. Table 1.7 presents the average post-reform estimates. Employment for all 924 CBSAs is measured as total private sector annual jobs, and population as the working-age population. In support of the identification assumption, the employment-to-population ratio evolved in parallel in high- and low-exposure CBSAs during the 8 years prior to 2011. Following a large tax increase for top earners, the employment-to-population ratio continued to evolve in parallel between high and low exposure local labor markets. The estimated average effect over the post-reform period of a one standard deviation increase in the exposure to the reform on the employment-to-population ratio is -0.06% with a 95% confidence interval of (-0.47%, 0.35%), equivalent to (-0.03, 0.02) standard deviations of the log employment-to-population ratio across CBSAs in 2011.

1.4.2 Bounding Local Trickle-Down Effects

Having documented a robust zero local trickle-down effect, I now bound the magnitude of effects that are statistically consistent with the estimates. The bounds facilitate a discussion of how strong the evidence is against alternative values of local trickle-down effects (Romer, 2020).

To compute the local trickle-down effect, $\widehat{\mathcal{L}}$, implied by an estimated wage impact of

[38] The CBSA capital stock is imputed based on the industry composition of CBSAs and the national capital stock per industry; see Appendix A.2.1.1 for details

a higher exposure to the top tax increase, $\hat{\beta}_h$, I use the functional form for wage changes due geographically concentrated spillovers in a given local labor market k, ϕ_k, presented in Section 1.2. Interpreting $\hat{\beta}_h$ as an estimate of the trickle-down factor in (1.2.3), I estimate the wage change due to local spillovers in a given local labor market k as the product of the top income share in k and the estimated trickle-down factor, $\hat{\phi}_k = S_t^k * \hat{\beta}_h$. Then I estimate the local trickle-down effect $\widehat{\mathcal{L}}$ as the worker weighted average wage change across all labor markets

$$\widehat{\mathcal{L}} = \sum_k \omega_k * \hat{\phi}_k = \sum_k \omega_k * \left(S_t^k * \hat{\beta}_h \right) \tag{1.4.1}$$

I bound the magnitude of local trickle-down effects $\mathcal{L}$ by determining the most pronounced wage decrease $l < 0$ such that in a hypothesis test of $H_0 : \mathcal{L} \leq l$ vs. $H_1 : \mathcal{L} > l$, I fail to reject H_0. Local trickle-down effects more severe than l are therefore unlikely given the estimates of this paper. The approach to estimating local trickle-down effects described by (1.4.1) implies that a hypothesis test for the magnitude of local trickle-down effects turns into a rescaled hypothesis test of the trickle-down factor: $H_0 : \beta_h \leq \frac{l}{\sum_k \omega_k S_t^k}$ vs. $H_1 : \beta_h > \frac{l}{\sum_k \omega_k S_t^k}$. I consider the confidence levels 90%, 92.5%, 95%, 97.5% and 99%. I present the bounds implied by two estimates using composition-controlled hourly wages for the ACS sample and two estimates using annual wages for the full sample. For the hourly wages I present the main specification and one that additionally controls for CBSA density. For the annual wages I present the average post-reform coefficient from the main specification and an additional specification that controls for demographics (to mirror the demographic controls in the composition-controlled wage) and density. I focus the discussion around the bound implied by the 95% threshold for hourly wages. Table 1.8 Panel A presents the point estimates and bounds for $\hat{\beta}_h$.

The estimates of this paper suggest it is unlikely that the 2013 tax increase for top 1% earners reduced the hourly wages of workers via local spillovers by more than 0.54-0.66% on average. Table 1.8 Panel B depicts the local trickle-down effects implied by the point estimates as well as the bounds for each of the four specifications at various levels of confidence with column (4) containing the bounds at 95% confidence. The average point estimates imply an insignificant local trickle-down effect on hourly wages of 0.20% and the average bound at 95% confidence is -0.6%. At 97.5% (92.5%) confidence, the mean bound for hourly wages is -0.75% (-0.50%).

To evaluate tax reforms targeting top earners that differ in magnitude from the 2013 reform, I scale the estimated local trickle-down effect, which is measured in logs, by the log change in the net-of-tax rate to arrive at an elasticity of worker wages with respect to the top net-of-tax rate. I focus on the income-weighted average marginal tax rate which increased from 32.2% to 39.7% resulting in a -0.117 decrease in the log net-of-tax rate.

The estimates of this paper suggest it is unlikely that a one percent decrease in the top net-of-tax rate reduces worker wages via local spillovers by more than 0.046-0.056% on average. Table 1.8 Panel C depicts the point estimates and bounds expressed in terms of elasticities at various levels of confidence. The average point estimate implies an insignificant elasticity of -0.017 and the average bound at 95% confidence is 0.051. At 97.5% (92.5%) confidence, the mean bound for hourly wages is 0.064 (0.040).

The headline bound of this paper is an elasticity of worker wages with respect to the top net-of-tax rate of 0.051, corresponding to the average bound at the 95% confidence level.

1.4.3 Discussion

Comparison to literature. The empirical literature that examines the aggregate economic impacts and spillover effects of top income taxation has used a variety of approaches and obtained a variety of results. One set of studies finds negative impacts of top income taxation. Using a cross-country panel approach, Gemmell et al. (2014) document an adverse impact of top personal income tax rates on GDP growth rates. Using a time-series approach, Mertens and Montiel Olea (2018) estimate an elasticity of bottom 99% wages with respect to the top net-of-tax rate of 0.44 in the year after the tax increase, almost 10 times the 0.051 bound. Using a firm-level approach, Risch (2021) finds that top income owner-managers pass 10-20 cents of each additional dollar in tax liability on to their employees. The estimates imply an elasticity of worker wages with respect to the top net-of-tax rate of approximately 0.02, which is within the 0.051 bound.[39] The estimates of this paper provide very strong evidence against an elasticity of 0.44, but only weak-moderate evidence against an elasticity of 0.02.

Another set of studies finds zero detectable impact of top income taxation. Using cross-country panel approaches, Lee and Gordon (2005), Piketty et al. (2014) and Hope and Limberg (2020) find a zero detectable impact of top income taxation on aggregate economic outcomes such as GDP and employment. Using a state-level exposure approach to study the impact of tax-induced income changes for top 10% earners, Zidar (2019) finds zero detectable impact on state-level economic activity. None of these studies focusses explicitly on the wages of non-top earners.

[39] Normalize total income to 1. Given a top 1% income share 20%, top 1% income is 0.2. A 5pp increase in the average tax rate (Figure A.1) results in a 0.01 increase in the tax liability of top 1% earners. If 0.2$ of each additional dollar is passed on to bottom 99% employees, the total decrease in bottom 99% annual wages is 0.002. Total bottom 99% income is 0.8, so that a 0.002 decrease results in a 0.25% decrease. Scaling by the size of the reform yields a bound on the elasticity of 0.02.

Rationalizing the absence of detectable trickle-down effects. The extent to which higher top income taxes trickle down and reduce worker wages depends both on the magnitude of top earner's real (effective labor supply) responses to the tax increase and the extent to which real responses impact worker wages. The estimates of this paper provide evidence against the joint hypothesis of a large real response combined with a large impact of responses on wages. However, they provide only weak or moderate evidence against alternative combinations of the two parameters. To make the argument more precise, I draw on the skill complementarity framework of Section 1.2. Within that framework, a given magnitude of the real response, governed by the elasticity of labor supply e, combined with a given impact of responses on wages, governed by the elasticity of substitution between top earners and workers σ, generates an elasticity of worker wages with respect to the top net-of-tax rate, denoted$\dot{w}_b$. A large elasticity of labor supply of $e = 0.84$, roughly the 90th percentile of the estimates reviewed by Keane (2011), combined with a low elasticity of substitution of $\sigma = 0.6$ (Dustmann et al., 2013) yields $\dot{w}_b = 0.09$, which is well outside of the 0.051 bound. However, a more moderate elasticity of labor supply of $e = 0.3$ (Tortarolo et al., 2020) combined with a moderate elasticity of substitution of $\sigma = 1.4$ (Katz and Murphy, 1992) yields $\dot{w}_b = 0.03$, which is well within the 0.05 bound.[40] Note that even a very large real response can be consistent with the estimates if the impact on workers is low. If the elasticity of labor supply is $e = 2.5$, as estimated by Rauh and Shyu (2019) and the elasticity of substitution is greater than 4.8, the implied worker wage elasticity is within the 0.051 bound. More generally, the 0.051 bound on the elasticity of worker wages wages with respect to the top net-of-tax rate implies that the ratio of a) the elasticity of substitution between top earners and workers and b) the elasticity of labor supply must be greater than 1.9.[41]

There is some suggestive evidence that the absence of detectable trickle-down effects is due to a small real response. The 2013 reform reduced the marginal return to effort for top earners while leaving it largely unchanged for others. If top earners did reduce their effort, larger changes in total income should be observe in local labor markets with a higher top income share, where more income is responding.[42] To test this, I use active income: AGI

[40]These elasticities of substitution are not estimated specifically for top earners. I use them as reference values because estimates specifically for top earners are, to the best of my knowledge, not available.

[41]To see this, note that interpreting the reduced-form estimate as an estimate of the trickle-down factor in Proposition 1 implies $\hat{\beta} = \frac{e}{\sigma+e} * \widehat{\Delta \ln(1-\tau_t)}$, so that $\widehat{\frac{\sigma}{e}} = \frac{1-\hat{\beta}/\Delta\ln(1-\tau_t)}{\hat{\beta}/\Delta\ln(1-\tau_t)}$. Using the mean bound on the reduced-form hourly wage estimate from Table 1.8 Panel A column (4) and setting $\Delta \ln(1-\tau_t) = -0.117$ implies that σ/e must be greater than 1.9.

[42]In the complementarity framework, the change in total income in a given local labor market, ΔY^k, can be formally written as $\Delta Y^k = S_t^k e$. This formally illustrates that an income measure which includes all top earner income should change across local labor markets in proportion to the top 1% income share.

less capital and transfer income. Active income includes pass-through business income, an important source of income for top earners. Figure 1.7 depicts the estimates from specification (2.3.1) using the log active income per worker as the outcome variable. The figure does not reveal that active income decreased in high exposure CBSAs. The estimated effect of a one standard deviation increase in the top 1% income share on average active income is 0.40% with a 95% confidence interval of (-0.40%, 1.17%).

Note that the evidence presented in the previous paragraph does not imply that top earners did not respond to tax increase at all. In fact, the results discussed in Section 1.4.1.1 suggests that top earners did respond to the tax increase by adjusting their capital gains realizations as well as their charitable contributions. Rather, the evidence suggests that real effort responses of top earners, the type of responses one might expect to systematically impact other workers, were limited.

Limitations. Although local trickle-down effects are arguably an important component of total trickle-down effects, especially in the short/medium run, they are not the only means via which higher income taxes on top earners can reduce worker wages. When wages equal marginal products, $w_b = A * \frac{\partial F(L_b, L_t)}{\partial L_b}$, trickle-down effects can arise via factor complementarity, $\frac{\partial F(L_b, L_t)}{\partial L_b \partial L_t} > 0$, and total factor productivity, $\frac{\partial A}{\partial L_t} > 0$.[43] Geographically concentrated spillovers from top earner responses to a tax increase are likely to capture spillovers due to factor complementarity, but less likely to capture spillovers due to total factor productivity, as these potentially affect the wages of workers everywhere.

1.5 Welfare Implications

This section examines the welfare implications of the estimated bounds on local trickle-down effects. To minimize extrapolations of the estimates, I focus on evaluating how trickle-down effects shape the social welfare benefits of a marginal increase in the top-bracket tax rate starting from the 2012 tax system which, as discussed in Section 1.3.1, corresponds closely to increasing the marginal tax rate of top 1% earners.[44,45]

[43] Changes in the consumption and savings/investment behavior of top earners in response to a lower post-tax income also potentially affect other workers. However, at the national level, these changes in the behavior of top earners need to be weighed against the changes in government spending and saving/investment due to higher tax revenues.

[44] In Appendix A.3.3, I combine my estimates with the optimal tax framework of Sachs et al. (2020) to calculate bounds on the adjustments to optimal top tax rates in a fully optimal non-linear tax schedule.

[45] In Appendix A.3.4, I use the 95% confidence bound on trickle-down effects to calculate incidence shares - what part of the distribution bears what share of the burden of a tax increase for top 1% earners.

Framework Societal welfare depends on agents' utility and tax revenue and is described by the dollar-valued welfare function

$$\mathcal{W} = \frac{1}{\lambda} \int G\left(U\left(y\right)\right) \mathrm{d}F\left(y\right) + T$$

where $G\left(U\right)$ is a concave transformation of individual utilities that captures the society's preference for redistribution, $T = \int T\left(y\right) \mathrm{d}F\left(y\right)$ is tax revenue and λ is the marginal value of public funds. Denote the marginal social welfare weight of an agent with income y — the welfare benefit of a marginal income increase for an agent with income y relative to the marginal value of public funds — by $g\left(y\right) = \frac{G'(U(y))U'(y)}{\lambda}$.

The welfare effect of a marginal increase in the top bracket tax rate τ_t above an income $\bar{y}$ has five components. The first component captures the mechanical revenue gain due to a higher tax rate when holding incomes constant. Formally,

$$\text{Mechanical revenue gain} \equiv \int_{\bar{y}}^{\infty} \left(y - \bar{y}\right) \mathrm{d}F\left(y\right)$$

where $\left(y - \bar{y}\right)$ is the increase in tax revenue at income y due to a marginal increase in τ_t.

The second and third components are the offsets to the mechanical revenue gain in standard optimal tax analysis. The second component captures the revenue loss due to income adjustments in response to higher taxes and the third captures the utility loss among top earners due to a decrease in post-tax income.[46]

The fourth and fifth component are the offsets to the mechanical revenue gain due to trickle-down effects. Recall that trickle-down effects amount to a regressive pre-tax redistribution of income. When top earners reduce their effective labor supply, their own marginal product increases and, because of complementarity, the marginal product of workers decreases.

The fourth component captures the *revenue impact* of trickle-down effects. Lower incomes among workers reduce tax revenue below the top bracket and higher incomes among top earners increase tax revenue within the top bracket. As pointed out by Sachs et al. (2020), if income within the top bracket is taxed at a higher rate than below,

[46]In the absence of trickle-down effects, the welfare benefit is determined by the sum the mechanical revenue gain, the revenue loss due to income adjustments and the utility loss among top earners. Setting the sum equal to zero and solving for τ_t yields the optimal top tax rate formula of Diamond and Saez (2011).

trickle-down effects can lower the revenue cost of higher top tax rates. Formally,

$$\text{Revenue impact} \equiv \underbrace{\int_0^{\bar{y}} T'(y)\, y \frac{-\dot{y}_b}{1-\tau_t} \mathrm{d}F(y)}_{\text{Revenue loss from workers}} + \underbrace{\int_{\bar{y}}^{\infty} \tau_t y \frac{-\dot{y}_t}{1-\tau_t} \mathrm{d}F(y)}_{\text{Revenue gain from top earners}}$$

where $-\dot{y}_b$ $(-\dot{y}_t)$ is the percentage decrease (increase) in income for workers (top earners) due to trickle-down effects from a percentage decrease in the top bracket net-of-tax rate, $y\frac{-\dot{y}_b}{1-\tau_t}$ is the income decrease for a worker with income y due to trickle-down effects which, multiplied by the marginal tax rate $T'(y)$, yields the revenue loss.[47]

The fifth component captures the *utility impact* of trickle-down effects. Lower wages due to trickle-down effects reduce the utility of workers whereas higher wages increase the utility of top earners. The sign of the utility impact depends on the society's preferences for redistribution. Formally,

$$\text{Utility impact} \equiv \underbrace{\int_0^{\bar{y}} g(y)\, y \left(1 - T'(y)\right) \frac{-\dot{w}_b}{1-\tau_t} \mathrm{d}F(y)}_{\text{Utility loss from workers}} + \underbrace{\int_{\bar{y}}^{\infty} g(y)\, y \left(1-\tau_t\right) \frac{-\dot{w}_t}{1-\tau_t} \mathrm{d}F(y)}_{\text{Utility gain from top earners}}$$

where $-\dot{w}_b$ $(-\dot{w}_t)$ is the percentage decrease (increase) in wages for workers (top earners) due to trickle-down effects from a percentage decrease in the top bracket net-of-tax rate, $y\left(1 - T'(y)\right)\frac{-\dot{w}_b}{1-\tau_t}$ is the decrease in post-tax income at income y due to lower wages from trickle-down effects which, multiplied by the social marginal welfare weight $g(y)$, yields the dollar-valued welfare impact of the utility loss.

The welfare impact of trickle-down effects on a marginal increase in the top tax rate is given by the sum of the revenue and utility impact. Normalizing the mechanical revenue gain to 1 and expressing each offset as a percentage of the mechanical revenue gain yields:

$$\text{Welfare impact of trickle down} = \widetilde{\text{Revenue impact}} + \widetilde{\text{Utility impact}} \tag{1.5.1}$$

where $\widetilde{x}$ indicates that x is expressed as a percentage of the mechanical revenue gain.

Calibration The estimated bounds on local trickle-down effects combined with implications of the theoretical framework in Section 1.2 and microdata on the U.S.

[47] Note that this expression assumes that both below and within the top bracket the percentage change in income due to trickle-down effects is constant. Empirical support comes from the absence of detectable heterogeneity in local trickle-down effects between different worker types documented in Section 1.4.1.2. Nonetheless, in Appendix A.3.2, I consider the implications of non-uniform trickle-down effects among bottom 99% earners.

distribution of income allow me to calibrate (1.5.1). This exercise facilitates interpreting the magnitude of the bounds and, because I consider an increase in the top rate starting from the 2012 tax system, yields a bound on the impact of local trickle-down effects on the welfare benefits of the 2013 tax reform.

I take the following steps to calibrate (1.5.1). I use the bounds at the 95% confidence level on the elasticity of hourly and annual worker wages with respect to the top net-of-tax rate to calibrate $\dot{w}_b$ and $\dot{y}_b$ ($\dot{w}_b = 0.051$ and $\dot{y}_b = 0.067$). I exploit the relationship between the wage decrease for workers and the wage increase for top earners in a given local labor market implied by the skill complementarity framework of Section 1.2 to calibrate $\dot{w}_t$ and $\dot{y}_t$ ($\dot{w}_t = -0.28$ and $\dot{y}_t = -0.37$). I assume that utility functions are quasi-linear and isoelastic, that $G(U) = \frac{U^{1-\kappa}}{1-\kappa}$ with $\kappa = 1$ and that the marginal value of public funds is such that the planner is indifferent about raising an additional dollar of tax revenue, $\lambda = \int G'(U(y))\,\mathrm{d}F(y)$.[48] This setting corresponds to one in which agents' utility functions are log-transformed and the social planner is utilitarian, so that marginal social welfare weights are inversely proportional to disposable income. I use the IRS SOI public use file for data on the distribution of income and to generate social marginal welfare weights. See Appendix A.3.1 for details.

Results The point estimates of this paper imply a zero welfare impact, and the bounds suggest that it is unlikely that local trickle-down effects offset the welfare benefit of a higher marginal tax rate for U.S. top 1% earners by more than 14% of the mechanical revenue gain. Figure 1.8 illustrates the calibrated version of (1.5.1). The green bar on the left indicates that the revenue impact of redistributing income from workers to top earners is unlikely to exceed 6% of the mechanical revenue gain. The red bar in the center indicates that the utility impact of local trickle-down effects is unlikely to exceed 20% of the mechanical revenue gain. The combined 14% offset can alternatively be interpreted as a bound on the share of the mechanical revenue gain that would be required to finance a lump-sum transfer to all agents that neutralizes the impact of local trickle-down effects.

The result that local trickle-down effects are unlikely to have more than a modest impact on the social welfare benefit of higher top tax rates is robust to alternative preferences for redistribution. Figure 1.9 Panel (a) reveals that the bounds on the welfare impact are a U-shaped function of the concavity of the social welfare function, as measured by κ, with

[48] Given the absence of income effects, the welfare impact of redistributing a dollar via lump-sum taxes from the agents to the government is given by $-\frac{1}{\lambda}\int G'(U(y))\,\mathrm{d}F(y) + 1$. Setting the expression to zero and solving for λ implies $\lambda = \int G'(U(y))\,\mathrm{d}F(y)$. Note that if the marginal value of public funds is higher than the value of redistributing a dollar among all agents, perhaps because government revenue is used to finance public goods such as national defense, the welfare impact of utility changes will be less pronounced.

a maximum offset of 17%. A value of $\kappa = 0$ implies constant welfare weights across all agents, $g(y) = g$, and as $\kappa \to \infty$ welfare weights approach Rawlsian weights where only the lowest income worker has a positive weight. Because the revenue impact is unaffected by κ, the U-shape must come from the utility impact. Panel (b) graphs both the total utility impact (solid blue line) as well as its two components, the utility loss for workers (dashed red line) and the utility increase for top earners (dash-dotted green line). The intuition for the U-shape is as follows. If all agents have equal welfare weight, the utility impact of trickle-down effects is small because workers' utility loss is valued as much as top earners' utility gain. If only the lowest income worker has positive weight, the utility impact is small because the lowest income worker has an income close to zero. As κ increases starting from $\kappa = 0$, the welfare impact of the utility gain of top earners falls more rapidly than the utility loss for workers so that the utility impact of trickle-down effects becomes negative.

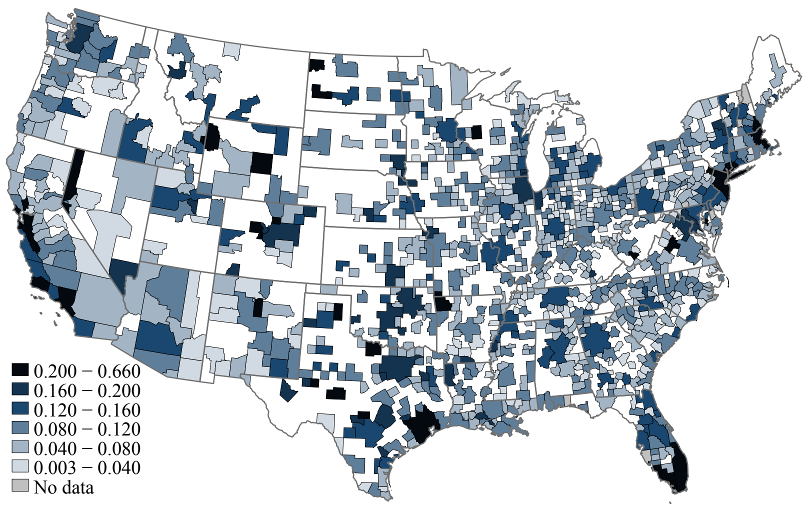
1.6 Conclusion

This paper developed a new approach to measuring how much higher income taxes on U.S. top 1% earners trickle down and reduce worker wages via geographically concentrated spillovers, an arguably important share of total trickle-down effects given the nature of U.S. top earners. Combining time-series variation in the federal marginal income tax rate for top 1% earners with cross-sectional variation in the income share of top 1% earners across local labor markets, the paper found very little evidence of local trickle-down effects.

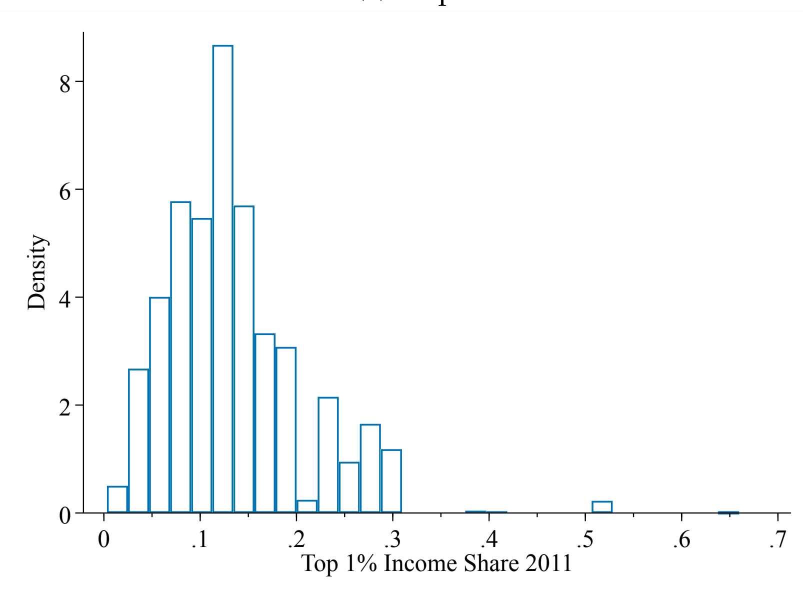
The results of this paper provide evidence against the claim that higher marginal tax rates for top 1% earners are undesirable because they make everyone worse off or, assuming symmetric effects of cuts and hikes, that lower taxes on top earners are an effective way of increasing other workers' wages. However, caution must be applied when extrapolating the results to considerably higher top marginal tax rates than those observed during the period of this study. Future work could apply the local labor markets to other major tax reforms for top earners that differed in sign and size from the reforms studied in this paper.

The approach of this paper to measuring general equilibrium effects of top income taxes can readily be adapted to measure the general equilibrium effects of income taxes for other income groups. For example, to measure the general equilibrium effects of the Earned Income Tax Credit (EITC), a researcher might combine a federal expansion of the EITC with variation in the income share of taxpayers eligible for the EITC across local labor markets.

Figures



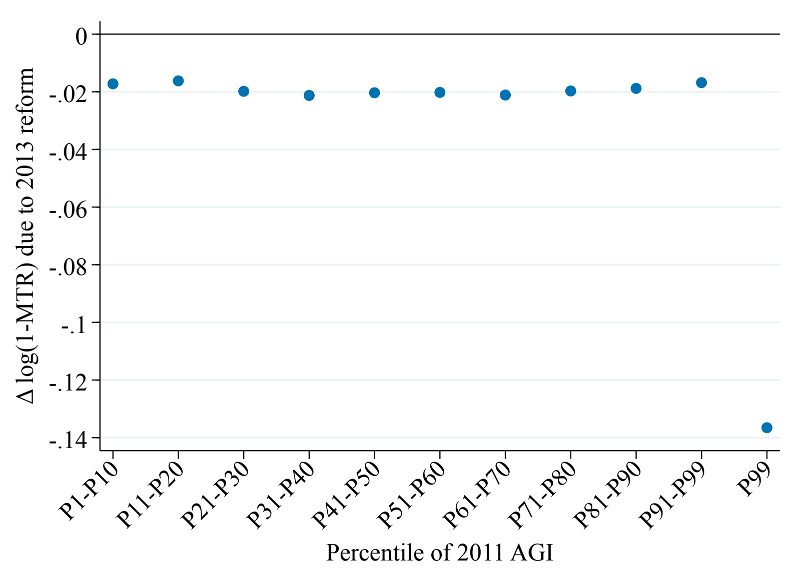
(a) Map



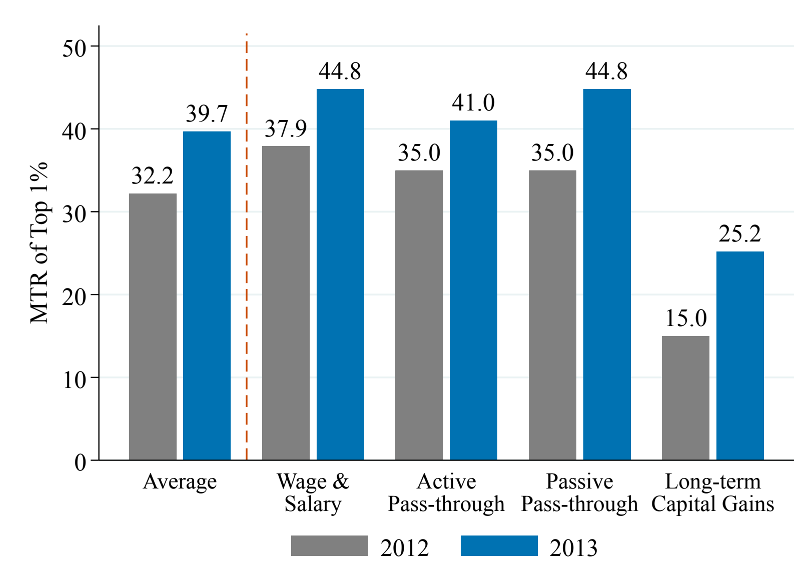
(b) Histogram

Figure 1.1: Variation in top 1% income shares across U.S. local labor markets

Note: This figure illustrates the considerable variation across U.S. local labor markets in the exposure to the 2013 top 1% tax increase (top 1% income share in 2011). Panel (a) depicts variation in top 1% income shares on a map of the continental U.S. and Panel (b) presents a population-weighted histogram. The concept of a local labor market is the core-based statistical area (CBSA). The top 1% income share in a CBSA in 2011 is the share of all CBSA 2011 income that is earned by CBSA residents with an income in the top 1 percentile of the 2011 national income distribution. For details on the data sources and calculation of the top 1% income share see Section 1.2.2.2 and 1.3.2. Note that the white areas in Panel (a) are rural regions that are not part of a CBSA. Panel (a) and (b) reveal that there is considerable variation in top 1% incomes shares across U.S. local labor markets. While some CBSAs have top income shares of less than 4%, others have top income shares of more than 20%. The population-weighted mean is 13.5% with a standard deviation of 7.2%.



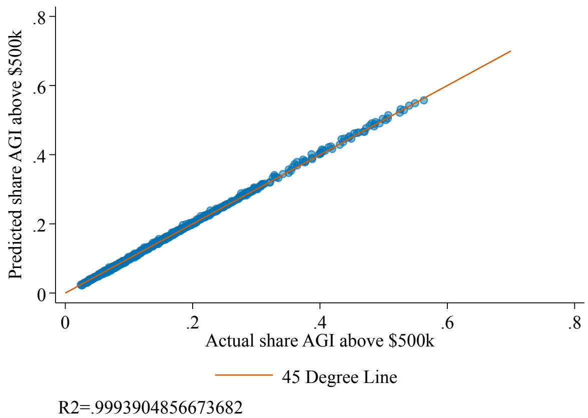
(a) MTR change by AGI size



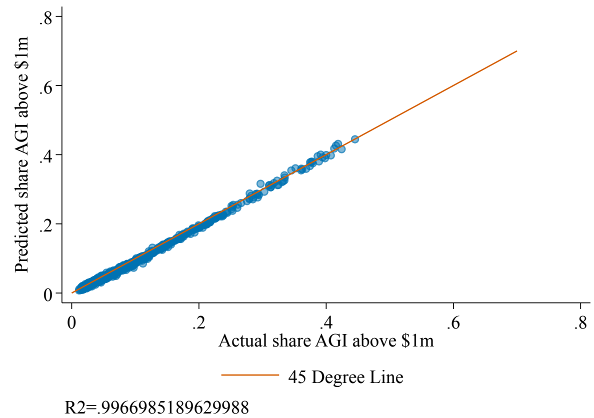
(b) MTR change by income type: Top 1%

Figure 1.2: Changes to income tax due to 2013 reforms

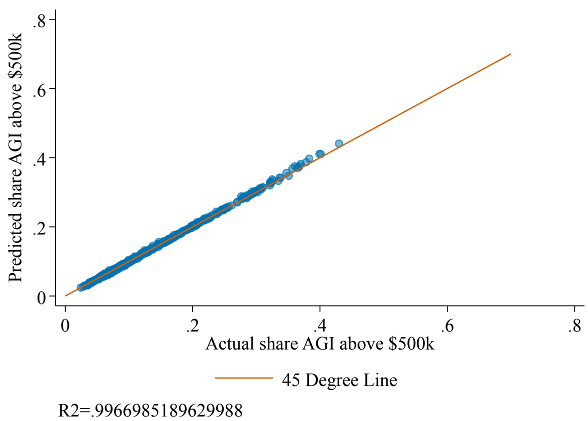
Note: This figure illustrates the large effects of the 2013 tax reforms on the marginal tax rates of top 1% earners. Panel (a) depcits the change in the log net-of-tax rate across the income distribution and Panel (b) depcits the effect on various income sources among top 1% earners. Panel (a) is created by using the 2011 Interal Revenue Service Statistics of Income public use microdata file for 2011 and applying NBER TAXSIM once for 2012 and once for 2013. In each case, 2011 data are inflated accordingly. Marginal tax rates are calculated with respect to primary earnings. The decrease in the net-of-tax rate below the top 1 percentile in Panel (a) is due to the expiration of a temporary Social Security tax cut that was introduced as one-year measure in 2011 and extended twice during 2012 before expiring in 2013. The decrease for the top 1 percentile is due to the American Taxpayer Relief Act and the Affordable Care Act surtax. Note that the decrease for the top 1 percentile in Panel (a) is slightly larger than would be expected given the changes Panel (b) due to taxpayers no longer being subject to the alternative minimum tax in 2013 as well as phaseouts or special provisions in the tax code. The figure reveals that the 2013 reforms induced a large decrease in the net-of-tax rate for top 1% with limited changes further down the income distribution. See Section 1.3.1 for details.



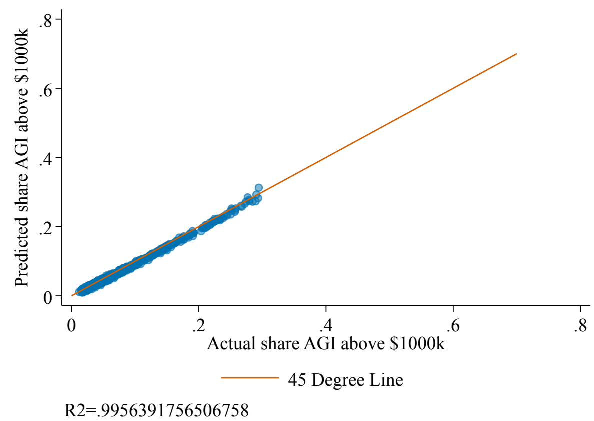
(a) Above $500k - county



(b) Above $1m - county



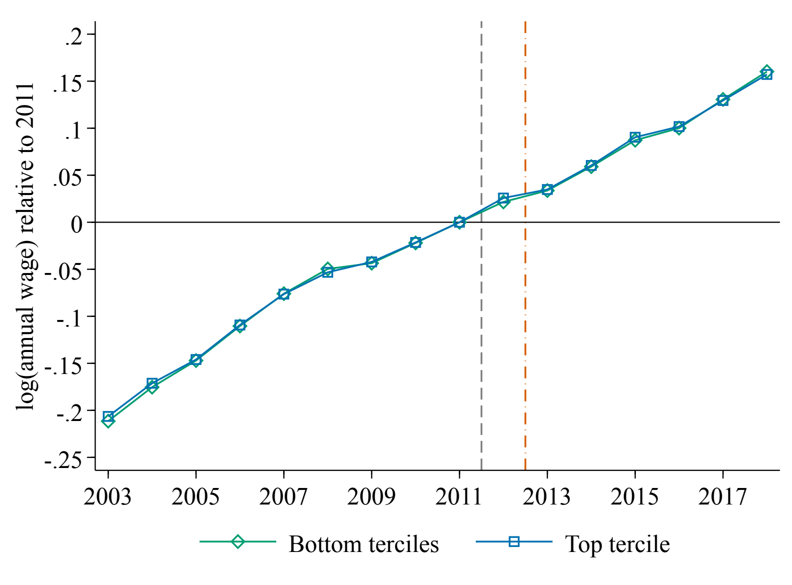
(c) Above $500k - CBSA



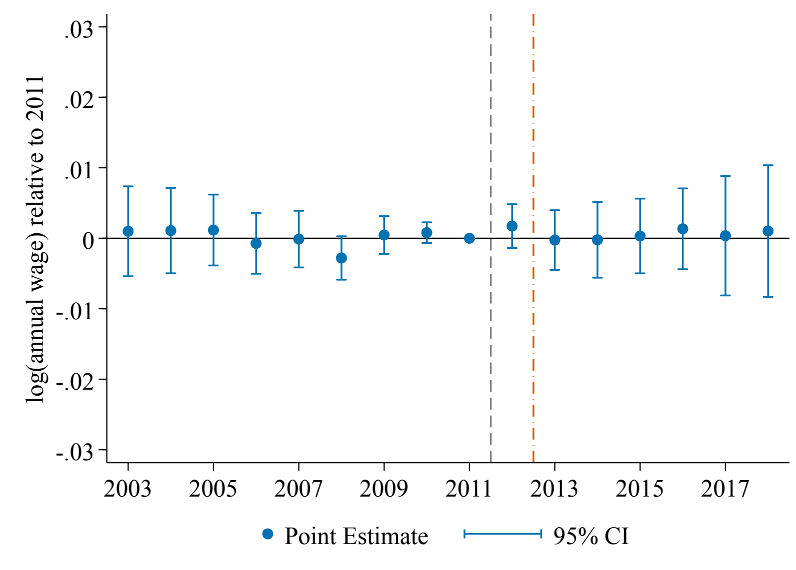
(d) Above $1m - CBSA

Figure 1.3: Pareto predicted vs. observed income shares at local level in California

Note: This figure validates the Pareto assumption used to calculate top 1% income shares. The figure illustrates the relationship between the predicted income shares above $500k and $1m using the Pareto assumption and the observed income shares. The figure uses data from Table B7 from the California Franchise Tax Board. The share of income above a given threshold is the share of total county/CBSA income earned by taxpayers whose income is above the threshold. The predicted share is calculated by assuming that income above $200k within a county/CBSA is Pareto distributed. The figure reveals that using the assumption that within local labor markets income above $200k is Pareto distributed generates highly accurate predictions of the share of income above $500k and $1m with an R2¿0.99 in each case. The figure therefore validates the procedure used to calculate top 1% income shares. See Section 1.3.2 for details.



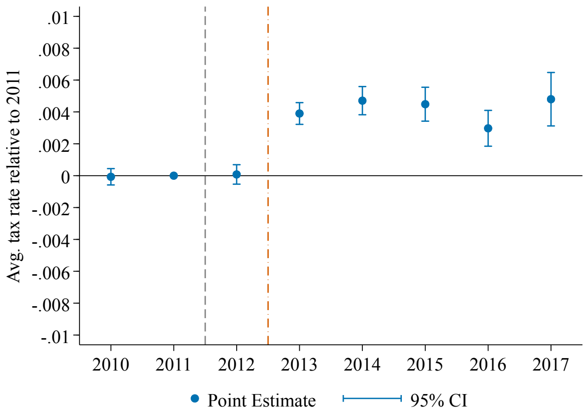
(a) Conditional means



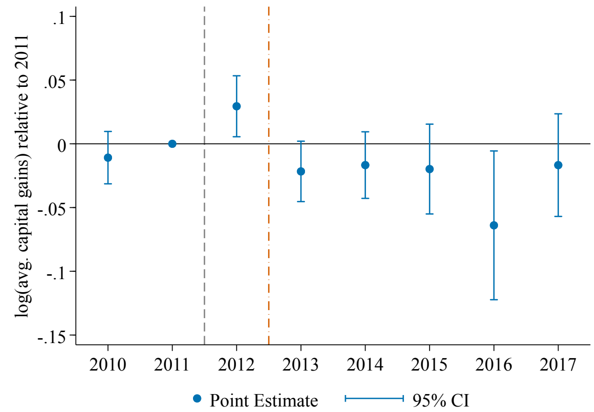
(b) Difference-in-differences coefficient

Figure 1.4: Effect of higher exposure to top tax increase on CBSA annual wages

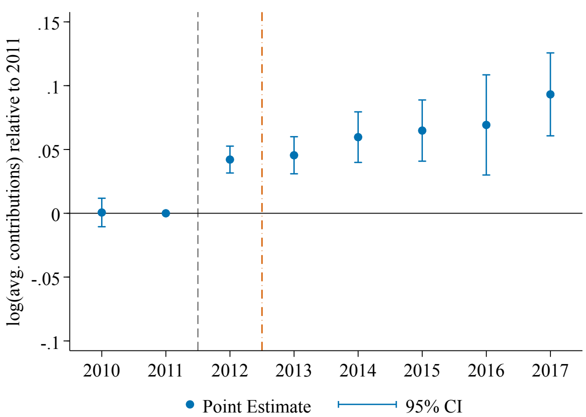
Note: This figure illustrates the zero detectable local trickle-down effects. The figure depicts effect of a higher exposure to the 2013 top tax increase on CBSA annual wages. Panel (a) depicts the conditional mean annual wage relative to 2011 separately for high and low exposure CBSAs. Panel (a) is generated in two steps. 1) I estimate for each year the average difference in annual wages relative to 2011 between CBSAs in the bottom two terciles vs. top tercile of the exposure distribution by including, instead of the continuous exposure measure, a dummy in (2.3.1) that is 1 for CBSAs in the top tercile. 2) I calculate the mean annual wage relative to 2011 of CBSAs in the bottom two terciles which yields the green diamond line and I generate the blue square line by adding to that in each year the estimated average difference in annual wage relative to 2011. Panel (b) depicts the point estimates and 95% confidence intervals from the main specification (2.3.1) of a one standard deviation increase in the exposure to the top tax increase (top 1% income share) on annual wages relative to 2011. The dashed gray line indicates the year the reforms could be anticipated (2012) and the dash-dotted red line indicates the year the reforms were implemented (2013). Both panels illustrate that 1) in support of the identification assumption, annual wages evolved in parallel between high and low exposure CBSAs prior to the tax increase and 2) a higher exposure to the top tax increase did not have a detectable adverse impact on annual wages, implying zero local trickle-down effects. See Section 1.4.1 and Table 1.1 for details.



(a) Average tax rate



(b) Average capital gains realization



(c) Average charitable contributions

Figure 1.5: Effect of higher exposure to top tax increase on CBSA average tax rate, capital gains realizations and charitable contributions

Note: This figure provides evidence of the validitiy of the exposure measure. It illustrates that CBSAs classified as high exposure by my research design were more affected by the 2013 tax increase than low exposure CBSAs and that my approach of comparing the evolution of outcomes across high and low exposure CBSAs is capable of detecting top earner responses to the reform documented using other methods. The figure illustrates the effects of a higher exposure to the top tax increase on the CBSA average tax rate (Panel (a)), (log) average capital gains realizations (Panel (b)) and (log) average charitable contributions (Panel (c)) relative to 2011. Each panel depicts for each year from 2010-2017 the estimate $\hat{\beta}_h$ from a population-weighted regression of the outcome change relative to 2011 on the standardized top 1% income share: $\Delta y_{i,h} = \beta_0 + \beta_h S_i + \epsilon_{i,h}$. The regression is estimated on the full sample of CBSAs. Consistent with the 2013 tax reform increasing the tax burdens of the top 1% and the behavioral incentives it created for capital gains realizations and charitable contributions, the panels indicate that CBSAs with a higher exposure to the reforn experienced an increase in the average tax rate, a temporary increase followed by a decrease in capital gains realizations, and an increase in charitable contributions. See Section 1.4.1 and Appendix A.2.5 for a more detailed discussion.

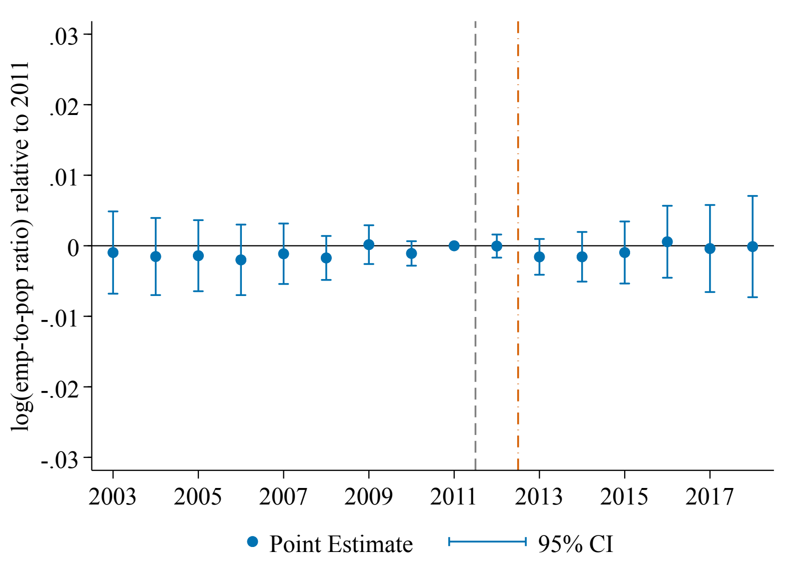


Figure 1.6: Effect of higher exposure to top tax increase on CBSA employment-to-population ratio

Note: This figure illustrates that the zero detectable local trickle-down effect on wages extends to employment. The figure depcicts the effects of a higher exposure to the top tax increase on the CBSA employment-to-population ratio. The figure plots the estimates $\hat{\beta}_h$ from the main specification (2.3.1) using the log employment-to-population ratio as an outcome and using a Bartik employment predictor instead of an income predictor. The estimates capture the effect of a one standard deviation increase in the exposure measure (top 1% income share) on the log employment-to-population ratio relatve to 2011. The dashed gray line indicates the year the reforms could be anticipated (2012) and the dash-dotted red line indicates the year the reforms were implemented (2013). The estimates indicate that a higher epxposure to the top tax increase did not a have a detectable adverse impact on CBSA employment. See Section 1.4.1.2 for a more detailed discussion.

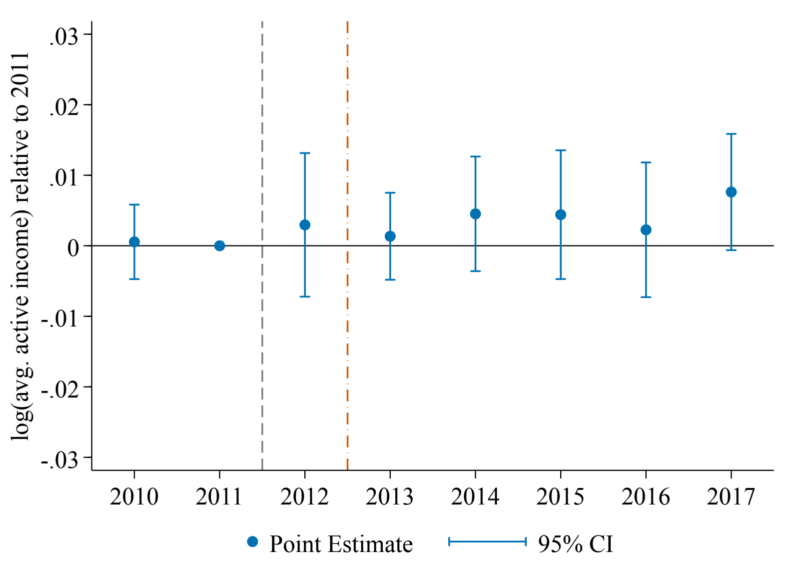


Figure 1.7: Effect of higher exposure to top tax increase on CBSA active income

Note: This figure provides suggestive evidence that the zero detectable local trickle-down effects is due to a small real response of top earners to the tax increase. The figure illustrates the effects of a higher exposure to the top tax increase on CBSA (log) average active income. Active income is calculated as total income (AGI) less capital and transfer income. It include pass-through business income, an important source of top income. The figure plots the estimates $\hat{\beta}_h$ from the main specification (2.3.1) using the log average active income as an outcome. The estimates capture the effect of a one standard deviation increase in the exposure measure (top 1% income share) on the log average active income relatve to 2011. The dashed gray line indicates the year the reforms could be anticipated (2012) and the dash-dotted red line indicates the year the reforms were implemented (2013). The reform reduced the marginal return to effort for top earners while leaving it largely unchanged for others. If top earners did reduce their effort, larger changes in total income should be observe in local labor markets with a higher top income share, where more income is responding. The figure does not reveal that active income decreased in high exposure CBSAs, providing evidence against a large real response.

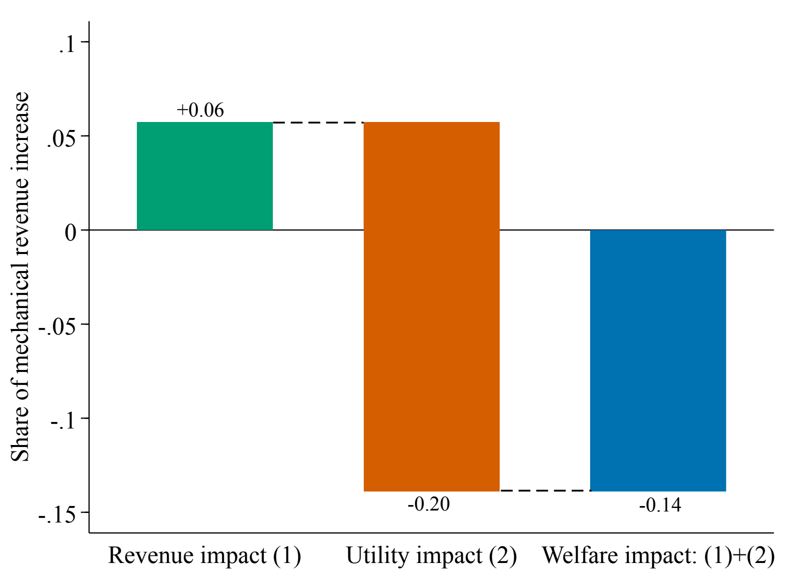
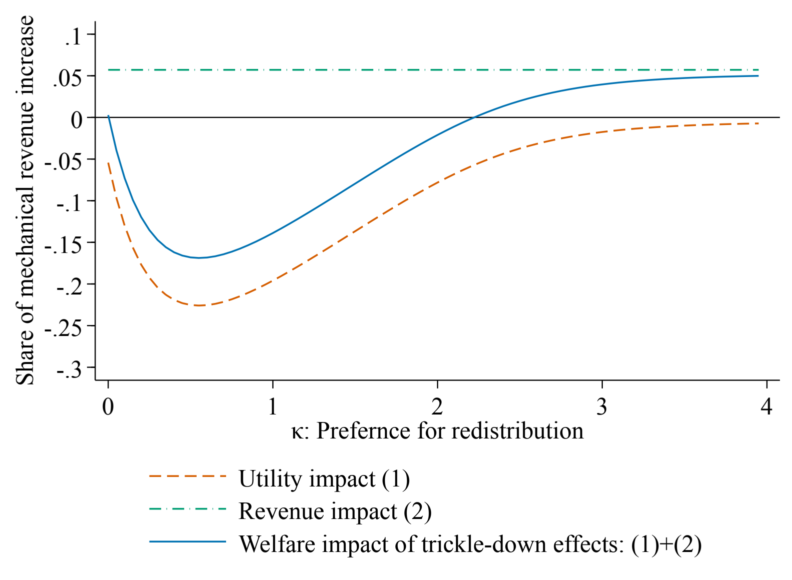
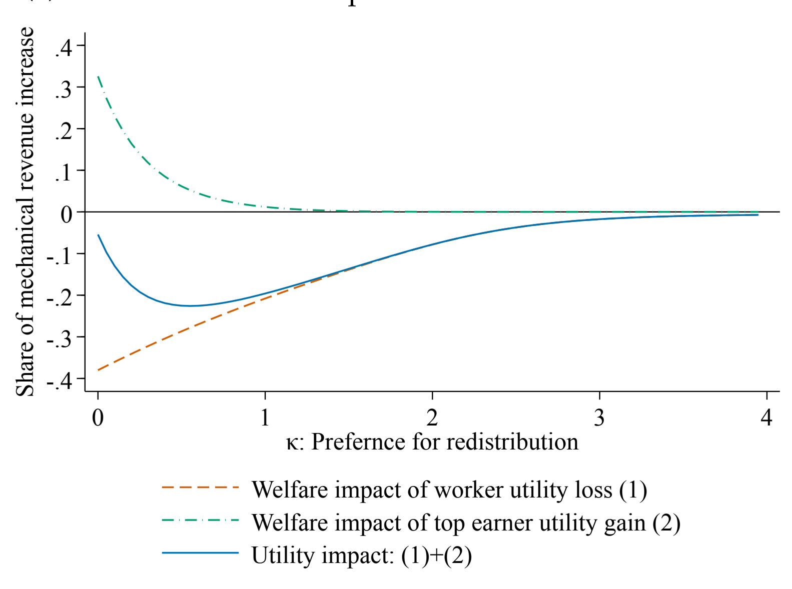


Figure 1.8: Bounds on the social welfare impact of local trickle-down effects

Note: This figure illustrates that local trickle-down effects are unlikely to have more than a modest impact on the social wefare benefit of higher top tax rates. The figure deptics the calibrated impact of the bound on local trickle-down effects on the welfare benefit of a marginal increase in the top bracket tax rate starting from the 2012 U.S. tax system. The estimated bound on trickle-down effects combined with implications of the theoretical framework in Section 1.2.2.1 and microdata on the U.S. distribution of income allow me to calibrate the welfare impact (1.5.1). The welfare impact is expressed as a percentage of the mechanical revenue gain – the revenue gain due to a higher tax rate when holding incomes constant. Welfare weights are inversely proportional to disposable income. See Section 1.5 and Appendix A.3.1 for more details. Trickle-down effects amount to a regressive pre-tax redistribution of resources. When top earners reduce their effective labor supply, their own marginal product increases and, because of complementarity, the marginal product of workers decreases. This regressive redistribution impacts the social welfare benefit of a higher top tax rate via a revenue and a utility impact. The green bar on the left indicates that the revenue impact of local trickle-down effects – the impact of redistributing income from workers to top earners – is unlikely to exceed 6% of the mechanical revenue gain. The red bar in the center indicates that the utility impact of the local trickle-down effects is unlikely to exceed 20% of the mechanical revenue gain. Combined, the bound on local trickle-down effects imply that local trickle-down effects are unlikely to decrease the welfare benefit of higher top tax rates by more than 14% of the mechanical revenue gain. Note that the point estimates imply a zero welfare impact.



(a) Bounds on welfare impact of local trickle-down effects



(b) Bounds on utility impact of local trickle-down effects

Figure 1.9: Robustness of welfare bounds to alternative welfare weights

Note: This figure illustrates that the result that local trickle-down effects are unlikely to have more than a modest impact on the social welfare benefit of higher top tax rates is robust to alternative welfare weights/preferences for redistribution. Panel (a) reveals that the bounds on the welfare impact are a U-shaped function of the concavity of the social welfare function, as measured by κ, with a maximum offset of 17%. $\kappa = 0$ implies constant welfare weights across all agents and as $\kappa \to \infty$ welfare weights approach Rawlsian weights where only the lowest income worker has a positive weight. Because the revenue impact is unaffected by κ, the U-shape comes from the utility impact. Panel (b) graphs both the total utility impact (solid blue line) as well as its two components, the utility loss for workers (dashed red line) and the utility increase for top earners (dash-dotted green line). If all agents have equal welfare weight, the utility impact of trickle-down effects is small because workers' utility loss is valued as much as top earners' utility gain. If only the lowest income worker has positive weight, the utility impact is small because the lowest income worker has an income close to zero. As κ increases starting from 0, the welfare impact of the utility gain of top earners falls more rapidly than the utility loss for workers so that the utility impact of trickle-down effects becomes negative.

Tables

	Mean 2012-2018	Mean 2012-2014	Mean 2015-2018
	(1)	(2)	(3)
$\hat{\beta}_h$	0.0006	0.0004	0.0008
	(0.0029)	(0.0021)	(0.0036)
Weights	Pop. 2011	Pop. 2011	Pop. 2011
Cyclicality Control	20 Quantiles	20 Quantiles	20 Quantiles
Region FE	State	State	State
Bartik	Yes	Yes	Yes
Housing Boom/Bust	Yes	Yes	Yes
CBSAs	924	924	924
Obs.	6468	2772	3696
R2 (within)	.367	.237	.396

Table 1.1: Effect of higher exposure to top tax increase on CBSA annual wages

Note: This table presents estimates that imply zero detectable local trickle-down effects. The table contains the estimated effects of a higher exposure to the top tax increase on CBSA annual wages averaged over three time periods: 2012-2018 (1), 2012-2014 (2) and 2015-2018 (3). Each estimate is given by the average of the coefficients $\hat{\beta}_h$ from the main specification (2.3.1) using CBSA annual wages as the outcome. Standard errors are clustered at the CBSA level. Each column presents the estimated effect of a one standard deviation increase in the exposure to the top tax increase (top 1% income share in 2011) on (log) CBSA annual wages relative to 2011. The average estimated wage effect over 2012-2018 of a one standard deviation increase in the the exposure to the reform is 0.06% with a 95% confidence interval of (-0.50%, 0.63%), equivalent to (-0.02, 0.03) standard deviations of log annual wages across all CBSAs in 2011. The estimates indicate zero local trickle-down effects. See Section 1.4.1 for a more detailed discussion.

	QCEW	ACS	
	Annual Wage	CC Annual Wage	CC Hourly Wage
	(1)	(2)	(3)
$\hat{\beta}_h$	0.0016	0.0004	0.0008
	(0.0061)	(0.0030)	(0.0024)
Weights	Pop. 2011	Pop. 2011	Pop. 2011
Cyclicality Control	20 Quantiles	20 Quantiles	20 Quantiles
Region FE	Division	Division	Division
Bartik	Yes	Yes	Yes
Housing Boom/Bust	Yes	Yes	Yes
CBSAs	361	361	361
Obs.	361	361	361
R2 (within)	.396	.342	.352

Table 1.2: Effect of higher exposure to top tax increase on CBSA annual and hourly composition-controlled wages excluding top earners

Note: This table presents estimates that imply zero detectable local trickle-down effects. The table contains the estimated effects of a higher exposure to the top tax increase on CBSA annual wages measured using the Quarterly Census of Employment and Wages (QCEW), column (1), annual (2) and hourly (3) composition-controlled (CC) wages that explicitly exlude any top earner labor income measured using the American Community Survery (ACS). The sample is restricted to subset of 361 CBSAs that are Metropolitan Statistical Areas and can be identified in the ACS. To increase precision, five annual ACS samples are pooled to create one pre-reform (2007-2011) and one post-reform observation (2014-2018). Each column indicates the effect of a one standard deviation increase in the exposure measure (top 1% income share in 2011) on (log) CBSA wages in 2014-2018 relative to 2007-2011. The estimated effect of a one standard deviation increase in the exposure to the top tax increase on CC hourly wages in column (3) is 0.08% with a 95% confidence interval of (-0.39%, 0.55%), equivalent to (-0.04, 0.05) standard deviations of log CC hourly wages across CBSAs. The estimates indicate zero local trickle-down effects. See Section 1.4.1 for a more detailed discussion and see Section 1.3.2 and Appendix A.2.1.2 for more details on the ACS data.

	Mean 2012-2018	Mean 2012-2014	Mean 2015-2018
	(1)	(2)	(3)
1. Main	0.0006	0.0004	0.0008
	(0.0029)	(0.0021)	(0.0036)
2. Division FE	0.0004	0.0004	0.0003
	(0.0037)	(0.0025)	(0.0048)
3. Region FE	0.0018	0.0010	0.0024
	(0.0042)	(0.0027)	(0.0054)
4. Nation FE	0.0007	0.0001	0.0012
	(0.0041)	(0.0026)	(0.0053)
5. 10 Cyclicality Quantiles	0.0026	0.0017	0.0033
	(0.0033)	(0.0024)	(0.0041)
6. Continuous Cyclicality Measure	0.0005	0.0007	0.0003
	(0.0034)	(0.0025)	(0.0041)
7. Cyclicality measured 1989-2011	0.0015	0.0011	0.0017
	(0.0034)	(0.0024)	(0.0042)
8. 2 Digit Bartik	-0.0001	0.0004	-0.0006
	(0.0031)	(0.0021)	(0.0039)
9. 10 Size Quantile FE	0.0016	0.0012	0.0020
	(0.0031)	(0.0022)	(0.0039)
10. Unweighted	0.0007	0.0002	0.0011
	(0.0018)	(0.0014)	(0.0024)
11. Demographic Controls	0.0010	0.0006	0.0013
	(0.0029)	(0.0021)	(0.0037)
12. Captial/GDP control	0.0006	0.0005	0.0007
	(0.0029)	(0.0021)	(0.0036)
13. Binary: Bottom terciles vs. top	0.0007	0.0015	0.0001
	(0.0020)	(0.0014)	(0.0027)
14. Binary: below median vs. above	0.0014	0.0018	0.0010
	(0.0019)	(0.0013)	(0.0025)
15. Avg. top 1% income share 2010-2011	0.0001	0.0003	-0.0001
	(0.0028)	(0.0020)	(0.0035)
16. Share income above $400k	0.0006	0.0004	0.0008
	(0.0027)	(0.0019)	(0.0034)
17. BEA annual wage data	0.0011	0.0002	0.0018
	(0.0026)	(0.0018)	(0.0033)

Table 1.3: Estimates from perturbations of main specification

Note: This table demonstrates the robustness of the zero detectable local-trickle to various perturbations of the main specification (2.3.1) using QCEW annual wages on the full sample of CBSAs. Row 1 presents the estimates from Table 1.1 for comparison. See Appendix A.2.2 for details on each perturbation.

	Mean 2012-2018	Mean 2012-2014	Mean 2015-2018
	(1)	(2)	(3)
1. Main	0.0085	0.0057	0.0106
	(0.0399)	(0.0287)	(0.0499)
2. Active income share	-0.0068	-0.0031	-0.0096
	(0.0403)	(0.0291)	(0.0506)
3. Wage & salary share	-0.0018	-0.0011	-0.0023
	(0.0618)	(0.0433)	(0.0790)
4. CSAs	0.0363	0.0254	0.0443
	(0.1141)	(0.0679)	(0.1506)

Table 1.4: Estimates from specifications that address measurement error concerns

Note: This table demonstrates the robustness of the zero detectable local trickle-down effect to multiple excercises intended to address measurement error concerns. The estimates in row 1-3 are based on QCEW annual wages for the full sample of CBSAs using the main specification (2.3.1). To facilitate comparisons across regressions, top 1% income shares enter unstandardized. Row 1 presents the estimates from the main speicification for comparison. Row 2 addresses the concern that the top 1% income share measures the top 1% human capital income share with error. The exposure measure in row 2 is the top 1% active income share where active income is defined as total income less capital and transfer income. Row 3 addresses concerns related to pass-through business income. The exposure measure in row 3 is the top 1% wage & salary income share. Row 4 addresses the concern of top earners working and living in different CBSAs by repeating the analysis as the CSA level which merges geographically proximate CBSAs (CSAs are linked by commuting flows, but to a lesser extent than CBSAs). The exposure measure in row 4 is the top 1% total income (AGI) share and is estimated using QCEW annual wages. The table contains the estimated effects of a higher exposure to the top tax increase on CBSA annual wages averaged over three time periods: 2012-2018 (1), 2012-2014 (2) and 2015-2018 (3). Each column presents the estimated effect of a one standard deviation increase in the exposure to the top tax increase (top 1% income share in 2011) on (log) CBSA annual wages relative to 2011. None of the attempts to address measurement error concerns change alter the conclusions from the main specification: there was no detectable wage decrease in high top 1% income share CBSAs relative to low top income share CBSAs following a large tax increase for top 1% earners. See Section 1.4.1 and Appendix A.2.3 for a more detailed discussion.

	Hourly Wage		CC Hourly Wage	
	Low Skill	High Skill	Low Skill	High Skill
	(1)	(2)	(3)	(4)
$\hat{\beta}_h$	-0.0015	0.0032	-0.0022	0.0005
	(0.0027)	(0.0033)	(0.0026)	(0.0029)
Weights	Pop. 2011	Pop. 2011	Pop. 2011	Pop. 2011
Cyclicality Control	20 Quantiles	20 Quantiles	20 Quantiles	20 Quantiles
Region FE	Division	Division	Division	Division
Bartik	Yes	Yes	Yes	Yes
Housing Boom/Bust	Yes	Yes	Yes	Yes
CBSAs	361	361	361	361
Obs.	361	361	361	361
R2 (within)	.319	.346	.285	.342

Table 1.5: Effect of higher exposure to top tax increase on CBSA wages by skill

Note: This table demonstrates that the zero detectable local trickle-down effect on average CBSA wages does not mask heterogeneity across worker types. The table presents the estimated wage effects of a higher exposure to the top tax increase separately for low- and high-skill workers. Low-skill workers are those with less than a college (Bachelor's) degree and high-skill workers are those with a Bachelor's degree or more. Column (1) presents the estimates for low-skill (log) hourly wages and (2) presents the estimates for high-skill (log) hourly wages. Columns (3) and (4) repeat the exercise but using composition-controlled hourly wages. Each column indicates the effect of a one standard deviation increase in the exposure measure (top 1% income share in 2011) on (log) CBSA wages in 2014-2018 relative to 2007-2011. The estimated wage effects of a higher exposure to the top tax increase are insignifcant for both types of workers and the p-value on a test of equality is 0.16 between (1) and (2) and 0.29 between (3) and (4). The estimates do not indicate that the zero local trickle-down effect on average wages masks heterogenous effects across worker types. See Section 1.4.1.2 for a more detailed discussion and see Appendix A.2.1.2 for more details on the ACS data.

Relative to Median	CBSA Size		CBSA Capital Intensity		CBSA Cyclicality	
	Below	Above	Below	Above	Below	Above
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\beta}_h$	0.0000	0.0009	0.0028	-0.0033	0.0040	0.0002
	(0.0036)	(0.0032)	(0.0044)	(0.0034)	(0.0037)	(0.0031)
Weights	Pop. 2011		Pop. 2011		Pop. 2011	
Cyclicality Control	20 Quantiles		20 Quantiles		20 Quantiles	
Region FE	State		State		State	
Bartik	Yes		Yes		Yes	
Housing Boom/Bust	Yes		Yes		Yes	
CBSAs	924		924		924	
Obs.	6468		6468		6468	
R2 (within)	.415		.419		.414	

Table 1.6: Effect of higher exposure to the top tax increase on CBSA annual wages by type of CBSA

Note: This table demonstrates that the zero detectable local trickle-down effect estimated across all CBSAs does not mask heterogeneity by types of CBSA. The table presents the estimated wage effects of a higher exposure to the top tax increase by type of CBSA using QCEW annual wages on the full sample of CBSAs. Columns (1) and (2) divide CBSAs in two equally-sized groups depending on whether their populaiton in 2011 was above or below the median CBSA population. Columns (3) and (4) divide CBSAs in two equally-sized groups depending on whether their capital-to-GDP ratio in 2011 was above or below the median ratio. Columns (5) and (6) divide CBSA in two equally-size groups depening on whether their cyclicality was above or below median. Differences across CBSAs in each of the three characteristics likely reflect differences in industrial composition. The estimates are the average of the β_h coefficients between 2012-2018 in (2.3.1) to which I add interactions between a dummy for above median interacted with the top 1% income share by year as well as with year dummies. Each column presents the estimated effect of a one standard deviation increase in the exposure to the top tax increase (top 1% income share in 2011) on CBSA (log) annual wages relative to 2011. The p-value on a test of equality for the estimates in (1) and (2) is 0.81, for the estimates in (3) and (4) is 0.28 and for the estimates in (5) and (6) is 0.40. The estimates do not inidicate systematic heterogeneity across CBSAs in the effects of a higher exposure to the top tax increase on CBSA annual wages.

	Mean 2012-2018	Mean 2012-2014	Mean 2015-2018
	(1)	(2)	(3)
$\hat{\beta}_h$	-0.0006	-0.0011	-0.0002
	(0.0021)	(0.0012)	(0.0028)
Weights	Pop. 2011	Pop. 2011	Pop. 2011
Cyclicality Control	20 Quantiles	20 Quantiles	20 Quantiles
Region FE	State	State	State
Bartik	Yes	Yes	Yes
Housing Boom/Bust	Yes	Yes	Yes
CBSAs	924	924	924
Obs.	6468	2772	3696
R2 (within)	.39	.298	.409

Table 1.7: Effect of higher exposure to top tax increase on CBSA employment-to-population ratio

Note: This table demonstrates that the zero local trickle-down effect on wages extends to employment. The table presents the estimated effects of a higher exposure to the top tax increase on the CBSA employment-to-population ratio. Each estimate is given by the average over the respective time period of the coefficients $\hat{\beta}_h$ from the main specification (2.3.1) using a Bartik employment predictor instead of an income predictor. The estimates captures the effect of a one standard deviation increase in the exposure to the reform (top 1% income share) on the (log) employment-to-population ratio relative to 2011. The average estimated effect over 2012-2018 of a one standard deviation increase in the exposure to the top tax increase on the employment-to-population ratio is -0.06% with a 95% confidence interval of (-0.47%, 0.35%), equivalent to (-0.03, 0.02) standard deviations of the log employment-to-population ratio across CBSAs in 2011. See Section 1.4.1.2 for a more detailed discussion.

		Bounds at confidence level				
	Point	90%	92.50%	95%	97.50%	99%
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Reduced-form estimates ($\hat{\beta}$)						
Panel A.1. ACS hourly wage						
1. Composition-controlled	0.0104	-0.0322	-0.0375	-0.0445	-0.0548	-0.0671
2. Composition-controlled + size	0.0168	-0.0244	-0.0295	-0.0363	-0.0462	-0.0581
Average	*0.0136*	*-0.0283*	*-0.0335*	*-0.0404*	*-0.0505*	*-0.0626*
Panel A.2. QCEW annual wage						
1. Main specificaiton	0.0085	-0.0426	-0.0490	-0.0573	-0.0697	-0.0845
2. Main + demo + size controls	0.0204	-0.0343	-0.0411	-0.0501	-0.0633	-0.0791
Average	*0.0144*	*-0.0384*	*-0.0450*	*-0.0537*	*-0.0665*	*-0.0818*
Panel B. Local trickle-down effect ($\widehat{\mathcal{L}} = \sum_k \omega_k * (S_t^k * \hat{\beta})$)						
Panel B.1. ACS hourly wage						
1. Composition-controlled	0.0015	-0.0048	-0.0056	-0.0066	-0.0081	-0.0099
2. Composition-controlled + size	0.0025	-0.0036	-0.0044	-0.0054	-0.0069	-0.0086
Average	*0.0020*	*-0.0042*	*-0.0050*	*-0.0060*	*-0.0075*	*-0.0093*
Panel B.2. QCEW annual wage						
1. Main specificaiton	0.0013	-0.0063	-0.0073	-0.0085	-0.0103	-0.0125
2. Main + demo + size controls	0.0030	-0.0051	-0.0061	-0.0074	-0.0094	-0.0117
Average	*0.0021*	*-0.0057*	*-0.0067*	*-0.0080*	*-0.0099*	*-0.0121*
Panel C. Elasticity of wages wrt top net-of-tax rate ($\widehat{\mathcal{L}}/\Delta \ln(1-\tau_t)$)						
Panel C.1. ACS hourly wage						
1. Composition-controlled	-0.0132	0.0407	0.0474	0.0562	0.0693	0.0848
2. Composition-controlled + size	-0.0212	0.0308	0.0373	0.0459	0.0585	0.0735
Average	*-0.0172*	*0.0358*	*0.0424*	*0.0511*	*0.0639*	*0.0792*
Panel C.2. QCEW annual wage						
1. Main specificaiton	-0.0107	0.0539	0.0619	0.0725	0.0882	0.1068
2. Main + demo + size controls	-0.0258	0.0434	0.0520	0.0634	0.0801	0.1001
Average	*-0.0183*	*0.0486*	*0.0570*	*0.0679*	*0.0841*	*0.1035*

Table 1.8: Bounds on reduced-form estimates, local trickle-down effects and wage elasticities wrt top net-of-tax rate

Note: This table demonstrates that at the 95% confidence level, the estimates of this paper are inconsistent with a one percent cut in the top net-of-tax rate reducing worker wages via local spillovers by more than 0.05%. The table presents the bounds on the reduced-form estimates (Panel A), local trickle-down effects (Panel B) and the elasticity of worker wages with respect to the top net-of-tax rate. Panel A presents the point estimates and bounds on the estimated effect of a higher exposure (unstandardized top 1% income share) to the top tax increase on CBSA wages. The bound at x% confidence is the smallest $b < 0$ such that in a hypothesis test of $H_0 : \beta \leq b$ vs. $H_1 : \beta > b$ I fail to reject H_0. Values below b can therefore be rejected at the x% confidence level. Panel B presents the point estimates and bounds on the local trickle-down effect – the average wage decrease across all workers due to local spillovers from the 2013 top tax increase. Interpreting the reduced-form estimate as an estimate of the trickle-down factor in the skill complementarity framework of Section 1.2.2.1, I estimate the wage change due to local spillovers in a given local labor market k as the product of the top income share in k and the estimated trickle-down factor. Then then estimate the local trickle-down effect $\widehat{\mathcal{L}}$ as the worker weighted average wage change across all labor markets, $\widehat{\mathcal{L}} = \sum_k \omega_k * (S_t^k * \hat{\beta})$ where ω_k is the share of workers in k. I bound the magnitude of local trickle-down effects by determining the most pronounced wage decrease l such that in a hypothesis test of $H_0 : \mathcal{L} \leq l$ vs. $H_1 : \mathcal{L} > l$ I fail to reject H_0. Local trickle-down effects more severe than l are therefore unlikely given the estimates of this paper. Scaling the bounds on local trickle-down effects by the magnitude of the tax change for top earners, $\Delta \ln(1 - \tau_t)$, yields the implied elasticity of worker wages with respect to the top net-of-tax rate.

CHAPTER II

Empirical Evidence on the Effect of Local Business Taxes on Business Entry

2.1 Introduction

Business entry features prominently in both theoretical and empirical economic research. Theoretically, business entry has been linked to productivity growth (Romer, 1990 and Aghion et al., 2015), the magnitude of business cycles (Clementi and Palazzo, 2016) and the labor market impact of business taxes (Sedlacek and Sterk, 2019). Empirically, new businesses are an important source of employment growth (Haltiwanger et al., 2013, Glaeser et al., 2015 and Walsh, 2019) and contribute to structural transformation (Dent et al., 2016) as well as productivity growth (Foster et al., 2008).[1] Given the importance attributed to business entry, assessing the economic implications of business taxes requires understanding how such taxes impact business entry.

This paper empirically studies the effect of local (municipal) business taxes on business (corporate employer establishment) entry by combining 5,111 German municipal business tax rate changes with administrative microdata on the universe of business entrants between 2004-2012. A unique feature of the institutional setting is that the base for taxation is constant across municipalities, making the policy variation cleaner than cross-country or cross-U.S. state variation. I use a dynamic difference-in-differences approach to estimate the effect of municipal business tax increases on business entry. Over the medium term, three-five years, a one percentage point increase in the local business tax rate reduces business entry by -4.0% (se: 1.57%). Cumulated over a six-year period, the impact of a one percentage point increase in the business tax rate corresponds to losing -18% (se: 7.1%) of an entry cohort. Heterogeneity analyses reveal that single-establishment entrants and

[1] The vague verbs used in this paragraph are intentional, as the studies cited do not necessarily uncover causal relationships.

entrants with less than three employees account for the entire decrease in business entry. Because of the local nature of the policy variation, this paper's estimates arguably reflect a combination of partial equilibrium and relocation effects.

Conceptually, business taxes impact business entry by decreasing the expected post-tax profit from entering relative to a fixed cost of entry or an outside option.[2] In theories of business dynamics in the spirit of Hopenhayn (1992), potential entrants weigh the expected post-tax profits associated with entering against a fixed cost of entry. In theories that incorporate an occupational choice margin (Lucas, 1978), potential entrepreneurs weigh the expected post-tax profits associated with creating a business against the wage they could earn as an employee. An increase in business taxes reduces post-tax profits relative to the costs of entry or the wage that can be earned as an employee, so that some entrants will no longer find it profitable to enter.

The institutional setting of this paper is the German municipal business tax. Each municipality each year determines the business tax rate that will apply the following year, and there is considerable variation in business tax rates across municipalities as well as within municipalities over time. Between 2000-2016, among the municipalities in the main sample, there are on average two local (municipal) business tax changes per year. The average tax change is +0.5 with a 75th percentile of +0.8 percentage points. Approximately 92% of tax changes are tax increases, so that I henceforth refer to tax changes as tax increases. The base for taxation, business profits, is determined at the national level and does not vary across municipalities.[3]

This paper focusses on the effect of local business taxes on corporate business entrants.[4] Although almost all types of business must remit municipal business taxes, the importance of municipal business taxes to a business' overall tax liability varies between corporate and unincorporated businesses. Corporate businesses pay additional national corporate taxes and cannot credit municipal business taxes against their national tax liability. In contrast, the profits of unincorporated businesses are passed through to the owner and are subject to the personal income tax. The owner can credit the municipal taxes they remit against their personal income tax liability, so that unincorporated businesses are less affected by municipal business taxes.

This paper uses administrative microdata from the German business register to construct municipality-level counts of business (corporate employer establishment) entrants between 2004-2012. The business register is a list of the population of firms

[2]This is also true of pure profit taxes as long as the cost of entry is non-deductible

[3]The tax administration, another dimension of tax policy as highlighted by Slemrod and Gillitzer (2013), varies at the local/regional level.

[4]Unless explicitly indicated otherwise, a business refers to a corporate business.

and their establishments operating in Germany in a given year, and includes data on characteristics such as legal form and number of employees. In this paper, a business corresponds to an employer establishment, which can be either a single-establishment firm or an establishment of a multi-establishment firm. I identify business entry by comparing establishment lists across consecutive years.

I implement a dynamic difference-in-differences approach to estimate the effects of local business tax increases on business entry. The approach uses the evolution of business entry in municipalities that don't change their business tax rate in a given year as a counterfactual for business entry in municipalities that increase their tax rate. In the main specification, I include commuting zone-by-year and state-by-year fixed effects to absorb regional economic shocks and differences in state-level institutions.[5] The identification assumption required for attributing any differential growth of business entry following a tax increase to the tax increase is the parallel-trends assumption: absent a tax increase, business entry would have evolved in parallel between municipalities that increase other municipalities within the same commuting zone that don't increase their tax rate. Econometrically, I estimate distributed-lag models on a first-differenced municipality-level panel.

An increase in the local business tax rate has a statistically significant adverse impact on local business entry. Business entry starts to decrease in the year that the tax increases, continues to decrease over the course of one-three years and remains constant thereafter. Five years after a one-percentage point increase in the local business tax rate, the number of business entrants in the municipality is -4.4% (se: 1.85%) lower than otherwise. The medium-term effect, the average of the three-, four- and five-year effect, is -4.0% (se: 1.57%). Given an average of 55 business entrants in the main sample, this implies a loss of -2.2 entrants. Cumulated over a six-year period, the impact of a one-percentage point increase in the business tax rate corresponds to losing -18% (se: 7.1%) of an entry cohort, or approximately -10 entrants out of approximately 330. These results are robust across a range of tests that assess the sensitivity of the results to the regression specification and sample.

Three exercises support a causal interpretation of the estimates. The first demonstrates that parallel-trends assumption was satisfied during the periods before a local business tax increase. The second demonstrates the robustness of the decrease in business entry to including more granular geography-by-year fixed effects. Finally, the third exercise demonstrates that there is no detectable decrease in unincorporated business entry following a local business tax increase, with point estimates close to zero. As discussed, unincorporated businesses are less affected by local business taxes, however they are

[5] In the main analysis sample, there are on average 20 municipalities per commuting zone-state cell

subject to the same economic conditions as corporate businesses. Combined, these exercises assuage concerns that the decrease in business entry reflects the continuation of a pre-existing trend or is the result of changes in economic conditions that are correlated with tax changes.

The drop in business entry following a tax increase is driven primarily by single-establishment firms with less than three employees which comprise 53-73% of all business entrants. Startups are heterogeneous, raising the question of whether the quality of entrants responds to a tax increase in addition to the quantity. The firm dynamics literature is careful to distinguish between new establishments of multi-establishment firms and new single-establishment firms, as only the latter are considered to reflect entrepreneurial activity (Decker et al., 2014). To investigate the quality of entrants along this margin, I focus on the subset of business entrants that are single-establishment firms, and I find that such firms can explain virtually all of the decrease in business entry. The entrepreneurship literature distinguishes between subsistence and transformational entrepreneurs (Schoar, 2010), where only the latter group are thought to contribute meaningfully to economic growth. To investigate the quality of entrants along the subsistence/transformational margin, I focus on the number of employees at entry, and I find that entrants with less than three employees can account for virtually all of the decrease in business entry.

The local nature of the tax changes studied here suggests that the estimates likely reflect partial equilibrium combined with spatial relocation effects. In general equilibrium, price changes, especially wage decreases, can attenuate the partial equilibrium effect of business taxes. Given the institutional setting, municipality-level business taxes where municipalities are part of larger regional economies, such price adjustments are less likely to attenuate the partial equilibrium impact of local business tax changes than national tax changes. Moreover, given that for any one municipality there is a set of geographically proximate municipalities that have identical institutions and face similar economic conditions, relocation of potential entrants likely further accentuates the effect of local business taxes above what might be expected for national business taxes. Consistent with a role for relocation effects, I find that the estimates are somewhat attenuated when substituting the commuting zone-by-year fixed effects with coarser governmental district-by-year or state-by-year fixed effects.

Connections to the literature This research connects to a number of existing studies that examine the effects of business taxes on the stock of as well as the entry and exit of businesses. Suarez Serrato and Zidar (2016) document that the number of establishments in a state decreases following a state corporate tax increase but do not examine whether the change is driven by reduced entry or increased exit. Giroud and Rauh (2019) study the effect of state corporate taxes on the number of establishments of multi-establishment firm. The paper documents that such firms decrease the number of establishments in a state following a tax increase, but does not examine whether the decrease is driven by more establishment exits or less entrants. Moreover, many establishments, in particular entrants, are single-establishment firms and these are not captured by their analysis.

Focussing specifically on new firms, Djankov et al. (2010) and Da Rin et al. (2011) use cross-country approaches to document that business taxes reduce business entry. Using a cross-U.S. state approach, Curtis and Decker (2018) find that the number of workers employed by startups decreases following a state corporate tax increase. Their results suggest an adverse impact of state corporate taxes on the number of entrants; however in response to higher state taxes, startups may also decide to hire fewer workers. Brosy (2021) examines the effect of state corporate taxes on establishment entry and exit and finds that corporate tax increases reduce establishment entry while having a less pronounced impact on exit. Riedel et al. (2020) estimate the impact of German local public good provision and local business taxes on business entry using an instrumental variable approach applied to a Poisson regression. Compared to their valuable contribution, this paper uses a different dataset covering a more recent time period and a different econometric approach that examines the dynamic impact of tax changes. Moreover, this paper focusses specifically on employer establishments and shows that the drop in business entry is driven primarily by small, single-establishment corporate firms.

The previous papers primarily exploit variation in country-level or U.S. state-level corporate tax rates over time. One issue with corporate tax variation at these levels is that not only the tax rate but also the base for taxation varies across units. Given that the impact of a tax change will depend on the tax base (Serrato and Zidar, 2018), variation in tax bases complicates the interpretation of cross-state and -country studies. This paper provides new evidence on the effects of business taxes on employer business entry, exploiting tax variation that is entirely driven by rate changes. Moreover, because countries as well as U.S. states correspond more closely to economic units than German municipalities, state- and country-level estimates should reflect general equilibrium price adjustments to a greater extent than the municipality-level estimates in this paper.

More broadly, this paper connects to the literature on the impact of business taxes on business investment. While this literature primarily focusses on the investment responses of existing businesses (for recent examples, see Yagan, 2015 and Zwick and Mahon, 2017), the intensive margin, this paper instead focuses on the investment responses of potential entrants, the extensive margin. Due to its relevance for productivity growth, a particular focus has been on the responses of R&D investment (Akcigit et al., 2021 and Lichter et al., 2021). This paper complements the R&D studies by showing that business entry, another type of investment linked to productivity growth, also responds to business taxation.

The remainder of this paper is structured as follows. Section 2.2 provides a brief conceptual discussion. Section 2.3 describes the institutional setting, data as well as the empirical approach. Section 2.4 presents the results and Section 2.6 concludes.

2.2 The Effect of Business Taxes on Business Entry in Theory

Partial equilibrium considerations Business taxes discourage entry by reducing the expected post-tax profit associated with starting a business compared to a fixed cost of entry or an outside option as an employee. In theories of business dynamics in the spirit of Hopenhayn (1992), potential entrants weigh the expected post-tax profits associated with entering against a fixed cost of entry, with the value V of creating a business given by the difference between these two elements. Denoting the expected profit by Π, the tax rate on business profits by τ and the fixed cost of entry by c_e, the optimal policy is to enter iff the expected profit exceeds the cost of entry, $\Pi(1-\tau) \geq c_e$, or, equivalently, iff the value of creating a business is positive, $V = \Pi(1-\tau) - c_e \geq 0$.[6] The fixed cost of entry can be understood as expenses that are incurred prior to beginning operations, such as developing a business plan, and are therefore not tax deductible, or as an upfront capital investment that is financed using equity.[7] An increase in τ reduces the value of creating a business by reducing post-tax profits relative to the cost of entry. Potential entrants that are at or close to the margin of entry prior to a tax increase will therefore be discouraged from entering by the tax increase.

Similarly, in theories that allow for an occupational choice margin (Lucas, 1978), potential entrepreneurs weigh the expected post-tax profits associated with creating a business against the wage they could earn as an employee. Entrepreneurs fully or partly earn their compensation in the form of business profits, so that an increase in business taxes reduces their post-tax compensation relative to their wage compensation as an employee. As a consequence, potential entrepreneurs that are at or close to the margin of entry prior to a tax increase will be discouraged from entering by the tax increase.

[6]This formulation highlights that the effect of business taxes on entry also applies to a pure profit tax. The insights would continue to hold if financing costs were not deductible.

[7]Creating a corporation in Germany, for example, requires an upfront equity investment of at least EUR25k.

General equilibrium considerations Business tax increases can put downward pressure on equilibrium wages, and lower wages can attenuate the partial equilibrium decrease in business creation. As discussed by Sedlacek and Sterk (2019), business tax increases can put downward pressure on wages via changes in the labor demand of incumbent businesses and of new businesses. Incumbent businesses are faced with an increase in the cost of capital and therefore reduce their investment and employment, while the drop in business entry reduces total employment at new businesses. Lower wages boost pre-tax profits and therefore attenuate the impact of business taxes on business creation. In addition, as pointed out in Neira and Singhania (2017), lower wages reduce the outside option of potential entrepreneurs, further offsetting the partial equilibrium effect of business taxes on the business entry margin.

However, given the local nature of business taxes studied in this paper, general equilibrium effects are less likely to attenuate the partial equilibrium decrease in business creation. Municipalities are small and workers are mobile across them, so that municipalities are components of larger regional economies and not standalone local labor markets. An increase in business taxes in one part of a regional economy will have a smaller impact on equilibrium wages than an increase in business taxes in the entire economy. Consequently, general equilibrium effects are less likely to attenuate the partial equilibrium effects for local business tax changes than for economy-wide changes.

Business relocation The institutional setting for this paper requires consideration of business location in addition to business entry decisions. A potential entrant decides both whether to enter as well as in what municipality to enter, and a business tax increase can impact both of these decisions. When a municipality increases its business tax rate, a business may no longer find it profitable to enter in that municipality, but it may still find it profitable to enter in a neighboring municipality. Moreover, even if it continues to be profitable for the business to enter in the municipality that raises its tax rate, it might now be more profitable to enter in a neighboring municipality. These two arguments suggest that relocation forces should accentuate the effect of local business taxes on local business entry compared to the effect of national business taxes on national business entry.

Business entry independent of exit Business entry responses to local business tax changes, independent of exit responses and adjustments in the stock of businesses, are interesting for at least two reasons. First, business entry can be conceptualized as sampling from a distribution of ideas (Kortum, 1997). The more business entry there is, the more ideas are sampled and the higher the probability of finding a high-productivity idea.

Second and relatedly, new businesses tend to have a higher productivity than incumbents and exiting firms (Foster et al., 2008), so that more entry implies faster productivity growth even if the total number of businesses stays constant. Indeed, declining entry and exit rates in the U.S. are a concern among economists (e.g. Decker et al., 2016 and Decker et al., 2017).

2.3 Empirical Setting and Strategy

This paper combines rich variation in German municipal business tax rates with administrative microdata from the business register and uses a difference-in-differences approach to estimate the effect of local (municipal) business tax increases on business (corporate employer establishment) entry.

2.3.1 The German Municipal Business Tax

Overview Each of the approximately 11,000 municipalities in Germany sets its own business tax rate and, apart from a few exceptions, all businesses are subject to municipal business taxes.[8,9] The municipal (local) business tax rate in municipality i, τ_i, is the product of a federal base rate, τ^{base}, and a local scaling factor, θ_i:[10]

$$\underbrace{\tau_i}_{\text{Municipal (local) business tax rate}} = \underbrace{\tau^{\text{base}}}_{\text{Federal base rate}} * \underbrace{\theta_i}_{\text{Local scaling factor}}$$

The base for taxation is regulated at the national level and consists of business profits.[11,12] Variation across municipalities in business taxes is therefore restricted to variation in the tax rate, which is determined by variation in the local scaling factors, θ_i. The scaling factor is set

[8] The average municipality has a population of roughly 7.5k a surface area of 12mi^2. Note that most municipalities are small: only roughly 1.6k municipalities have a population of more than 10k.

[9] Freelancers (Freiberufler) are exempt. The most common professions among freelancers are doctors and lawyers; other eligible professions are engineers, journalists and artists. Note, though, that if a freelancer decides to incorporate, they are subject to the local business tax.

[10] Note that prior to 2008, the local business tax was deductible from the business tax base, so that the local business tax rate was given by $\tau_i = \tau^{\text{base}}\theta_i / \left(1 + \tau^{\text{base}}\theta_i\right)$.

[11] Interest costs are partly tax deductible. Prior to 2008, 50% of long-term debt interest was tax deductible. Since 2008, 25% of interest costs above EUR100k are added back to profits.

[12] Compliance is monitored by approximately 600 local and regional tax administrations. There can be more than one tax administration per municipality in very large municipalities (e.g. Berlin), and less than one in smaller, less dense municipalities.

each year by the municipal council.[13,14] For multi-establishment firms with establishments in multiple municipalities, taxable profits are allocated across municipalities based on payroll shares.

Variation There is considerable cross-sectional variation in local business tax rates.[15] Between 2004-2008, the average scaling factor was 3.3 and the interquartile range (iqr) was 3.1-3.5 which, given a federal base rate of $\tau^{\text{base}} = 5\%$ and taking into account the deductibility of the local business tax from the tax base, translates into an average local business tax rate of 14.1% and an iqr of 13.4-14.9%. In 2008, the federal base rate decreased to $\tau^{\text{base}} = 3.5\%$ and the deductibility of the business tax was eliminated. Between 2008-2012 the average scaling factor was 3.4 and the iqr was 3.2-3.6 translating into an average local business tax rate of 11.9% and an iqr of 11.6-12.6%.

In addition to the variation across municipalities in any given year, and more importantly for the empirical strategy of this research, there is ample variation in local business tax rates within municipalities over time. Between 2000-2016, among the 10,227 municipalities that were not part of a municipal merger, there were on average 1,088 local business tax changes triggered by scaling factor changes per year.[16] Over this time period, 15% of municipalities had zero tax changes, 31% had exactly 1 tax change, 44% had 2 or 3 tax changes and 10% had more than 3 changes. Approximately 95% of tax changes were tax increases, with an average tax increase of .8 percentage points and a 75th percentile of one-percentage point, see Figure 2.1 Panel (a) for a histogram. Moreover, these local business tax changes are persistent: 10 years after a one-percentage point increase in the tax rate, approximately 83% of the increase persists, see Figure 2.1 Panel (b).

Corporate vs. unincorporated businesses Because local business taxes primarily affect corporate businesses, this research focusses on estimating the impact of local business taxes on corporate business entry. Unless explicitly indicated otherwise, business entry refers to corporate business entry. In addition to local business taxes, corporate businesses must remit additional national corporate taxes on the profits they earn, where the base

[13] The scaling factor for year t is typically determined towards the end of year $t-1$ but can be determined up to 06.30 of $t+1$.

[14] In 2004, a minimum scaling factor of 2 was implemented. Only a small number of municipalities was affected by the policy (approximately 30) and these are dropped from the main analysis sample.

[15] Note that the variation described here refers to variation across all German municipalities, as opposed to municipalities in the main analysis sample. As a consequence, the numbers reported here can vary from the numbers reported elsewhere in the paper.

[16] I focus on non-merged municipalities as these are the starting point for the analysis sample (see Section 2.3.2 for a discussion). I focus on the years 2000-2016 because tax changes within this time window are used to identify the dynamic effects of local business tax changes in the regressions.

for taxation is the same at the local and national level. Local business taxes account for approximately 40-45% of total business taxes for corporate firms.[17]

In contrast, the profits of unincorporated businesses are passed through to the owner where they are subject to the personal income tax. The owner of the businesses can credit the municipal taxes they remit against their personal income tax liability. The maximum credit is capped and is based on the municipal tax liability that would have been due given a certain scaling factor.[18] Moreover, unincorporated business benefit from a standard deduction of EUR24.5k. As a consequence local business taxes have considerably less of an impact on the total business tax liability of unincorporated businesses.[19]

Local business taxes in the context of municipal public finances Municipal governments in Germany have limited control over revenue and expenditure policies. On the revenue side, in addition to setting the local business tax rate, municipalities also set property tax rates. Local business taxes account for 75% of total municipal tax revenues, but total municipal tax revenues account for only 20% of overall revenue. The other main sources of municipal revenue are shares of federal income and sales taxes as well as transfers from higher levels of government. Both the share of federal taxes and the transfers are determined in a way that redistributes from municipalities with a stronger budget to those with a weaker budget.[20] On the expenditure side,

[17]From 2004-2008 the average scaling factor was 3.3. The average local business tax rate, taking into account the deductibility of the business tax from the business tax base, was therefore approximately 14.1%. The share of local business taxes in the total tax liability, taking into account that prior to 2008 the national corporate tax rate was 25% and applied to the after-local-tax profits, was therefore $\frac{0.14}{0.25(1-0.14)+0.14} \approx 0.4$. From 2008-2012 the average scaling factor was 3.4. The average local business tax rate was therefore approximately 12%. Given a national tax rate of 15%, the share of local business taxes in the total tax liability was therefore $\frac{0.12}{0.12+0.15} \approx 0.45$.

[18]For example, between 2008-2020, the scaling factor used to determine the maximum credit was 3.8 from 2008. If the business is located in a municipality with a scaling factor above 3.8, the owner can only credit the business tax liability that would have been due given a scaling factor of 3.8.

[19]Less, but not necessarily zero. There are three reasons why local business taxes can still impact unincorporated businesses. First, other sources of income can prevent the owner from fully crediting the local business tax liability. Second, for some municipalities, around 10%, the local business tax liability can exceed the maximum allowed credit. Third, local business taxes are remitted quarterly and can create cash-flow issues for credit-constrained firms.

[20]The share of income taxes, for example, is based on the overall income of municipality residents, however the income is capped at a certain threshold (for example EUR35k for individual filers in 2015 in Baden-Württemberg). The transfer payments are based on the difference between a hypothetical expenditure and a hypothetical tax revenue. The hypothetical expenditure is a function of the municipal population. The hypothetical tax revenue is the sum of the share of income and sales taxes as well as hypothetical business and property tax revenues. The hypothetical business and property tax revenues are determined by dividing the actual business and property tax revenue in a municipality by the municipality's scaling factor and then multiplying them by fixed state-level scaling factors. For a given tax revenue, municipalities with a higher scaling factor are therefore attributed a lower hypothetical tax revenue and therefore a larger transfer.

transfer policies as well as the provision of many public goods, such as most educational institutions and inter-municipality transportation infrastructure, are provided by state or national governments. Approximately 70% of municipal expenditures go towards public administration, maintaining local roads/parks/buildings (including schools), intra-municipality public transport, waste removal as well as the payment and provision of certain services for welfare recipients as mandated by federal law.[21] Municipalities are required by law from to prepare a balanced budget proposol in any given year.[22,23]

Reasons for local business tax changes As documented by Foremny and Riedel (2014) and Fuest et al. (2018a), the overall trend of local business tax increases is driven by expenditure shocks, while the timing of tax increases is driven by electoral cycles. Foremny and Riedel (2014) argue that the trend towards local business tax increases is a response of municipal governments to increased spending requirements resulting from increases in federally mandated expenditures, such as the expansion of early childhood education, as well as increases in social security payments to the elderly and unemployed. Moreover, they document that the timing of tax increases is driven by electoral cycles: the probability of a tax increase in an election year drops, whereas it increases in the post-election year. Fuest et al. (2018a), in their study of the corporate tax incidence that exploits local business tax variation, provide evidence that local business tax increases are not correlated with local business cycles.

2.3.2 German Business Register

Overview This research uses 11 years (2002-2012) of administrative microdata from the German business register (Unternehmensregister - System 95), accessed via the Research Data Centres of the Federal Statistical Office and the Statistical Offices of the Federal States, to construct municipality-level measures of business entry between 2004-2012. The business register is a list of the population of firms and establishments operating in Germany in a given year. It is constructed mainly by combining administrative data on establishments and their employees from the Federal Employment Agency with data on firms and their taxable sales from the tax authorities. The database is maintained by the

[21] Approximately 15% go towards daycare centers for kids, youth support and athletic facilities such as public pools. The remainder goes towards a combination of cultural institutions (e.g. theaters), education institutions (e.g. vocational training) and health care facilities.

[22] A budget deficit needs to be approved by state authorities and can only be approved if the municipality presents a plan to improve its fiscal situation.

[23] The data and information in this paragraph are based on Statistisches Bundesamt (2011) and Ministerium fuer Finanzen und Wirtschaft Baden-Wuerttemberg (2015).

statistical office of each state and is intended to serve both as a source for official economic statistics as well as a sampling frame for firm/establishment-level surveys.

The data contain information on the size and various other characteristics of establishments and firms. An observation in the data is either an establishment or a firm, and it is characterized by both an establishment and firm id. An establishment is either a single-establishment firm or an establishment of a multi-establishment firm. For establishments, the data contain information on the number of workers subject to social security contributions, the location (municipality) and a 4-digit industry code. Note that establishments can be assigned to firms via the firm id. For firms, the data contain information on the number of workers, the amount of taxable sales, the legal form, wether the firm is part of a tax group or firm group, the location (municipality) and a 4-digit industry code.

Measuring entry The concept of a business in this paper corresponds to an employer establishment. Employer establishments hire at least one worker subject to social security contributions and are either single-establishment firms or establishments of multi-establishment firms. I identify business entry by comparing establishment lists in consecutive years. I define a business as entering in t if its establishment id is not found in the list of businesses in $t-1$ and $t-2$. Entry is therefore measured each year from 2004-2012.

The business entry definition attributes economic entry to the appearance of id numbers in the business register.[24] The continuity rules of the business register for id numbers provide a justification for this linkage. They state that, for an underlying economic unit, the establishment id should change iff at least two out of the following three criteria are satisfied: the location (municipality) changes, the ownership changes, the economic activity (industry) changes. Pure ownership, location or industry changes should therefore generally not result in false business entry and exit. Indeed, I observe ownership, location and industry changes in the data. However, in practice these continuity rules could likely not always be followed, so that a subset of business entry events are likely due to pure ownership/location/industry changes.[25] I provide some evidence that the drop in entry

[24] Economic entry here is understood as the creation of new production resources or the implementation of a new idea.

[25] A key issue is that for the Federal Employment Agency ownership, legal form and industry changes do trigger new ids. Therefore, if a single-establishment firm changes ownership between t and $t+1$, the Federal Employment Agency will send the information on the number of workers of the establishment to the Statistical Offices of the Federal States in $t+1$ using a new id number, and it will be the responsibility of the statistical offices to reconcile the old and the new id number. The tax authorities also follow different continuity rules.

following a tax increase is due to a drop in true entry by investigating how the total number of businesses in a municipality responds to a tax change (Appendix B.1.1). Whereas true business entry increases the total number of businesses, events such as ownership or industry changes do not.

I construct a number of additional variables to classify and describe businesses. I use the legal form to classify single-establishment firms as incorporated or unincorporated and I classify establishments of multi-establishment firms as incorporated.[26] For businesses that enter, I construct size measures based on the number of workers at entry, I determine whether the establishment is a single-establishment firm and if they are operating in the non-tradable sector.

Descriptive statistics There are approximately 22m business-year observations in the data from 2002-2012.[27] Single-establishment firms account for 85.7% of these observations and account for 56.7% of all workers. Corporate businesses are larger than non-corporate businesses, with the average corporate business hiring twice as many workers as the average across all businesses (see Table B.1 Panel (a) for details). Approximately 1.75m total (corporate, unincorporated and other) business entries are observed in the data from 2004-2012, implying an average total business entry rate of 9.7% (see Table B.2 Panel (a) for details). Corporate entrants are larger than non-corporate entrants, with the average corporate entrant hiring twice as many workers as the average across all entrants (see Table B.1 Panel (b) for details).[28]

Other data The municipal tax data come from the Federal Statistical Office and the Statistical Offices of the Laender.[29] I crosswalked the municipality identifiers in the tax data to the 2017 version of the Business Register using crosswalks from the Federal Institute for Research on Building, Urban Affairs and Spatial Development.

The main regression specification controls for commuting zone-by-year fixed effects.

[26]Note that the set of corporate and unincorporated establishments are not a partition of the set of all establishments. Some establishments are neither corporate nor unincorporated. These include public institutions and non-profit organizations as well as establishments for which the legal form was missing.

[27]Note that the variation described here refers to variation across all businesses and all German municipalities, as opposed to municipalities in the main analysis sample.

[28]Unfortunately, a comparison to existing public statistics on employer establishment entry is not possible as these statistics do not exit. Rink et al. (2013) use additional data to disentangle id changes from true entry. However, their unit of observation is the firm and they consider employer and non-employer firms. As a consequence, the levels of entry cannot be readily compared. Reassuringly though, the time series pattern of entry are similar between their series (Figure 1 in their paper) and the series in Table B.2. In the US, according to the Business Dynamics Statistics, the average entry rate between 2004-2012 was 10.8%.

[29]I thank Sebastian Siegloch for providing the data for certain years and states where the data was not readily available at the statistical offices.

The commuting zones are determined by the Federal Institute for Research on Building, Urban Affairs and Spatial Development. These areas are composed of counties and are determined based on commuting flows across counties. On average, 26 municipalities comprise a county and 40 municipalities comprise a commuting zone.

Analysis sample I construct a municipality-level panel by collapsing the business register microdata to municipality-year cells. I calculate the number of (corporate) business entrants in a municipality each year, as well as the number for all (corporate, unincorporated and other) and unincorporated business entrants. To investigate whether the quality of entrants adjusts, I add counts of the number of small business entrants, the number of entrants that are single-establishment firms, and the number of entrants in the non-tradable sector. I merge the tax data and commuting zones to the municipality-level version of the business register.

The main analysis sample consists of on non-merged municipalities that have at least 1,000 workers on average between 2004-2012. I focus on non-merged municipalities because for merged municipalities the tax rate is not well defined prior to the merger. Moreover, post-merger, the tax rate can vary within the municipality across the former independent municipalities. I restrict attention to municipalities with at least 1,000 workers on average in order to focus on economically meaningful municipalities. I examine the robustness of the results to alternative municipality-size restrictions.

Table B.3 contains the descriptive statistics of the main analysis sample.[30] The main analysis sample consists of 2,567 municipalities and 20,356 municipality-year observations. There are on average 28,094 workers and 55 business entrants in a municipality-year cell (medians: 11,138 and 22, respectively). Approximately 73% of business entrants are single-establishment firms, 38% have less than two employees and 53% have less three employees at entry. Across municipalities in the main sample, there are 5,111 local business tax changes between 2000-2016 with an average change of +0.5 and an iqr of +0.4 to +0.8 percentage points. The average tax rate between 2004-2012 is 14.2%.

2.3.3 Empirical Strategy and Econometric Implementation

Empirical strategy This paper uses a difference-in-differences approach to study the effect of local (municipal) business taxes on business (corporate employer establishment) entry. The approach examines whether, following a local business tax increase, business entry grows at different rates in municipalities that increase their tax rate compared to

[30] Municipalities are weighted by their regression weight: the median number of workers in the municipality over the period 2004-2012, winsorized at the 99th percentile.

control municipalities that don't. The control municipalities in the main specification are restricted to the set of municipalities that are in the same commuting zone as well as state, and therefore subject to similar economic conditions and political institutions. The identification assumption required for attributing differential growth rates of business entry following a local business tax increase to the increase is the parallel-trends assumption: in the absence of the tax increase, business entry in municipalities that increase their tax rate would have grown at the same rate as entry in municipalities that don't. I empirically assess the plausibility of the identification assumption and provide evidence in support of a causal interpretation of the estimates in Section 2.4.

Econometric implementation I implement the empirical strategy by estimating distributed lag models and generating cumulated coefficients. The regression equation reads

$$\Delta y_{i,t} = \sum_{k \in \mathcal{K}} \delta_k \Delta \tau_{i,t-k} + \gamma_{g(i),t} + \epsilon_{i,t} \tag{2.3.1}$$

where $\Delta y_{i,t}$ measures the year-on-year change in the outcome variable in municipality i in year t, $\Delta \tau_{i,t-k} = \tau_{i,t-k} - \tau_{i,t-k-1}$ is the year-on-year change in the local business tax rate in municipality i k periods from t, $\mathcal{K}$ is a set of leads and lags, $\gamma_{g(i),t}$ is a year fixed effect that varies by geographic region where $g(i)$ is a mapping from municipalities to geographic regions, and $\epsilon_{i,t}$ is an error term which captures the combined impact of all other factors that determine $\Delta y_{i,t}$, clustered at the municipality level.[31]

The primary outcome variable is the year-on-year change in scaled business (corporate employer establishment) entry, scaled by the average population of all businesses in the municipality between 2004-2012. Formally, $\Delta y_{i,t} = \frac{y_{i,t} - y_{i,t-1}}{\bar{n}}$, where $y_{i,t}$ is business entry in i at time t and $\bar{n}$ is the average population of all (corporate, unincorporated and other) businesses in the municipality between 2004-2012. The scaled change in business entry has the advantage of accommodating zeros while allowing the level change in entry to vary by the size of the municipality, and it facilitates comparisons across outcomes because of the common denominator. It can also be understood as assuming that the effect of the right-hand-side variables, in particular tax changes, on the level change in the number of entrants will be proportional to the municipality size, as measured by its average population of all businesses.

[31] I restrict attention to changes in the local business tax rate that are due to scaling factor changes. As a consequence, I do not consider changes due to the 2008 reform that changed the base rate and eliminated the deductibility of the local business tax from its own base. As discussed in Section 2.4, the results are robust to including both local and national changes.

The primary coefficients of interest are the cumulated post-reform coefficients, $\hat{\beta}_t = \sum_{k=0}^{t} \hat{\delta}_k$ with $t \in \mathcal{T} = \{0, \ldots, 5\}$. Each $\hat{\delta}_k$ indicates the impact that a business tax change k periods prior to the current period has on the current year-on-year change in scaled business entry. For example, the effect on the year-on-year change in entry during the year of the tax increase is given by $\hat{\delta}_0$, and the effect on the year-on-year change in entry one year after a tax increase is given by $\hat{\delta}_1$. The total effect on entry one year after relative to one year before a tax increase is therefore given by the sum of the two year-on-year effects, $\hat{\beta}_1 = \hat{\delta}_0 + \hat{\delta}_1$.

The main specification makes the following decisions regarding the geographic unit, the lead/lag structure and the regression weights. The year fixed effect is allowed to vary at the commuting zone and state level, where there are on average 20 municipalities in a commuting zone-state cell. The specification includes 4 leads and 5 lags, so that the coefficients in the main specification are identified using tax reforms between 2000-2016. Each municipality is weighted by the median number of workers in the municipality over the period 2004-2012, winsorized at the 99th percentile. I examine the robustness of the results to these modeling choices.

2.4 Local Business Taxes Reduce Business Entry

2.4.1 Main Result

Non-parametric evidence Before presenting the estimates from the distributed lag model, I show that the adverse impact of local business taxes on business entry can also be uncovered using a non-parametric approach.[32] is as follows. I first select a set of treated and untreated municipalities. Treated municipalities experience one tax increase between 2005-2007 and no other confounding tax changes within a five-year window. Control municipalities do not experience a tax change between 2000-2012. I then compare four-, five- and six-year changes in business entry between treated municipalities, those that increase their tax rate, and control municipalities in the same state that don't.[33]

[32] A number of papers have highlighted extrapolation issues that can arise with staggered adoption designs when treatment effects are heterogeneous (e.g. Borusyak et al., 2021 and Sun and Abraham, 2021). The non-parametric evidence presented here is immune to these criticisms and therefore assuages concerns that the estimates from the distributed lag model are the product of econometric issues with staggered adoption designs.

[33] The six-year comparison, for example, is constructed as follows. For municipalities that experience a tax increase in 2005, I calculate the difference in entry between 2010 and 2004, the pre-reform year. I then calculate the difference between this change in entry and the change for control municipalities in the same state that experience zero tax changes during 2000-2012. Similarly for municipalities that experience a tax increase in 2006 and 2007. The six-year effect is a weighted average of these differences.

Municipality weights are as in the regression.

Table 2.1 presents the results. Panel A presents the average four-, five- and six-year changes in scaled business (corporate employer establishment) entry both for municipalities that increase their tax rate and for control municipalities that don't. The third row presents the difference between the two groups. The difference is negative across all lags and fairly stable, indicating that, compared to control municipalities, municipalities that increase their business tax rate experience a decrease in business entry. Panel B presents information on the sample that is used to compute the implied effects of a one-percentage point increase in the local business tax rate in Panel C. The exercise suggests that a one-percentage point increase in the local business tax rate reduces scaled business entry by -0.25 to -0.31pp. Taking into account that the average scale variable — mean population of all (corporate, unincorporated and other) businesses in the municipality between 2004-2012 — in this sample is 1,625, a one-percentage point business tax increase reduces business entry by -4.1 to -5 businesses per year, or approximately -7.5 to -9.5% per year. To exploit all of the tax variation in the data and to examine how the adverse impact of local business taxes on business entry generalizes, I now turn to the distributed lag model.

Distributed lag model estimates Figure 2.2 Panel (a) depicts the cumulated post-reform coefficients from the distributed lag regression (2.3.1), $\hat{\beta}_t = \sum_{k=0}^{t} \hat{\delta}_k$ with $t \in \mathcal{T} = \{0, \ldots, 5\}$ and with $\hat{\beta}_{-1} = 0$, using year-on-year changes in scaled business (corporate employer establishment) entry as the outcome variable. The estimates represent the percentage point change in scaled business entry between one period prior to and t periods post a one-percentage point local business tax increase. The estimates reveal that, following a local business tax increase, business entry decreases. The decrease starts in the year of the tax increase, becomes more pronounced during the next two years and remains constant thereafter. Five years after a one-percentage point increase in the local business tax rate, scaled business entry is -0.149pp (se: 0.063pp) lower than otherwise. Taking into account that the average scale variable — mean population of all (corporate, unincorporated and other) businesses in the municipality between 2004-2012 — in the sample is 1,628, a one-percentage point business tax increase reduces business entry by -2.4 businesses per year, or approximately -4.4% per year. The estimated semi-elasticity five years after a tax increase is therefore -4.4. Table 2.2 Column (1) contains the estimated effect five years after a tax increase and Column (2) contains the average estimated effect three, four and five years after a tax change (medium-term effect). The medium-term effect implies a semi-elasticity of -4.0.

In addition to the impact on business entry in any given post-reform year, it is also interesting to ask what the cumulated loss of business entry is over the post-reform period. Column (3) of Table 2.2 presents the cumulated estimate, $\sum_{t=0}^{5} \hat{\beta}_k$. The estimate indicates that, cumulated over a six-year period, a one-percentage point increase in the local business tax rate reduces scaled business entry by -0.61pp (se: 0.24pp), corresponding to a loss of -9.9 business entrants or approximately 18% of business entry in a given period. Put differently, the estimates indicate that, cumulated over a six-year period, the impact of a one-percentage point increase in the local business tax rate is equivalent to losing 18% of an entry cohort.

I also consider an alternative outcome variable that equals one in the case of a year-on-year increase in business entry and zero otherwise. Compared to the scaled change in business entry, the indicator variable has the advantage of being less impacted by large changes in the number of entrants. Figure 2.2 Panel (b) depicts the post-reform coefficients from the distributed lag regression (2.3.1), $\hat{\delta}_k$ with $k \in =\{0, \ldots, 5\}$. Each estimate therefore indicates the percentage point impact of a local business tax increase of one-percentage point on the probability of experiencing a year-on-year increase in entry k periods after the tax increase. The horizontal lines spanning the figure indicate the average of the coefficients as well its 95% confidence interval. The estimate implies that a one-percentage point business tax increase reduces the probability of experiencing a year-on-year increase in business entry during the five years following a tax increase by on average -1.99pp (se: 0.77pp). Given a 45% probability of experiencing an increase in entry across all municipalities and years, this implies a decrease of -4.4%.

Identification The effect of local business tax increases on business entry is identified by comparing changes in business entry between municipalities that increase their tax rate and others that don't. The identification assumption is the parallel-trends assumption: absent a tax increase, business entry in municipalities that increase their tax rate would evolve in parallel with business entry in municipalities that don't increase their tax rate.

One concern is that municipalities that increase their tax rate are experiencing a general decline in business entry that precedes the tax increase. If this were the case, the drop in entry observed after a tax increase would be due to differential trends between municipalities as opposed to the tax increase itself. To address this concern, I investigate the cumulated pre-reform coefficients from the distributed lag regression (2.3.1), $\hat{\beta}_{-t} = \sum_{k=-(t-1)}^{-1} \hat{\delta}_k$ with $t \in \mathcal{T} = \{2, \ldots, 5\}$ and with $\hat{\beta}_{-1} = 0$, using the year-on-year change in scaled business entry as the outcome variable. The estimates represent the percentage point change in scaled business entry between k periods prior to and one period prior to a one-percentage point local business tax increase. The estimates, depicted in Figure 2.3 Panel (a), reveal that, during the five years leading up to a tax increase, municipalities that increase their tax rate experience similar changes in business entry compared to municipalities that don't increase their tax rate. This exercise assuages concerns that the decrease in business entry following a tax increase reflects the continuation of a pre-existing trend.

Another concern is that unobserved economic shocks are triggering both tax increases as well the subsequent decline in business entry. I address this concern using two approaches. First, I consider more granular geography-by-year fixed effects. The main specification already includes state-by-year fixed effects that absorb state-level political and economic shocks as well as commuting zone-by-year fixed effects that absorb economic shocks at the level of commuting zones. To further probe the threat of economic shocks that coincide with tax increases, I instead include county-by-year fixed effects.[34] The estimates are depicted in Figure 2.3 Panel (a) and are very similar to the estimates from the main specification. After controlling for county-by-year fixed effects, in order for unobserved economic shocks to confound the results, these shocks must narrowly affect municipalities that increase their tax rate.

The second approach consists of investigating the impact of local business tax increases on the entry of unincorporated businesses. As discussed in Section 2.3, because the owners of unincorporated businesses can credit their local business tax liability against their personal income tax liability, unincorporated businesses should be less affected by local business tax increases. However, within a municipality, unincorporated businesses are subject to the same economic conditions as corporate businesses. Therefore, if unobserved economic shocks at the municipality level are confounding the estimates, we should expect to see a similar decrease in entry among unincorporated businesses. Figure 2.3 Panel (b) depicts the estimates from the main specification using year-on-year changes in scaled unincorporated business entry as the dependent variable. The estimates do not reveal a decrease in unincorporated business entry, suggesting that unobserved economic shocks are not confounding the results.[35]

Robustness I assess the robustness of the main result to changes in the regression specification as well as the estimation sample. For the regression specification, I consider alternative outcome transformations, alternative weights, including municipality size-by-year controls, alternative tax specifications, a fixed effect as opposed to a first

[34] In the sample, there are on avg. 12 municipalities per county vs. 20 per commuting zone & state cell.

[35] The estimates suggest an insignificant medium-term effect of -0.037. This compares to -0.136 for corporate entrants and -0.186 for all entrants.

differenced regression and an alternative lead/lag structure. The alternative outcomes I consider are the DHS growth of business entrants (Davis et al., 1998) and the year-on-year change in the inverse-hyperbolic sine transformed number of entrants.[36] Both outcomes are similar to log changes but have the advantage of accommodating zeros. Figure 2.4 Panel (a) depicts the estimates. Panel (b) depicts the estimates when using either non-winsorized weights or no weights. Panel (c) depicts the estimates when including municipality size-by-year controls that allow more and less urban municipalities to experience differential time trends.[37] Panel (d) depicts the estimates when controlling for leads and lags of local business tax changes due to the 2008 national tax reform, and when including both the effect of the national tax reform and local scaling factor changes in $\Delta\tau_{i,t}$. Panel (e) depicts the estimates when estimating a panel regression with municipality fixed effects as opposed to a first-differenced regression, and Panel (f) depicts the estimates when including one more lead and lag. The main takeaways, parallel pre-trends and a decrease in business entry following a local business tax increase, are robust across these perturbations.

For the estimation sample, I consider larger and smaller samples by altering the size requirement, I drop municipalities that are in the top one percentile of the municipality-size distribution, I restrict the sample to municipalities that experience less than a fixed number of tax changes between 2000-2016, and I drop municipalities that experienced a tax decrease between 2000-2016. Figure 2.5 Panel (a) depicts the estimates for a larger set of municipalities, including municipalities with more than 200 workers on average between 2004-2012, as well as a smaller set of municipalities, including only municipalities with more than 3,000 workers on average. Panel (b) depicts the estimates when dropping municipalities that are in the top one percentile of the municipality-size distribution (more than 33,000 workers on average). Panel (c) depicts the estimates when restricting attention to municipalities that experience less than five, four, three or two tax changes between 2000-2016. Panel (d) depicts the estimates when dropping municipalities that experienced a tax decrease between 2000-2016 both for the year-on-year change in the local business tax rate as well as for a dummy variable that is one in the case of a tax increase. Note that for the latter regression, the estimates are scaled by the average local business tax change conditional on the change being a tax increase (+0.683 percentage points). The main takeaways, parallel pre-trends and a decrease in business entry following a local business tax increase, are robust across these perturbations.

[36]For an outcome y_t, the DHS growth rate between t and $t-1$ is given by $\frac{y_t - y_{t-1}}{0.5*(y_t + y_{t-1})}$ if $y_t + y_{t-1} \neq 0$ and 0 otherwise. The inverse-hyperbolic sine transformation is $\ln\left(y_t + \sqrt{y_t + 1}\right)$.

[37]Municipality size is measured as the median number of workers in the municipality between 2004-2012.

2.4.2 Examining Spillovers

Spillovers to other legal forms New businesses can choose to not incorporate, raising the question to what extent the drop in corporate business entry reflects new businesses choosing an alternative legal form. The identification discussion above already investigated the impact of local business tax increases on unincorporated business entry, finding no effect. In the data, the legal form is not perfectly observed, so that some businesses are classified neither as unincorporated nor incorporated. I therefore also investigate the impact of a business tax increase on the total (corporate, unincorporated and other) number of new businesses.

The exercise suggests that the decrease in corporate business entry following a local business tax increase does not reflect new businesses choosing an alternative legal form. Figure 2.6 Panel (a) depicts the estimates. There is no detectable effect on total business entry prior to a tax increase, and total business entry drops following a tax increase. An advantage of the scaled outcome is that it is straightforward to calculate what share of the drop in total business entry is due to the drop in corporate business entry. The medium-term effect of a one-percentage point increase in the local business tax rate on scaled total business entry is -0.186pp, see Table 2.3 Column (3). Recalling that the impact on scaled corporate business entry is -0.136pp, the decrease in corporate business entry accounts for approximately 73% of the total decrease in business entry.[38]

Spillovers to neighboring municipalities When a given municipality increases its business tax rate, potential entrants can choose to enter in a neighboring municipality. While the distinction between one less entrant in total and one less entrant in her municipality is potentially not a first-order concern for a local policy maker, the same is not true for a national policy maker. Moreover, understanding to what extent the estimates are driven by relocation effects is important for the implications of the estimates for business tax changes at the state or national level. To examine how important relocation effects are, I consider coarser geography-by-year fixed effects in the regression. If relocation decisions to neighboring municipalities are important, the estimates should be larger when making comparisons with a more geographically proximate set of municipalities. Compared to other municipalities, geographically proximate municipalities would experience an increase in business entry due to relocation effects. Because neighboring municipalities act as control municipalities in the regression, the increase in neighboring municipalities will make the decrease in entry in the municipality that increases its tax rate appear more

[38] Approximately 20% is accounted for by unincorporated business entry, with the remainder due to other businesses (e.g. unclassified, public institution or charitable organizations).

pronounced.

The exercise suggests that spillovers to geographically proximate municipalities partly, although not entirely, offset the decrease in business entry in a municipality that increases its business tax rate.[39] Figure 2.6 Panel (b) depicts the estimated medium-term effects of a local business tax increase on scaled business entry for various levels of geographic controls. The main specification includes state-by-year and commuting zone-by-year fixed effects. The estimates are very similar when instead making comparisons with the narrower set of municipalities that are in the same county (county-by-year fixed effects), suggesting a limited role for spillovers. The estimates are somewhat attenuated when instead making comparisons with a larger set of municipalities, either all municipalities in the same state or municipalities in the same governmental district, suggesting that the total decrease in business entry is less than the decrease in the municipality that increases its tax rate. Finally, compared to including state-by-year effects, the estimates are not further attenuated when including only year fixed effects and making comparisons with all German municipalities.[40]

2.4.3 Examining Heterogeneity

Firms vs. establishments The decrease in business entry following a local business tax increase is primarily driven by a decrease in new single-establishment firms. New businesses can either be new establishments of multi-establishment firms or new single-establishment firms. Startups and the concept of entrepreneurship are more accurately captured by single-establishment firms (Decker et al., 2014), raising the question to what extent the decrease in business entry is due to a decrease in the entry of single-establishment firms. To investigate this question, I replace the year-on-year change in scaled business entry with the scaled change in (corporate) single-establishment firm entry. Table 2.4 Column (2) reveals that the estimated medium-term effect of a one-percentage point increase in the local business tax increase on scaled single-establishment firm entry is -0.135pp. The medium-term effect for scaled business entry is -0.136pp, so that single-establishment firms account for approximately 100% of the decrease. Because single-establishment firms account for approximately 100% of the decrease in business entry but account for only 73% of business entry on average, the estimates imply a disproportionate impact of local business taxes on single-establishment

[39] A back-of-the-envelope calculation in Appendix B.1.2 suggests that approximately 30% of the decrease in business entry is due to relocation effects

[40] In the main analysis sample, there are on average 12 municipalities per county, 20 per commuting zone-state cell, 94 per governmental district and 367 per state.

firm entrants.

Small vs. large entrants The decrease in business entry following a local business tax increase is primarily driven by entrants with less than three employees. Startups vary in size, and, all else equal, policymakers arguably care more about a decrease in the entry of large than small businesses. I operationalize this analysis by separately considering the year-on-year change in the scaled entry of businesses with one employee and the scaled change in the entry of businesses with one or two employees. Table 2.4 Column (3) presents the estimated medium-term effect for business entrants with less than two and Column (4) presents the estimated effect for business entrants with less than three employees. The results indicate that entrants with less than two employees account for approximately 48% of the total decrease in business entry, and entrants with less than three employees account for approximately 100% of the decrease. Because entrants with less than three employees account for approximately 53% of business entry on average, the estimates implies a disproportionate impact of local business taxes on small entrants.

Non-tradable vs. tradable Entrants in the non-tradable sector contribute somewhat more to the decrease in business entry following a local business tax increase than entrants in the tradable sector. I classify following sectors as non-tradable: hospitality, construction, retail sales and real estate. All other sectors are classified as tradable. The dependent variable in the main specification now corresponds to the year-on-year scaled change of business entrants in the non-tradable sector. Table 2.4 Column (5) presents the estimated medium-term effect. The estimate suggests that entrants in the non-tradable sector account for approximately 60% of the total decrease in business entry. Because business entrants in the non-tradable sector account for approximately 76% of business entry on average, the estimates implies a weaker impact of local business taxes on entrants in the non-tradable vs. tradable sector.

2.5 Discussion

Dynamic pattern of estimates in Figure 2.2 Business entry starts to decrease in the reform year and continues to decrease over the subsequent two-three years. One year after a local business tax increase, the decrease is entry is approximately 60% of the medium-term effect, with the full medium-term effect only materializing three years after the tax increase. While a gradual adjustment of the stock of capital to changes in taxes is unsurprising, a gradual adjustment of investment raises questions.

One possible explanation for the gradual decrease in business entry is that the costs of starting a business are largely incurred a year or more before the business actually enters. For the entrepreneur who intends to open her business in the reform year, the cost of entry is sunk, and the business tax increase has less of an impact on the marginal decision of whether to enter or not. In contrast, a potential entrepreneur who has not yet incurred any costs of entry will be more impacted by the tax increase, but her decision not to open a business will only appear as one less entry a number of years in the future.

Disproportionate impact on small and single-establishment firms Small and single-establishment firm entry is disproportionately impacted by a local business tax increase. As discussed above, this subset of business entrants can account for almost the entire decrease in business entry following a tax increase, despite accounting for only 53%-73% of business entry on average.

I consider two possible explanations for the disproportionate impact of local business taxes on small and single-establishment firm entry. The first explanation is that small firms are closer to the margin of entry, and therefore more easily discouraged from entering by a local business tax increase. According to this explanation, small firms are small because they have a low productivity, and the profits they earn are only marginally larger than the non-deductible fixed cost of entry. The second explanation is that the asymmetry in how the tax system treats profits and losses is particularly distortive for small businesses, so that a local business tax increase reduces expected profits more for small than for large entrants. According to the second explanation, small firms are more risky than large firms and more likely to incur losses. Moreover, conditional on experiencing a loss, small firms are more likely to exit, so that they benefit less from the ability to offset losses against profits than larger firms.

Note that in the first explanation the disproportionate impact of a local business tax increase on small and single-establishment firm entry is due to a primitive feature of the economic environment, whereas in the second explanation it is due to a malleable feature of the policy environment.

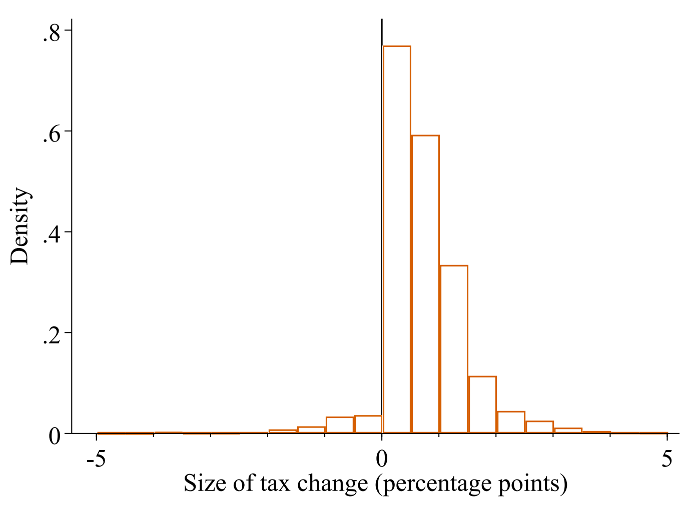
Partial equilibrium, general equilibrium and relocation effects The estimates in this paper arguably reflect a combination of partial equilibrium and relocation effects. Business taxes discourage entry by reducing the expected post-tax profit associated with starting a business compared to a fixed cost of entry or an outside option as an employee. In general equilibrium, price changes, especially wage decreases, can attenuate the partial equilibrium effect of business taxes. Given the institutional setting of this paper, municipality-level business taxes where individual municipalities are part of larger local and regional economies, such general equilibrium price adjustments are less likely to offset the direct impact of local business taxes than they would for national business taxes. Moreover, given that for any one municipality there is a set of geographically proximate municipalities that have identical institutions and face similar economic conditions, relocation of potential entrants likely further accentuates the effect of local business taxes above what might be expected for national business taxes. The estimates depicted in Figure 2.6 and discussed above are consistent with a role for relocation effects.

2.6 Conclusion

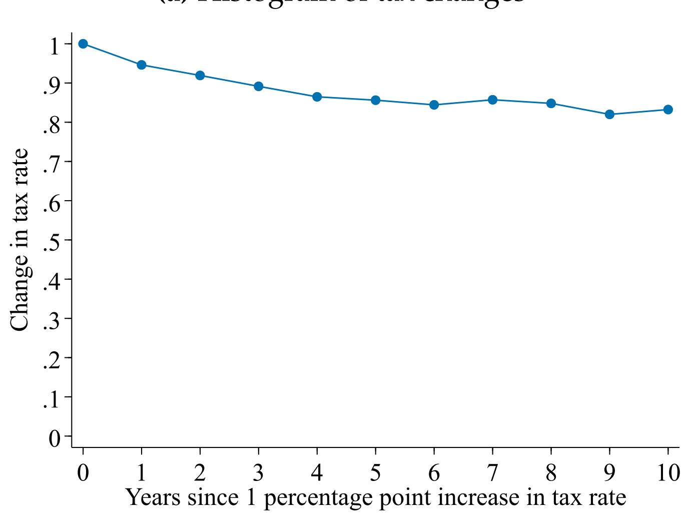
This paper estimated the effect of local (municipal) business taxes on business (corporate employer establishment) entry by combining rich and clean tax rate variation with administrative data on the population of German business entrants between 2004-2012. The results indicate that an increase in the local business tax rate reduces the amount of business entry, and that the decrease is almost entirely accounted for by single-establishment firms with less than three employees. Given the role that business entry plays in theories of endogenous growth (Romer, 1990) as well as recent evidence on the contribution of startups to local growth (Walsh, 2019), the estimates in this paper suggest that local business tax increases might have meaningful impacts on the long-run growth of municipalities that increase their tax rate. Using the estimated impact of local business taxes on business entry to quantify the long-term economic impact of such taxes would be an interesting exercise for future research.

Compared to estimates based on higher-level tax changes, e.g. U.S. state or national tax changes, the estimates based on local business tax changes are likely less affected by general equilibrium price adjustments and more affected by relocation effects. Decomposing the total effect of a local business tax change into a partial and general equilibrium as well as a relocation effect would be a fruitful avenue for future research, as it would clarify what the estimates from this paper imply for tax changes at higher levels.

Figures



(a) Histogram of tax changes



(b) Persistence of tax changes

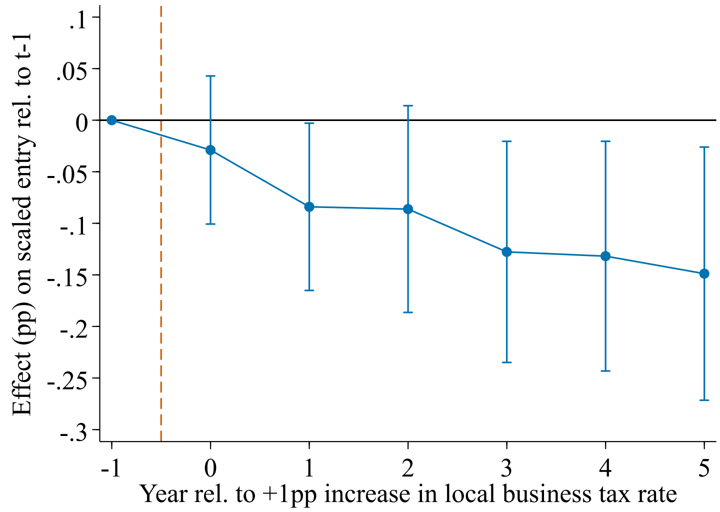
Figure 2.1: Local business tax variation

Note: This figure illustrates variation in the local business tax rate within municipalities over time and the persistence of local business tax rate changes. The sample consists of all non-merged municipalities. The data include all scaling factor-induced local business tax changes between 2000-2016. Panel (a) contains a histogram for year-on-year changes in the local business tax rate induced by changes in the municipal scaling-factor. The deductibility of the municipal tax from the tax base prior to 2008 is taken into account. For 2008, the year in which the deductibility was eliminated and the federal base rate changed, I calculate the year-on-year change assuming that the 2008 federal policies applied in 2007. Panel (b) depicts the persistence of local business tax changes induced by changes in the municipal scaling factor. The figure depicts the β_h coefficients from the regression

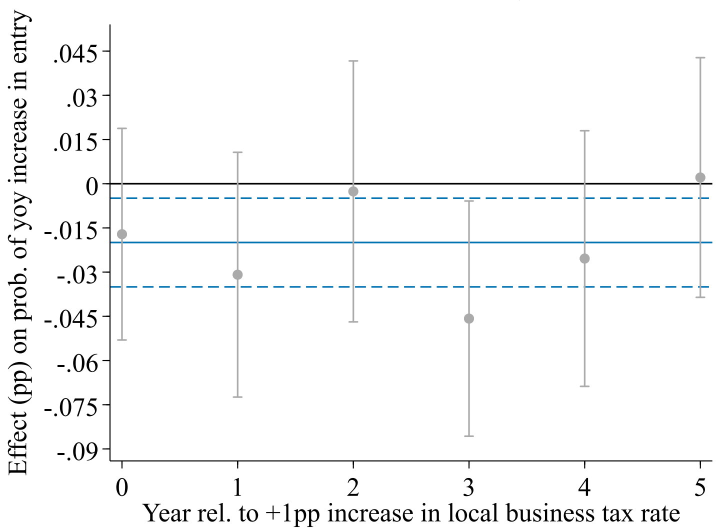
$$\theta_{i,t+h} - \theta_{i,t-1} = \beta_h \left(\theta_{i,t} - \theta_{i,t-1}\right) + \gamma_t + \epsilon_{i,t}$$

for $h \in \{1,2,3,4,5,6,7,8,9,10\}$ where $\theta_{i,t}$ is the scaling factor in municipality i in year t and γ_t is a year fixed effect. At any horizon h, $\hat{\beta}_h$ indicates how much of a given change in the scaling factor between t and $t-1$ persists h periods in the future.

Source: Federal Statistical Office and Statistical Offices of the Laender, Hebesaetze der Realsteuern, 1996-2016, own calculations.



(a) Effect (pp) on scaled business entry rel. to $t-1$

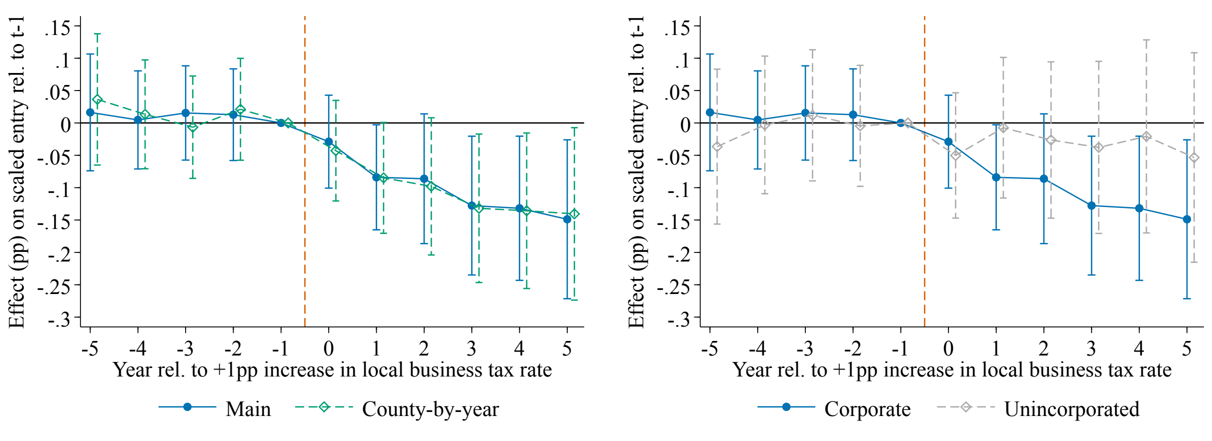


(b) Effect (pp) on prop. of yoy increase in business entry

Figure 2.2: Effects of local business taxes on business entry

Note: This figure depicts estimates of how a local business tax increase impacts business entry. A business entrant corresponds to a new corporate employer establishment. Panel (a) depicts the cumulated post-reform coefficients from the distributed lag regression (2.3.1), $\hat{\beta}_t = \sum_{k=0}^{t} \hat{\delta}_k$ with $t \in \mathcal{T} = \{0, \ldots, 5\}$ and with $\hat{\beta}_{-1} = 0$, where the outcome variable in the regression is the scaled year-on-year change in business entrants, scaled by the average population of all (corp., unincorp. and other) businesses in the municipality between 2004-2012. The estimates indicate the percentage point change in scaled business entry between one period prior to and t periods post a local business tax increase of one-percentage point. Panel (b) depicts the post-reform coefficients from the distributed lag regression (2.3.1), $\hat{\delta}_k$ with $k \in =\{0, \ldots, 5\}$, where the outcome variable is an indicator variable that equals one in the case of a year-on-year increase in business entry and zero otherwise. Each estimate therefore indicates the percentage point impact of a local business tax increase of one-percentage point on the probability of experiencing a year-on-year increase in entry k periods after the tax increase. The horizontal lines that span the figure indicate the average of the coefficients as well as the 95% confidence interval of the average. Both panels indicate that a local business tax increase reduces business entry. See Section 2.3.3 for more information on the regression and 2.4 for a discussion of the results.

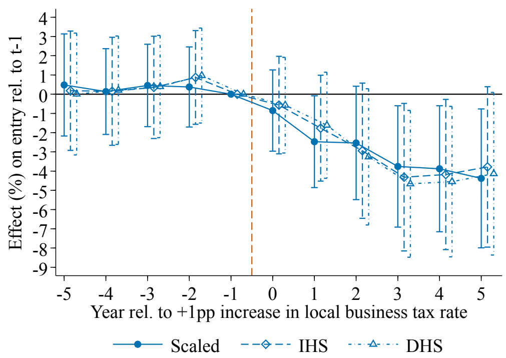
Source: RDC of the Federal Statistical Office and Statistical Offices of the Laender, AFiD-Panel Unternehmensregister - System 95, Berichtsjahre 2002-2012, own calculations.



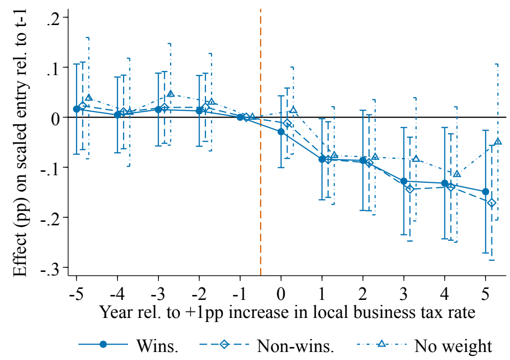
(a) Pre-reform est. & county-by-year fe **(b)** Unincorp. business entry vs. corp. entry

Figure 2.3: Probing identification

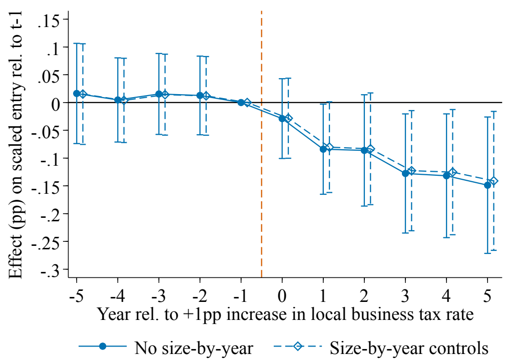
Note: This figure depicts estimates of how (corporate) business entry evolves in municipalities that increase their local business tax rate prior to a tax increase, estimates using more granular geography-by-year fixed effects and estimates for unincorporated business entry. The purpose is to investigate the credibility of the identification assumption necessary for a causal interpretation of the results. Panel (a) depicts the cumulated pre- and post-reform coefficients from the distributed lag regression (2.3.1), where the outcome variable is the scaled year-on-year change in (corporate) business entrants. The estimates are depicted both for the main specification and for one that includes county-by-year fixed effects. Panel (b) depicts the cumulated post-reform coefficients from the distributed lag regression (2.3.1) using either the scaled year-on-year change in (corporate) business entry or unincorporated business entry as the outcome variable. Unincorporated businesses are less impacted by local business taxes, but are similarly impacted by local economic conditions. Panel (a) indicates that the parallel trends assumption was satisfied in the pre-reform period, and that controlling for more granular geography-by-year fixed effects does not impact the result. This suggests the post-reform decrease in (corporate) business entry does not reflect the continuation of a pre-existing trend or the impact of county-level economic shocks. Panel (b) indicates that the drop in (corporate) business entry is not mirrored by a drop in unincorporated business entry. This suggests that the post-reform decreases in (corporate) business entry is not attributable to economic shocks at the municipality level that are also triggering tax increases. See Section 2.4 for details.
Source: RDC of the Federal Statistical Office and Statistical Offices of the Laender, AFiD-Panel Unternehmensregister - System 95, Berichtsjahre 2002-2012, own calculations.



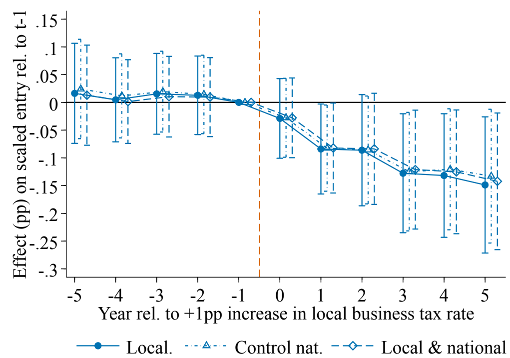
(a) Alternative outcome



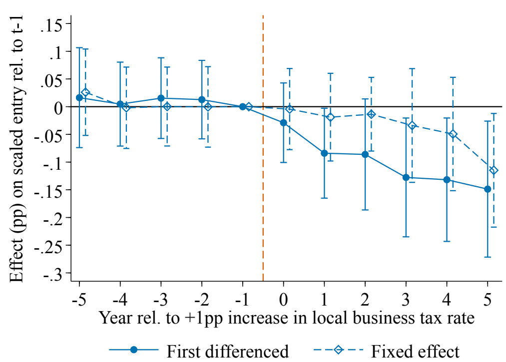
(b) Alternative weights



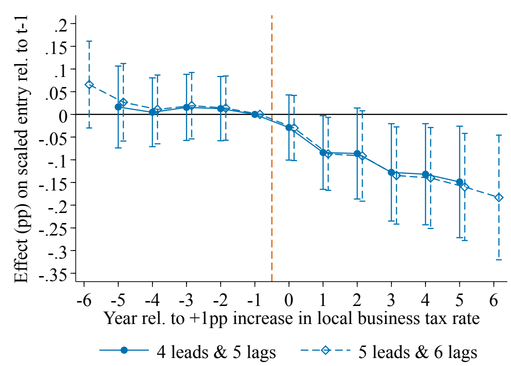
(c) Size-by-year controls



(d) Tax rates



(e) Fixed effect

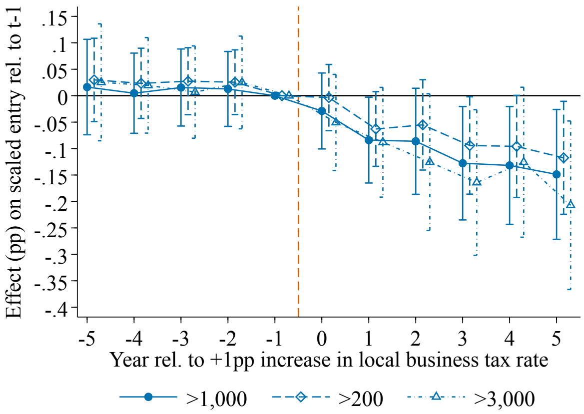


(f) Lead and lag structure

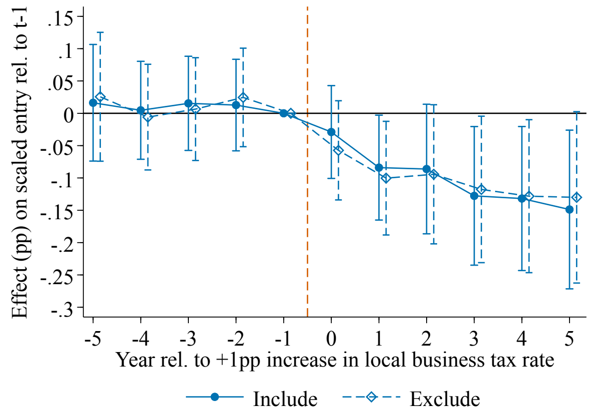
Figure 2.4: Robustness 1/2

Note: This figure depicts estimates from a variety of alternative specifications that are intended to assess the robustness of the main result. The estimates correspond to cumulated coefficients from the distributed lag regression. The outcome, unless otherwise noted, is the scaled year-on-year change in business entry. Panel (a) depicts estimates using alternative outcome transformations. Note that the scaled estimate is transformed to arrive at the implied semi-elasticity and that IHS is the inverse hyperbolic sine transformation. Panel (b) depicts estimates using alternative weights. Panel (c) depicts estimates when controlling for municipality size-by-year effects. Panel (d) depicts estimates when including both local and national policy induced changes to the local business tax rate, and when separately controlling for changes in the local tax rate due to national tax policy. Panel (e) depicts estimates from a fixed effect regression. Panel (f) depicts estimates when including one additional lead and lag of local tax rate changes. The main results, parallel pre-trends and a decrease in business entry following a local business tax increase, are robust across specifications. See Section 2.4 for more details.

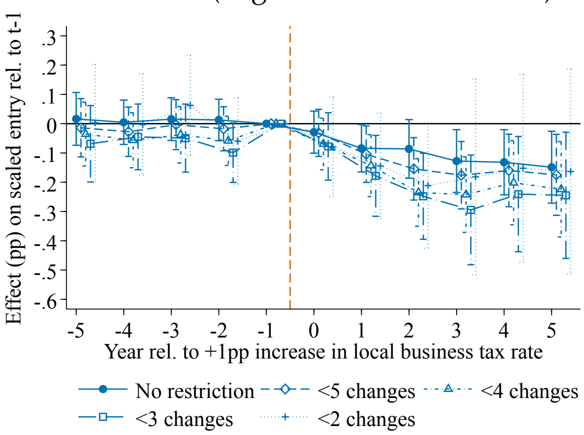
Source: RDC of the Federal Statistical Office and Statistical Offices of the Laender, AFiD-Panel Unternehmensregister - System 95, Berichtsjahre 2002-2012, own calculations.



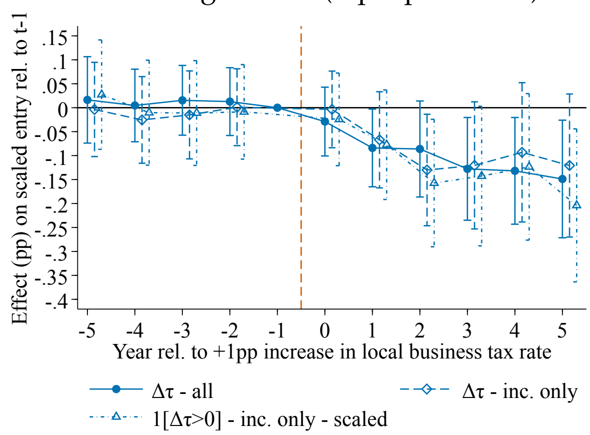
(a) Muni. size (avg. # workers 2004-2012)



(b) Excl. large muni. (top 1 percentile)



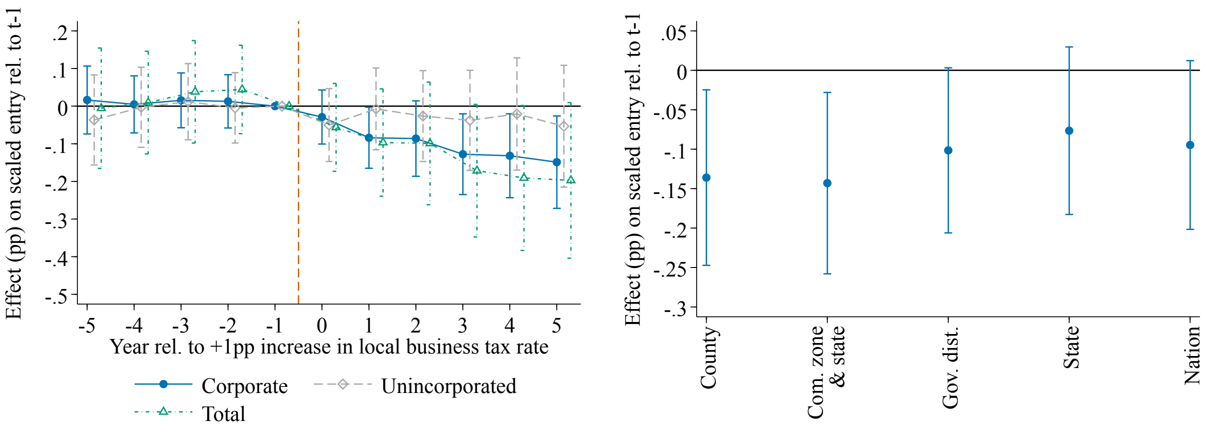
(c) Restrict number of tax changes



(d) Exclude tax decreases

Figure 2.5: Robustness 2/2

Note: This figure depicts estimates from a variety of alternative samples that are intended to assess the robustness of the main result. The estimates correspond to cumulated coefficients from the distributed lag regression. The outcome is the scaled year-on-year change in business entry. Panel (a) depicts estimates when adding smaller municipalities, those with more than 200 workers on average between 2004-2012, to the sample or excluding medium-sized municipalities, those with less than 3,000 workers on average between 2004-2012, from the sample. Panel (b) depicts estimates when excluding municipalities with an average number of workers in the top one precentile of the municipality-size distribution. Panel (c) depicts the estimates when restricting the sample by the number of tax changes in the municipality. Panel (d) depicts the estimates when dropping municipalities that experienced a tax decrease between 2000-2016 both for the year-on-year change in the local business tax rate as well as for a dummy variable that is one in the case of a tax increase. Note that for the latter regression, the estimates are scaled by the average local business tax change conditional on the change being a tax increase (+0.683 percentage points). The main results, parallel pre-trends and a decrease in business entry following a local business tax increase, are robust across specifications. See Section 2.4 for more details.
Source: RDC of the Federal Statistical Office and Statistical Offices of the Laender, AFiD-Panel Unternehmensregister - System 95, Berichtsjahre 2002-2012, own calculations.



(a) Spillovers to other legal forms **(b)** Spillovers to neighboring municipalities

Figure 2.6: Examining spillover effects

Note: This figure depicts estimates of how a local business tax increase impacts (corporate) business entry, unincorporated business entry, total business entry and the medium-term effects on (corporate) business entry using a variety of geography-by-year fixed effects. The purpose is to investigate spillover effects to alternative legal forms, Panel (a), and to neighboring municipalities, Panel (b). Panel (a) depicts the cumulated post-reform coefficients from the distributed lag regression (2.3.1) using either the scaled year-on-year change in (corporate) business entry, scaled change in unincorporated business entry or the scaled change in total (corporate, unincorporated and other) business entry as the outcome variable. The purpose here is to examine whether the drop in corporate business entry simply reflects new businesses chosing to not incorporate. Panel (b) depicts the medium-term effects of a local business tax increase on scaled (corporate) business entry, where the medium-term effect is the average of the three-, four- and five-year effect ,$\frac{1}{3}\sum_3^5 \hat{\beta}_k$, using either county-by-year, commuting zone-by-year and state-by-year, government district-by-year, state-by-year or only year fixed effects. If spillovers to neighboring municipalities are present, then the estimates based on more granular geography-by-year fixed effects should be accentuated compared to estimates using coarser controls. Panel (a) reveals that the drop in (corpoarte) business entry does not reflect new businesses chosing to not incorporate, as the drop in (corporate) business entry is accompanied by a drop in total business entry. Panel (b) suggests that the drop in business entry in the municipality that increases its tax rate somewhat overstates the total decrease in entry. See Section 2.4 for details.
Source: RDC of the Federal Statistical Office and Statistical Offices of the Laender, AFiD-Panel Unternehmensregister - System 95, Berichtsjahre 2002-2012, own calculations.

Tables

	Five-year effect ($\hat{\beta}_5$)	Med.-term effect ($\frac{1}{3}\sum_3^5 \hat{\beta}_k$)	Cumulated effect ($\sum_0^5 \hat{\beta}_k$)
	(1)	(2)	(3)
Panel A. Scaled entrants			
Estimate	-0.149	-0.136	-0.607
Standard error	(0.063)	(0.053)	(0.240)
Panel B. Sample characteristics (avg.)			
# Scale variable	1,628	1,628	1,628
# Entrants	55	55	55
Panel C. Implied effect of +1pp local busines tax rate on			
# Entrants	-2.4	-2.2	-9.9
% Entrants	-4.4	-4.0	-18.0
Weights	Yes	Yes	Yes
State x year FE	Yes	Yes	Yes
Com. zone x year FE	Yes	Yes	Yes
Municipalities	2,567	2,567	2,567
Obs.	20,536	20,536	20,536

Table 2.2: Effects of local business taxes on business entry

Note: This table presents estimated effects of a local business tax increase on business entry. A business entrant corresponds to a new corporate employer establishment. Panel A presents estimates based on cumulated coefficients from the distributed lag model (2.3.1), where the outcome is the scaled year-on-year change in business entrants, scaled by the average population of all (corp., unincorp. and other) establishments in the municipality between 2004-2012. Column (1) presents the estimated effect on business entry five years after a tax increase, $\hat{\beta}_5 = \sum_0^5 \hat{\delta}_k$. Column (2) presents the medium-term estimated effect on business entry, defined as the average of the three-, four- and five-year effect,$\frac{1}{3}\sum_3^5 \hat{\beta}_k$. Column (3) presents the cumulated effect over the post-reform period, $\sum_0^5 \hat{\beta}_k$. Panel B presents the average scale variable and the average number of business entrants acorss municipalities. Panel C presents the implied effect of a one-percentage point increase in the local business tax rate on the number of entrants and the percentage change in the number of entrants. The former is obtained by multiplying the scaled estimate by the average scale variable. The latter is obtained by dividing the change in the number of entrants by the average number of entrants. The estimates indicate that an increase in the local business tax rate reduces business entry. See Section 2.3.3 for more information on the regression and 2.4 for a discussion of the results.
Source: RDC of the Federal Statistical Office and Statistical Offices of the Laender, AFiD-Panel Unternehmensregister - System 95, Berichtsjahre 2002-2012, own calculations.

	Lag 3 $\left(\widetilde{\text{Entry}}_{t+3} - \widetilde{\text{Entry}}_{t-1}\right)$	Lag 4 $\left(\widetilde{\text{Entry}}_{t+4} - \widetilde{\text{Entry}}_{t-1}\right)$	Lag 5 $\left(\widetilde{\text{Entry}}_{t+5} - \widetilde{\text{Entry}}_{t-1}\right)$
	(1)	(2)	(3)
Panel A. Differences			
Tax Increase Municipalities	0.0001	0.0000	-0.0007
Control Municipalities	0.0017	0.0013	0.0006
Difference	*-0.0016*	*-0.0013*	*-0.0013*
Panel B. Sample charatersitics			
# Municipalities	847	847	847
Avg. tax increase (pp)	+0.52	+0.52	+0.52
Avg. scale variable	1,625	1,625	1,625
Avg. # (corp.) business entrants	55	55	55
Panel C. Implied effect of +1pp local busines tax rate on			
Scaled entry (pp)	-0.31	-0.25	-0.25
# of (corp.) business entrants	-5.0	-4.1	-4.1
% of (corp.) business entrants	-9.1	-7.5	-7.5

Table 2.1: Non-parametric evidence on the effect of local business taxes on business entry

Note: This table presents non-parameteric evidence on how local business taxes affect business entry. A business entrant corresponds to a new corporate employer establishment. The outcome variable corresponds to scaled change in business entrants between one year prior to and three, four or five years after a tax increase, scaled by the average population of all (corp., unincorp. and other) businesses in the municipality between 2004-2012. Scaled business entry is denoted by $\widetilde{\text{Entry}}$. The approach is as follows. Among municipalities in the analysis sample, I restrict attention to the subset of municipalities that experience either zero or one tax change between 2000-2012. Among municipalities that experience one tax change, I further restrict attention to those that experience a tax increase between 2005-2007. I then compare four-, five- and six-year changes in business entry between municipalities that increase their tax rate and other municipalities in the same state that did not. Formally, for the six-year change (lag 5), I proceed as follows. For municipalities that increase their tax rate in 2005, I calculate the change in entry between 2010 and 2004. I do the same for all control municipalities. I proceed similarly for municipalities that increase their tax rate in 2006 and 2007. I then stack these differences and regress the differences on a dummy which equals 1 in the case of a tax increase, controlling for state-by-year fixed effects. Municipalities are weighted as in the main regression. The difference is the estimated coefficient on the dummy, and I calculate the change in entry for municipalities that increased their tax rate by adding the coefficient to the mean change among control municipalities. See Section 2.4 for details.
Source: RDC of the Federal Statistical Office and Statistical Offices of the Laender, AFiD-Panel Unternehmensregister - System 95, Berichtsjahre 2002-2012, own calculations.

	Spillovers to other legal forms			Spillovers to neighboring municipalities				
	Corp.	Unincorp.	Total	County	Com. zone & state (main)	Gov. dist.	State	Nation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Med.-term effect ($\frac{1}{3}\sum_3^5 \hat{\beta}_k$)	-0.136	-0.037	-0.186	-0.136	-0.143	-0.101	-0.077	-0.095
Standard error	(0.053)	(0.069)	(0.088)	(0.057)	(0.059)	(0.053)	(0.054)	(0.055)
Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geo. x year FE	Main	Main	Main	County	Main	Gov Dist.	State	Nation
Municipalities	2,567	2,567	2,567	2,476	2,476	2,476	2,476	2,476
Obs.	20,536	20,536	20,536	19,808	19,808	19,808	19,808	19,808

Table 2.3: Examining spillover effects

Note: This table presents estimated medium-term effects of a local business tax increase on (corporate) business entry, unincorporated business entry, total business entry as well as (corporate) business entry when using alternative regional controls. The purpose is to investigate spillover effects to alternative legal forms, Columns (1)-(3), and to neighboring municipalities, Columns (4)-(8). Column (1) reprints the estimate from the main specification (2.3.1) using year-on-year scaled change in (corporate) business entry, Column (2) uses the change in scaled unincorporated business entry and Column (3) uses the change in scaled total (corporate, unincorporate and other) business entry as the outcome variable. The purpose here is to examine whether the drop in (corporate) business entry simply reflects new businesses chosing to not incorporate. Columns (4)-(8) present estimated medium-term effects of local business tax increases on scaled (corporate) business entry using a variety of geography-by-year controls. Column (4) replaces the commuting zone-by-year and state-by-year fixed effects from the main specification with county-by-year fixed effects. The decrease in the number of municipalities is due to singleton cells. Column (5) uses the sample from (4) but commuting zone-by-year and state-by-year instead of county-by-year fixed effects. Column (6) uses government district-by-year fixed effects, Column (7) uses state-by-year fixed effects and Column (9) uses only year fixed effects. If spillovers to neighboring municipalities are present, then the estimates based on more granular geography-by-year fixed effects should be accentuated compared to estimates using coarser controls. The results from Columns (1)-(3) do not reveal a role for spillovers to alternative legal forms of operation. The results from Columns (4)-(8) suggest that spillovers to neighboring municipalities are partly responsible for the decrease in entry in the municipality that increases its tax rate. See Section 2.4 for more details.
Source: RDC of the Federal Statistical Office and Statistical Offices of the Laender, AFiD-Panel Unternehmensregister - System 95, Berichtsjahre 2002-2012, own calculations.

	Business entrants	Single-est. firms	Small (< 2 empl.)	Small (< 3 empl.)	Non-tradable
	(1)	(2)	(3)	(4)	(5)
Med.-term effect ($\frac{1}{3}\sum_3^5 \hat{\beta}_k$)	-0.136	-0.135	-0.065	-0.137	-0.082
Standard error	(0.053)	(0.041)	(0.029)	(0.037)	(0.042)
Weights	Yes	Yes	Yes	Yes	Yes
State x year FE	Yes	Yes	Yes	Yes	Yes
Com. zone x year FE	Yes	Yes	Yes	Yes	Yes
Municipalities	2,567	2,567	2,567	2,567	2,567
Obs.	20,536	20,536	20,536	20,536	20,536

Table 2.4: Heterogeneous effects by type of entrant

Note: This table presents estimated medium-term effects of a local business tax increase on the number of business entrants, the number of business entrants that are firms, the number of business entrants that are small with less than two employees, the number of business entrants that are small with less than three employees and the number of business entrants that are in the non-tradable sector. The purpose is to investigate whether the quality of entrants responds in addition to the quantity. The estimates are based on the main specification (2.3.1), where the outcome in Column (1) is the scaled year-on-year change in business entry, in Column (2) is the scaled change in the number of business entrants that are firms, in Column (3) is the scaled change in the number of business entrants that have less than two employees at entry, in Column (4) is the scaled change in the number of business entrants that have less than three employees at entry and in Column (5) is the scaled change in the number of business entrants that are in the tradable sector. The results reveal that single-establishment firms and small entrants with less than three employees account for almost the entire decrease in business entry following a tax increase. For more details, see Section 2.4.
Source: RDC of the Federal Statistical Office and Statistical Offices of the Laender, AFiD-Panel Unternehmensregister - System 95, Berichtsjahre 2002-2012, own calculations.

CHAPTER III

The Role of Firms in Transmitting Worker-Level Policies into Wages: Evidence from Payroll Taxes

3.1 Introduction

The effect of a worker-level policy (e.g. mandated benefits, employee protection laws, payroll taxes) on wages is typically studied in a competitive framework at an aggregate level. In a competitive framework, the impact of a worker-level policy on wages depends on characteristics of aggregate labor supply and demand, in particular the supply and demand elasticities. Since firms take market wages as given, there is no systematic variation in the wage effect of a policy across firms. However, a growing body of empirical research provides evidence that labor markets are not perfectly competitive and that individual firms play an important role in the wage determination process.[1] When wages are determined at the firm level, how does the effect of a worker-level policy on wages vary across firms?

This paper provides a theoretical and empirical answer to the question by studying the role of firms in transmitting a particularly prominent worker-level policy into wages: payroll taxes. Payroll taxes are commonly used to collect social security contributions.[2] The average OECD member in 2016 collected 9.2% of GDP and 26.2% of total tax revenue in payroll taxes. For the average worker in the average OECD country, the wedge created by payroll taxes, defined as the ratio of total payroll tax liability to total labor costs, amounted to approximately 22% in 2018 (OECD, 2019).

[1]See Manning (2011) and Card et al. (2018) for surveys.

[2]If the social security system is actuarially fair, then social security contributions are not a tax. Many social security systems are not actuarially fair. For example, in Germany, England, and France public health insurance is part of the social security system and benefits in the public health insurance system do not increase with contributions.

The first part of this paper develops a static wage-posting model to study payroll tax incidence in an environment characterized by firm labor market power. The model illustrates that the determinants of payroll tax incidence at the firm level are: 1) the effect of payroll taxes on labor costs, 2) the labor intensity of a firm, 3) the ability of a firm to substitute between production inputs, 4) the extent to which payroll taxes are passed through to output prices, and 5) the elasticity of labor supply to the firm. When the payroll tax applies uniformly to all workers, the effect on labor costs is constant across firms. The incidence of payroll taxes on wages is then generally more pronounced at firms that are more labor intensive, that are more easily able to substitute away from labor, that cannot pass payroll taxes through to higher output prices, and that have more labor market power. When the payroll tax varies across different worker types, the labor composition of firms determines the extent to which payroll taxes increase labor costs.[3] Although these results apply to payroll taxes, the model can easily be adapted to study other worker-level policies and the results reinterpreted accordingly.

Given the magnitude of payroll taxes, variation in the incidence of payroll taxes across firms can lead to considerable variation in observed wages across firms. Moreover, firm-level payroll tax incidence can lead to the type of systematic wage differences across firms captured by firm wage premiums (Card et al., 2013). In the model, productivity and amenity differences across firms do not lead to variation in the incidence of payroll taxes across firms. Wage dispersion across firms due to differences in the incidence of payroll taxes is thus orthogonal to wage dispersion due to productivity and amenity differences.[4] In contrast, because the incidence of payroll taxes on workers is increasing in the labor market power of a firm, wage dispersion across firms with different labor market power is accentuated by payroll taxes.

The second part of this paper empirically studies to what extent the incidence of payroll taxes is a firm-level phenomenon. In particular, I combine linked employer-employee data based on administrative social security earnings records with a German payroll tax reform to examine whether the wage responses of workers who were similarly affected by the payroll tax reform varied across firms.

Payroll taxes in Germany are used to collect social security contributions. There are four different payroll taxes: health care, long-term care and unemployment insurance as well as pensions. Each payroll tax features a constant marginal rate up to an earnings

[3]Firms that experience a greater increase in labor costs decrease the total amount of labor more, however they also have different substitution patterns across different worker types. While the former increases the share of the payroll tax burden that falls on workers, the latter can reduce it.

[4]Amenity and productivity differences across firms are two commonly discussed sources of wage dispersion across firms. See Card et al. (2018) and Sorkin (2018) for examples.

threshold beyond which the marginal rate drops to zero. In 2000, the year before the reform, the total payroll tax rate for the average worker amounted to 41.1%.

The reform studied in this paper increased the payroll tax cap in East Germany in 2001 for health care and long-term care from EUR32.6k to EUR40k and created a set of treated and not-treated workers.[5] The effect of the reform on a worker's payroll tax liability depended on her earnings. The reform treated workers with high earnings (those with earnings above the cap prior to the reform) but did not treat workers with lower earnings (those with earnings below the cap prior to the reform). Among treated workers, the reform increased the payroll tax liability by up to 6.9% or EUR1,126. The reform did not change the benefits for either set of workers.

I use the wage-posting model from the theoretical analysis to develop an empirical strategy that compares the wage responses of workers who are similarly affected by the reform between firms with different exposure levels to the reform in a difference-in-differences style framework. The approach holds the worker-level effect of the reform constant while varying a characteristic of the worker's employer. The exposure measure, the percentage increase in the firm's payroll tax liability due to the reform, is closely related to the effect of the reform on a firm's labor costs. The empirical analysis therefore tests whether the wage responses vary across firms along one of the dimensions highlighted by the wage-posting model. Because exposure to the reform is correlated with worker and firm characteristics, I exploit variation of the exposure measure within worker and firm characteristics. The identifying assumption is that, conditional on worker and firm characteristics, the wage growth of treated (not-treated) workers would not vary with the exposure level of a firm in the absence of the reform. To assess the plausibility of this assumption I compare the wage growth of workers who are similarly affected by the reform between high- and low-exposure firms prior to the reform. I find that wage growth was parallel, which is evidence in support of the identifying assumption.

Post reform, treated workers at high-exposure firms experienced wage decreases compared to treated workers at low-exposure firms. For a treated worker, a one standard deviation increase in the exposure measure of the firm is associated with a -1.2% decrease in the daily wage two years after the reform. For not-treated workers, I find that, post reform, workers at high-exposure firms experienced wage increases compared to workers at low-exposure firms. A one standard deviation increase in the exposure measure of the firm is associated with a 1.2%-1.4% increase in the daily wage of a not-treated worker.[6] The

[5]Benefits are not tied to contributions in either insurance so that the contributions more closely correspond to a tax.

[6]I find that working at a higher exposure firm has a more negative effect on the wage response of a worker if the firm operates in a labor intensive sector. I also provide suggestive evidence that the variation in

results provide evidence that the workplace matters for how a worker's wage is affected by a worker-level policy. In particular, they provide evidence that the workplace matters because of the composition of co-workers: for a given worker, how her wage is affected by the reform depends on the treatment status of her co-workers. The results stand in contrast to the prediction of a model without firm-level wage determination: no systematic variation across firms in the wage responses of workers who are similarly affected by the reform. The pattern of wage responses implies that the incidence of the payroll tax increase on the wages of treated (not-treated) workers is more (less) pronounced at high-exposure firms.

The wage increases for not-treated workers at high-exposure firms relative to low-exposure firms are consistent with firms increasing their demand for not-treated workers in order to substitute not-treated for treated workers in production. To examine this mechanism further, I test whether the relationship between firm exposure and the wage responses of not-treated workers depends on the substitutability of treated and not-treated workers. I operationalize this by idea by assessing substitutability based on occupation. I take a broad view of occupation and classify workers as having either a blue-collar occupation or a white-collar occupation. I compute the exposure separately for blue- and white-collar workers at each firm and then define, for each worker, the own exposure and the other exposure (e.g. for a blue-collar worker the own (other) exposure is the percentage increase in payroll tax liability among blue- (white-) collar workers).[7] I find that increases in the own exposure are the main driver of wage increases for not-treated workers, consistent with a substitution mechanism. Furthermore, I document that not-treated workers at high-exposure firms are less likely to leave the firm after the reform, consistent with high-exposure firms increasing the wages of not-treated workers to increase their retention probability.

Connections to the literature This paper connects to three literatures. The first is the nascent literature on the role of firms in transmitting policies into worker-level outcomes. Chetty et al. (2011), Best (2014), and Galassi (2018) document evidence that non-wage responses to worker-level policies are mediated through firms. Fuest et al. (2018b) show that the wage incidence of corporate taxes varies across workplaces. Two recent papers explicitly address worker-level policies: Bovini and Paradisi (2019) and Saez et al. (2019).

wage responses between high- and low-exposure firms is not due to liquidity constraints at high-exposure firms, and that the variation is more pronounced among non-unionized firms.

[7]This approach is similar in spirit to the one pursued by Jäger and Heining (2019), who show that the effect of an unexpected coworker death on an incumbent's wage depends on whether the incumbent and the deceased coworker have the same occupation.

Saez et al. (2019) is the closest paper to this one. The paper studies a large payroll tax cut for young workers in Sweden and finds that the wages of older workers (not treated) at firms which were more exposed to the reform increased in response to the reform. This paper contributes to the literature by systematically exploring the implications of firm-level wage determination on the mechanism through which worker-level policies are transmitted to wages. In particular, it provides a theoretical framework for studying the effects of a worker-level policy on wages at the firm level and illustrates the key mechanisms via the example of payroll taxes, and it provides new empirical evidence that the choice of workplace matters for how a worker's wage is affected by a worker-level policy.

By exploring how the incidence of payroll taxes is determined at the firm-level and providing evidence of variation across firms, the paper also contributes literature on the incidence of payroll taxes. A number of papers exploit natural experiments to empirically study payroll tax incidence using micro-level data at the market level. While Gruber (1994), Gruber (1997), and Deslauriers et al. (2018) find that the incidence is borne fully by workers, Saez et al. (2012a), Neumann (2017), and Saez et al. (2019) find that the incidence is shared between workers and firms. Neumann (2017) studies the same natural experiment considered here and concludes that the incidence is share evenly between workers and firms. The results of this paper suggest that the market-level estimates may mask considerable heterogeneity across firms.

The third is the growing literature on the role of individual firms in the labor market, particularly the wage setting process. Recent examples include Kline et al. (2019b) who use initial patent allowances as a source of idiosyncratic profit shocks to firms and document wage increases for incumbent workers at the firm, and Caldwell and Harmon (2019) who find that workers receive wage increases when the labor demand increases at a former coworker's current firm, consistent with workers' outside option influencing their bargaining position with their current employer. Evidence of systematic differences in compensation across firms come from firm wage premiums (e.g. Card et al. (2013) in West Germany and Song et al. (2018) in the USA). These wage premiums are commonly considered to reflect productivity differences across firms (Card et al., 2018) and/or compensating differentials (Sorkin, 2018). This paper provides further evidence of firm-level wage determination. In particular, while most of the current evidence comes from idiosyncratic shocks to workers or firms, there is less evidence of differential wage responses to market-wide policies. Moreover, the paper raises the possibility that firm wage premiums partly reflect variation in the incidence of various policies across firms.

The paper is organized as follows. Section 3.2 contains the theoretical analysis and Sections 3.3-3.5 contain the empirical analysis. Section 3.3 sets forth the institutional setting and describes the natural experiment as well as the data used in the analysis. The empirical approach is developed in Section 3.4. Section 3.5 presents and discusses the results and Section 3.6 concludes.

3.2 Theoretical Analysis

This section theoretically studies the role of firms in transmitting payroll taxes into wages. I develop a simple and tractable static wage-posting model that is rich enough to accommodate various sources of wage dispersion across firms.[8] The remainder of the section presents the model and studies the incidence of two types of payroll taxes: one which is the same for all workers and one which is different for different types of workers. Although the analysis is based on payroll taxes, a similar analysis can be applied to other government policies which change the labor costs of firms.

3.2.1 Environment

There are two types of workers, $i \in \{a, b\}$. There are J firms, $j \in \{1, \dots, J\}$. Payroll taxes in the model are ad valorem and potentially vary across worker type (τ_a and τ_b). The legal incidence is fully on firms.[9] The total labor cost to a firm is thus $(1 + \tau_i)w_i^j$ and the after payroll tax wage the worker receives is w_i^j. I abstract from other types of taxes such as personal and corporate income taxes.

Each firm $j \in \{1, \dots, J\}$ faces an upward sloping labor supply schedule for each worker type.[10] Because workers' preferences are not observed by firms, firms are unable to perfectly price discriminate and offer each worker their reservation wage. Instead, within firm and within worker type each worker is paid the same wage. As the firm posts higher wages, more workers are willing to work for the firm. Rents in this model arise due to a firm's inability to observe workers' preferences. While the marginal worker of each type at each firm is indifferent about working at the firm, inframarginal workers earn a rent from their job. The labor supply of labor type i to firm j is

$$N_i^j(w_i^j) = \Omega_i^j (w_i^j)^{\epsilon^j}.$$

[8] In their survey article, Card et al. (2018) use a similar model as a framework to reconcile multiple empirical facts about the role of firms in wage inequality.

[9] As is true in the standard competitive framework, the economic incidence of payroll taxes is invariant to the legal incidence. For details see Appendix C.1.1.

[10] As discussed by Manning (2011), the upward sloping labor supply schedule is a result of workers' idiosyncratic preferences over non-pecuniary match factors: conditional on an offered wage, a worker has a strict preference ordering over firms due to variation in the non-pecuniary components of a job match. For example, the location of firms makes some firms more attractive than others.

The labor supply schedules to the firm are characterized by three parameters: the elasticity of labor supply with respect to the wage, ϵ^j, as well as shift parameters, Ω_i^j. While the labor supply elasticities are the same for both types of workers, the shift parameter can vary.[11] The former captures how sensitive the labor supply is to wage changes and the latter captures the overall 'appeal' of the firm.[12] Ω_i^j is allowed to depend on the outside option of worker type i. I assume that that the outside option is constant across firms, is captured by the average wage of worker type i across all firms, $\bar{w}_i$, and that firms are small so that a change in the wage offered by an individual firm has a negligible effect on the average wage.

Firm $j \in \{1, \ldots, J\}$ produces output using workers of type a and b as well as capital:

$$Q^j = T^j F^j[K^j, L^j[N_a^j, N_b^j]]$$

where F^j and L^j are constant returns to scale, N_i^j denotes the number of workers of type i, K^j denotes the units of capital, and T^j denotes total factor productivity at firm j. Firms can rent capital at a price r. Output is sold on a monopolistically competitive product market characterized by a constant price elasticity of demand schedule with the price elasticity denoted by $\beta^j > 1$.[13]

Each firm j chooses how much capital to rent and which wages to post. Posted wages (w_a^j, w_b^j) result in numbers of workers according to $N_i^j(w_i^j)$. The firm's maximization problem reads:

$$\max_{w_a^j, w_b^j, K^j} \; P^j(Q^j)T^j F^j[K^j, L^j[N_a^j, N_b^j]] - \sum_{i \in \{a,b\}} w_i^j(1+\tau_i)N_i^j(w_i^j) - rK^j.$$

The wages of type a and b workers at firm j are implicitly determined by the first order conditions

$$q_i^j = \Big(\frac{1+\epsilon^j}{\epsilon^j}\Big) w_i^j(1+\tau_i),$$

where q_i^j is the marginal revenue product of type i at firm j. On the right hand side, $w_i^j(1+\tau_i)$ is the cost of hiring an additional worker of type i at the current wage and

[11] Allowing the elasticities to vary does not change the qualitative conclusions of the model. However, because the exposition is complicated, the discussion of the case with different elasticities is relegated to Appendix C.1.3.

[12] A 1% increase in Ω_i^j increases labor supply by 1% irrespective of the wage. For example, firms in geographic areas with more favorable local amenities might face a higher labor supply at any wage than firms in an area with less favorable amenities.

[13] A low (high) price elasticity implies that changes in the price charged by the firm will have a small (large) affect on the quantity demanded.

$\frac{1+\epsilon_j}{\epsilon_j}$ is the monopsonist's wedge between this cost and the marginal revenue product of worker type i.[14] A firm that has more labor market power places a larger wedge between a worker's marginal revenue product and their wage. As the labor supply schedule to the firm becomes infinitely elastic, $\epsilon_i^j \to \infty$, the wedge disappears.

Firms in this model differ in their production technology, the labor supply schedules they face, and the demand for the final good they produce, and each of these differences is a source of wage dispersion across firms. In particular, a firm which is more appealing to workers (higher Ω_i^j) will pay lower wages and a firm which is more productive (higher T^j) will pay higher wages. The model therefore accommodates two theories of wage dispersion across firms frequently discussed in the literature (Card et al., 2018).

3.2.2 Studying Payroll Tax Incidence

Let $w_i^j(\tau_a, \tau_b)$ denote the wage of worker type i at firm j given payroll tax rates (τ_a, τ_b).[15] The incidence of payroll taxes (τ_a, τ_b) on the wage of worker type i at firm j, denoted $\mathcal{I}_i^j(\tau_a, \tau_b)$, can be thought of as the difference between the wage in the presence payroll taxes and the wage in the absence of payroll taxes

$$\mathcal{I}_i^j(\tau_a, \tau_b) = w_i^j(\tau_a, \tau_b) - w_i^j(0, 0).$$

The question this paper asks is how $\mathcal{I}_i^j(\tau_a, \tau_b)$ varies across firms. The approach will be to study the incidence of marginal payroll tax changes. To see that variation in the incidence of marginal payroll tax changes is tied to variation in the incidence of total payroll taxes, note that $\mathcal{I}_i^j(\tau_a, \tau_b)$ corresponds to the integral over infinitely many wage responses to marginal payroll tax rate changes:

$$\mathcal{I}_i^j(\tau_a, \tau_b) = w_i^j(\tau_a, \tau_b) - w_i^j(0, 0) = \int_0^{(\tau_a, \tau_b)} \nabla w_i^j(x) dx,$$

where $\nabla w_i^j = (\frac{\partial w_i^j(\tau_a, \tau_b)}{\partial \tau_a}, \frac{\partial w_i^j(\tau_a, \tau_b)}{\partial \tau_b})$ captures the incidence of a marginal payroll tax change.

3.2.3 The Incidence of a Uniform Payroll Tax

This subsection considers a uniform payroll tax. To study the variation in incidence of such a tax at the firm level, I consider a marginal rate change and derive expressions for

[14] The wedge reflects the notion that to hire an additional worker the firm must increase the wage of all incumbent workers.

[15] Since the legal incidence is fully on firms, $w_i^j(\tau_a, \tau_b)$ corresponds to the after-tax wage.

the wage responses. Formally, I assume $\tau_a = \tau_b = \tau$ and consider a marginal increase in τ. I begin by assuming that the outside option of a worker does not respond to the change and subsequently discuss the implications of relaxing this assumption.

Note that when payroll taxes change a firm responds and optimally adjusts its production process. The adjustment leads to changes in prices, and these price changes trigger a further production adjustment. The wage-posting model developed above yields closed form expressions for the adjustment of the endogenous variables of the model as functions of changes in the exogenous variables.[16,17] The wage response of worker type i at firm j to a marginal increase in τ is given by

$$\frac{\partial \ln w_i^j}{\partial \ln(1+\tau)} = -\Big[\frac{S_L^j \beta^j + (1-S_L^j)\sigma^j}{S_L^j \beta^j + (1-S_L^j)\sigma^j + \epsilon^j}\Big] \leq 0, \tag{3.2.1}$$

where σ^j is the elasticity of substitution between capital and labor at firm j and S_L^j is the output share of labor at firm j.[18] A uniform payroll tax increases labor costs which leads to an increase production costs and a change in the relative cost of labor and capital.[19] The incidence of the payroll tax change is fully on workers if the expression equals minus one and is fully on firms if the expression equals zero.

Determinants of incidence The determinants of incidence at the firm level are: 1) the output share of labor, S_L^j, 2) the substitutability of capital and labor, σ^j, 3) the price elasticity of output demand, β^j, and 4) the elasticity of labor supply to the firm, ϵ^j.

The output share of labor, S_L^j, captures the labor intensity of firms. Firms that are more labor intensive experience a larger increase in production costs in response to payroll taxes and this puts downward pressure on the wages of both types of workers. At the same time, a higher labor intensity affects how capital and labor are substituted in production. In particular, a firm with a higher labor intensity decreases labor less and increases capital

[16]The production technology, the first order conditions, the output demand, and the labor supply equations yield a system of 7 equations. Linearizing the equations by taking logs and totally differentiating each equation with respect to $\ln(1+\tau)$ results in a system of 7 equations in 7 unknowns, where the unknowns are the responses of the endogenous outcomes to the tax change. See Appendices C.1.1-C.1.3 for more details on the derivations in this Section.

[17]Some of the expressions will look familiar from market-level incidence analysis. There are two important differences. First, the level of analysis here is a firm, not a market. Second, since firms post wages rather than taking them as given, the concept of a firm-level labor demand elasticity is not defined.

[18]Formally the elasticity of substitution is $\sigma^j = \frac{F_1^j F_2^j}{F_{12}^j F^j}$ and the output share of labor is $S_L^j = \frac{F_2^j L^j}{F^j}$ where F_i^j denotes the ith derivate of F^j.

[19]If the elasticities of labor supply of the different types of workers vary, the incidence of a uniform tax will vary between the two types of workers at the same firm. See Appendix C.1.3 for details.

more. This puts upward pressure on the wages of both types of workers. If capital and labor are not very substitutable in production, the cost effect dominates and the incidence falls more on workers at more labor intensive firms.

The elasticity of substitution between capital and labor, σ^j, determines how easy it is to substitute capital and labor in production. The increase in payroll taxes increases the relative cost of labor. The easier it is to substitute between labor and capital, the larger the wage decrease and hence the larger the share of the payroll tax burden borne by workers.

The price elasticity of output demand, β^j, determines how responsive output demand is to price changes. An increase in the payroll tax rate increases the production costs of a firm. If output demand is not very responsive to price changes (low β^j), the firm can pass through the higher production costs to output prices without triggering large decreases in output demand. In contrast, when output demand is very responsive to price changes, a given pass through of the production costs to output prices will lead to large demand decreases.[20] Note that the change in the output price in response to the payroll tax increase can also be thought of as the pass through of higher production costs to consumers. The more of the burden is passed on to consumers, the less of the burden falls on wages.[21]

The elasticity of labor supply, ϵ^j, determines how responsive labor supply is to wage changes. When labor supply is not very responsive to wage changes (low ϵ^j), the firm can pass through most of the payroll tax to wages without triggering a large decrease in labor supply. In contrast, when labor supply is very responsive to wage changes, a given pass through of the payroll tax burden will lead to large labor supply and thus production decreases. A higher elasticity of labor supply therefore reduces the incidence of payroll taxes on wages. Put differently, firms which have more monopsony power in the sense that they face a less elastic labor supply schedule are able to pass a greater share of the payroll tax burden on to workers.

Implications for wage variation To the extent that $(S_L^j, \sigma^j, \epsilon^j, \beta^j)$ vary across firms, the incidence of payroll taxes will vary across firms. Some of the observed variation in wages of similar workers across different firms can therefore be attributed to variation in the incidence of payroll taxes. Since payroll taxes can be quite high (e.g. approximately 40% in Germany), this source of wage dispersion is potentially substantial. Put differently, in the absence of payroll taxes the pattern of wage differences across firms could be quite different from what has been documented so far. This raises the question how wage variation due to payroll taxes interacts with other sources of wage variation.

[20] The latter might be true, for example, for a small firm in a global market for a homogeneous good.

[21] Because workers consume a variety of goods, an increase in the price of output at firm j does not lead to a one for one decrease in the real wage of workers at firm j.

Productivity (T^j) and amenity (Ω_i^j) differences across firms are two sources of wage dispersion commonly discussed in the literature.[22] However, because they do not alter the effect of payroll taxes on production costs and do not affect the ability of firms to pass higher payroll taxes on to wages or output prices, productivity and amenity differences do not lead to variation in the incidence of payroll taxes across firms. Wage dispersion across firms due to differences in the incidence of payroll taxes is thus orthogonal to wage dispersion due to productivity and amenity differences.

The elasticity of labor supply (ϵ^j) to a firm is a measure of the firm's labor market power. Firms with more labor market power are able to pass a greater share of the payroll tax burden on to workers. Payroll taxes therefore decrease the wages of workers at firms with more labor market power. To the extent that these firms pay lower wages to start with, payroll taxes exacerbate wage inequality between workers of the same type at high and low labor market power firms.

Accounting for changes in the outside option I assume that the payroll tax decreases the outside option of both types of workers. If the outside option of both worker types responds to the payroll tax, then the wage response of worker type i is given by

$$\frac{\partial \ln w_i^j}{\partial \ln(1+\tau)} = \frac{\partial \ln w_i^j}{\partial \ln \bar{w}_{-i}} \Delta \bar{w}_{-i} + \frac{\partial \ln w_i^j}{\partial \ln \bar{w}_i} \Delta \bar{w}_i - \Big[\frac{S_L^j \beta^j + (1 - S_L^j)\sigma^j}{S_L^j \beta^j + (1 - S_L^j)\sigma^j + \epsilon^j} \Big], \tag{3.2.2}$$

where $-i$ indicates the other worker type and $\Delta \bar{w}_i = \frac{\partial \ln \bar{w}_i}{\partial \ln(1+\tau)}$ is the response of the outside option to a marginal increase in the payroll tax rate. For ease of interpretation, let $i = a$.

The first term on the right hand side captures how the increase in the payroll tax rate changes the outside option of worker type b and how that change affects the wage of worker type a. A decrease in the outside option of worker type b leads to 1) a decrease in the overall labor costs, and 2) an increase the cost of type a workers relative to type b. 1) puts upward pressure on the wages of type a workers and 2) puts downward pressure on the wages of type a workers. The overall effect is therefore ambiguous. The second term on the right hand side captures how the increase in the payroll tax rate changes the outside option of worker type a and how that change affects the wage of worker type a. When the outside option of worker type a decreases, the wages of type a workers unambiguously decrease. The third term on the right hand side corresponds to the wage change in the absence of a change in the outside option. In summary, changes in the outside option can either attenuate or accentuate the wage responses at firm j.

[22]See Sorkin (2018) for amenity differences and Card et al. (2018) for productivity differences.

Note that the change in the outside option for a given worker type is constant across firms. However, if wages at some firms are more responsive to changes in the outside option than wages at other firms, the link between payroll taxes and the outside option adds further variation in the incidence of payroll taxes across firms.

Competitive equilibrium benchmark In a standard model of a perfectly competitive labor market, each firm faces a perfectly elastic labor supply curve at the market wage. Deviations from the market wage either attract all workers in the market (positive) or deter any individual from working at the firm (negative). Therefore, each firm pays the market wage and this will be true at any level of payroll taxes. There is no variation in wages across firms and also no variation in distribution of the payroll tax burden across firms.

Although the effect of payroll taxes on wages is the same across workplaces in a competitive labor market, the effect on employment may vary due to differences in the labor demand elasticities of firms. For example, firms which can more easily pass through changes in labor costs to output prices will adjust their employment less. Employment responses to payroll taxes are therefore transmitted through firms whether or not wages are determined at the firm level, while the incidence of payroll taxes is transmitted through firms only when wages are determined at the firm level.

3.2.4 The Incidence of a Non-Uniform Payroll Tax

This Subsection considers a non-uniform payroll tax. To study the variation in incidence of such a tax at the firm level, I consider a marginal rate change and derive expressions for the wage responses. Formally, I assume $\tau_a > \tau_b$ and consider a marginal increase in τ_a. The discussion focusses on insights that are not present in the case of a uniform tax. In particular, I do not separately address the the implications of incidence variation for observed wage variation, the case in which the outside option varies, and the competitive equilibrium benchmark. The wage responses of worker types a and b at firm j to a marginal increase in τ_a are given by:

$$\frac{\partial \ln w_a^j}{\partial \ln(1+\tau_a)} = \gamma \frac{s_a^j \epsilon^j + \phi^j}{\epsilon^j + \phi^j} - \frac{(1-s_a^j)\epsilon^j \phi^j}{\epsilon^j + \phi^j} \frac{1}{S_L^j \beta^j + (1-S_L^j)\sigma^j + \epsilon^j} \leq 0, \tag{3.2.3}$$

$$\frac{\partial \ln w_b^j}{\partial \ln(1+\tau_a)} = \gamma \frac{s_a^j \epsilon^j}{\epsilon^j + \phi^j} + \frac{s_a^j \epsilon^j \phi^j}{\epsilon^j + \phi^j} \frac{1}{S_L^j \beta^j + (1-S_L^j)\sigma^j + \epsilon^j} \lesseqgtr 0, \tag{3.2.4}$$

where γ corresponds to the wage response to a uniform payroll tax increase (3.2.1), ϕ^j is the elasticity of substitution of type a and b workers, and s_a^j is the labor share of type a

workers at firm j.[23] A non-uniform payroll tax increases labor costs which leads to an increase in production costs and a change in the relative cost of labor and capital, and also changes the relative costs of different types of workers. The first term on the RHS of (3.2.3) and (3.2.4) captures the overall effect of the labor cost increase and the second term on the RHS captures the effect of the change in relative costs.

Determinants of incidence The determinants of incidence at the firm level are: 1) the labor share of type a workers, s_a^j, 2) the output share of labor, S_L^j, 3) the elasticity of substitution between type a and b workers, ϕ^j, 4) the elasticity of substitution between capital and labor, σ^j, 5) the price elasticity of output demand, β^j, and 6) the elasticity of labor supply to the firm, ϵ^j. The effect of the output share of labor, the elasticity substitution between capital and labor, and the price elasticity of output demand on wage responses at a firm mirror those in the case of a uniform payroll tax.

The labor share of type a, s_a^j, workers determines how much labor costs increase. A higher s_a^j implies that a larger share of labor is subject to the payroll tax increase so that labor costs increase more. This puts downward pressure on the wages of both types of workers. At the same time, a higher s_a^j affects how type a and b workers are substituted in production. In particular, a firm with a higher s_a^j decreases labor of type a less and increases labor of type b more. This puts upward pressure on the wages of both types of workers. Which effect dominates depends on how substitutable workers of type a and b are.

The elasticity of substitution between type a and b workers, ϕ^j, determines how easy it is to substitute the two types of workers in production. The increase in the payroll tax rate for type a workers increases the relative cost of using type a workers. The easier it is to substitute between the two worker types, the larger the wage decrease of a workers and the smaller the wage decrease (larger the wage increase) of b workers.

The elasticity of labor supply, ϵ^j, determines how responsive labor supply is to wage changes. As the labor supply to the firm becomes more sensitive there are two effects. First, the increase in labor costs results in a smaller wage adjustment. Second, the substitution between a and b workers requires a smaller wage adjustment. Both effects put upward pressure on the wages of worker type a. However, the second effect puts downward pressure on the wages of worker type b. Therefore, at a firm with more labor market power the wages of type a decrease more and the effect on the wages of type b workers is

[23]Formally $\gamma = -\frac{S_L^j\beta^j+(1-S_L^j)\sigma^j}{S_L^j\beta^j+(1-S_L^j)\sigma^j+\epsilon^j}$, the elasticity of substitution between a and b workers is $\phi^j = \frac{L_a^j L_b^j}{L^j L_{ab}^j}$, and the labor share of type a workers is $s_a^j = \frac{L_a^j N_a^j}{L^j}$ where L_a^j is the derivative of L^j with respect to N_a^j.

ambiguous.

Differences between worker types Within a firm, the wages of type a workers weakly decrease while the wages of type b workers can either increase or decrease. The increase in τ_a increases the overall cost of labor and changes the relative cost of type a and b workers. Both of these effects work to decrease the wages of a workers. For b workers the increase in labor costs puts downward pressure on wages while the change in the relative cost of a and b workers puts upward pressure on wages. Therefore, it is possible that a higher payroll tax on type a workers increases the wages of type b workers.

3.2.5 Summary

In summary, the ' model illustrates that the incidence of payroll taxes at the firm level is determined by the effect of payroll taxes on a firm's labor costs, its labor intensity, its ability to substitute between production inputs, the extent to which it can pass through the payroll tax to output prices, and its labor market power. Although the analysis focussed on payroll taxes, the results apply more broadly to policies that change the labor cost of all or a subset of workers. Indeed, the preceding analysis could be generalized by replacing $(1 + \tau_i)$ with λ_i where λ_i captures the ability of governments to change the labor costs of a given worker type through policies. For example, a law which increases the costs associated with dismissing a worker can be modeled as an increase in λ_i $\forall i$ and a law which requires more generous paternity leave policies can be modeled as increase in λ_m where m indicates men.

The remainder of the paper presents an empirical exercise that compares the wage responses of workers who are similarly affected by a payroll tax change across firms which experience differential increases in their labor costs.

3.3 Institutional Setting, Natural Experiment, and Data

The empirical approach in this paper builds on the insight from the previous section that variation in the incidence of marginal payroll tax changes across firms is linked to variation in the incidence of total payroll taxes across firms. This section presents the payroll tax change which will be used in the empirical analysis.

3.3.1 Institutional Setting

This paper studies a reform to the payroll tax system in Germany in 2001. Payroll taxes in Germany are used to collect social security contributions. Total payroll taxes collected amounted to 14.1% of GDP in 2000 (this compares to 9.2% for income taxes) and 39% of total tax revenue (this compares to 25.3% for income taxes). The corresponding numbers in 2017 were 14.2% of GDP and 37.9% of total tax revenue.

The payroll tax is divided into four different taxes which correspond to the four arms of the social security system. There is a payroll tax for health insurance, for long-term care insurance, for unemployment insurance, and for pensions. The structure is the same across each tax: except for very low earnings levels the marginal rate is constant up to a threshold, the cap, beyond which it drops to zero and no additional payroll tax liability is incurred. The payroll tax liability is a function of a worker's pre-tax earnings in a given calendar year and the liability is evenly distributed between the employer and the worker. The total labor cost to a firm is thus the pre-tax wage plus 50% of the payroll tax liability and the net wage prior to income taxes for the worker is the pre-tax wage less 50% of the payroll tax liability. In 2000 the payroll tax rates were 19.3% for pensions, 6.5% for unemployment insurance, 13.6% for health insurance, and 1.9% for long-term care insurance. The cap for health and long-term care insurance was EUR32.7k in East Germany and EUR39.6k in West Germany. The cap for pension and unemployment insurance was EUR43.6k in East Germany and EUR52.8k in West Germany.

In the unemployment insurance and the pension system benefits are tied to contributions. A worker with higher earnings receives higher benefits in the form of higher pension payments or higher benefit payments during unemployment. Benefits are not tied to contributions in the health and long-term care insurance system: conditional on contributing, each worker receives the same health and long-term care benefits.

3.3.2 Natural Experiment

Panel (a) and (b) of Figure 3.1 depict the evolution of payroll tax rates and caps in Germany between 1996 and 2003. Over this time period the rates remained fairly constant but there were changes in the payroll tax caps. In 2001 the cap for health and long-term care insurance in East Germany increased by EUR7.3k (from EUR32.7k to EUR40k) and in 2003 the cap for pensions and unemployment insurance in East and West Germany increased by a similar amount. This paper studies the former change — the increase in the health and long-term care insurance cap in East Germany — because earnings in the data are top-coded at the pension cap. To isolate the effects of the 2001 reform in the analysis, I focus on the years 2001 and 2002.

The increase in the health and long-term care insurance cap in East Germany in 2001 was implemented to equalize the caps between East and West Germany. Panel (c), (d), and (e) of Figure 3.1 display the effect that the reform had on a worker's marginal rate, her payroll tax liability, and the percentage increase in the payroll tax liability as a function of her (pre-tax) yearly earnings in 2001, respectively. The reform divided workers into two broad categories: treated and not-treated. A worker with earnings in 2001 below the payroll tax cap (EUR32.6k) was *not treated* by the reform, as neither her marginal rate nor her payroll tax liability changed. A worker with earnings in 2001 above the cap was *treated* and experienced an increase in her payroll tax liability as well as a 15.4 percentage point increase in her marginal payroll tax rate if her earnings were below the new cap. The treatment intensity varied amongst treated workers. The change in payroll tax liability is linearly increasing in earnings up to the new payroll tax cap at which it amounts to EUR1,126. The increase remains at that level for higher earnings. The percentage increase in the payroll tax liability is increasing in earnings up to the the new payroll tax cap (at which point it reaches 6.85%), decreasing in earnings between the new cap for health and long-term care insurance and the cap for UI and pension insurance (at which point it reaches 6.3%), and constant for higher earnings. Finally, the change in the marginal payroll tax rate is constant between the old and the new cap, after which it drops to zero. Because benefits are not tied to contributions in the health and long-term care insurance system, the reform did not affect the benefits of either group of workers.

In the empirical analysis I classify workers as treated and not-treated based on their earnings in 2000, as the earnings in 2001 are potentially impacted by the reform. I apply the same approach for firm-level outcomes related to the reform. Earnings and wages are always net of the employer's share of payroll taxes and gross of the worker's share.

3.3.3 Data

The empirical analysis uses linked employer-employee data (LIAB LM 9310) provided by the Institute of Employment Research (IAB). The data are sourced from a combination of social security earnings records and survey responses of firms. For information on how the dataset is constructed see Appendix C.2.1.

At the firm level the analysis uses data on a firm's industry, location, labor force size and composition, union status, firm age, and self-reported profitability.[24] At the worker level the analysis uses information on compensation (daily wages), part-time or full-time status, education, occupation, gender, and age. The worker-level data is spell data. Spells are generated when firms send notifications to the social security system. In most cases a

[24]The survey data also contain other operational measures such as investment and total revenue. However, since the purpose of the survey is to provide an accurate representation of the state of labor demand in Germany, questions which are not related to the labor force receive less attention and their response rates are lower.

worker has one spell per year. However, if she becomes unemployed or switches jobs, she will have more than one spell. The daily wage is calculated as the total earnings reported for the spell divided by the length of the spell in calendar days. If a worker has more than one spell per year I follow the standard approach in the literature and select the spell which generates the highest earnings in that year.[25]

The data have two limitations. The first is that there is no information on hours worked. This implies that any wage responses are potentially due to intensive margin adjustments of the number of hours worked. To limit the ability for hours to vary the analysis focusses on workers who are full-time employed. The second limitation is that earnings are top-coded at the payroll tax cap for unemployment insurance and pensions. The empirical analysis can therefore not consider firm-level variation in the wage responses of workers with earnings above the payroll tax cap for UI and pensions. Importantly, the UI and pensions cap is above the new cap for health and long-term care insurance, so that both treated and not-treated workers can be studied in the empirical analysis.

3.4 Empirical Strategy

This section develops the empirical strategy to test whether there is evidence of firm-level payroll tax incidence. The idea is to compare the wage responses of workers who are similarly affected by the reform but work at different firms. In the context of the payroll tax cap increase, this corresponds to comparing the wage responses of treated (not-treated) workers at different firms. This section develops the empirical strategy, presents summary statistics and the identifying variation, and outlines the regression specifications as well as the identifying assumptions.

3.4.1 Theoretical Motivation

To theoretically motivate the empirical analysis, I map the institutional setting and natural experiment onto the wage-posting model discussed in Section 3.2. Let a treated worker be type a and a not-treated worker type b. Let the increase in the payroll tax cap correspond to an increase in the payroll tax rate for type a workers. Although the mapping is imperfect — there is considerable heterogeneity amongst workers above and below the cap and a cap increase is different from a rate increase — the exercise provides useful insights into an appropriate empirical strategy.

The wage responses are given by (3.2.3) and (3.2.4) for treated and not-treated workers, respectively. The reform increases the labor costs at a firm and increases the relative cost of

[25] For recent examples see Card et al. (2013) and Jäger and Heining (2019).

treated workers. The firm characteristics that determine the incidence of an increase in τ_a are the elasticity of output demand (β^j), the elasticity of labor supply to the firm (ϵ^j), the elasticity of substitution between capital and labor (σ^j), the elasticity of substitution between treated and not-treated workers (ϕ^j), the output share of labor (S_L^j), and the labor share of treated workers (s_a^j). In the Data, none of the elasticities can be observed separately for each firm and there is no information on production inputs other than labor. The remaining characteristic, the labor share of treated workers, determines the the extent to which a firm's labor costs increase and how a firm substitutes between treated and not-treated workers.

A firm with higher labor share of treated workers experiences a larger increase in labor costs and this puts downward pressure on the wages of treated and not treated workers. At the same time, a higher s_a^j affects how treated and not treated workers are substituted in production. In particular, a firm with a higher s_a^j decreases its demand for treated workers less and increases its demand for not-treated workers more. This puts upward pressure on the wages of both types of workers.

The empirical approach of this paper is to compare the wage responses of workers who are similarly affected by the reform across firms that experience differential increases in their labor costs. I refer to firms that experience a large (small) increase in labor costs as a consequence of the reform as high (low) exposure firms.

3.4.2 Empirical Implementation

Exposure measure The paper uses the percentage increase in payroll tax liability due to the reform, $\Delta\mathcal{T}$, as the exposure measure and compares the wage responses of workers who are similarly affected by the reform between firms with different exposures according to $\Delta\mathcal{T}$ in a difference-in-differences style framework.

$\Delta\mathcal{T}$ is an appropriate measure if does a good job at preserving the ranking of firms according to their percentage increase in labor costs.[26] For firms in which there are no workers with earnings above the pension cap (earnings top code), $\Delta\mathcal{T}$ is closely related to the increase in labor costs. In particular, the ranking of firms according to exposure is preserved so that firms with a larger increase in labor costs will also experience a larger increase $\Delta\mathcal{T}$.[27] However, for firms in which there are workers with earnings above the cap,

[26] Note that since earnings are top-coded, the percentage increase in labor costs can not be calculated.

[27] To see this consider two firms with an equal fraction of treated workers who have the same earnings above the cap. The not-treated workers in the first firm have earnings close to the old cap while in the second firm they have earnings far below the cap. The first firm will experience a smaller increase in labor costs due to the reform and also a smaller increase in $\Delta\mathcal{T}$.

$\Delta\mathcal{T}$ overstates the increase in labor costs.[28] Since few workers have earnings above the cap in any given year, the bias is likely small.

Calculating $\Delta\mathcal{T}$ I calculate $\Delta\mathcal{T}$ in two steps. First, I calculate the total earnings subject to social security contributions of each worker in 2000, the payroll tax liability in 2000 and the increase in payroll tax liability due to the reform. Next, I aggregate this at the firm-level which yields the total payroll tax liability of a firm in 2000 and the total increase in payroll tax liability due to the reform. $\Delta\mathcal{T}$ is the ratio of the two.

Worker sets Firm-level incidence is only well defined for workers who remain employed at the firm and do not experience intensive margin adjustments in their hours worked. Recall that the hours worked are not observed, but that workers are classified as part-time or full-time employed and that their is less variation in hours amongst full-time workers. The paper therefore focuses on workers who remain employed full-time at the same firm from 2000 to 2002. I consider three sets of workers (worker types). The first is treated workers with earnings in 2000 within a EUR8k window above the old cap, [EUR32.6k, EUR40.6k]. I do not consider treated workers with higher earnings because the top coding of wages limits the ability of wages to grow at higher earnings levels. The second set is not-treated workers whose earnings in 2000 are within a EUR8k window below the old cap, [EUR24.6k, EUR32.6]. The third set is not-treated workers whose earnings in 2000 are more than EUR8k and less than EUR16k below the old cap, [EUR16.6k, EUR24.6k]. I split the set of not-treated workers so as to reduce the worker-level heterogeneity within the set. Because workers within each set are similarly affected by the reform, variation in their wage responses to the reform across firms implies variation in the incidence of the reform.

Limitations The empirical strategy developed in this section is subject to a number of limitations. One limitation, which was discussed above, is the potential mis-measurement of exposure due to top-coded wages. I now address two further limitations.

The first is a consequence of the two-tiered nature of the German health insurance system, which is divided into private and public health insurance providers. In the public system contributions are tied to earnings while in the private system they are tied to age and health status. Workers with earnings above the payroll tax cap for health and long-term care insurance are eligible to leave the public and join the private insurance system. The effect of the payroll tax reform for workers in the private was different from

[28]The reason for the difference between these two examples is that the payroll tax liability is a strictly increasing function of earnings up to the payroll tax cap, after which the payroll tax liability no longer increases as earnings increase.

the effect for workers in the public insurance system. In particular, the total insurance premium did not change for privately insured workers but the worker's contribution weakly decreased and the employer's contribution weakly increased. Neumann (2017) calculated that in 2001 roughly 10% of workers above the old cap were privately insured. Appendix C.2.2 contains more details.

The private health insurance system confounds the research design in two ways. First, workers with the same earnings are potentially affected by the reform differentially due to their status in the health insurance system. Second, firms with the same exposure according to $\Delta\mathcal{T}$ potentially experience different increases in labor costs due to differences in the health insurance status of their workers. The former issue can be addressed by considering workers whose earnings are in a narrow window around the cap. The latter issue can not be addressed empirically. If enrollment in the private health insurance system is not correlated with the increase in labor costs, then the private health insurance system is a source of measurement error which will attenuate the estimates.

The second limitation is that production inputs other than labor are not observed in the data. The effect of labor cost increases on firms depends on their labor intensity. Intuitively, a labor intensive firm will be more affected by a higher exposure to the reform than a capital intensive firm. If labor intensity is negatively correlated with $\Delta\mathcal{T}$ it is possible that the estimated effect of a higher exposure is zero even though the effect among firms with the same labor intensity is non-zero. To reduce the extent to which variation in other production inputs impact the results I compare firms within the same industry.[29]

3.4.3 Sample Restrictions, Summary Statistics, and Identifying Variation

Sample restriction I restrict the sample of firms to those that operate in East Germany and are active from 1996 to 2002. I drop firms with less than 3 employees, state-owned firms, and firms that operate in the public sector (e.g. schools and universities). Furthermore, I require firms to have at least one treated worker in 2000. The last step eliminates 462 firms and results in a remaining firm sample size of 631. I focus on workers who were employed full-time by a sample firm in 2000. I restrict the sample of workers to those that are observed in the data in each year from 1996 to 2000 as a full-time employee and

[29]More generally, because other unobserved firm characteristics that also determine wage responses are potentially correlated with the exposure measure, any variation in wage responses between firms with a high and low exposure is due to the combined effect of exposure and all other unobserved firm characteristics that covary with the exposure measure. As the goal of the empirical analysis is only to test whether the wage responses of workers who are similarly affected by the reform vary across firms, the composition of the mechanism driving variation in wage responses between high and low exposure firms is not of primary importance. This discussion also highlights that the empirical analysis does not lead to estimates of an underlying structural parameter.

who remain in data throughout 2002. For the wage analysis, as discussed above, I further restrict attention to workers who remain full-time employed at the firm they are assigned to in 2000 throughout 2002. Workers are required to be older than 20 and younger than 63 in 2000.

Summary statistics Table C.1 presents summary statistics for workers as of 2000, by worker type. The worker-level outcomes are age, gender, occupation, and skill. The total number of workers in the sample is 39,006. Since the original cap is at a high earnings level relative to the earnings distribution in East Germany, most workers are not-treated (31,917 vs. 7,089). Across worker types, treated workers tend to be older, are more likely to have white collar occupations, and are more likely to be high skilled. Within worker type, most workers are between 30 and 50 years old, medium skilled and male. Among not-treated workers there are considerably more workers in blue than white collar occupations.[30]

Table C.2 presents summary statistics for firms as of 2000. The firm-level outcomes are size, sector, state, age, profit, and union status. Apart from firms with more than 500 workers, the distribution of firms across size groups is relatively uniform.[31] The largest sector in the sample is the manufacturing sector (45% of firms) followed by the service sector (21% of firms). Apart from East Berlin and Thuringia, the distribution of firms across states is relatively uniform. There are few firms in East Berlin and more firms in Thuringia. Most firms in the sample are more than 9 years old and very few are less than 6 years old. The distribution across self-reported profitability is fairly uniform. 58% of firms report being unionized vs. 41% of firms which are not unionized.

Table 3.1 presents information on the variation in exposure to the reform, $\Delta\mathcal{T}$, amongst firms in the sample. The minimum exposure among firms with at least one treated worker in 2000 is 0.0002% and the maximum exposure is 6.4193%, which closely corresponds to the increase when all workers at a firm are treated and have earnings in 2000 close to the new cap. The standard deviation is 1.22%. The mean exposure is 1.24% and the median exposure is 0.81%. The distribution of exposure across firms is skewed: there is a large mass of firms which have a small exposure to the reform and then a relatively fat tail. In particular, less than 10% of the firms which have a positive exposure have an exposure above 2.87%. These firms rely on high earnings workers in production to a much greater extent than other firms in the sample. Because these firms are arguably fundamentally different from firms with more modest production technologies, I focus on firms below the 90th percentile of exposure in the main analysis. I discuss the sensitivity of the results to

[30] 77% vs. 23% and 85% vs. 15% for workers in the groups Not-Treated 1 and Not-Treated 2, respectively.

[31] The size groups are <20, 20-50, 50-100, 100-500, >500.

this restriction.

Identifying variation One concern in difference-in-differences research designs is whether the characteristics of treated and control units are balanced. Translated into this setting the concern is that worker and firm characteristics are potentially correlated with the exposure measure.

Table C.3 presents, for each worker type, and for each worker characteristic, the mean exposure, the standard deviation, and the min and max. The table reveals that younger, female, blue collar, and low skilled treated workers have a higher exposure to the reform on average than older, male, white collar, and higher skilled treated workers.[32] The characteristics of not-treated workers are similarly correlated with exposure to the reform.

Table C.4 presents, for each firm characteristic, the mean exposure, the standard deviation, and the min and max. As with worker characteristics, the firm characteristics covary with exposure. For example, firms which self-report having a good profitability level in 2000 tend to have a higher exposure to the reform than firms which report a lower profitability. More broadly, the mean exposure measure varies across firm size groups, sectors, states, firm age groups, self-reported profitability status, and union status.

Because the worker and firm characteristics are correlated with the exposure level to the reform, it is necessary to control for the characteristics in the analysis. As the the tables reveal there is considerable variation in the exposure to the reform within each characteristic value. For example, while the mean exposure amongst female treated workers and high profit firms is higher than amongst male workers and low profit firms, there is considerable variation within each gender and profit category in the exposure. In the empirical analysis I therefore leverage variation within worker and firm characteristics to obtain identification.

3.4.4 Estimation and Identification

I estimate a dynamic difference-in-differences regression using the continuous exposure measure $\Delta\mathcal{T}$ that includes 5 pre-reform leads and 2 post-reform lags.[33] I omit the coefficient

[32] This implies, for example, that while it is less likely that a worker is treated if she is young than if she is old, conditional on being a treated worker (and thus having high earnings), it is more likely that a young worker works for a high exposure (and thus high paying) firm than a old worker.

[33] I end the event window in 2002 due to potentially confounding reforms in 2003. In particular the payroll tax liability for very low earnings workers (below EUR 800 a month) decreased and the payroll tax cap for pension insurance increased. The latter reform affected only a very small number of workers in East Germany.

in the year prior to the reform, 2000. The regression equation reads

$$y_{i,j,t} = \alpha_i + \sum_{s \in \mathcal{S}} \beta_s \mathbb{1}[t = s] \Delta \mathcal{T}_j + \Gamma X_{i,t} + \Psi E_{j,t} + \epsilon_{i,j,t}, \tag{3.4.1}$$

where $y_{i,j,t}$ is the log daily wage of worker i assigned to firm j in year t, α_i is an individual fixed effect, $X_{i,t}$ is a vector of worker-level controls, $E_{j,t}$ is a vector of firm-level controls, and $\epsilon_{i,j,t}$ is the error term which reflects the combined effect of all other factors that impact $y_{i,j,t}$, clustered at the firm-level. I assign workers to the firm they worked for in 2000, but do not require them to have worked at the firm in the years prior to 2000; the only requirement is that they were full-time employed somewhere and remain full-time employed at the firm from 2000-2002. The regression is run separately for the different worker groups (Treated, Not-Treated 1, Not-Treated 2). In order to assign firms a constant weight across regressions while still controlling for worker characteristics, I weight each observation by the inverse of the number of workers in the cluster.

$\{\beta_s\}_{s \in \mathcal{S}}$ with $\mathcal{S} = \{1996, 1997, 1998, 1999, 2001, 2002\}$ are the parameters of interest. They capture the differential wage growth between workers at firms with different exposure levels relative to 2000. The estimates for the years prior to 2000 indicate to what extent the wage growth varied between workers at high and low exposure firms prior to the reform and the estimates post 2000 indicate to what extent wage growth varied post reform.[34] The estimates from this regression therefore provide both a test of pre-existing trends in the relative wage growth as well as a test of firm-level payroll tax incidence.

To obtain a summary measure of the differences in the wage growth post reform between workers at high and low exposure firms I regress the log wage growth between 2002 and 2000 for full-time stayers on the exposure measure, controlling for firm and worker characteristics:

$$\Delta y_{i,j} = \alpha + \beta \Delta \mathcal{T}_j + \Gamma X_i + \Psi E_j + \epsilon_{i,j} \tag{3.4.2}$$

$\Delta y_{i,j} = y_{i,j,2002} - y_{i,j,2000}$ is the difference in the log daily wage between 2002 and 2000 for worker i employed at firm j. β captures the effect on the growth in daily wages from 2000 to 2002 of working at a firm with a higher exposure to the reform.

The worker controls include a quadratic and cubic in (age-40) interacted with skill, a gender by year, skill by year, and occupation by year fixed effect. Skill and occupation

[34] Recall, however, that any differences in wage growth are not necessarily attributable purely to exposure differences. Instead, they reflect the combined effect of all firm characteristics which covary with exposure and affect wage growth.

are set to their value in 2000 and held constant throughout. I consider various sets of firm controls with different levels of 'strictness'. The baseline specification includes state by year, sector by year, as well as grouped measures of firm size, age, self reported profitability, and union status by year.[35] The firm characteristics are set to their 2000 values and held constant throughout. A stricter version includes industry as opposed to sector by year.[36] A less strict version includes only state by year and sector by year as firm controls. I show that the results are qualitatively consistent between the sets of firm controls. The firm and worker characteristic by year fixed effects ensure that the effect of firm exposure on wage growth is identified using variation of exposure within worker and firm characteristics as opposed to across.

The worker-level controls serve an additional purpose. Recall that even in a competitive labor market the incidence of payroll taxes can vary across workers with different characteristics (e.g. occupations). Therefore, to the extent that the workforce composition is correlated with the exposure of firms (e.g. white collar workers tend to work for high exposure firms), not controlling for worker characteristics could falsely attribute differential wage responses across workers with different characteristics to differences in the exposure of their employer.

Identification The identifying assumption reads: Conditional on worker and firm characteristics, in the absence of the reform the daily wages of the worker type under consideration would grow at the same rate between high and low exposure firms. Under this assumption, any differences in the wage growth between high and low exposure firms after the reform can be attributed to firm-level differences in the exposure to the reform as opposed to some unobserved confounder.

Since the estimation equation leverages variation in exposure within worker characteristics and within firm characteristics, some obvious threats to identification are mitigated. In particular, unobserved trends in or shocks to earnings by gender, occupation, or skill are not an issue as they are absorbed by the worker characteristic by year fixed effect. Similarly, unobserved trends in or shocks to earnings by sector, state, firm age, profitability as of 2000, union status, and firm size are not an issue as they are absorbed by firm characteristic by year fixed effects. Furthermore, since all firms are located in East Germany, an unobserved shock or trend at the level of East Germany does not confound the results.

Two threats to identification remain. The first is that differences in wage growth

[35] In 3.4.2 the firm and worker controls enter as fixed effects for the characteristics as well as a quadratic and cubic in (age-40) interacted with skill.

[36] There are 13 different industries and 5 different sectors.

between high and low exposure firms after the reform are due to differences in the trend of wages at these firms. Comparing the trajectory of wages between high and low exposure firms in the years before the reform is informative about whether or not this is the case. The second threat is that a time-varying unobserved confounder is correlated with the exposure of a firm even after controlling for firm characteristics.

3.5 Results

3.5.1 Firm-Level Incidence

Treated workers Panel (a) of Figure 3.2 depicts the estimates $\{\hat{\beta}_s\}_{s \in \mathcal{S}}$ from the dynamic difference-in-differences regression (3.4.1) for the set of treated workers using the baseline set of firm controls. The figure reveals that treated workers who were employed at high exposure firms and treated workers who were employed at low exposure firms experienced very similar wage growth in the four years before the reform. The parallel wage trajectory increases the confidence with which any differences in the wage growth after the reform can be attributed to differential wage responses to the reform as opposed to an underlying trend or an unobserved confounder.

In the year of the reform (2001), the wage growth of treated workers at high and low exposure firms began to diverge. In particular, treated workers at high exposure firms experienced wage *decreases* relative to treated workers at low exposure firms. The staggered nature of the relative wage decrease suggests that treated workers at high exposure firms experienced real rather than nominal wage decreases. Column 1 of Table 3.2 contains the point estimate from the two year differenced regression (3.4.2), $\hat{\beta}$. By 2002 a one standard deviation increase in the exposure of a firm ($\sigma = 1.22$) was associated with a statistically significant daily wage decrease for treated workers of -1.2%. As indicated in Table 3.2, the mean total earnings of treated workers in 2000 was EUR36.0k, which implies that a one standard deviation increase in exposure was associated with a yearly earnings loss by 2002 of EUR430. [37]

The parallel wage trajectory of treated worker at high and low exposure firms prior to the reform in combination with the divergence of wages starting in the year of the reform are evidence of differences in the wage responses of workers who are similarly affected by the reform but work at different firms. In the context of payroll taxes, they are evidence of variation in the incidence of a payroll tax change across firms. In particular, the results imply that the incidence of the payroll tax reform on treated workers was more

[37] Because most workers have only one spell per year, the percentage change in the daily wage is closely related to the percentage change in total earnings.

pronounced at high than at low exposure firms.

Not-Treated workers Panel (b) of Figure 3.2 depicts the estimates $\{\hat{\beta}_s\}_{s\in\mathcal{S}}$ from the dynamic difference-in-differences regression (3.4.1) for both sets of not-treated workers using the baseline set of firm controls. The figure reveals that not-treated workers who were employed at high exposure firms and not-treated workers who were employed at low exposure firms experienced very similar wage growth in the four years before the reform. The parallel wage trajectory increases the confidence with which any differences in the wage growth after the reform can be attributed to differential wage responses to the reform as opposed to an underlying trend or an unobserved confounder.

In the year of the reform (2001), the wage growth of not-treated workers at high and low exposure firms began to diverge. In particular, not-treated workers at high exposure firms experienced wage *increases* relative to not-treated workers at low exposure firms. The increase was gradual and evenly spread out between 2001 and 2002. Columns 2 and 3 of Table 3.2 contain the point estimates from the two year differenced regression (3.4.2), $\hat{\beta}$. By 2002 a one standard deviation increase in the exposure of a firm ($\sigma = 1.22$) was associated with a statistically significant daily wage increase for not-treated workers of 1.2% and 1.4% in groups 1 and 2, respectively. Table 3.2 indicates that the mean total earnings of not-treated workers in 2000 were EUR27.9k and EUR20.8k in groups 1 and 2, respectively, which implies that a one standard deviation increase in exposure was associated with a yearly earnings increase by 2002 of EUR330 and EUR290 in groups 1 and 2, respectively.

The parallel wage trajectory of not-treated worker at high and low exposure firms prior to the reform in combination with the divergence of wages starting in the year of the reform are evidence of differences in the wage responses of workers who are similarly affected by the reform but work at different firms.

Discussion The results reveal that the wage responses of workers who were *similarly* affected by the payroll tax reform varied across firms with a *differential* exposure to the reform. The results provide evidence that the workplace matters for how a worker's wage is affected by a worker-level policy. In particular, the results provide evidence that the workplace matters because of the composition of co-workers: for a given worker, how her wage is affected by the reform depends on the treatment status of her co-workers. The findings are not consistent with a perfectly competitive labor market, but are consistent with a labor market in which firms face upward sloping labor supply schedules. The results uncovered here address one of a number of firm characteristic that determine how worker-level policies are transmitted to wages at the firm level. Given the variation in wage responses across high and low exposure firms, it is plausible that some of the other characteristics highlighted in Section 3.2 and not analyzed in this paper due to data limitations are also empirically relevant.

Comparison with Saez et al. (2019) The relationship between firm exposure and the wage responses of not-treated workers uncovered above is qualitatively different from the one reported in Saez et al. (2019). They study a large payroll tax cut for young workers (younger than 26) in Sweden and document that older (not-treated) workers at firms with a high exposure to the reform experienced wage increases following the payroll tax cut. The paper argues that the wage increases are a consequence of high exposure firms sharing the profit windfall of the tax cut with their workers. Applied to the natural experiment studied in this paper the profit sharing theory suggests that not-treated workers at high exposure firms should experience wage decreases relative to not-treated workers at low exposure firms, the opposite of the results presented above. However, the results are not per se inconsistent. In particular, it is possible that in the Swedish context the upward pressure on wages from the overall increase in the demand for labor could have offset downward pressure from an increase in the relative cost of older workers. The relationship between firm exposure and the wage responses of treated workers can not be compared as the paper does not separately report these results.

Evaluating the model The wage-posting model illustrated that a higher exposure to the reform affects the wage responses of workers through two channels: 1) overall increase in labor costs and 2) change in the relative cost of different types of labor. An increase in the exposure puts downward pressure on wages through the first channel and upward pressure on wages through the second channel. The positive relationship between firm exposure and the wage responses of not-treated workers suggests the second channel dominates.[38] The negative relationship between firm exposure and the wage responses of treated workers suggests the first channel dominates. Since the model requires the relationship between firm exposure and wage responses to be the same for both types of workers, the results above appear inconsistent with the model.

However, two other pieces of empirical evidence are broadly consistent with the predictions of the model. Subsection 3.5.3 presents the results of a heterogeneity analysis

[38]The wage increases for not-treated workers at high exposure firms are also in line with imperfect substitutability between incumbent workers and outside workers as documented in Jäger and Heining (2019). In response to the reform, high exposure firms substitute not-treated for treated workers in production. Because outside workers are imperfect substitutes for incumbent workers (due, for example, to firm-specific human capital), firms increase the retention probability of incumbent not-treated workers by increasing their wages.

that examines whether the relationship between exposure and wage responses varies depending on the capital intensity of the sector within which the firm operates. The model suggests that the exposure matters more among labor intensive than capital intensive firms and indeed, this is what the analysis suggests. Moreover, the transition responses documented in Subsection 3.5.4 are also broadly consistent with the model. The empirical results in the paper do therefore not allow for a definitive judgement of the model.

3.5.2 Worker Substitution

The wage increases for not-treated workers at high exposure firms relative to low exposure firms are consistent with firms increasing their demand for not-treated workers in order to substitute not-treated for treated workers in production. To examine this mechanism further, I test whether the relationship between firm exposure and the wage responses of not-treated workers depends on the substitutability of treated and not-treated workers. The idea is as follows. Suppose there are two firms, *a* and *b*, that have the same exposure level to the reform. In firm *a* treated and not-treated workers perform very similar tasks whereas in firm *b* they perform very different tasks. If the wage increases for not-treated workers at high exposure firms are due to a substitution of not-treated for treated workers, the wage increases of not-treated workers should be more pronounced at firm *a* than at firm *b*.

I operationalize this by idea by assessing substitutability based on occupation. I take a broad view of occupation and classify workers as having either a blue collar occupation or a white collar occupation.[39] At each firm I then impute the percentage increase in payroll tax liability amongst blue and white collar workers separately. I use these measures to define for each worker the own exposure, $\Delta\mathcal{T}_{\text{own}}$, which corresponds to the percentage increase in payroll tax liability amongst workers in the same occupation group, and the other exposure, $\Delta\mathcal{T}_{\text{other}}$, which corresponds to the percentage increase in payroll tax liability amongst workers in the other occupation group. I then estimate the following regression equation:

$$\Delta y_{i,j} = \alpha + \beta_1 \Delta\mathcal{T}_{own,j} + \beta_2 \Delta\mathcal{T}_{other,j} + \Gamma X_i + \Psi E_j + \epsilon_{i,j}. \tag{3.5.1}$$

If wage increases for not-treated workers at high exposure firms are indeed due to the substitution of not-treated for treated workers in production, the increase in exposure among workers who have similar occupations should be more important than the increase in the exposure among workers in different occupations. In other words, the coefficient for the own exposure, $\Delta\mathcal{T}_{own}$, should be larger than the coefficient for the other exposure,

[39] I follow the classification of Fuest et al. (2018b).

$\Delta\mathcal{T}_{other}$. Columns 1 and 2 of Table 3.4 report the estimates $\hat{\beta}_1$ and $\hat{\beta}_2$ for not-treated workers in groups 1 and 2, respectively. For both sets of not-treated workers it is indeed the case that the own exposure matters more than the other exposure. Because collar color is a broad classification of occupations some blue and white collar workers are likely close substitutes for one another and it is not surprising that increases in the other exposure are also associated with wage increases.

Note that a substitution between treated workers and not-treated workers would also be predicted by a model in which firms take wages as given. However, in such a model a firm that increases its demand for not-treated workers moves horizontally along the labor supply curve without changing wages. In contrast, when firms face upward sloping labor supply schedules they must increase the wage in order to attract more workers. Moreover, in the wage-posting model presented in Section 3.2, when a firm increases the wage it offers new hires it must also increase the wage of incumbents.

3.5.3 Heterogeneity

Labor vs. capital intensive The effect of a higher exposure to the reform on the wage responses of workers should vary between capital and labor intensive firms. Intuitively, a higher exposure to the reform has more of an effect on production costs in a labor than in a capital intensive firm. Moreover, since a larger increase in production costs leads to a larger reduction in output, the fact that the relative cost of using not-treated workers in production has decreased becomes less important. The combination of these two mechanisms implies that a higher exposure to the reform should have a more negative effect on wages in labor intensive than in capital intensive firms.[40]

Because the data do not contain information on production inputs other than labor I cannot directly distinguish between labor and capital intensive firms. Instead, I exploit variation in labor intensity across sectors and examine whether the effect of exposure varies between more and less labor intensive sectors. I classify the services and retail/wholesale sectors as labor intensive and the agriculture/mining/energy, construction, and manufacturing sectors as capital intensive. Columns 6-7 of Table 3.3 report the estimates from a version of the two year differenced regression 3.4.2 in which

[40]The model in Section 3.2 illustrates that the reasoning is slightly more subtle than laid out here. Recall that a higher exposure to the reform implies that 1), the overall labor cost of labor increases more and 2), that more of the substitution between treated and not-treated workers must come from an increase in the use of not-treated rather than a decrease in the use of treated workers. 1) puts downward pressure on the wages of treated and not-treated workers and 2) upward pressure. Under the assumption that the elasticity of substitution between capital and labor is low, a given increase in labor costs reduces the demand for labor more in labor intensive firms. The the effect of 1) is thus accentuated and the effect of 2) attenuated since there are less units of output for which a substitution must occur.

exposure is interacted with a dummy which takes on the value 1 for capital intensive sectors. As predicted, the effect of firm exposure on wage responses is more negative (less positive) in labor intensive industries. Among treated workers, the effect of firm exposure on wage responses is negative in labor intensive sectors and indistinguishable from zero in capital intensive sectors. Among not-treated workers, the effect of firm exposure on wage responses is positive in capital intensive sectors and indistinguishable from zero in labor intensive sectors.

Good vs. poor profitability One possible reason why the incidence of the payroll tax change falls more on the wages of treated workers at high exposure firms is that high exposure firms are liquidity constrained. If this is the case, then the relationship between firm exposure and the wage response of treated workers should be more negative at firms which self report a low profitability status. Panel 1 Columns 1-3 of Table 3.3 report the estimates from a version of the two year differenced regression 3.4.2 in which exposure is interacted with self reported profitability. Perhaps surprisingly, the exposure of a firm to the reform seems to matter most among firms that self report a good profitability level. This suggests that liquidity constraints at high exposure firms are not driving the wage decreases for treated workers. The result is similar to one obtained by Fuest et al. (2018b), who find that the incidence of corporate taxes on wages is more pronounced amongst high profit firms. Panel 2 and 3 Columns 1-3 of Table 3.3 reveal that, in contrast to treated workers, the wages of not-treated workers in group 1 and 2 are more sensitive to the exposure of their employer among firms that self report a satisfactory or low profitability level.

Union vs. no union Although there has been a decrease over time, collective bargaining agreements (CBA) are an important part of the wage determination process in Germany (Dustmann et al., 2014). Recall from Table C.2 that approximately 60% of sample firms are covered by some type of CBA. Because the ability of firms to change wages is reduced by collateralized bargaining agreements, the relationship between firm exposure and wage responses might be attenuated among unionized firms. Columns 4-5 of Table 3.3 report the estimates from a version of the two year differenced regression 3.4.2 in which exposure is interacted with a dummy which takes on the value 1 for non-unionized firms (i.e. firms that are not part of a sectoral agreement and do not have a firm-specific agreement). The point estimates imply that the relationship between firm exposure and wage responses applies both among unionized and non-unionized firms and that, as expected, the relationship is more pronounced amongst non-union firms (although the difference is insignificant).

3.5.4 Transition Responses

This subsection examines the relationship between the probability of leaving a firm and the exposure of the firm. As discussed in Section 3.2 the probability of leaving theoretically varies across firms with different exposure levels both in a perfectly competitive and in a monopsonistic labor market. In combination with the wage responses, the probability of leaving a firm can be used to further evaluate the model. In the wage-posting model presented in Section 3.2, firms control the amount of labor used in production via the wage. Wage increases (decreases) for a given type of worker are tied to employment increases (decreases), which suggests that treated workers at high exposure firms should be more likely to leave the firm and not-treated workers less likely. A similar result emerges in a model in which incumbent workers at a firm receive outside offers and firms control their retention probability through the wage they pay them, see for example Kline et al. (2019b).

I examine whether the probability of leaving a firm varied with the exposure of the firm by estimating a version of the two year differenced equation 3.4.2. The regression equation reads:

$$y_{i,j} = \alpha + \beta \Delta \mathcal{T}_j + \Gamma X_i + \Psi E_j + \epsilon_{i,j}, \tag{3.5.2}$$

where $y_{i,j}$ is a dummy which takes on the value 1 if the worker has left the firm by 2002 and 0 otherwise. The estimates are based on the set of workers from the wage regressions as well as those workers who by 2002 left the firm they were employed for in 2000. A worker who left the firm may either have moved to another firm or transitioned to unemployment.

Table 3.5 presents the estimates. Column 1 presents the results for treated workers. While the point estimate implies that a one standard deviation increase in exposure is associated with a 1.3pp decrease in the separation probability, the estimate is insignificant ($p = 0.32$) and the 95% confidence interval includes increases in the separation probability of up to 2pp. Columns 2 and 3 present the results for not-treated workers in group 1 and 2, respectively. Not-treated workers in group 1 experience a statistically significant 3.6pp decrease in their separation probability, while not-treated workers in group 2 experience a statistically insignificant 0.8pp increase in their separation probability, see Column 2. Overall, the effect of a higher exposure to the reform on the transition probabilities of treated and not-treated workers does not refute a substitution of treated and not-treated workers in production at high exposure firms. Indeed, the effect of a higher exposure on the separation probability of not-treated workers is consistent with high exposure firms increasing the wages of not-treated workers in order to increase their retention probability.

3.5.5 Robustness

Different sets of firm controls Figure 3.3 Panel (a) depicts the estimates $\{\hat{\beta}_s\}_{s \in \mathcal{S}}$ from the dynamic difference-in-differences regression (3.4.1) for the set of treated workers using the baseline set of firm controls, a weaker, and a stricter set of controls. Reassuringly, the results are qualitatively consistent across specifications. The point estimates in the post period are very similar across specifications, but differ in some pre periods. In particular, the point estimates using the strictest set of controls in some pre periods are larger (in absolute terms) than using weaker controls, however they are insignificant in each case and not suggestive of an underlying trend. Figure 3.4 Panels (a) and (b) depicts the estimates $\{\hat{\beta}_s\}_{s \in \mathcal{S}}$ from the dynamic difference-in-differences regression (3.4.1) for both not-treated workers 1 and 2, respectively, using the baseline set of firm controls, a weaker, and a stricter set of controls. The results are qualitatively consistent across specifications and the quantitative differences in the point estimates across specifications are minor.

Smaller set of treated workers As discussed in the previous section, the effect of the reform at the worker-level varies due to earnings differences and status in the health insurance system. To reduce the individual level variation I consider a subset of treated workers with earnings closer to the cap (EUR32.6-35.6k). Figure 3.3 Panel (c) depicts the estimates $\{\hat{\beta}_s\}_{s \in \mathcal{S}}$ from the dynamic difference-in-differences regression (3.4.1) using the subset of treated workers. The results are quantitatively consistent with those from the full set of treated workers and the estimates in the post-reform period are quantitatively similar to the ones based on all workers.

Excluding ever-censored workers Since for some treated workers the observable wage growth is potentially limited due to the top coding of earnings at the pension and UI cap, I reestimate (3.4.1) using only workers whose wages are never top-coded between 1996 and 2002. Figure 3.3 Panel (b) depicts the estimates $\{\hat{\beta}_s\}_{s \in \mathcal{S}}$ from the dynamic difference-in-differences regression (3.4.1) using the subset of treated workers who are never censored. The results are consistent with those from the full set of treated workers.

Firms with very high exposure The main analysis focusses on firms which were below the 90th percentile of the distribution of firms according to their exposure measure. Figure 3.3 Panel (d) depicts the estimates $\{\hat{\beta}_s\}_{s \in \mathcal{S}}$ from the dynamic difference-in-differences regression (3.4.1) using all treated workers and all firms. The estimates for the period after the reform are not consistent with the results from the main analysis: relative to 2000, wage growth (relative to 2000) at high exposure firms was slightly higher (although not significant) in 2001 than at low exposure firms and equal in 2002. Panel 1 of Table 3.6 reports the estimates from the two year differenced (3.4.2) for the full set of firms. The point estimate is 0.0005. To investigate this result further I estimate a version of (3.4.2) with a linear spline in the exposure measure and a knot at the 90th percentile. Panel 1 of Table 3.6 reports the estimates. The effect of an increase in exposure is negative below the 90th percentile, but positive above. The Table also reveals that when the set of firms is restricted to those below the 95th percentile of exposure, the estimates continue to suggest treated workers at high exposure firms experienced wage decreases compared to treated workers at low exposure firms. This suggests that a very small share of firms has a considerable influence on the results.

Figure 3.4 Panel (c) depicts the estimates $\{\hat{\beta}_s\}_{s \in \mathcal{S}}$ from the dynamic difference-in-differences regression (3.4.1) using both sets of not-treated workers and all firms. For not-treated workers in both groups, the estimates continue to indicate statistically significant significant wage increases for not-treated workers at high exposure firms after the reform and parallel wage growth before the reform. The point estimates in some of the pre periods are larger (in absolute terms) then in the main specification, however they are always insignificant and do not reveal a pre existing trend. I also estimate a version of (3.4.2) with a linear spline in the exposure measure and a knot at the 90th percentile. Panels 2 and 3 of Table 3.6 reports the estimates. Unlike for treated workers, there is no difference in the effect of an increase in exposure above and below the 90th percentile.

One possible explanation for the non-monotonicity of the exposure effect among treated workers is that firms and workers at firms with very high exposure levels are not comparable to firms and workers at firms with more moderate exposure levels. Recall that the exposure measure is related to the importance of high earnings workers in production. Firms with a very high exposure measure thus have production technologies that differ considerably from the production technologies of other firms. These firms constitute only 10% of the sample of firms with positive exposure and thus considerably less than 10% of the sample of all firms. Differences in the wage growth of workers at very high and at moderate exposure firms are therefore not necessarily related to differences in the wage responses to the reform across these firms. Instead, they potentially reflect an unobserved confounder which is unrelated to the reform. Panels (a)-(c) of Figure 3.5 provide support for this argument. They depict the estimates from a version of the dynamic difference-in-differences regression with a linear spline in the exposure measure and a knot at the 90th percentile. The estimates reveal that the wage growth of workers at very high exposure firms differs from the wage growth of workers at more moderate exposure firms even in the pre treatment period, whereas the wage growth of workers at moderate exposure firms does not differ substantially from the wage growth of workers at low exposure firms.

Hours responses The wage measure used in the empirical analysis is the daily wage. Because the daily wage is computed by dividing the total earnings for a job spell by the number of days of the spell it is possible that daily wage changes partly reflect changes in hours worked. In order to minimize the wage variation due to hours variation, this paper focussed on workers who are classified as working full-time. As noted by Jäger and Heining (2019), the variation in hours worked amongst full-time workers is likely limited, due in part to German labor laws that set strict upper bounds on work hours.

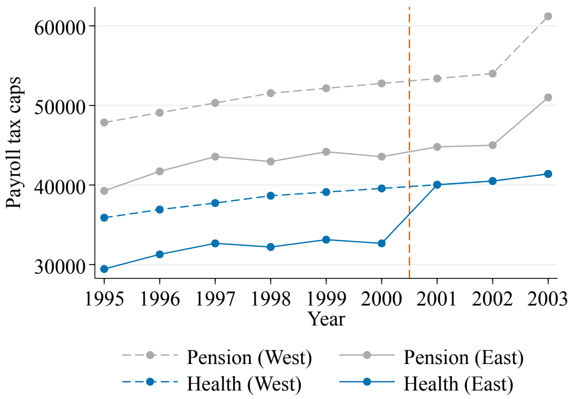
3.6 Conclusion

This paper theoretically and empirically studied the role of firms in transmitting a prominent worker-level policy, payroll taxes, into wages. While in a competitive labor market the incidence of payroll taxes is determined at an aggregate level, in a labor market characterized by firm-level wage determination the incidence of payroll taxes is determined at the firm level. The theoretical analysis illustrates that determinants of payroll tax incidence at the firm level are: 1) the effect of payroll taxes on labor costs, 2) the labor intensity, 3) the ability to substitute between production inputs, 4) the extent to which payroll taxes are passed through to output prices, and 5) the elasticity of labor supply to the firm. The theoretical framework developed in this paper can readily be applied to other government policies that change the labor costs of some or all workers.

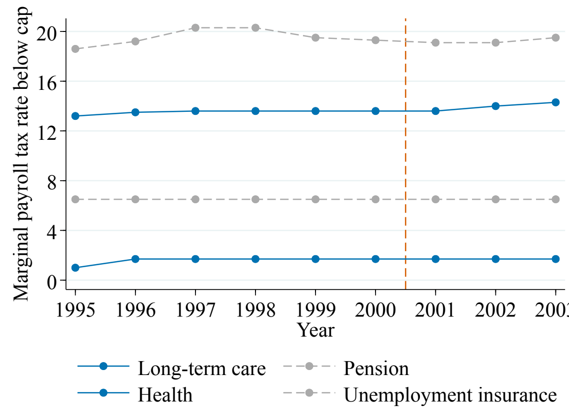
The empirical analysis documents evidence in favor of firm-level payroll tax incidence. Exploiting a reform that increased the payroll tax liability of some workers (treated) but not of others (not-treated), the paper shows that the wage responses of workers similarly affected by the reform varied across firms that experienced differential increases in labor costs. In particular, treated (not-treated) workers at firms with a high exposure to the reform experienced wage decreases (increases) relative to treated (not-treated) workers at low exposure firms. These results imply that the incidence of the payroll tax increase on the wages of treated (not-treated) workers was more (less) pronounced at high exposure firms.

The contrast between the results in this study and Saez et al. (2019) highlight the importance of additional empirical evidence on the role of firms in transmitting worker-level policies, in particular payroll taxes, into wages. Of particular interest is further evidence on the relative weight of the rent-sharing and substitution mechanisms in determining firm-level incidence.

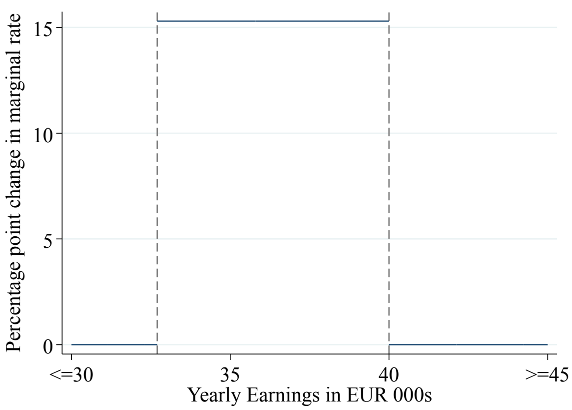
Figures



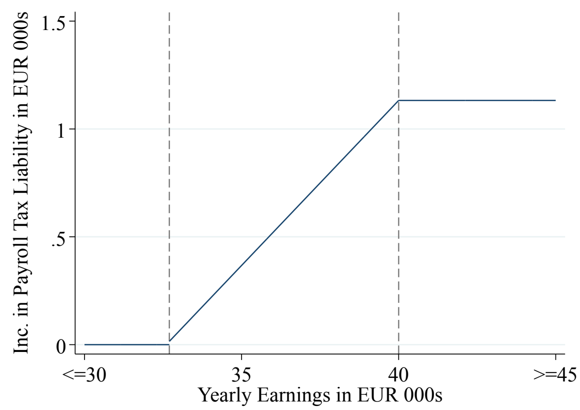
(a) Change in payroll tax caps



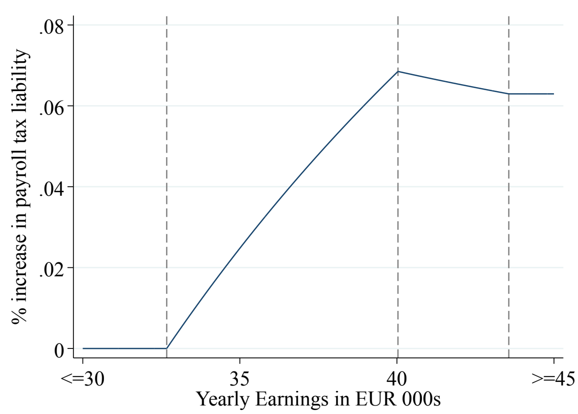
(b) Change in payroll tax rate



(c) Change in marginal payroll tax rate



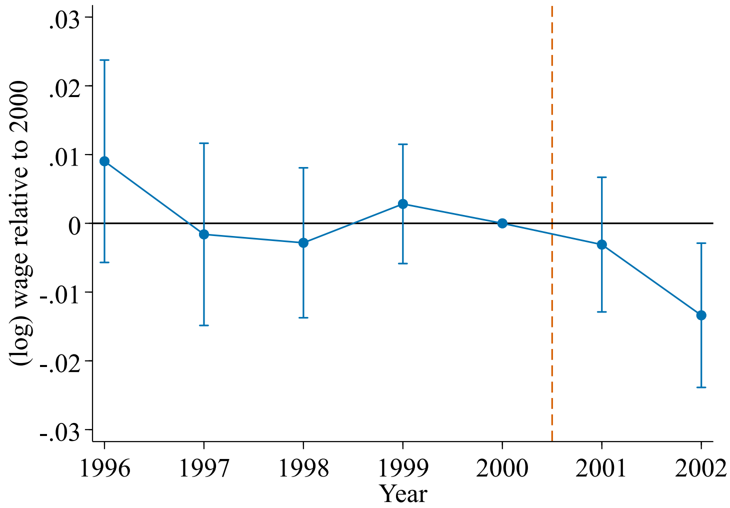
(d) Change in payroll tax liability



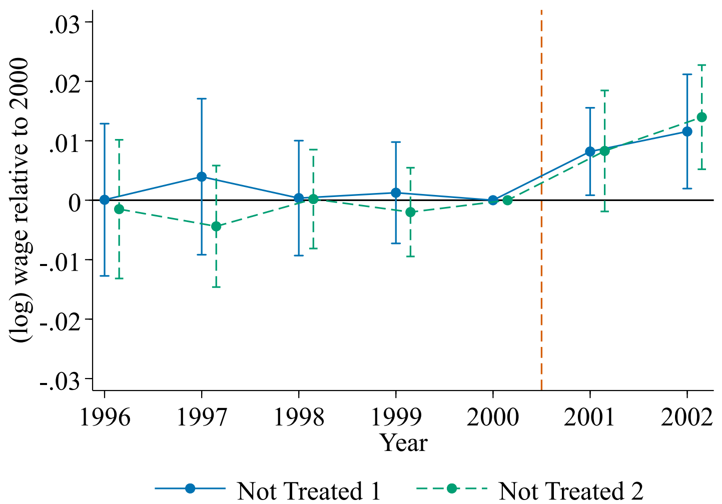
(e) % change in payroll tax liability

Figure 3.1: Institutional environment and natural experiment

Note: The Figure presents the institutional setting and natural experiment. Panel (a) depicts the time path of tax rates in for the four payroll tax categories: pensions, unemployment insurance (UI), long-term care insurance (LTCare), and health insurance. Panel (b) depicts the time path of the payroll tax caps in East and West Germany (the rates do not vary between East and West). Pensions and UI share a cap as do health and long-term care insurance. Panel (c) depicts the effect of the 2001 cap increase in East Germany on the marginal rate as a function of yearly earnings. Panel (d) depicts the effect of the 2001 cap increase in East Germany on a workers' payroll tax liability as a function of yearly earnings. Panel (e) depicts the effect percentage increase in payroll tax liability due to the 2001 cap increase in East Germany as a function of yearly earnings.



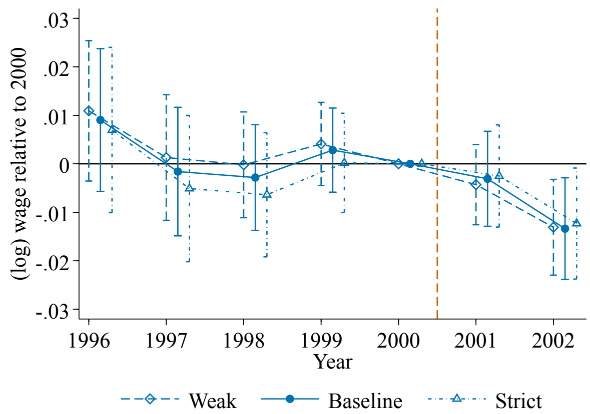
(a) Treated



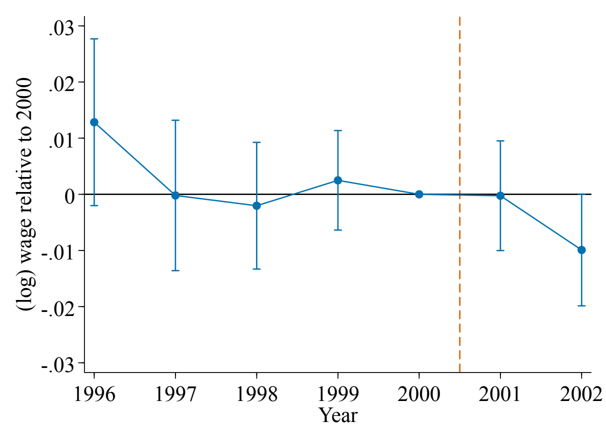
(b) Not-Treated

Figure 3.2: Dynamic difference-in-differences

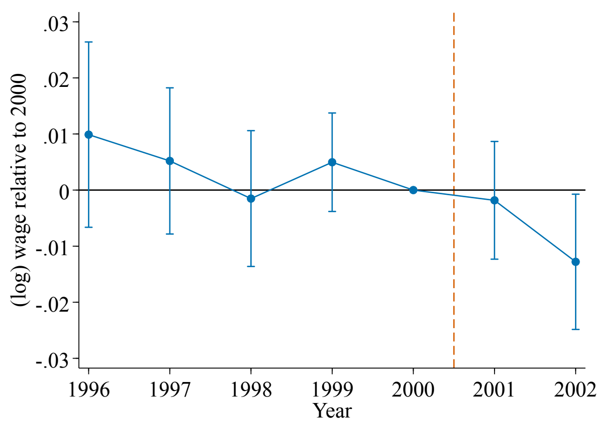
Note: The Figure depicts the estimates from the dynamic difference-in-differences regression (3.4.1). Panel (a) depicts the estimates for treated workers and and Panel (b) depicts the estimates for not treated workers. A worker is Treated if her total earnings in 2000 were in the range [EUR32.6k, EUR40.6k], where the lower bound corresponds to the pre-reform payroll tax cap for health and long-term care insurance. A worker is Not Treated 1 if her total earnings in 2000 were in the range [EUR24.6k, EUR32.6k]. A worker is Not Treated 2 if her total earnings in 2000 were in the range [EUR16.6k, EUR24.2k]. Based on her 2000 earnings, the reform did not increase the payroll tax liability of a not treated worker, but did increase the payroll tax liability of a treated worker. Workers are required to have been full-time employed at any firm prior to 2000 and to remain full-time employed at their 2000 employer throughout 2002. The estimates are based on regressions including worker-level controls and the baseline set of firm-level controls. Standard errors are clustered at the establishment level. Observations are weighted by the inverse number of units in the cluster.



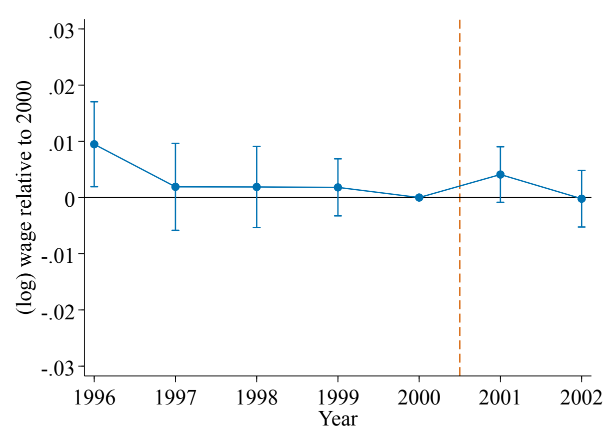
(a) Various sets of controls



(b) Never censored workers



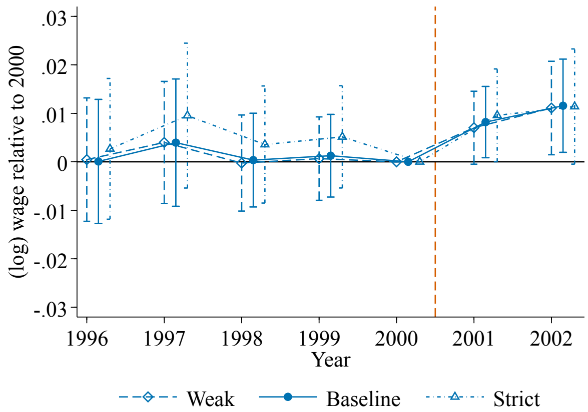
(c) Workers with 2000 earnings below EUR35.6k



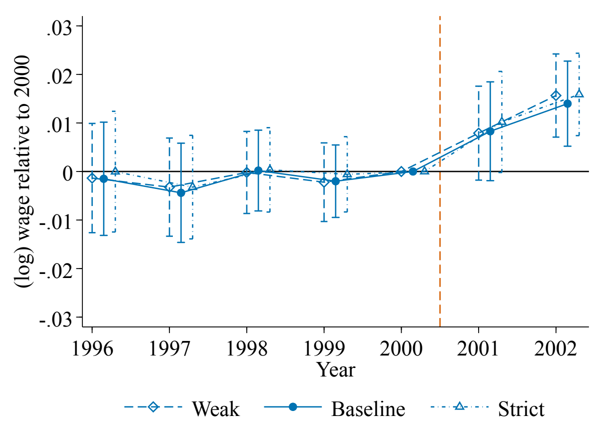
(d) Incl. firms above the 90th percentile of $\Delta\mathcal{T}$

Figure 3.3: Robustness — Treated workers

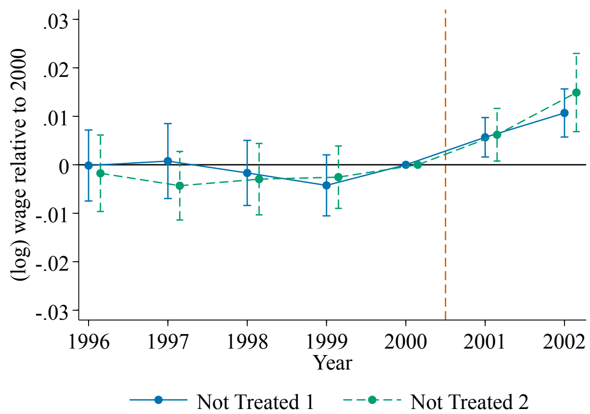
Note: The Figure depicts the estimates from the dynamic difference-in-differences regression (3.4.1). Each Panel depicts the estimates of a robustness check for Treated Workers. Panel (a) depicts the estimates using alternate sets of firm-level controls and the main analysis sample. The Weak set includes only sector by year and state by year. The Baseline set includes sector by year, state by year, and the firm size (grouped), self-reported profitability, union status, and age (grouped) as of 2000 interacted with years. The Strict set includes 1 digit industry by year instead of sector by year and is otherwise equivalent to the baseline set. Panel (b) considers only workers who are never censored. The regression include the baseline set of firm controls, worker fixed effects and worker characteristics. Panel (c) restricts attention to Treated Workers with earnings in 2000 in the range of [EUR32.6k, EUR35.6k]. The regression include the baseline set of firm controls, worker fixed effects and worker characteristics. Panel (d) includes firms above the 90th percentile of the distribution of the exposure measure. The regressions include the baseline set of firm controls, worker fixed effects and worker characteristics. Standard errors are clustered at the establishment level. Observations are weighted by the inverse number of units in the cluster.



(a) Various sets of controls — Not Treated 1



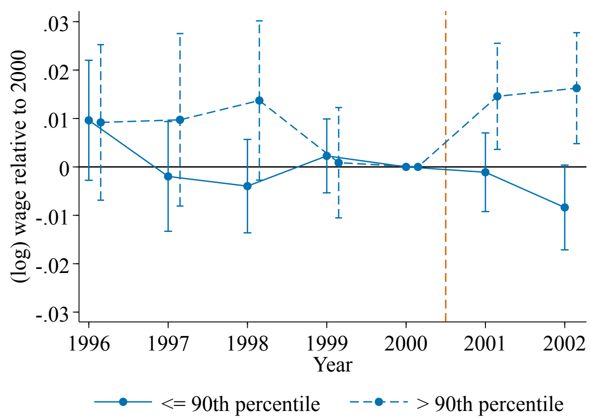
(b) Various sets of controls — Not Treated 2



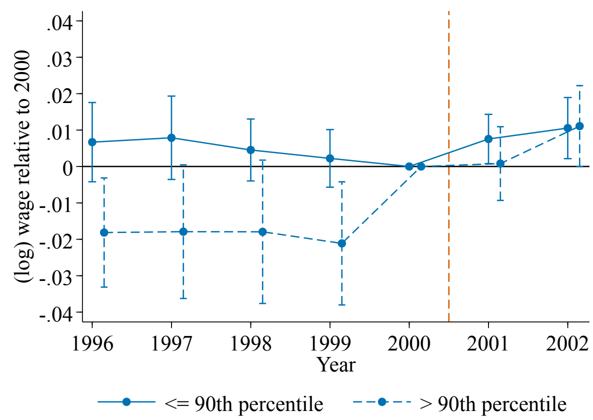
(c) Incl. firms above the 90th percentile of $\Delta\mathcal{T}$

Figure 3.4: Robustness — Not-Treated workers

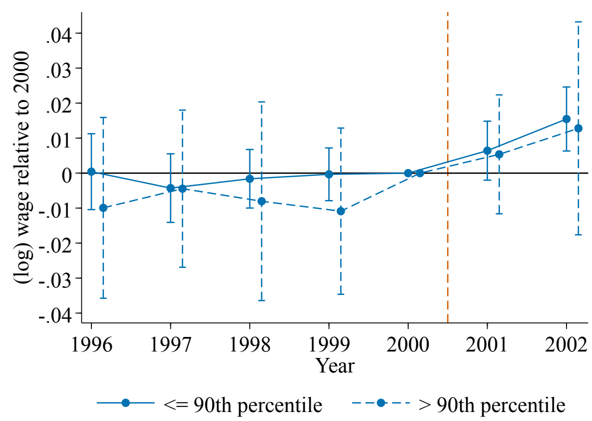
Note: The Figure depicts the estimates from the dynamic difference-in-differences regression (3.4.1). Each Panel depicts the estimates of a robustness check for Not Treated Workers. Panels (a) and (b) depict the estimates using alternate sets of firm-level controls and the main analysis sample. The Weak set includes only sector by year and state by year. The Baseline set includes sector by year, state by year, and the firm size (grouped), self-reported profitability, union status, and age (grouped) as of 2000 interacted with years. The Strict set includes 1 digit industry by year instead of sector by year and is otherwise equivalent to the baseline set. Panel (c) includes firms above the 90th percentile of the distribution of the exposure measure. The regressions include the baseline set of firm controls, worker fixed effects and worker characteristics. Standard errors are clustered at the establishment level. Observations are weighted by the inverse number of units in the cluster.



(a) Treated



(b) Not-Treated 1



(c) Not-Treated 2

Figure 3.5: Dynamic Difference-in-differences: splined

Note: The Figure depicts the estimates from the dynamic difference-in-differences regression (3.4.1) which includes a linear spline with a knot at the 90th percentile. The coefficients measure the slope in each interval. Thus, the coefficient above the 90th percentile captures the effect of a marginal increase in the exposure measure on wages, rather than the difference in the slope to the coefficient below the 90th percentile. The estimates in each panel are based on regressions which include the baseline set of firm controls, worker fixed effects, and worker level controls. Panel (a) reports the estimates for Treated workers, (b) for Not Treated 1, and (c) for Not Treated 2. Standard errors are clustered at the establishment level. Observations are weighted by the inverse number of units in the cluster.

Tables

	Min	Max	Mean	Std Dev	10th	25th	50th	75th	90th
$\Delta\mathcal{T}$	0.0002	6.4193	1.24	1.22	0.16	0.39	0.81	1.75	2.87

Table 3.1: Variation in exposure

Note: The Table reports statistics on the distribution of the exposure measure, $\Delta\mathcal{T}$. Firms are required to employ at least one treated worker in 2000 and operate between 1996-2002.

	Treated	Not Treated 1	Not Treated 2
$\Delta\mathcal{T}$	-0.0123	0.0118	0.0139
	(0.0054)	(0.0049)	(0.0044)
Mean Earnings in 2000	EUR36.0k	EUR27.9k	EUR20.8k
# of Workers	5,442	15,473	14,362
# of Firms	401	455	522

Table 3.2: Difference-in-differences regression

Note: The Table reports estimates from (3.4.2). A worker is Treated if her total earnings in 2000 were in the range [EUR32.6k, EUR40.6k], where the lower bound corresponds to the pre-reform payroll tax cap for health and long-term care insurance. A worker is Not Treated 1 if her total earnings in 2000 were in the range [EUR24.6k, EUR32.6k]. A worker is Not Treated 2 if her total earnings in 2000 were in the range [EUR16.6k, EUR24.2k]. Based on her 2000 earnings, the reform did not increase the payroll tax liability of a not treated worker, but did increase the payroll tax liability of a treated worker. Workers are required to have been full-time employed at any firm prior to 2000 and to remain full-time employed at their 2000 employer throughout 2002. The estimates are based on regressions including worker-level controls and the baseline set of firm-level controls. Standard errors are clustered at the establishment level. Observations are weighted by the inverse number of units in the cluster.

	Profit			Union		Labor Intensity	
	Good	Sat.	Poor	Yes	No	High	Low
Panel 1: Treated							
$\Delta\mathcal{T}$	-0.0268	-0.0094	0.0035	-0.0102	-0.0171	-0.0284	0.0015
	(0.0077)	(0.0094)	(0.0096)	(0.0057)	(0.0109)	(0.0095)	(0.0058)
# of Workers	5,442	5,442	5,442	5,442	5,442	5,442	5,442
# of Firms	401	401	401	401	401	401	401
Panel 2: Not Treated 1							
$\Delta\mathcal{T}$	0.0006	0.0182	0.0187	0.0078	0.0174	0.0023	0.0178
	(0.0065)	(0.0076)	(0.0115)	(0.0050)	(0.0102)	(0.0099)	(0.0050)
# of Workers	15,473	15,473	15,473	15,473	15,473	15,473	15,473
# of Firms	455	455	455	455	455	455	455
Panel 3: Not Treated 2							
$\Delta\mathcal{T}$	0.0095	0.0219	0.0096	0.0108	0.0178	0.0017	0.0205
	(0.0061)	(0.0059)	(0.0100)	(0.0062)	(0.0065)	(0.0065)	(0.0053)
# of Workers	14,362	14,362	14,362	14,362	14,362	14,362	14,362
# of Firms	522	522	522	522	522	522	522

Table 3.3: Difference-in-differences regression: heterogeneity

Note: The Table reports estimates from heterogeneity analysis using (3.4.2). The point estimates correspond to the coefficients on the interaction of the relevant firm characteristic and exposure measure. Columns 1-3 report estimates of the effect of firm exposure at firms with different self-reported profitability levels. Columns 4-5 report the estimates of the effect of firm exposure at firms with different unionization status. Columns 6-7 report the estimates of the effect of firm exposure at firms in sectors with different labor intensity. The set of workers corresponds to the main analysis sample. The estimates are based on regressions including worker-level controls and the baseline set of firm-level controls. In each case the relevant firm characteristic is included as a control. Standard errors are clustered at the establishment level. Observations are weighted by the inverse number of units in the cluster.

	Not Treated 1	Not Treated 2
$\Delta\mathcal{T}_{\text{own}}$	0.0082	0.0086
	(0.0035)	(0.0038)
$\Delta\mathcal{T}_{\text{other}}$	0.0050	0.0046
	(0.0035)	(0.0019)
# of Workers	15,473	14,362
# of Firms	455	522

Table 3.4: Difference-in-differences regression: own vs. other

Note: The Table reports estimates from (3.5.1). The set of workers corresponds to the main analysis sample. Workers are classified as either blue collar or white collar and the exposure measure (percentage increase in payroll tax liability at the firm-level) is calculated separately for blue and white collar workers. For a blue collar worker $\Delta\mathcal{T}_{\text{own}}$ is the percentage increase in payroll tax liability amongst blue collar workers at the firm and $\Delta\mathcal{T}_{\text{other}}$ is the percentage increase in payroll tax liability amongst white collar workers at the firm. Similarly for white collar workers. The estimates are based on regressions including worker-level controls and the baseline set of firm-level controls. Standard errors are clustered at the establishment level. Observations are weighted by the inverse number of units in the cluster.

	Treated	Not Treated 1	Not Treated 2
Leave Firm	-0.0106	-0.0355	0.0082
	(0.0155)	(0.0139)	(0.0155)
# of Workers	6,128	17,427	17,246

Table 3.5: Transition regressions

Note: The Table reports estimates from (3.5.2). The set of workers corresponds to all workers who are full-time employed at a sample firm in 2000 and are full-time employed in the years before 2000. The outcome is a dummy variable which takes on the value one if a worker left the firm by 2002. The estimates are based on regressions including worker-level controls and the baseline set of firm-level controls. Standard errors are clustered at the establishment level. Observations are weighted by the inverse number of units in the cluster.

				Linear Spline	
	Main Sample	All Firms	<= 95th percentile	<= 90th percentile	> 90th percentile
Panel 1: Treated					
$\Delta\mathcal{T}$	-0.0123	0.0005	-0.0079	-0.0075	0.0168
	(0.0054)	(0.0026)	(0.0041)	(0.0045)	(0.0060)
# of Workers	5,442	7,011	6,547	7,011	7,011
# of Firms	401	458	429	458	458
Panel 2: Not Treated 1					
$\Delta\mathcal{T}$	0.0118	0.0117	0.0112	0.0102	0.0155
	(0.0049)	(0.0026)	(0.0039)	(0.0043)	(0.0067)
# of Workers	15,473	16,979	16,653	16,979	16,979
# of Firms	455	506	483	506	506
Panel 3: Not Treated 2					
$\Delta\mathcal{T}$	0.0139	0.0148	0.0161	0.0152	0.0133
	(0.0044)	(0.0040)	(0.0049)	(0.0046)	(0.0151)
# of Workers	14,362	14,562	14,473	14,562	14,562
# of Firms	522	560	544	560	560

Table 3.6: Difference-in-differences regression: all firms and linear spline

Note: Column 1 reports the estimates from (3.4.2) for workers at firms below the 90th percentile in the distribution of firms according to their exposure. Column 2 reports the results including workers at firms above the 90th percentile. Columns 3 and 4 report the estimates from a version of (3.4.2) which includes a linear spline with a knot at the 90th percentile. The coefficients measure the slope in each interval. Thus, the coefficient above the 90th percentile captures the effect of a marginal increase in the exposure measure on wages, rather than the difference in the slope to the coefficient below the 90th percentile. The estimates are based on regressions including worker fixed effects, worker-level controls, and the baseline set of firm-level controls. The set of workers is restricted to workers who are full-time employed between 1996 and 2002 and remain full-time employed at the firm they work for in 2000 throughout 2002 (stayers). Standard errors are clustered at the establishment level. Observations are weighted by the inverse number of units in the cluster.

APPENDICES

APPENDIX A

Appendix to Chapter 1

A.1 Theoretical Appendix

A.1.1 Proof of Proposition 1

In general, solving for the log wage change of each worker type given a log change in the top net-of-marginal-tax rate requires solving a system of $|\Theta|$ equations in $|\Theta|$ unknowns. The proof illustrates that, in this setting, the task simplifies to solving a system of two equations in two unknowns.

The tax increase for top 1% earners can be thought of as an increase in the top bracket tax rate where the top bracket thresholds corresponds to the floor of the top 1 percentile. In each local labor market k, there are two sets of types. The first set, Θ_1^k, contains the set of worker types that are not affected by the top bracket tax increase. Formally, $\Theta_1^k = \left\{\theta | y_\theta^k < \bar{y}\right\}$ where y_θ^k is the income of type θ in local labor market k and $\bar{y}$ is the income cutoff for the top bracket. The second set, Θ_2^k, contains the set of worker types that are affected by the top bracket tax increase. Formally, $\Theta_2^k = \left\{\theta | y_\theta^k > \bar{y}\right\}$. Denote by $\dot{x}$ the log change in x given a log change in the top bracket net-of-marginal-tax rate, $(1 - \tau_t)$.

The wage for a given worker type is given by

$$w_\theta^k = A^k \alpha_\theta^k \left(\frac{L_\theta^k}{F^k}\right)^{-\frac{1}{\sigma}}$$

The log wage change for a given worker type due to a log change in $(1-\tau_t)$ is given by

$$\dot{w}^k_\theta = -\frac{1}{\sigma}\left(\dot{L}^k_\theta - \dot{F}^k\right)$$

Because there is no extensive labor supply margin the log change in the total labor supply of type θ in k corresponds to the log change in labor supply of a given worker of that type, $\dot{L}^k_\theta = \dot{N}^k_\theta + \dot{l}^k_\theta = \dot{l}^k_\theta$. Given preferences over consumption and leisure that are represented by a quasilinear and isoelastic utility function, the log change in labor supply for a worker type below the top bracket, $\theta \in \Theta^k_1$, is given by $\dot{l}^k_\theta = e\dot{w}^k_\theta$. The log change in labor supply for a worker type within the top bracket, $\theta \in \Theta^k_2$, is given by $\dot{l}^k_\theta = e\dot{w}^k_\theta + e$. Consequently, the log wage changes for worker types in the two sets can be written as

$$\theta \in \Theta^k_1: \; \dot{w}^k_\theta = -\frac{1}{\sigma}\left(e\dot{w}^k_\theta - \dot{F}^k\right) \quad \text{and} \quad \theta \in \Theta^k_2: \; \dot{w}^k_\theta = -\frac{1}{\sigma}\left(e\left(1+\dot{w}^k_\theta\right) - \dot{F}^k\right) \tag{A.1}$$

Equation (A.1) implies that the log wage changes for all types with incomes below the top bracket, $\theta \in \Theta^k_1$, are equal and that the log wage changes of all types within the top bracket, $\theta \in \Theta^k_2$, are equal. To see this, first consider arbitrary $\theta, \theta' \in \Theta^k_1$ and subtract their wage changes:

$$\dot{w}^k_\theta - \dot{w}^k_{\theta'} = -\frac{e}{\sigma}\left(\dot{w}^k_\theta - \dot{w}^k_{\theta'}\right) \iff \dot{w}^k_\theta = \dot{w}^k_{\theta'}$$

The same argument applies for arbitrary $\theta, \theta' \in \Theta^k_2$.

Denote worker types in Θ^k_1 as θ^k_1 and types in Θ^k_2 as θ^k_2 and denote the wage change for types in Θ^k_1 by $\dot{w}^k_{\theta_1}$ and for types in Θ^k_2 by $\dot{w}^k_{\theta_2}$. In each labor market k, θ^k_1 types are those unaffected by the tax increase (bottom 99% earners) and θ^k_2 types are affected by the tax increase (top 1% earners). Because F is constant returns to scale we can write $\dot{F}^k = \sum_\theta S^k_\theta \dot{L}^k_\theta$ where $S^k_\theta = \frac{F^k_\theta L^k_\theta}{F^k}$ with $F^k_\theta = \frac{\partial F^k}{\partial L^k_\theta}$ is the output share of type θ in k. Given the arguments above we can write $\dot{F}^k = e\dot{w}^k_{\theta_1} \sum_{\theta' \in \Theta^k_1} S^k_{\theta'} + e\left(1+\dot{w}^k_{\theta_2}\right) \sum_{\theta' \in \Theta^k_2} S^k_{\theta'}$. Denote by S^k_t the income share of worker types in the top bracket in k, $S^k_t = \sum_{\theta' \in \Theta^k_2} S^k_{\theta'}$. Since $\sum_{\theta \in \Theta} S^k_\theta = 1$ and $\Theta^k_1 \cup \Theta^k_2 = \Theta$ it follows that $\sum_{\theta \in \Theta^k_1} = 1 - S^k_t$. The log wage changes for θ^k_1 and θ^k_2 can now be written as

$$\dot{w}^k_{\theta_1} = -\frac{1}{\sigma}\left(e\dot{w}^k_{\theta_1} - e\dot{w}^k_{\theta_1}\left(1-S^k_t\right) - e\left(1+\dot{w}^k_{\theta_2}\right)S^k_t\right)$$
$$\dot{w}^k_{\theta_2} = -\frac{1}{\sigma}\left(e\left(1+\dot{w}^k_{\theta_2}\right) - e\dot{w}^k_{\theta_1}\left(1-S^k_t\right) - e\left(1+\dot{w}^k_{\theta_2}\right)S^k_t\right)$$

Solving the system yields

$$\dot{w}^k_{\theta_1} = S^k_t \frac{e}{e+\sigma} \tag{A.2}$$

$$\dot{w}^k_{\theta_2} = -\left(1 - S^k_t\right) \frac{e}{e+\sigma} \tag{A.3}$$

To arrive at (1.2.4), multiply (A.2) by the size of the tax reform: $\Delta \ln(1-\tau_t) = \ln(1-\tau_t) - \ln(1-\tau_t')$ where τ_t' denotes the top marginal tax rate prior to the reform.

A.1.2 Distinct Elasticity of Substitution Between Top Earners and Other Workers

This appendix illustrates that the insight from Proposition 1 — trickle-down effects vary across local labor markets in proportion to the top 1% income share — continues to apply if the elasticity of substitution between top earners and other workers is distinct from the elasticity of substitution among other worker types.

The economic environment in Section 1.2.2.1 assumes that the elasticity of substitution across all worker types is constant. Consider instead the following environment where the elasticity of substitution between top earners and other workers is distinct from the elasticity of substitution among other types of workers, for example high- and low-skill. To operationalize this notion, I partition the type space Θ into a set of top-skill agents and a set of non-top-skill agents, referred to as workers. Formally: $\Theta = \Theta_T \cup \Theta_N$ where Θ_T denotes the set of top-skill agents and Θ_N denotes the set of workers. The production function now depends on the labor of top-skill agents and workers and is given by

$$F^k = A^k \left(\alpha^k_N \left(\left(\sum_{\theta \in \Theta_N} \alpha^k_\theta \left(L^k_\theta\right)^{\frac{\eta-1}{\eta}} \right)^{\frac{\eta}{\eta-1}} \right)^{\frac{\sigma-1}{\sigma}} + \alpha^k_T \left(L^k_T\right)^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}$$

where $\left(\sum_{\theta \in \Theta_N} \alpha^k_\theta \left(L^k_\theta\right)^{\frac{\eta-1}{\eta}} \right)^{\frac{\eta}{\eta-1}}$ is a constant elasticity of substitution aggregator of worker labor and $L^k_\theta = N^k_\theta l^k_\theta$ is the total labor supply of any given labor type. For example, one possibility is that workers are differentiated into high- and low-skill workers. In that case, $\Theta_N = \{\theta_l, \theta_h\}$ where θ_l denotes low-skill workers and θ_h denotes high-type workers and η would denote the elasticity of substitution between high- and low-skill workers. Note that for simplicity the production function assumes that all top-skill agents are perfect substitutes for one another: $L^k_T = \sum_{\theta \in \Theta_T} \alpha^k_\theta L^k_\theta$. This assumption can be relaxed without changing the insights of the analysis.

Preferences, the tax schedule and behavioral responses are unchanged compared to

Section 1.2.2.1. The following result can then be proven.

Proposition 2. *Consider an increase in the marginal tax rate for top 1% earners, τ_t. Denote the income floor of the top 1 percentile by $P1$. Assume that agents with an income in the top one percentile of the income distribution are top-skill agents. Then, given the economic environment described above, the log wage change of worker types in labor market k due to a change in the rate for top 1% earners of $\Delta \ln (1 - \tau_t) < 0$ is given by*

$$\Delta w_N^k = S_t^k * \left[\frac{e}{\sigma + e} * \Delta \ln (1 - \tau_t) \right] \leq 0 \tag{A.4}$$

where $S_t^k = \sum_{\{\theta : \theta \in \Theta_T \wedge w_\theta^k l_\theta^k \geq P1\}} S_\theta^k = \sum_{\{\theta : w_\theta^k l_\theta^k \geq P1\}} \frac{w_\theta^k L_\theta^k}{\sum_{\theta \in \Theta} w_\theta^k L_\theta^k}$ is the income share of top 1% earners in k, e is the elasticity of labor supply, and σ is the elasticity of substitution between top-skill agents and workers.

Proof. See Appendix A.1.2.1. □

The result illustrates that the key insight from Section 1.2.2.1 — trickle-down effects vary across local labor markets in proportion to the top 1% income share — continues to apply if the elasticity of substitution between top earners and workers is distinct from the elasticity of substitution among other worker types.

There are two differences. First, the result in this appendix applies only to non-top skill bottom 99% earners, whereas the result in Section 1.2.2.1 applies to all bottom 99% earners. The difference is that in this appendix, some of the top-skill agents can be bottom 99% earners. To the extent that some top-skill agents have an income below the top one percentile, the result in this appendix suggests a robustness test that focusses specifically on bottom 99% earners that are not top-skill agents. The heterogeneity analysis in Section 1.4.1.2 addresses this issue by investigating whether there are wage effects of a higher exposure to the tax increase for low-skill or low-paid workers.

Second, σ in this setting captures the elasticity of substitution between top-skill agents and workers, which can be different from the elasticity of substitution among other worker types, η. In contrast, in Section 1.2.2.1, the elasticity of substitution between top earners and worker types is the same as the elasticity of substitution among different worker types. This does not impact the empirical analysis, but does impact the interpretation of the estimates.

A.1.2.1 Proof of Proposition 2

The proof is similar to the proof in Appendix A.1.1.

The tax increase for top 1% earners can be thought of as an increase in the top bracket tax rate where the top bracket thresholds corresponds to the floor of the top 1 percentile. In each local labor market k, there are three sets of types. The first set, Θ_N, contains all worker types. By assumption, all top 1% earners are top skill, so that worker types are not affected by the tax increase. The second set, Θ_{T1}^k, contains the set of top-skill types that are unaffected by the tax increase because their income is below the top one percentile. Formally, $\Theta_{T1}^k = \{\theta | \theta \in \Theta_T \wedge y_\theta^k < P1\}$. The third set, Θ_{T2}^k, contains the set of top-skill types with an income in the top one percentile of the income distribution. Formally, $\Theta_2^k = \{\theta | \theta \in \Theta_T \wedge y_\theta^k \geq P1\}$. Denote by $\dot{x}$ the log change in x given a log change in the top bracket net-of-marginal-tax rate, $(1 - \tau_t)$.

Consider first Θ_N. Denote the aggregator of worker types by $L_N^k = \left(\sum_{\theta \in \Theta_N} \alpha_\theta^k \left(L_\theta^k\right)^{\frac{\eta-1}{\eta}}\right)^{\frac{\eta}{\eta-1}}$ and note that it is constant returns to scale. The log wage change for a given worker type $\theta \in \Theta_N$ due to a log change in $(1 - \tau_t)$ is given by

$$\dot{w}_\theta^k = \frac{1}{\sigma}\left(\dot{F^k} - \dot{L_N^k}\right) + \frac{1}{\eta}\left(\dot{L_N^k} - \dot{L_\theta^k}\right) \tag{A.5}$$

Because there is no extensive labor supply margin the log change in the total labor supply of type θ in k corresponds to the log change in labor supply of a given worker of that type, $\dot{L}_\theta^k = \dot{N}_\theta^k + \dot{l}_\theta^k = \dot{l}_\theta^k$. Given preferences over consumption and leisure that are represented by a quasilinear and isoelastic utility function, the log change in labor supply for a worker, $\theta \in \Theta_N$, is given by $\dot{l}_\theta^k = e\dot{w}_\theta^k$. The wage change for all worker types is therefore equal. To see this, consider two arbitrary types $\theta, \theta' \in \Theta_N$ and subtract their wage changes:

$$\dot{w}_\theta^k - \dot{w}_{\theta'}^k = -\frac{e}{\eta}\left(\dot{w}_\theta^k - \dot{w}_{\theta'}^k\right)$$

which holds when $\dot{w}_\theta^k = \dot{w}_{\theta'}^k$. Denote the wage change of worker types in k by $\dot{w}_N^k$. Because L_N^k is constant returns to scale, the wage change in (A.5) simplifies to

$$\dot{w}_N^k = \frac{1}{\sigma}\left(\dot{F^k} - e\dot{w}_N^k \sum_{\theta \in \Theta_N} \frac{\partial L_N^k}{\partial L_\theta^k}\frac{L_\theta^k}{L_N^k}\right) + \frac{1}{\eta}\left(e\dot{w}_N^k \sum_{\theta \in \Theta_N} \frac{\partial L_N^k}{\partial L_\theta^k}\frac{L_\theta^k}{L_N^k} - e\dot{w}_N^k\right) = \frac{1}{\sigma}\left(\dot{F^k} - e\dot{w}_N^k\right) \tag{A.6}$$

Consider next Θ_{T1}^k. The log wage change for a given worker type $\theta \in \Theta_{T1}$ due to a log change in $(1 - \tau_t)$ is given by

$$\dot{w}_\theta^k = \frac{1}{\sigma}\left(\dot{F^k} - \dot{L_T^k}\right) \tag{A.7}$$

A similar argument to the one above establishes that the wage change for all top-skill agents unaffected by the tax increase is the same. Denote the wage change by $\dot{w}_{T1}^k$. The log change in labor supply for a top-skill agent unaffected by the tax increase, $\theta \in \Theta_{T1}$, is given by $\dot{l}_{T1}^k = e\dot{w}_{T1}^k$.

Consider next Θ_{T2}^k. The log wage change for a given worker type $\theta \in \Theta_{T2}$ due to a log change in $(1-\tau_t)$ is given by

$$\dot{w}_\theta^k = \frac{1}{\sigma}\left(\dot{F^k} - \dot{L_T^k}\right) \tag{A.8}$$

A similar argument to the one above establishes that the wage change for all top-skill agents unaffected by the tax increase is the same. Denote the wage change by $\dot{w}_{T2}^k$. The log change in labor supply for a top-skill agent affected by the tax increase, $\theta \in \Theta_{T2}$, is given by $\dot{l}_{T2}^k = e\dot{w}_{T2}^k + e$. Note that this set of top-skill agents responds directly to the tax change as well as to the wage change, whereas the other sets of agents respond only to wage changes.

Note that (A.7) and (A.8) imply that the wage change for both types of top-skill agents is identical: $\dot{w}_{T2}^k = \dot{w}_{T1}^k = \dot{w}_T^k$. The intuition for this result is that all top-skill agents are perfect substitutes for one another, so that a change in the labor supply of top-skill agents affected by the tax reform has no direct impact on the marginal product of other top-skill agents. Rather, the wage effect for top-skill agents unaffected by the tax change stems purely from the fact that the relative supply of top-skill agents and workers has changed. We can therefore write the change in total top-skill agent labor supply, $\dot{L_T^k}$, as

$$\dot{L_T^k} = e\dot{w}_T^k \sum_{\theta \in \Theta_{T1}^k} \frac{\alpha_\theta^k L_\theta^k}{L_T^k} + e\left(\dot{w}_T^k + 1\right) \sum_{\theta \in \Theta_{T2}^k} \frac{\alpha_\theta^k L_\theta^k}{L_T^k} = e\dot{w}_T^k + eS_{T2}^k \tag{A.9}$$

where $S_{T2}^k = \sum_{\theta \in \Theta_{T2}^k} \frac{\alpha_\theta^k L_\theta^k}{L_T^k}$ is the share of all top-skill income accounted for by top-skill agents in the top one percentile of the income distribution.

There are now two unknowns, the wage change for worker types and the wage change for top-skill agents, and two equations:

$$\dot{w}_T^k = \frac{1}{\sigma}\left(\dot{F^k} - \dot{L_T^k}\right) = \frac{1}{\sigma}\left(\left(1 - S_T^k\right)\dot{L_N^k} + S_T^k \dot{L_T^k} - \dot{L_T^k}\right) \tag{A.10}$$

$$\dot{w}_N^k = \frac{1}{\sigma}\left(\dot{F^k} - \dot{L_N^k}\right) = \frac{1}{\sigma}\left(\left(1 - S_T^k\right)\dot{L_N^k} + S_T^k \dot{L_T^k} - \dot{L_N^k}\right) \tag{A.11}$$

where $S_T^k = \frac{\partial F^k}{\partial L_T^k}\frac{L_T^k}{F^k}$ is the share of all labor market income accounted for by top-skill agents and where the expression after the second equal sign follows because the production function is constant returns to scale. Plugging in the expressions for $\dot{L_T^k}$ and $\dot{L_N^k}$ and simplifying yields

$$\dot{w}_T^k = \frac{1}{\sigma}\left(1 - S_T^k\right)\left(e\dot{w}_N^k - e\dot{w}_T^k - eS_{T2}^k\right) \tag{A.12}$$

$$\dot{w}_N^k = \frac{1}{\sigma}S_T^k\left(e\dot{w}_T^k + eS_{T2}^k - e\dot{w}_N^k\right) \tag{A.13}$$

Solving this system yields

$$\dot{w}_N^k = S_T^k S_{T2}^k \frac{e}{e+\sigma} = S_t^k \frac{e}{e+\sigma} \tag{A.14}$$

where multiplying the top-skill income share by the share of top-skill income accounted for by top 1% earners yields the top 1% income share $S_t^k = S_T^k S_{T2}^k$.

A.2 Empirical Appendix

A.2.1 Data

A.2.1.1 Variables and Data Sources

This appendix provides details on the variables that are used in the empirical part of the paper and are not described in detail in Section 1.3.2. The American Community Survey microdata used to construct CBSA-level wage measures are described separately in Appendix A.2.1.2.

- Bartik income predictor. I use QCEW data on private-sector payroll and employment by 2- and 3-digit NAICS to generate the Bartik annual wage predictor at the 2- and 3-digit level for each horizon $h \neq 2011$. The QCEW data is at the county level and I generate CBSA-by-industry payroll and employment by summing across counties within a CBSA. Annual wages in a CBSA-by-industry cell is the ratio of payroll and employment. The Bartik annual wage predictor at horizon h is the change in annual wages that would be expected given the industry composition of labor market i in 2011 and national annual wage changes within industry. Formally, $b_{i,h}^{inc} = \sum_{j \in NAICS3}\left(\frac{Y_{i,j,2011}}{\sum_{j \in NAICS3} Y_{i,j,2011}}\right) * \left(\frac{y_{j,h} - y_{j,2011}}{y_{j,2011}}\right)$ where $Y_{i,j,2011}$ are total payroll in industry j in CBSA i in 2011 and $y_{j,h}$ is the average nationwide annual wage in industry j at horizon h.

- Bartik employment predictor. I use QCEW data on private-sector employment by 2- and 3-digit NAICS to generate the Bartik employment predictor at the 2- and 3-digit level for each horizon $h \neq 2011$. The QCEW data is at the county level and I generate CBSA-by-industry employment by summing across counties within a CBSA. The Bartik employment predictor at horizon h is the change in employment that would be expected given the industry composition of labor market i in 2011 and national employment changes within industry. Formally, $b_{i,h}^{emp} = \sum_{j \in NAICS3} \left(\frac{n_{i,j,2011}}{\sum_{j \in NAICS3} n_{i,j,2011}} \right) * \left(\frac{n_{j,h} - n_{j,2011}}{n_{j,2011}} \right)$ where $n_{i,j,2011}$ is employment in industry j in CBSA i in 2011 and $n_{j,h}$ is the nationwide employment in industry j at horizon h.

- Cyclicality measure. I use BEA data on personal income (Table CAINC4 linecode 10) and population (Table CAINC4 linecode 20) to construct the cyclicality measure. The BEA data is at the county level and I generate CBSA-level personal income per capita by summing personal income and population across counties within a CBSA and taking the ratio. The cyclicality control is based on the estimated β_1 from a regression for each CBSA over the period 1999-2011 of $\Delta y_{i,t} = \beta_0 + \beta_1 \Delta y_t + e_{i,t}$ where $\Delta y_{i,t}$ is the log change in income-per-capita in i between t and $t-1$ and Δy_t is the national change. I also generate cyclicality controls based on a regression for each CBSA over the period 1989-2011. The cyclicality controls I consider are 10 and 20 quantiles of the distribution of $\hat{\beta}_1$ across CBSAs. Whenever I consider subsamples of the main sample, I generate the quantiles separately for the subsample to ensure balance across quantiles.

- Housing boom & bust. I use the house price index of the Federal Housing Finance Agency to construct the housing boom and bust measure. The index is available at the county level and I generate a CBSA-level index by setting a base year of 2003 and then using population weights to aggregate the year-on-year changes of the county-level index to a CBSA-level change. The housing boom is the log change of the index between 2003-2007 while the housing bust is the log change of the index between 2007-2011.

- Population weights. I use population data from the National Cancer Institute's Surveillance, Epidemiology, and End Results Program (SEER) to calculate population weights. The data contain population estimates for various age groups at the county level. I aggregate to age-group-by-CBSA cells by summing across counties within a CBSA. The population weights are based on the population in 2011 and are windsorized at the top 1 percentile.

- Population controls. The population controls consist of 10 quantiles of the distribution of the population weight. For the ACS sample they consist of 5 quantiles.

- Demographic controls. I use tabulated statistics based on a 5-year pooled sample of ACS data from 2007-2011 to generate the share of the population 25 and above that has a college education as well as the share of the total population that is white. The data are at the county level and I aggregate to the CBSA level by summing across counties within a CBSA. I also use the SEERs population data to generate the median age. I first sum across counties within a CBSA and then use the age distribution within CBSAs to generate the median age in 2011.

- Capital control. I use 2011 BEA data on the current-cost net stock of private equipment, structures and fixed assets by 2-digit NAICS industry and distribute the capital stock across CBSAs according to CBSA-by-industry employment shares. I use the County Business Patterns to calculate total CBSA employment for each 2-digit industry. CBP data has the advantage that, in the case of suppressed cells, an employment range is indicated. I use the midpoint of the range in case employment is suppressed. I scale the total amount of capital in a CBSA by the total CBSA GDP, also taken from the BEA.

- BEA wage & salary income. I use BEA wage & salary annual wages as an alternative to QCEW wage & salary income. The BEA measure captures a slightly broader range of private sector jobs than the QCEW and also includes public sector jobs. The data are at the county level and I generate CBSA-level annual wages by summing total wage & salary income (Table CAINC4 linecode 50) and total wage & salary employment (Table CAINC4 linecode 7020) across counties within a CBSA and taking the ratio.

- Local income share. I use BEA payroll by place of work (Table CAINC4 linecode 45) as well as the residence adjustment (Table CAINC4 linecode 42) to calculate the share of income generated in a CBSA that is accounted for by the income of residents in the CBSA. The data are at the county level and I aggregate to the CBSA level by summing across counties within a CBSA. The local income share is the ratio of total payroll by place of residence and total payroll by place of work. If the ratio is less than one, not all of the income generated in a CBSA is accounted for by CBSA residents.

- Top 1% active income share. Recall that the highest AGI class that is separately reported in the data is AGI above $200k. The top 1% active income share is therefore not directly observed. I calculate the top 1% active income by first calculating the top 1% total income using the Pareto assumption (see Section 1.3.2) and then assuming that the share of total income that is active income among the top 1% is the same as the share of total income that is active income for those with an AGI above $200k. I have verified using state-level data that contain statistics on tax returns with an AGI above $500k that this prediction works well - in a regression of the observed active income share for taxpayers with an AGI above $500k on the predicted active income share the R-squared is 0.97.

- Top 1% wage & salary income share. Recall that the highest AGI class that is separately reported in the data is AGI above $200k. The top 1% wage & salary share is therefore not directly observed. I calculate the top 1% wage & salary income by first calculating the top 1% total income using the Pareto assumption (see Section 1.3.2) and then assuming that the share of total income that is wage & salary among the top 1% is the same as the share of total income that is wage & salary income for those with an AGI above $200k. I have verified using state-level data that contain statistics on tax returns with an AGI above $500k that this prediction works well - in a regression of the observed wage & salary income share for taxpayers with an AGI above $500k on the predicted wage & salary income share the R-squared is 0.97.

- Employment. Employment is measured as the total number of private sector jobs reported by the QCEW. The data are at the county level and I generate CBSA-level employment by summing across counties within a CBSA.

- Employment-to-population ratio. The employment-to-population ratio is the ratio of QCEW private sector jobs to the working-age population where the latter is based on the SEER county-level population data. The data are at the county level and I generate CBSA-level employment and working-age population by summing across counties within a CBSA.

- Population. Population is measured as the total population (all ages) within a CBSA. The data source is the SEER county-level population data. The data are at the county level and I generate CBSA-level population by summing across counties within a CBSA.

A.2.1.2 American Community Survey

This appendix describes how I use the American Community Survey (ACS) public use microdata (Ruggles et al., 2021) to calculate CBSA-level wage outcomes.

To increase precision, I use 5-year pooled samples from 2007-2011 and from 2014-2018 to create one pre-reform and one post-reform observation per CBSA. In the data, the most disaggregated geographic identifier is the Public Use Microdata Area (PUMA). A PUMA covers a population of at least 100k. CBSAs can be either a MiSA or a MeSA. Because an MiSA by definition covers a population of less than 100k, I restrict attention to MeSAs, resulting in a sample of 361 unique CBSAs. In cases where a PUMA is not fully contained with an MeSA, I modify the weight of each observation in the PUMA by the share of the PUMA's population in the MeSA. In cases where a PUMA is in multiple MeSAs, I duplicate each observation in the PUMA by the number of MeSAs it is a part of and weight each duplicated observation by the share of the PUMA in the respective MeSA. I drop observations that have imputed values of labor income, hours or weeks worked. I restrict attention to non-institutionalized workers who report being employed (empstat=1), report positive labor income (incwage¿0), report working 50-52 weeks last year (wkswork2=6), work in the private sector and are older than 25 and younger than 54. For each sample year, I drop observations whose total earnings (inctot) are in the top 1 percentile of the weighted total earnings distribution that year.

I create six wage measures at the CBSA level. Each measure is created separately for the pre- and post-reform period.

- Average annual wage (log). This outcome is calculated as the CBSA fixed effect in a regression of log total labor income on year dummies where observations are weighted by their sampling weight. Calculating average annual wage this way as opposed to a simple weighted average across observations in a CBSA accounts for potential differences in sampling across CBSAs. For example, it accounts for situations where one CBSA has 40% of its observations for the 2007-2011 sample during 2007 and the remaining 60% evenly spread across 2008-2011 whereas another CBSA has 40% of its observations during 2011.

- Average hourly wage (log). For each observation, the hourly wage is calculated in two steps. First, I assign each observation a number of weeks worked. In the ACS, weeks worked are indicated as intervals and I assign each interval the midpoint as the number of weeks worked. Second, I calculate the hourly wage as the total labor income divided by the product of weeks worked and usual hours worked. The process of aggregating wages to the CBSA level is the same as the process for annual wages..

- Average composition controlled annual and hourly wage (log). The CBSA level composition controlled annual and hourly wage are given as the CBSA fixed effect in a regression of (log) wages on a quartic in age, education dummies (6), gender dummies (2), race dummies (3), and ethnicity dummies (2), all interacted with year dummies. Each observation is weighted by its sampling weight.

- Average hourly wage by skill (log). A worker is classified as high-skill if she has a college degree (Bachelor) or more and low-skill otherwise. CBSA level average wages by skill are calculated in two steps. First, I regress (log) hourly wages on year dummies and save the residuals. Second, I generate CBSA level average wages by skill level by calculating the weighted average of the residuals separately for each skill level and each CBSA.

- Quartiles of hourly wage distribution (log). CBSA level quartiles of the hourly wage distribution are calculated in two steps. The first step is the same as for the average hourly wage by skill. Second, I generate CBSA level quartiles by calculating the quartiles of the weighted hourly wage distribution within each CBSA.

A.2.2 Robustness to Perturbations of Main Specification

This appendix describes various tests intended to asses the robustness of the results to perturbations of the main specification.

Table 1.3 presents the estimated effect of a one standard deviation increase in the top 1% income share in 2011 on CBSA QCEW annual wages relative to 2011 for the full sample of CBSAs over three time periods: 2012-2018, 2012-2014 and 2015-2018.

- The main specification includes state-by-year fixed effects. This is done for a number of reasons. First, state tax rates differ and interact with federal tax rates. As a consequence, the impact of the tax increase will differ across states. Second, states have their own contemporaneous policy changes, including top tax rates (e.g. California). However, I show that the results are qualitatively unaffected by using either region, division or nation by year fixed effects. See rows 2, 3 and 4.

- The main specification include 20 quantiles of the cyclicality measure estimated over the time period 1999-2011. I show that the results are robust to using 10 quantiles, the continuous measure, as well as 20 quantiles based on 1989-2011. The point estimates increase slightly when using deciles or quantiles based on 1989-2011 and are similar when using the continuous measure. See rows 5, 6 and 7.

- The main specification includes a Bartik predictor based on 3-digit industry composition. I examine the robustness of the results to instead using a Bartik predictor based on 2-digit industry shares. The results are qualitatively unchanged. See row 8.

- The main specification compares wage changes relative to 2011 across CBSAs that differ drastically in size. Moreover, larger CBSAs tend to have a higher top 1%

income share. A concern is that differential trends across CBSAs of different size might confound the wage comparisons. Moreover, a concern is that the transition mechanism might vary between CBSAs of different size due to differences in the organization of the local economy. For example, if trickle-down effects are less pronounced in dense CBSAs, then the main specification will not necessarily capture the average effect. To address these concerns, I include controls for CBSA size. In particular, I allow the year effect to vary across deciles of CBSAs by size. The results are qualitatively unchanged. See row 9.

- The main specification is weighted. Since CBSAs differ drastically in size, some receive much more weight than others. I examine the robustness of the results to the inclusion of weights by reestimating without weights. The results are qualitatively unchanged. See row 10.

- The main specification does not include controls for differences in demographics across CBSAs. I examine the robustness of the results to this exclusion by adding controls for median age, share with a college degree and share white, all measured using ACS data for the period 2007-2011. The results are quantitatively and qualitatively similar. See row 11.

- The main specification does not include controls for potential differences in capital intensity across CBSAs. Similar to CBSA size, there are concerns both about differential trends by capital intensity as well as transition mechanisms varying across CBSAs with different capital intensity. To address these concerns, I include the private capital-stock-to-GDP ratio as a control interacted with year dummies. The results are qualitatively unchanged. See row 12.

- The top 1% income share enters the main specification as a continuous regressor. I examine the robustness of the results to using two versions of a binary top 1% income share. The first divides CBSAs depending on whether or not they are in the top tercile of the top 1% income share distribution. This division corresponds to the one used to generate Figure 1.4 Panel (a). The second divides CBSAs depending on whether or not they are above or below the median top 1% income share. In both cases, I scale the estimates by the difference in the top 1% income share mean within each group multiplied by the population-weighted standard deviation of the top 1% across CBSAs in 2011 to facilitate a comparison to the main specification. The estimates are quantitatively and qualitatively very similar compared to the main specification. See row 13 and 14.

- The main specification uses the top 1% income share in 2011. I examine the robustness to instead using the average of the 2010 and 2011 top 1% income share. The average is standardized by the population-weighted standard deviation of the top 1% across CBSAs in 2011 to facilitate a comparison to the main specification. The estimates are quantitatively and qualitatively very similar compared to the main specification. See row 15.

- The main specification measures the top 1% income share as the share of total AGI earned by those with an AGI above $500k. I examine the robustness to instead using the share of total AGI earned by those with an AGI above $400k. The share is standardized by the population-weighted standard deviation of the top 1% across CBSAs in 2011 to facilitate a comparison to the main specification. The estimates are quantitatively and qualitatively very similar compared to the main specification. See row 16.

- Finally, I examine the robustness to using BEA wage & salary income instead of QCEW. The estimates are quantitatively and qualitatively very similar compared to the main specification. See row 17.

Overall, the tests do not indicate that the lack of trickle-down effects is an artifact of the regression specification.

A.2.3 Robustness to Concerns about Exposure Measurement

A potential concern is that mismeasured exposure to the reform is responsible for the zero local trickle-down result. I address three different measurement concerns. To facilitate the comparison of estimates across different exposure measures, I focus on unstandardized measures. Table 1.4 row 1 reports the estimates of the main specification for QCEW annual wages on the full sample of CBSAs using the unstandardized top 1% income share. The average estimate over the post-reform period is 0.0085 (se: 0.0399).

The first measurement concern is that the top 1% total income share measures the human capital income share with noise because total income (AGI) includes income such as capital gains that are potentially not the return to human capital.[1] I address this concern by calculating the top 1% active income share and using it instead of the top 1% AGI share. Active income is calculated as AGI less capital gains, interest, dividend, retirement and transfer income. The top 1% active income share is the share of CBSA active income earned

[1] Ex-ante it is unclear to what extent realized capital gains reflect returns to capital as opposed to shifted returns to human capital, particularly at the top of the distribution (Scheuer and Slemrod, 2020).

by those whose total income is in the top 1% of the national distribution as these are the taxpayers treated by the 2013 tax increase.[2] The average effect on annual wages over the post-reform period is -0.0068 (se: 0.0403), see row 2.

A second potential measurement concern is due to pass-through business income. First, pass-through business income potentially reflects returns to physical capital as opposed to human capital.[3] Second, top 1% earners potentially own many businesses in many different places, breaking the geographic link between the source and recipient of top 1% income. Third, the distribution of partnership income is opaque and incomplete (Cooper et al., 2016), potentially causing spurious variation in top 1% income shares across CBSAs. I address these issues two ways. First, I examine the correlation between the location of pass-through business activity and income at the state level. Data on the location of pass-through activity are available from the County Business Patterns (CBP) that breaks out total payroll by legal form of business. Data on the location of pass-through income come from IRS SOI tabulations of tax returns at the state level. If pass-through owners systematically live in one place but own businesses elsewhere, the correlation between a state's share of total pass-through payroll and pass-through income should be low. Figure A.2 instead reveals a high correlation (R-squared≈0.98) between the two shares. Second, I use the top 1% wage & salary share as the exposure measure. The average effect on annual wages over the post-reform period is -0.0018 (se: 0.0618), see row 3. Finally, the findings of Smith et al. (2019) that the median top 1-0.1% and top 0.1% business owner owns one business in a labor intensive industry and that approximately 75% of pass-through profits are returns to human capital also assuage concerns about the first two issues.

A third potential measurement concern is that top earners live and work in different CBSAs. To address this concern, I repeat the analysis at the Combined Statistical Area (CSA) level.[4] CSAs are groupings of adjacent CBSAs that are economically connected as measured by commuting patterns, however to a lesser extent than counties within CBSAs. For example, Princeton, Greenwich and New York City are separate CBSAs however

[2]Because the highest AGI class that is separately reported in the data is AGI above $200k, I calculate the top 1% active income by first calculating the top 1% total income using the procedure from Section 1.3.2 and then assume that the share of active income in total income among the top 1% is the same as the share of active income in total income among those with an AGI above $200k.

[3]If top pass-through business income is a return to physical capital, the 2013 tax reform reduced the post-tax return to equity-financed capital for these businesses which can lead to lower wages via a lower capital stock (e.g. Suarez Serrato and Zidar, 2016 and Fuest et al., 2018a). How this effect varies across CBSAs will depend on the local capital share of top pass-through businesses, which is not observed.

[4]An alternative would be to repeat the analysis at the state level. I prefer CSAs because CSA boundaries are intended to align with economic boundaries, which is not true for states. For example, the New York City, Boston and Chicago CSAs are all split across three states. In Appendix A.2.4 I discuss an additional approach that relies on identifying CBSAs where the majority of income generated in the CBSA is accounted for by CBSA residents. The exercise yields similar conclusions.

they are combined in the same CSA. There are a total of 125 CSAs and to accommodate the reduced sample size, I include region fixed effects instead of state fixed effects and cyclicality controls based on 5 quantiles instead of 20. The average effect on annual wages over the post-reform period is 0.0363 (se: 0.1141), see row 4.

A.2.4 Addressing Exposure Measurement Concern due to Mismatch Between Place of Work and Place of Residence

This appendix discusses an additional approach to addressing the concern that top earners live and work in different CBSAs. The results are consistent with and complement those of Appendix (A.2.3).

I use additional data from the Bureau of Economic Analysis (BEA) to determine what share of total payroll in a CBSA is earned by CBSA residents. The further the ratio is from 1, the more pronounced the threat of measurement error attenuating the estimates. I therefore estimate (2.3.1) on the sample of CBSAs where the ratio falls in the interval [0.95, 1.05]. 394 CBSAs satisfy the sample restriction.[5] The estimates are presented in row 2 of Table A.3. The average effect on annual wages relative to 2011 over the post-reform period is 0.0882 (se: 0.0856). The estimates do not alter the conclusion that there is no detectable decrease of worker incomes in CBSAs with a higher exposure to the 2013 top tax increase.[6]

A.2.5 Evidence on the Validity of the Exposure Measure

This appendix elaborates the argument in Section 1.4.1.1. To further assuage concerns about the validity of the top 1% income share as an exposure measure, I test whether it predicts CBSA-level outcome changes due to the 2013 reform. These tests assuage concerns that the 2011 top 1% income share does not accurately describe differences in the exposure of CBSA residents to the 2013 reform because 1) the Pareto assumption from Section 1.3.2 is inappropriate, 2) errors in my code, 3) errors in the geocoding of tax returns by the IRS SOI resulting in random year-to-year variation in the assignment of tax returns to counties, 4) mean reversion that results in 2011 high exposure CBSAs having a low exposure when the reform is implemented in 2013.

[5] To accommodate the reduced sample size, I include division fixed effects instead of state fixed effects and cyclicality controls based on 10 quantiles instead of 20.

[6] Note that compared to CBSAs in which top earners live but don't work, the mechanisms via which trickle down occurs are potentially different in CBSAs in which top earners live and work. In particular, in CBSAs in which they live and work, trickle down might occur due a decrease in demand triggered by a lower after-tax income as well as due to mechanisms such as skill complementarity that occur in the work place. These considerations would suggest that trickle-down effects are more pronounced when focussing on CBSAs in which top earners live and work. However, the estimates, which if anything are inflated compared to the main specification, do not support these considerations.

I test whether CBSAs with a higher exposure in 2011 experienced differential changes in their average tax rate, capital gains realizations and charitable contributions post 2011 that would be expected given the nature of the reform and evidence on the responses of top earners documented by Auten et al. (2016) and Saez (2017). Figure 1.5 presents the results. Each panel in Figure 1.5 depicts the estimates $\hat{B}_h$ from the regression $\Delta y_{i,h} = \beta_0 + \beta_h S_i + \epsilon_{i,h}$ where each CBSA is weighted by 2011 population (windsorized at the top 1 percentile), the top 1% income share is standardized and standard errors are clustered at the CBSA level. For these tests, I rely on the IRS SOI county-level tabulations of tax returns, aggregated to the CBSA level. Because the tax data is only available starting in 2010 and there was a change to the tax data in 2018, the estimates are depicted for $h \in \{2010, 2012, \ldots, 2017\}$. Table A.4 presents the estimates. See Appendix A.2.6 for one approach to deriving the elasticity of capital gains realizations and charitable with respect to the net-of-top-tax rate implied by the estimates.

Panel (a) presents the estimates for the average tax rate. There were no differential changes in the average tax rate in 2010 or 2012. Consistent with the 2013 tax reform increasing the taxes of the top 1%, CBSAs with larger top 1% income shares in 2011 experienced larger average tax rate increases in 2013. Panel (b) presents the estimates for log average capital gains realizations. The 2013 reform increased the capital gains tax rate from 15% to 25%, creating an incentive to accelerate realizations into 2012 and reduce realizations post 2012 due to the lock-in effect. Because incentives remained unchanged for lower income taxpayers, larger responses of aggregate CBSA-level capital gains realizations should be observed in high exposure CBSAs where more income is responding. Panel (b) presents evidence consistent with a spike in high-exposure CBSAs in 2012 and a reduction post 2012. The spike is consistent with time-series evidence on accelerated realizations in Saez (2017) and the drop with evidence on capital gains responses to taxes from Agersnap and Zidar (Forthcoming). Panel (c) presents the estimates for log average charitable contributions. Saez (2017) documents that top 1% earners increased their charitable contributions in 2012 along with their capital gains realizations. The increase in top 1% contributions should result in CBSAs with a higher top 1% income share experiencing an increase in contributions in 2012. Panel (c) confirms this prediction. Post 2012, the lower after-tax-cost of contributions increased the incentives to make contributions via the substitution effect. Panel (c) reveals that, consistent with evidence of charitable contribution responses to changes in the tax price (Bakija and Heim, 2011 and Duquette, 2016), contributions increased in high exposure CBSAs after 2012.

These tests support the notion that CBSAs classified as high exposure by my research design were more affected by the 2013 tax increase than low exposure CBSAs. They also illustrate that my approach of comparing the evolution of outcomes across high and low exposure CBSAs is capable of detecting top earner responses to the reform documented using other methods.

A.2.6 Elasticities Implied by CBSA Capital Gains and Charitable Contributions Responses

This appendix discusses one approach to deriving an elasticity of capital gains realizations and charitable contributions with respect to the net-of-top-tax rate based on the differential responses of capital gains realizations and charitable contributions across CBSAs with a differential exposure to the 2013 reform. See Section A.2.5 for the discussion and Figure 1.5 and Table A.4 for the estimates.

Translating the estimates into elasticities requires knowledge of how the top 1% capital gains share/charitable contributions share increases as the top 1% income share increases. Formally, the change in log capital gains realization, Δc, in CBSA i assuming no change in the realizations of bottom 99% earners can be written as $\Delta c_i = \theta_i^t \Delta c_i^t$ where θ_i^t is the share of top 1% capital gains realizations in i. Denote the share of income that is capital gains among top 1% and bottom 99% earners as ω^j. Then the top 1% capital gains share can be expressed as a function of the top 1% income share: $\theta_i^t = \frac{\omega^t y^t}{\omega^t y^t + \omega^b y^b} = \frac{\omega^t S_i^t}{\omega^t S_i^t + \omega^b \left(1 - S_i^t\right)}$. The derivative of this expression with respect to the top 1% income share is given by $\frac{\partial \theta_i^t}{\partial S_i^t} = \frac{1/\kappa}{\left(S_i^t + 1/\kappa\left(1 - S_i^t\right)\right)^2}$ where $\kappa = \frac{\omega^t}{\omega^b}$ is the ratio of the share of income that is capital gains among top 1% and bottom 99% earners. According to IRS SOI Table 1.4, $\kappa \approx 10$, so that evaluated at the mean top 1% income share, the derivative would be approximately 2.1. The unscaled point estimate over the period 2013-2017 evaluates to -0.38 which given a change in the net-of-tax rate from 0.85 to 0.75 implies an elasticity of around 1.5 with a 95% confidence interval of (-0.16, 3.3). Adding controls increases the elasticity to approximately 3.5 with a 95% confidence interval of (1.4, 5.5). In comparison, the elasticity reported by Agersnap and Zidar (Forthcoming) over a 0-5 year horizon is approximately 2.2 (Table 2).

A similar exercise as the one for capital gains suggests $\kappa \approx 0.7$ and unscaled point estimate of 0.92. Given a change in the net-of-tax rate from 0.65 to 0.59 implies an elasticity of approximately -7.9 with a 95% confidence interval of (-10.6, -5.2). Adding controls changes the elasticity to -5.5 with a 95% confidence interval of (-7.5, -3.6). The estimates are bigger than the -4 elasticity reported by Duquette (2016). See Saez (2017) for a discussion of how to interpret top 1% charitable contributions to the 2013 reform.

A.2.7 Social Security Tax Increase

This appendix addresses the concern that the 2013 Social Security tax increase is contaminating the research design. The expiration of the 2011-2012 Social Security tax holiday resulted in an almost across-the-board 2pp increase in the marginal and average tax rate, raising the question of whether the expiration is contaminating the comparison of annual wage changes between high and low exposure CBSAs. It should be noted that the Social Security tax increase was the consequence of a one-year temporary decrease in 2011 that was twice extended in 2012 before expiring in 2013. Research on adjustment frictions (Chetty, 2012) and the response of labor supply to temporary wage changes (Martínez et al., 2021) suggests such a temporary reform is unlikely to trigger labor supply adjustments. Nonetheless, the remainder of this appendix both theoretically and empirically argues that Social Security tax increase is not contaminating the research design

Theoretically, it is unlikely that the expiration of the Social Security tax cuts is contaminating the comparison of wage changes between high and low exposure CBSAs because, compared to the tax increases for top 1% earners, the effect on the marginal and average tax rates were small. The expiration potentially affected both the labor supply and the consumption behavior of taxpayers. As detailed in below (Appendix A.2.7.1), labor supply responses of bottom 99% earners to the reform are unlikely to contaminate a comparison between high and low exposure CBSAs. The intuition is that what matters for trickle down is the difference between the labor supply responses of top 1% earners and bottom 99% workers and, because the impact of the 2013 reform was more than five times as pronounced for top 1% earners, the difference should only be marginally smaller compared to a situation without the Social Security tax increase.[7] As discussed above, the temporary nature of the reform also raises the question of wether there were any labor supply responses and, to the best of my knowledge, there is no research that demonstrates labor supply responses to the 2011 Social Security tax cut. The expiration is also unlikely to contaminate the empirical design via lower post-tax incomes that trigger consumption responses. Although the expiration did decrease the post-tax income of most workers, the combined effect of the tax increases for top earners and the Social Security tax increase resulted in post-tax income declining considerably more at the top of the distribution (Figure A.1 Panel (c)). Therefore, CBSAs with a higher exposure also experienced larger decreases in post-tax income.

Two empirical tests likewise suggest that the threat of contamination from the Social Security tax increase is limited. First, if a lower exposure to the tax increase for top earners

[7] This implicitly assumes that the labor supply elasticities of both groups are similar.

is offset by a larger impact of the Social Security tax increase, then the introduction of the tax holiday in 2011 should have resulted in differential wage and employment responses in low exposure CBSAs. The estimates in Figure 1.4 and 1.6 do not indicate any differential changes in low vs. high exposure CBSAs between 2010 and 2011. A second test proceeds as follows. Given empirical evidence on the lack of a labor supply response to payroll taxes (Lehmann et al., 2013) combined with evidence that the 2011 Social Security tax cut increased spending (Graziani et al., 2016), the main concern is that consumer demand decreased more in labor markets with a low top 1% income share. To address this concern, I focus on wages in the tradable sector as local demand conditions should have a smaller impact on the local tradable sector while trickle-down mechanisms still apply.[8] Table A.5 presents the estimates. The point estimates (2012-2018 mean: 0.17%, se: 0.37%) are qualitatively unchanged and qualitatively similar compared to the main estimates.

A.2.7.1 Theoretical Framework

This appendix examines the effects of the Social Security tax increase in a similar framework to that of Section 1.2. The key takeaway is that labor supply responses of bottom 99% earners to the Social Security tax increase are unlikely to interfere with the empirical design because the impact of the 2013 reform on the net-of-tax rate was much more pronounced for top 1% earners.

There are top 1% earners, t, and bottom 99% workers, b, that each supply labor according to $L_i = \Omega_i \left(1-\tau_i\right)^e w_i^e$. Wages are given by the marginal products of an aggregate production function which is constant-returns-to-scale and has a constant elasticity of substitution σ

$$w_b = F_b\left(L_t, L_b\right)$$
$$w_t = F_t\left(L_t, L_b\right)$$

To study the response of wages to the combined 2013 tax increase for top 1% earners and the Social Security tax increase, I take logs of the previous expressions and totally differentiate yielding

$$\dot{w_b} = \frac{1}{\sigma} S_t \left(\dot{L_t} - \dot{L_b}\right)$$
$$\dot{w_t} = -\frac{1}{\sigma}\left(1-S_t\right)\left(\dot{L_t} - \dot{L_b}\right)$$

[8] I follow Chodorow-Reich et al. (2021) and classify NAICS 11 (Agriculture, Forestry, Fishing and Hunting), 21 (Mining) and 31-33 (Manufacturing) as tradable.

where the change in the labor supply of either worker type is given by $\dot{L}_i = e(\Delta \ln(1-\tau_i) + \dot{w}_i)$. That is, each agent responds both to the tax change and the wage change, and the equations above illustrate how the wage changes in turn depend on the labor supply changes. Solving this system yields the following expression for the wage change of bottom 99% workers

$$\dot{w}_b = S_t \frac{e\Delta}{\sigma + e} \tag{A.1}$$

where $\Delta = \Delta \ln(1-\tau_t) - \Delta \ln(1-\tau_b)$ is the difference in the (log) net-of-tax rate changes for top 1% earners and bottom 99% earners.

The key motivation of the empirical approach in the absence of the Social Security tax increase, $\Delta = \Delta \ln(1-\tau_t) < 0$, was that the wage impact would vary across local labor markets as a function of top 1% income shares. Conceptualize each labor market as an autonomous economy, then (A.1) indicates that the research design continues to be valid as long as $\Delta < 0$. Using the data from NBER Taxsim suggests that the log change in the net-of-tax rate for bottom 99% workers was around -0.02 whereas it was around -0.137 for top 1% earners implying a $\Delta = -0.117$. As a consequence, the empirical approach is still valid.

In summary, although labor supply responses of bottom 99% workers to the Social Security tax increase in theory can attenuate differences in wage impacts across high and low exposure CBSAs, the attenuation should be small given that the impact of the 2013 reform on top 1% earners was more than 5 times as pronounced as the impact of the Social Security tax increase.

A.2.8 Heterogeneity Within Bottom 99%

This appendix presents additional evidence on the absence of detectable heterogeneity in trickle-down effects among workers (bottom 99% earners). The results are consistent with and complement those of Section 1.4.1.2.

Section 1.4.1.2 tested for heterogeneous impacts between high and low skilled workers as determined by their education. An alternative approach to testing for heterogeneous impacts consists of examining whether wage changes varied across the distribution. Table A.6 presents the estimates at quartiles of the within-MSA hourly wage distribution. The coefficients, particularly at the median, are inflated compared to the mean estimate, however all are indistinguishable from zero. An F-test of equality across quartiles has a p-value of 0.26. The table also presents the estimates for the mean composition-controlled wage within terciles. I first assign workers within each CBSA to terciles of the wage distribution and then estimate the composition-controlled wages for each tercile. These estimates again have the advantage of absorbing differential wage changes due to differences in workforce composition. The estimates are 0.28% (se: 0.34%), 0.57% (se: 0.44%) and 0.19% (se: 0.34%) for the bottom, middle and top tercile, respectively. Each estimate is indistinguishable from zero and an F-test of equality across quartiles has a p-value of 0.35. The estimates therefore indicate zero trickle-down effects across the wage distribution.

A.3 Welfare Appendix

A.3.1 Details of Calibration

This appendix presents the details of how I calibrate (1.5.1) using the estimated bounds on trickle-down effects combined with implications of the theoretical framework in Section 1.2.2.1 and microdata on the U.S. distribution of income.

- I use the 2011 IRS SOI public use file housed at NBER and generate tax liabilities and marginal tax rates for 2012. I use 2011 IRS SOI data as opposed to 2012 data because the distribution of income in 2012 is potentially distorted by anticipation responses to the 2013 reform.
- I calculate the utility for each tax return using $U(y) = y\left(\frac{1+T'(y)e}{1+e}\right) - T(y)$ which is the indirect utility function given preferences that are described by a quasilinear and isoelastic utility function. I use AGI as the income concept for determining utility as it more closely corresponds to disposable income than taxable income and I use the income tax liability and federal marginal tax rate calculated by NBER Taxsim.[9] I set $e = 0.25$ as in Diamond and Saez (2011).
- I calculate the marginal welfare weight by first calculating $G'(U) = U^{-\gamma}$ for each tax return, then calculating the marginal value of public funds as $\lambda = E[G'(U)]$ and finally generating the marginal welfare weight for each tax return as $g(y) = U(y)^{-\gamma}/\lambda$.
- I set the top bracket cutoff to a taxable income of \$388.5k as this corresponds to the top bracket cutoff in 2012. I set the tax rate in the top bracket to the average marginal tax rate within the top bracket as calculated using the NBER data: 0.3109.

[9] For example, it is not clear that if two taxpayers have the same AGI but one has a lower taxable income because of higher charitable contributions the taxpayer with the lower taxable income has a lower utility.

- I use the bound at the 95% confidence level on $\dot{w}_b$ implied by the average ACS hourly wage estimates, $\dot{w}_b = 0.051$ (Table 1.8 Panel C1) and the bound on $\dot{y}_b$ implied by the average of the QCEW annual wage estimates, $\dot{y}_b = 0.067$ (Table 1.8 Panel C2).

- To calibrate $\dot{w}_t$, I exploit the relationship implied by skill complementarity framework of Section 1.2.2.1 between wage decreases for bottom 99% and wage increases for top 1% earners in any given labor market of $\dot{w}_t^k = -\frac{1-S_t^k}{S_t^k}\dot{w}_b^k$. I then calculate the bound as $\dot{w}_t = \sum_k \psi^k \dot{w}_t^k$ where ψ^k is the share of all nationwide top earners who work in k. I proceed similarly for $\dot{y}_t$. The exercise yields $\dot{w}_t = -0.28$ and $\dot{y}_t = -0.37$.

- I use taxable income to calibrate the mechanical revenue gain.

- I use market income, AGI less transfer income less capital income, to calculate income changes due to trickle-down effects (d and e in (1.5.1)). I use market income because trickle-down effects should apply to income that reflects returns to human capital. More precisely, I calculate market income as: AGI - IRA distributions - pensions - Social Security distributions - unemployment insurance - interest income - dividend income - capital gains.

A.3.2 Trickle-Down Effects that Vary Below Top Bracket

This appendix studies the welfare implications of non-constant trickle-down effects among workers (bottom 99% agents).

While I do not find evidence in support of the hypothesis that trickle-down effects vary among bottom 99% agents (see Section 1.4.1.2), there is a range of differential impacts that I can't reject. In this appendix, I make ad-hoc assumptions about how trickle-down effects vary below the top bracket. I consider both cases where trickle-down effects are more and less pronounced among high-income bottom 99% agents. Formally, I consider situations where the elasticity of bottom 99% wages varies with income: $\dot{w}_b(y) = \dot{w}_b h(y)$ where $h()$ is some function.

I require that the total income change of bottom 99% agents is the same as in the situation with a constant wage elasticity. Formally, the total income change with a constant trickle-down elasticity is given by

$$N\int_0^{\bar{y}} y\frac{\dot{y}_b(y)}{1-\tau_t}\mathrm{d}F(y) = N\frac{1+e}{1-\tau_t}\dot{w}_b\int_0^{\bar{y}} y\mathrm{d}F(y).$$

I require that

$$N\frac{1+e}{1-\tau_t}\dot{w}_b\int_0^{\bar{y}} y\mathrm{d}F(y) = N\frac{1+e}{1-\tau_t}\dot{w}_b\int_0^{\bar{y}} yh(y)\,\mathrm{d}F(y) \iff \int_0^{\bar{y}} y\mathrm{d}F(y) = \int_0^{\bar{y}} yh(y)\,\mathrm{d}F(y)$$

I consider a simple and tractable form of heterogeneity in trickle-down effects whereby trickle-down effects are either more or less pronounced above some income. Formally,

$$h(y) = \begin{cases} \phi & \text{if } y \geq x \\ \eta\phi & \text{if } y < x \end{cases}$$

where x denotes the cutoff and $\phi > 0$ determines how much more ($\phi > 1$) or less ($\phi < 1$) pronounced trickle-down effects are above x.

I set the income cutoff so that half of bottom 99% income is located above and half below the cutoff. Formally, I set x such that $\int_0^x y\mathrm{d}F(y) = 0.5 * \int_0^{\bar{y}} y\mathrm{d}F(y)$. In the calibration, this corresponds to approximately $92k.

Given the requirement that the total income change is constant and that bottom 99% income is equally split above and below x, it is possible to solve for η as a function of ϕ. Formally,

$$\begin{aligned} N\frac{1+e}{1-\tau_t}\dot{w}_b\int_0^{\bar{y}} y\mathrm{d}F(y) =& N\frac{1+e}{1-\tau_t}\dot{w}_b\eta\phi\int_0^{x} y\mathrm{d}F(y) + N\frac{1+e}{1-\tau_t}\dot{w}_b\phi\int_x^{\bar{y}} y\mathrm{d}F(y) \\ 1 =& 0.5 * \eta\phi + 0.5 * \phi \\ \eta =& \frac{2-\phi}{\phi} \end{aligned}$$

I repeat the analysis of Section 1.5 for values of $\phi \in [0, 2]$. If $\phi = 2$ ($\phi = 0$) then trickle-down effects exist only for agents with an income above (below) $92k. If $\phi = 1.5$, then the percentage wage change given a percentage change in the top net-of-tax rate is 3 times as pronounced above $92k than below. More generally, $\frac{\phi}{2-\phi}$ determines the ratio of trickle-down effects above vs. below $92k.

The solid blue line in Figure A.3 presents the results. If only workers with an income above $92k are affected by trickle down, the bound on local trickle-down effects implies a bound on the welfare offset of 5% of the mechanical revenue gain; 9 percentage points less than with an equal distribution. In contrast, if only workers with an income below $92k are affected by trickle down, the bound on local trickle-down effects implies a bound on the welfare offset of 23% of the mechanical revenue gain; 9 percentage points more than with an equal distribution. If trickle-down effects are 5x, 4x, 3x, 2x as pronounced above (below) \$92k than below (above), the welfare bound is 6pp, 5.5pp, 4pp, 3pp lower (higher) than with an equal distribution.

Figure A.3 illustrates that two opposing forces determine how the bound on the welfare impact of local trickle-down effects changes as the distribution of trickle-down effects among workers changes. On the one hand side, as trickle-down effects become more concentrated among high-income worker, the utility impact decreases because welfare weights are decreasing in income. On the other hand side, given that marginal tax rates are increasing in income, more tax revenue below the top bracket is lost due to trickle-down effects when these are concentrated among high-income workers. Overall, the first effect dominates the second effect so that the welfare bound becomes smaller as trickle-down effects become more concentrated among high-income workers.

A.3.3 Optimal Tax Implications

This appendix combines the estimates of local trickle-down effects from this paper with the optimal tax framework from Sachs et al. (2020) (STW) to study the implications of the estimates for optimal top marginal tax rates in a fully optimal non-linear tax schedule.

STW characterize the fully optimal non-linear tax schedule when workers are imperfectly substitutable, the elasticity of substitution denoted by σ is constant across all worker types, the top tail of the skill distribution is Pareto, and worker preferences over leisure and consumption are quasi-linear and isoelastic with elasticity of labor supply denoted by e. Corollary 5 in STW reveals that the optimal top marginal tax rate in a fully optimal non-linear tax schedule is given by

$$\tau^* = \frac{1}{1 + \Pi \epsilon_r \zeta}, \quad \text{with} \quad \epsilon_r = \frac{e}{1 + \frac{e}{\sigma}} \quad \text{and} \quad \zeta = \frac{1}{1 - \Pi \frac{\epsilon_r}{\sigma}}$$

where Π is the Pareto parameter.

When interpreted as an estimate of the trickle-down factor in Proposition 1, the estimated wage effect of a higher exposure to the reform form implies an estimate of the ratio of the elasticity of labor supply e and the elasticity of substitution σ. Formally, if $\hat{\beta} = \frac{e}{\sigma + e} * \widehat{\Delta \ln (1 - \tau_t)}$, then $\widehat{\frac{e}{\sigma}} = \frac{\hat{\beta} / \Delta \ln(1-\tau_t)}{1 - \hat{\beta} / \Delta \ln(1-\tau_t)}$.

My estimates imply that at the 95% confidence level, I can reject $H_0 : \frac{e}{e+\sigma} \geq 0.345$ or alternatively $H_0 : \frac{e}{\sigma} \geq 0.527$. For a small elasticity of labor supply, the bound places very little restriction on σ. For larger values of e, however, the restrictions become more interesting. For example, if the elasticity of labor supply is 0.5, the elasticity of substitution must be at least 0.95 to fall within the bound.

Figure A.4 depicts, as functions of the elasticity of labor supply, the optimal top tax rate with fixed wages (blue), the rate implied by the smallest σ consistent with the estimates for that value of e (green), and the difference between the two (red). The difference between the two can be interpreted as the largest adjustment to the optimal top tax rate due to trickle-down effects that is statistically consistent with the estimates.

The bound on the required adjustment is U-shaped. The bound is most pronounced for a value of $e \approx 0.5$. For $e = 0.5$, the optimal top rate with fixed wages is 0.571 and the smallest value of σ consistent with $e = 0.5$ implies an optimal top rate of 0.495. At the 95% confidence level, the estimates are therefore inconsistent with a downward adjustment of the top tax rate due to trickle-down effects in excess of 7.6 percentage points.

Note that the estimates in this paper do not provide evidence in favor of adjustments within the bound. Rather, they provide strong evidence against adjustments in excess of the bound but only modest evidence against smaller adjustments.

A.3.4 Incidence Shares

This appendix uses the estimated bound on local trickle-down effects to calculate bounds on the share of the burden of a marginal increase in the tax rate for top 1% earners borne by various parts of the income distribution.

I proceed as follows. Using the IRS SOI public use file, I calculate for each tax return the change in post-tax income due to price changes triggered by a marginal increase in the tax rate for top 1% earners. I focus on income changes that are due to price changes as income changes due to behavioral responses have no first-order utility impact. For workers below the top 1%, the change in post-tax income arises due to lower wages from trickle-down effects. For top 1% earners, the change in post-tax income has two components. First, the decrease in post-tax income due to a higher marginal tax rate. Second, an increase in post-tax income due to higher wages of top earners (recall that when top earners reduce their effective labor supply, their own marginal product increases). I calculate the total burden as the sum over post-tax income changes across all taxpayers. I calculate shares of the burden for various groups of the income distribution by calculating the total post-tax income change for that group and dividing by the total burden. The calibration assumptions correspond to those described in Appendix A.3.1.

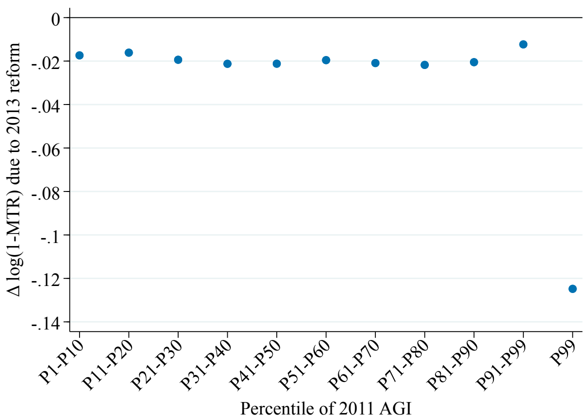
The 95% confidence bound on trickle-down effects implies that the percentage decrease in disposable income for an average top 1% earner is approximately 10 times as pronounced as the decrease in disposable income for the average worker. The percentage change in disposable income for an average worker below the top 1% is a decrease of -0.051%. In contrast, the average change for a top 1% earner is a decrease in disposable income of

-0.515%.[10]

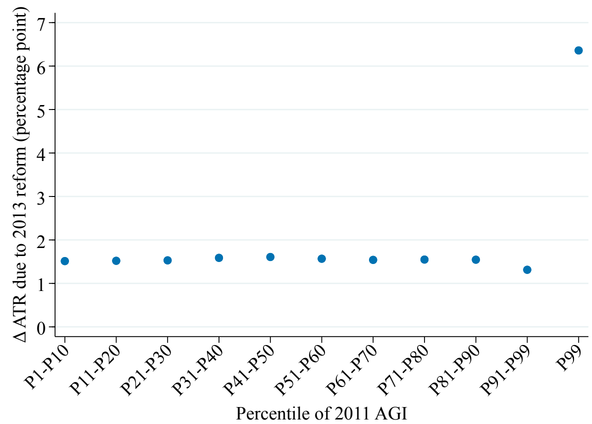
Figure A.5 depicts what share of the burden is borne by each decile of the income distribution with the top 1 percentile being separated from the top decile. The figure indicates that if trickle-down effects correspond to the 95% confidence bound, then the total share of the burden borne by top 1% earners is approximately 64% and the share borne by workers below the top is 36%. The reason that a -0.051% disposable income decrease can translate into a non-trivial share of the burden is that the bottom 99% account for the vast majority of disposable income, approximately 85% in this calculation. Because the percent change in the wage given a percent change in the net-of-tax rate for top 1% earners is constant among the bottom 99%, the share of the burden borne by a given decile increases with the amount of disposable income within the decile. As a consequence, the bottom 50% of the income distribution account for less than 10% of the total burden.

[10] The decrease is less than 1% because the higher tax rate only applies to income above the bracket cutoff and because higher wages partly offset the tax increase.

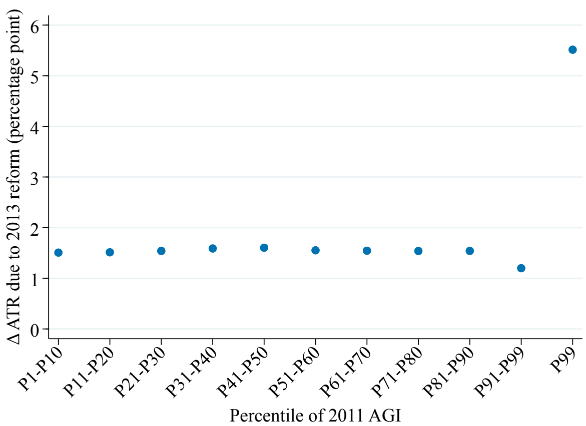
Appendix Figures



(a) MTR Changes by AGI Size - Excl. AMT



(b) ATR Changes by AGI Size - Excl. AMT



(c) ATR Changes by AGI Size - All Filers

Figure A.1: Changes to income tax due to 2013 reforms

Note: This figure illustrates the effects of the 2013 tax reform on marginal and average tax rates across the income distribution for non-AMT taxpayers, Panel (a) and (b), respecitvely, as well as on the average tax ratae for all filers, Panel (c). Each panel is created by using the 2011 IRS SOI public use microdata file housed at NBER and applying TAXSIM twice, once for 2012 and once for 2013. In each case, 2011 data are inflated accordingly; marginal tax rates are calculated with respect to primary earnings.

Figure A.2: State share of pass-through payroll vs. share of pass-through income in 2012

Note: This figure provides evidence consistent with the hypotheis that top owner-managers live close to their business. The figure depcits the relationship between a state's share of pass-through payroll and the share of pass-through income in 2012. Pass-through payroll is taken from the County Business Patern data and includes total 2012 payroll by S-Corps and Partnerships by state of economic activity. Pass-through payroll is measured at the location of economic activity. Pass-through income is taken from IRS SOI tabulations of tax returns by state for 2012 and includes total pass-through income by state of residence. Pass-through income is measured by location of income receipt. The figure illustrates that pass-through income is concentrated in states where pass-through activity is concentrated, suggesting a link between the location of pass-through oweners and their businesses.

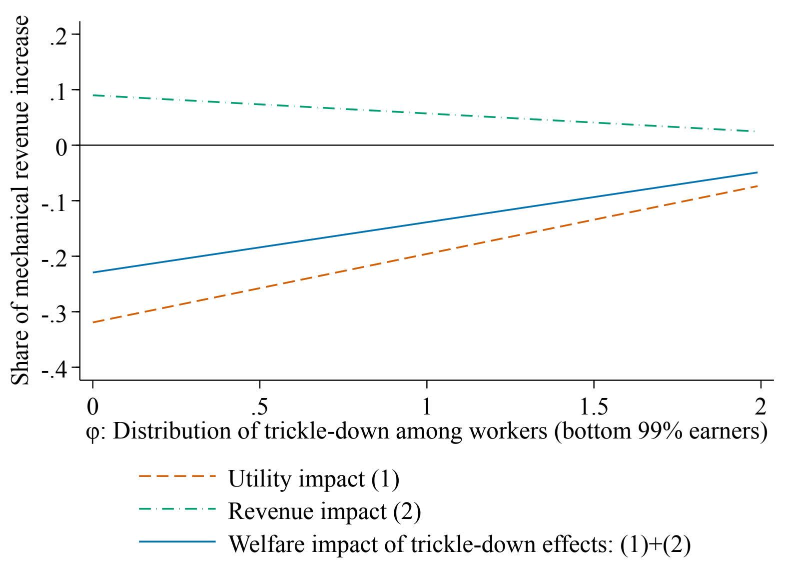


Figure A.3: Bounds on welfare impact as distribution of trickle-down effects varies among workers

Note: This figure illustrates how the bound on the welfare impact of trickle-down effects varies as the distribution of trickle-down effects among workers (bottom 99% earners) varies. I divide bottom 99% workers in two groups so that an equal amount of income is in each group, require that the total income change due to trickle-down effects among the bottom 99% remains constant and then vary the distribution of effects across the two groups. See Appendix A.3.2 for details. The solid blue line indicates that as trickle-down effects become more concentrated (ϕ increases) among high-income workers (bottom 99% earners), the bound on the welfare impact of trickle-down effects becomes smaller. Note that $\phi = 1$ corresponds to an equal split. The dashed red line indicates that, because welfare weights decrease with income, as ϕ increases, the welfare decrease via lower utility among bottom 99% workers becomes less pronounced. The dash-dotted green line indicates that, because marginal tax rates increase with income, as trickle-down effects become more concentrated among high-income workers (ϕ increases), the revenue loss from lower incomes below the top bracket becomes more pronounced, so that the revenue gain from trickle-down effects shrinks.

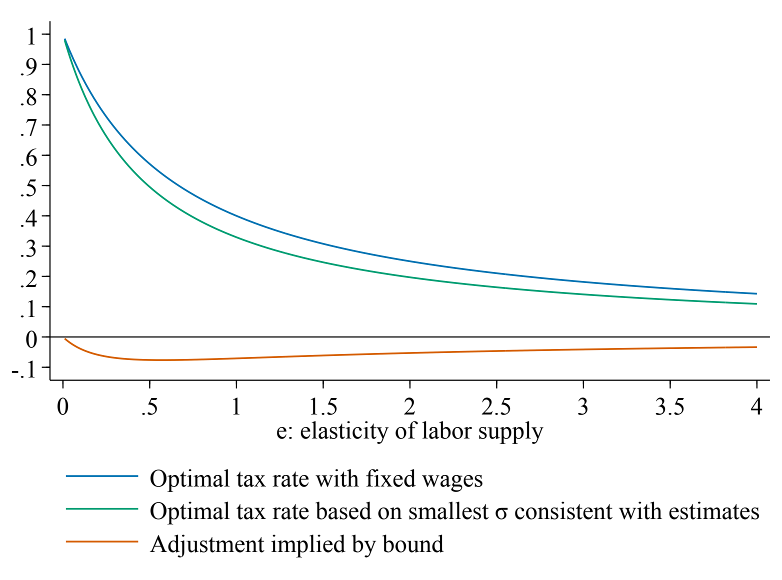


Figure A.4: Implications of bounds for optimal top marginal tax rate

Note: This figure illustrates the implications of the bounds on local trickle-down effects for optimal top tax rates in a fully optimal non-linear schedule using the framework of Sachs et al. (2020) (STW). Corollary 5 in STW presents an expression for the optimal top tax rate assuming that depends on the ratio of the elasticity of labor supply e and the elasticity of substitution σ. When interpreted as an estimate of the trickle-down factor in Proposition 1, the estimated wage effect of a higher exposure to the reform form implies an estimate of e/σ. The estimated bound on local trickle-down effects therefore essentially places a bound on the smallest possible value of σ for a given e. The green line in this figure, for each value of e, uses the smallest possible value of σ to calculate the optimal top tax rate. The blue line for comparison depicts the case with perfect subsitutability and the red line depicts the difference. The difference can be interpreted as the largest adjustment to the optimal top tax rate due to trickle-down effects that is statistically consistent with the estimates. Note that the estimates in this paper do not provide evidence in favor of adjustments within the bound. Rather, they provide strong evidence against adjustments in excess of the bound but only modest evidence against smaller adjustments. See Appendix A.3.3 for details.

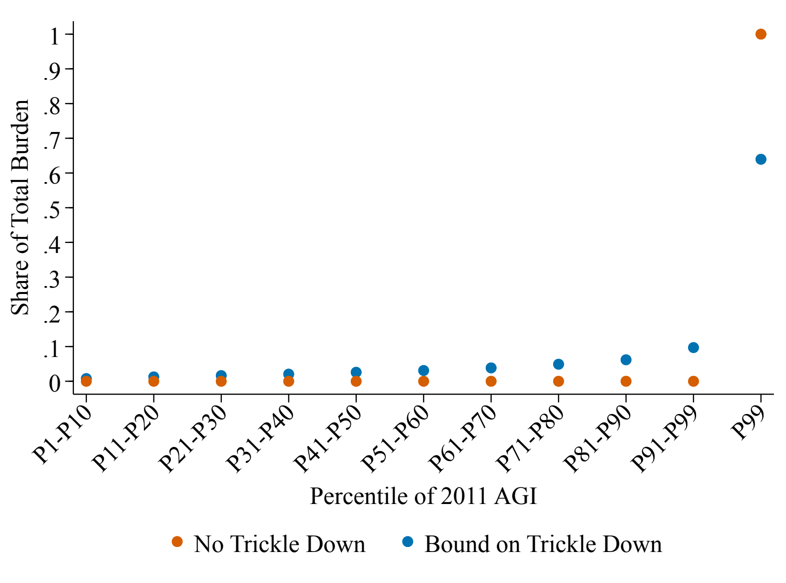


Figure A.5: Implications of bounds for incidence shares

Note: This figure illustrates the implications of the bounds on local trickle-down effects for how the burden of a marginal increase in the tax rate for top 1% earners is distributed across the income distribution. I proceed as follows. Using the IRS SOI public use file, I calculate for each tax return the change in post-tax income (disposable income) due to price changes triggered by a marginal increase in the tax rate for top 1% earners. I focus on income changes that are due to price changes as income changes due to behavioral responses have no first-order utility impact. For workers below the top 1%, the change in post-tax income arises due to lower wages from trickle-down effects. For top 1% earners, the change in post-tax income has two components. First, the mechanical decrease in post-tax income due to a higher marginal tax rate. Second, an increase in post-tax income due to higher wages of top earners (recall that when top earners reduce their effective labor supply, their own marginal product increases). I calculate the total burden as the sum over post-tax income changes across all taxpayers. I calculate shares of the burden for various groups of the income distribution by calculating the total post-tax income change for that group and dividing by the total burden. See Appendix A.3.4 for details.

Appendix Tables

	Mean	Sd	P25	P50	P75
	(1)	(2)	(3)	(4)	(5)
Panel A: QCEW Sample ($N = 924$)					
Top 1% income share	0.134	0.072	0.083	0.122	0.164
Cyclicality beta	0.949	0.424	0.685	0.895	1.177
Housing boom	0.277	0.164	0.145	0.253	0.410
Housing bust	-0.218	0.206	-0.307	-0.155	-0.061
Labor income log change 2011-2018	0.169	0.049	0.144	0.164	0.183
Bartik income predictor 2018	0.204	0.012	0.197	0.204	0.211
Labor income log change 2011-2003	-0.229	0.055	-0.257	-0.223	-0.202
Bartik income predictor 2003	-0.219	0.013	-0.226	-0.220	-0.209
Panel B: ACS Sample ($N = 361$)					
Top 1% income share	0.143	0.070	0.097	0.132	0.170
Cyclicality beta	0.993	0.404	0.732	0.927	1.178
Housing boom	0.283	0.167	0.147	0.258	0.438
Housing bust	-0.234	0.208	-0.356	-0.170	-0.075
CC labor income log change pre vs. post	-0.001	0.032	-0.020	-0.004	0.017
CC hourly wage log change pre vs. post	-0.001	0.028	-0.020	-0.002	0.016
Bartik income predictor 2018	0.205	0.012	0.198	0.205	0.211

Table A.1: Summary statistics

Note: This table presents summary statistics for the primary outcome variables in the QCEW and ACS sample as well as the controls in the baseline specification. Note that summary statistics are based on 2011 population-weighted observations. Note that the change in composition-constant wages between the pre- and post-reform period is zero because composition-constant wages are estimated as the CBSA fixed effect in a Mincerian wage regression. Composition-constant wages are therefore demeaned in each period.

Top 1% Income Share	CBSA Name
15 CBSAs with lowest top 1% income share	
1.37%	Hinesville-Fort Stewart, GA
2.65%	Vallejo-Fairfield, CA
2.96%	Anderson, IN
3.11%	Longview, WA
3.20%	Pine Bluff, AR
3.22%	Yuba City, CA
3.25%	Kokomo, IN
3.26%	Vineland-Millville-Bridgeton, NJ
3.28%	Cumberland, MD-WV
3.33%	Warner Robins, GA
3.41%	Jacksonville, NC
3.51%	Lewiston-Auburn, ME
3.55%	Sumter, SC
3.58%	Pueblo, CO
3.59%	Hagerstown-Martinsburg, MD-WV
15 CBSAs with highest top 1% income share	
23.86%	Trenton-Ewing, NJ
23.88%	Charlottesville, VA
24.12%	Houston-Sugar Land-Baytown, TX
24.53%	Eau Claire, WI
25.00%	Fayetteville-Springdale-Rogers, AR-MO
25.96%	Boston-Cambridge-Quincy, MA-NH
27.09%	Miami-Fort Lauderdale-Pompano Beach, FL
27.47%	San Angelo, TX
28.64%	San Francisco-Oakland-Fremont, CA
29.04%	San Jose-Sunnyvale-Santa Clara, CA
29.71%	New York-Northern New Jersey-Long Island, NY-NJ-PA
39.12%	Sebastian-Vero Beach, FL
41.79%	Midland, TX
51.38%	Naples-Marco Island, FL
52.31%	Bridgeport-Stamford-Norwalk, CT

Table A.2: List of CBSAs with lowest and highest top 1% income share

Note: This table lists the CBSAs with the 15 smallest and 15 largest top 1% income shares in 2011. Note that this table only considers CBSAs that are both in the ACS and QCEW sample.

	Mean 2012-2018	Mean 2012-2014	Mean 2015-2018
	(1)	(2)	(3)
1. Main	0.0085	0.0057	0.0106
	(0.0399)	(0.0287)	(0.0499)
2. Self-contained CBSAs	0.0882	0.0641	0.1059
	(0.0858)	(0.0642)	(0.1035)

Table A.3: Addressing measurement error concerns

Note: This table presents the results of an additional excercise intended to address measurement error concerns due to top earners working and living in different CBSAs. To facilitate comparisons across regressions, the top 1% income share enters (2.3.1) unstandardized. Row 1 presents the estimates from the main speicification for comparison (see also Table 1.1). Row 2 restricts attention to CBSAs where most labor income generated within the CBSA is earned by CBSA residents. Each estimate is given by the average of the coefficients $\hat{\beta}_h$ from the main specification (2.3.1) using CBSA QCEW annual wages as the outcome. Standard errors are clustered at the CBSA level. Each column presents the estimated effect of a one standard deviation increase in the exposure to the top tax increase (top 1% income share in 2011) on (log) CBSA annual wages relative to 2011. The estimates of the second row do not alter the conclusions from the main specification. See Appendix A.2.4 for a more detailed discussion.

	Avg. Tax Rate		Avg. Capital Gains		Avg. Charitable Contr.	
	2012	2013-2017	2012	2013-2017	2012	2013-2017
	(1)	(2)	(3)	(4)	(5)	(6)
1. No Controls	0.0001	0.0042	0.0295	-0.0278	0.0421	0.0665
	(0.0003)	(0.0005)	(0.0122)	(0.0157)	(0.0054)	(0.0117)
2. Baseline Controls	-0.0008	0.0025	-0.0140	-0.0613	0.0334	0.0467
	(0.0002)	(0.0003)	(0.0195)	(0.0186)	(0.0090)	(0.0083)

Table A.4: Effect of higher exposure to top tax increase on CBSA average tax rate, capital gains realizations and charitable contributions

Note: This table presents the estimated effects of a higher exposure to the 2013 tax increase for top earners on the CBSA average tax rate (1)-(2), (log) average capital gains realizations (3)-(4) and (log) average charitable contributions (5)-(6) relative to 2011. The esimates in row 1 stem from a population-weighted regression of the outcome change relative to 2011 on the standardized exposure measure: $\Delta y_{i,h} = \beta_0 + \beta_h S_i + \epsilon_{i,h}$. The regression is estimated on the full sample of CBSAs. The first column for each outcome presents the estimated effect on the outcome in 2012 relative to 2011 and the second column presents the average estiamted effect on the outcome over the period 2013-2017 relative to 2011. Row 2 contains the estimates from a regression with the same controls as the baseline specification (2.3.1). Consistent with the effects of the 2013 tax reform on the tax burden of top earners and the behavioral incentives it created, the panels indicate that CBSAs with a higher exposure to the reforn experienced an increase in the average tax rate, a temporary increase followed by a decrease in capital gains realizations, and an increase in charitable contributions. See Appendix A.2.5 for a more detailed discussion.

	Mean 2012-2018	Mean 2012-2014	Mean 2015-2018
	(1)	(2)	(3)
$\hat{\beta}_h$	0.0017	0.0003	0.0028
	(0.0037)	(0.0027)	(0.0054)
Weights	Pop. 2011	Pop. 2011	Pop. 2011
Cyclicality Control	20 Quantiles	20 Quantiles	20 Quantiles
Region FE	State	State	State
Bartik	Yes	Yes	Yes
Housing Boom/Bust	Yes	Yes	Yes
CBSAs	914	914	914
Obs.	6398	2742	3656
R2	.188	.108	.21

Table A.5: Effect of higher exposure to top tax increase on CBSA annual wages in tradable sector

Note: This table presents the estimated effects of a higher exposure to the 2013 tax increase for top earners on CBSA annual wages in the tradable sector relative to 2011 over three time periods: 2012-2018 (1), 2012-2014 (2) and 2015-2018 (3). Each estimate is given by the average of the coefficients $\hat{\beta}_h$ from the main specification (2.3.1) using CBSA QCEW annual wages in the tradable sector as the outcome. For example, the mean for 2012-2018 is: $1/7 \left(\sum_{h=2012}^{2018} \hat{\beta}_h \right)$. The tradable sector is the union of NAICS 11 (Agriculture, Forestry, Fishing and Hunting), 21 (Mining) and 31-33 (Manufacturing). Each column presents the estimated effect of a one standard deviation increase in the exposure to the top tax increase (top 1% income share in 2011) on (log) CBSA annual wages relative to 2011. The focus on the tradable sector is intended to address concerns that CBSAs with a low exposure to the 2013 tax increase for top earners are more exposed to the 2013 Social Security tax increase. Because the Social Security tax increase potentially affected labor markets primarily via shifts in the demand for goods and services, the tradable sector should be less impacted by the Social Security tax increase. However, the mechanisms via which a higher exposure to the 2013 tax incrase for top earners potentially impact local labor markets still operate in the tradable sector. The estimates are close to the estimates from the main specification using average CBSA annual wages across all sectors and do not indicate that differential exposure to the Social Security tax increase are biasing the estiamtes. See Appendix A.2.7 for a more detailed discussion.

	Wage at Percentile			Mean CC Wage within Tercile		
	25th	50th	75th	Bottom	Middle	Top
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\beta}_h$	0.0020	0.0075	0.0039	0.0018	0.0056	0.0019
	(0.0034)	(0.0043)	(0.0035)	(0.0033)	(0.0037)	(0.0031)
Weights	Pop. 2011	Pop. 2011	Pop. 2011	Pop. 2011	Pop. 2011	Pop. 2011
Cyclicality Control	20 Quantiles	20 Quantiles	20 Quantiles	20 Quantiles	20 Quantiles	20 Quantiles
Region FE	Division	Division	Division	Division	Division	Division
Bartik	Yes	Yes	Yes	Yes	Yes	Yes
Housing Boom/Bust	Yes	Yes	Yes	Yes	Yes	Yes
CBSAs	361	361	361	361	361	361
Obs.	361	361	361	361	361	361
R2 (within)	.308	.386	.410	.243	.411	.398

Table A.6: Effect of higher exposure to top tax increase on CBSA hourly wage by quartile

Note: This table demonstrates that the absence of detectable heterogeneity in the magnitude of local trickle-down effects across worker types as measured by education extends to the case when heterogeneity is measured by income. The table presents the estimated effects of a higher exposure to the top tax increase on CBSA hourly wages for quartiles of the within CBSA hourly wage distribution. American Community Survery (ACS) public use microdata are used to calculate CBSA level wage measures that explicitly exlude any top earner labor income. The sample is restricted to subset of 361 CBSAs that are Metropolitan Statistical Areas and can be identified in the ACS. To increase precision, five annual ACS samples are pooled to create one pre-reform (2007-2011) and one post-reform observation (2014-2018). For each CBSA, I calculate the (log) hourly wage at the 25th, 50th and 75th percentile separetely for the pre-reform and post-reform period. Column (1) presents the estimates (log) hourly wages at the 25th percentile, (2) presents the estimates for at the 50th percentile and (3) presents the estimates at the 75th percentile. For columns (4)-(6) I first divide workers in each CBSA into three groups depending on their position in the wage distribution and then estimate composition-controlled wages separately for each tercile. Each column indicates the effect of a one standard deviation increase in the exposure measure (top 1% income share in 2011) on (log) CBSA wages in 2014-2018 relative to 2007-2011. The estimates do not indicate that the lack of an average wage response masks heterogenous impacts across the wage distribution. Each estimate is insignifcant and the p-value on an F-test of equality is 0.18 for (1)-(3) and 0.28 for (4)-(6). See Appendix A.2.1.2 for more details on the ACS data.

APPENDIX B

Appendix to Chapter 2

B.1 Empirical Appendix

B.1.1 Less Entry or Less Events that Trigger Id Changes?

This appendix provides some evidence that the drop in business entry reflects a true decrease in new businesses, as opposed to a decrease in events such as ownership changes that trigger id changes but do not represent true economic entry. The approach is as follows. While less entry should lead to a decrease in the number of businesses, less ownership changes or other events that trigger id changes do not impact the number of businesses. Table B.4 Column (1) presents the medium-term effect of a one-percentage point increase in the local business tax on scaled business entry and Column (3) presents the medium-term effect on the scaled number of businesses. The estimates indicate that the drop in business entry is accompanied by a drop in the number of businesses. Because the number of businesses is a stock and entry is a flow, comparing the magnitude of the estimates requires comparing the effect on the number of businesses at time t with the cumulated effect on the number of entrants up to point t. As discussed above, the cumulated effect of a one-percentage point increase in the local business tax rate on scaled business entry -0.61 percentage points. Column (4) of Table B.4 shows that the five-year effect on the scaled number of businesses is -0.61, so that the drop in the number of businesses corresponds closely to what would be expected given the drop in entry.

B.1.2 Quantifying Relocation Effects

This appendix presents a back-of-the-envelope calculation of what share of the decrease in business entry in the municipality that increases its tax rate is due to relocation effects. The starting point are the estimates presented in Table 2.3 that use more granular and coarser geography-by-year fixed effects.

Denote by y the decrease in entry in a municipality that increases its tax rate and by x the average change in entry in municipalities in the same county. The estimated effect of the local business tax increase on entry is then given by $y - x$ and corresponds to -0.136, Column 4. Denote the average change in entry in municipalities that are in the same governmental district by $\tilde{x}$. The estimated effect of the local business tax increase on entry is then given by $y - x$ and corresponds to -0.101, Column 6. Note that $\tilde{x}$ will be an average of the change in entry in municipalities that are in the same county, x, and the change in entry in municipalities that are in the same governmental district but not county, $\hat{x}$. Suppose that the weights correspond to the number of municipalities: $\tilde{x} = \frac{N_x}{N_x+N_{\hat{x}}} x + \frac{N_{\hat{x}}}{N_x+N_{\hat{x}}} \hat{x}$. Further, suppose that the change in entry in control municipalities is only due to relocation effects, and that relocation happens only to municipalities in the same county. Formally: $x = \text{relocation effect}$ and $\hat{x} = 0$. The change in entry in the municipality that increases its tax rate corresponds to a true decrease in entry and a relocation effect: $y = \text{true decrease} - \text{relocation effect}$. We now have a system of two equations in two unknowns:

$$y - x = \text{true effect} - 2 * \text{relocation effect} = -0.136 \tag{B.1}$$

$$y - \tilde{x} = \text{true effect} - \left(1 + \frac{N_x}{N_x + N_{\hat{x}}}\right) \text{relocation effect} = -0.101 \tag{B.2}$$

Setting N_x to the average number of municipalities in the same county, $N_x = 12$, and $N_x + N_{\hat{x}}$ to the average number of municipalities in the same governmental district, $N_x + N_{\hat{x}} = 94$, implies a relocation effect of approximately 0.04, or approximately 30% of the total effect. Repeating this exercise using the counties in the same commuting zone as the baseline for comparison against the governmental district implies a relocation effect of approximately 0.05 or approximately 33% of the total effect.

Appendix Tables

	All	Corporate	Non-Corporate
	(1)	(2)	(3)
Panel A: All			
# Businesses (2002-2012)	22,003,543	8,727,781	13,275,762
Mean # workers per business	13.61	26.82	4.93
Panel B: Entrants			
# entrants (2004-2012)	1,749,769	538,871	1,210,898
Mean # workers per entrant	4.20	9.03	2.05

Table B.1: Descriptive statistics of microdata

Note: This table presents descirpitve statistics of the microdata. The level of observation is an establishment year. The sample consists of all businesses, establishments (single-establishment firms or establishments of multi-establishment firms) that employed at least one worker on 31-Dec of a given calendar year. For single-establishment firms, the legal form determines whether the firm is incorporated. Establishments of multi-establishment firms as well as firms that are part of a tax group are classified as incorporated. Note that non-corporate includes both unincorporated establishments as well as establishments that are either a public/non-profit institution or for which the legal form is missing. Approximately 85% of non-corporate establishments are unincorporated, approximately 78% of non-corporate entrants are unincorporated and approximately 77% of non-corporate exits are unincorporated. The number of workers corresponds to the number of workers subject to social security contributions employed at an establishment on 31-Dec of a given calendar year.
Source: RDC of the Federal Statistical Office and Statistical Offices of the Laender, AFiD-Panel Unternehmensregister - System 95, Berichtsjahre 2002-2012, own calculations.

	2004	2005	2006	2007	2008	2009	2010	2011	2012	**Mean**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
# (000s)	200	198	198	202	203	190	190	190	178	**194**
Rate (%)	10.2	10.1	10.0	10.2	10.1	9.4	9.3	9.3	8.7	**9.7**

Table B.2: Overview of all business entry

Note: This table presents the number and rate of all (corporate, unincorporated and other) business (employer establishment) entry. An employer establishment is an establishment that employs at least one worker at the end of a given calendar year. An employer establishment enters in t if its establishment id is not found in the list of employer establishments in $t-1$ and $t-2$.
Source: RDC of the Federal Statistical Office and Statistical Offices of the Laender, AFiD-Panel Unternehmensregister - System 95, Berichtsjahre 2002-2012, own calculations.

	Mean	25th Percentile	Median	75th Percentile	*N*
	(1)	(2)	(3)	(4)	(5)
Panel A: Business Register (per municipality year)					
# Entrants	55	9	22	50	20,536
# Entrants that are firms	40	6	15	35	20,536
# Entrants w. $<$ 2 employees	21	3	8	19	20,536
# Entrants w. $<$ 3 employees	29	5	12	27	20,536
# Entrants in non-tradable sector	42	6	16	39	20,536
# Unincorporated entrants	83	17	37	77	20,536
# All entrants	162	30	69	151	20,536
# Businesses	704	140	323	706	20,536
# Unincorporated businesses	778	180	378	758	20,536
# All businesses	1,631	351	753	1603	20,536
# Workers	28,094	4,363	11,138	28,166	20,536
Panel B: Tax Data					
Tax rate (%)	14.2	12.6	14.1	15.4	20,536
# Tax changes	2	1	2	3	20,536
Size of tax change (pp)	+0.54	+0.35	+0.53	+0.80	5,111

Table B.3: Descriptive statistics of main estimation sample

Note: This table presents summary statistics of the dependent and indendent variables in the regression model for the main analsysis sample. Municipalities are weighted by their regression weight: the median number of workers in the municipality over the period 2004-2012, winsorized at the 99th percentile. The main analysis sample consists of non-merged municipalities that have at least 1,000 workers on average between 2004-2012. There are 2,567 unique municipalities in the main analysis sample. A business corresponds to an employer establishment. Unless otherwise noted, a buiness/entrant refers to a corporate business/entrant. For the business register variables, the statistics are based on the years 2005-2012 as these are the years that enter the first-differenced regression (2004 is dropped as the difference to 2003 is undefined). The tax rate statistics are based on 2005-2012. The number and size of changes are based on 2000-2016, as these are the years used to identify leads and lags of tax changes.
Source: RDC of the Federal Statistical Office and Statistical Offices of the Laender, AFiD-Panel Unternehmensregister - System 95, Berichtsjahre 2002-2012, own calculations. Federal Statistical Office and Statistical Offices of the Laender, Hebesaetze der Realsteuern, 1996-2016, own calculations.

	Business entrants		Businesses	
	Med.-term ($\frac{1}{3}\sum_3^5 \hat{\beta}_k$)	Cum. ($\sum_0^5 \hat{\beta}_k$)	Med.term ($\frac{1}{3}\sum_3^5 \hat{\beta}_k$)	5-year ($\hat{\beta}_5$)
	(1)	(2)	(3)	(4)
Estimate	-0.136	-0.607	-0.520	-0.614
Standard error	(0.053)	(0.240)	(0.128)	(0.151)
Weights	Yes	Yes	Yes	Yes
State x year FE	Yes	Yes	Yes	Yes
Com. zone x year FE	Yes	Yes	Yes	Yes
Municipalities	2,567	2,567	2,567	2,567
Obs.	20,536	20,536	20,536	20,536

Table B.4: Less business entry or less other events that trigger id changes?

Note: This table presents estimated medium-term effects of a local business tax increase on the number of business entrants and the number of businesses. The purpose is to investigate whether the drop in entry documented in Table 2.2 reflects a true drop in business entry as oposed to a drop in other events, such as ownership changes, that trigger changes in establishment ids. The idea is that while a decrease in true business entry has an impact on the number of businesses, a decrease in events such as ownership changes does not. Columns are differentiated by the outcome that enters the main specification (2.3.1) and the combination of estimates they represent. Column (1) presents the medium-term estimated effect on the scaled number of businesses entrants and Column (2) presents the cumulated effect over the post-reform period. Column (3) presents the estimated medium-term effect on the scaled number of businesses and Column (4) presents the five-year effect. The estimates indicate that the number of businesses decreases along with the number of entrants. Because entry reflects the inflow to the stock of businesses, comparing the estimates between entry and the number of businesses requires comparing the cumulated entry effect up to year t with the efffect on the number of businesses in year t. Comparing the 5-year effect of a local business tax increase on the number of businesses with the cumulated effect on the number of entrants reveals that the drop in the number of businesses is what would be expected given the decrease in entry. The exercise therefore supports the notion that the drop in entry is driven by a true decrease in economic entry. For more details see Section 2.4.
Source: RDC of the Federal Statistical Office and Statistical Offices of the Laender, AFiD-Panel Unternehmensregister - System 95, Berichtsjahre 2002-2012, own calculations.

APPENDIX C

Appendix to Chapter 3

C.1 Theoretical Appendix

C.1.1 Baseline

The economic environment is as described in Section 3.2. Firm j produces output using workers of type a and b as well as capital: $Q^j = T^j F^j(K^j, L^j(N_a^j, N_b^j))$ where F^j and L^j are constant returns to scale, N_i^j denotes the number of workers of type i, K^j denotes the units of capital, and T^j denotes total factor productivity at firm j. Firms can rent capital at a price r. A firm's output is sold on a monopolistically competitive product market characterized by a constant price elasticity of demand schedule with the price elasticity denoted by $\beta^j > 1$. Firms face upward sloping labor supply schedules for each type of worker: $N_i^j = \Omega_i^j (w_i^j)^{\epsilon^j}$. Ω_i^j is allowed to depend on the outside option of worker type i. I assume that that the outside option is constant across firms, is captured by the average wage of worker type i across all firms, $\bar{w}_i$, and that firms are small so that a change in the wage offered by an individual firm has a negligible effect on the average wage. A payroll tax of τ_i is levied on the wage of worker type i with legal incidence fully on firms. A firm's profit maximization problem reads

$$\max_{w_a^j, w_b^j, K^j} P^j(Q^j)F^j(K^j, L^j(N_a^j, N_b^j)) - \sum_{i \in \{a,b\}} w_i^j (1+\tau_i) N_i^j(w_i^j) - rK^j.$$

Suppose that the outside option is constant. Note that when payroll taxes change, a firm responds and optimally adjusts its production process. The adjustment leads to changes in prices, and these price changes trigger a further production adjustment. The endogenous variables in the model are: $(P^j, Q^j, K^j, N_a^j, N_b^j, w_a^j, w_b^j)$. The first order conditions, the market clearing conditions, and the production equation constitute a system of seven equations that pins down the endogenous variables of the system. Consider a marginal increase to a uniform payroll tax rate τ. Let $\dot{X}^j = \frac{\partial \ln X^j}{\partial \ln(1+\tau)}$. Taking the logarithm of the model's seven equations and totally differentiating with respect to $\ln(1+\tau)$ yields:

$$\dot{P}^j + S_L^j \frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) = 0, \tag{C.1}$$

$$\dot{P}^j - (1 - S_L^j)\frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) - (1 - s_a^j)\frac{1}{\phi^j}(\dot{N}_a^j - \dot{N}_b^j) = 1 + \dot{w}_a^j, \tag{C.2}$$

$$\dot{P}^j - (1 - S_L^j)\frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) + s_a^j \frac{1}{\phi^j}(\dot{N}_a^j - \dot{N}_b^j) = 1 + \dot{w}_b^j, \tag{C.3}$$

$$\dot{Q}^j = (1 - S_L^j)\dot{K}^j + S_L^j(s_a^j \dot{N}_a^j + (1 - s_a^j)\dot{N}_b^j), \tag{C.4}$$

$$\dot{Q}^j = -\beta^j \dot{P}^j, \tag{C.5}$$

$$\dot{N}_a^j = \epsilon^j \dot{w}_a^j, \tag{C.6}$$

$$\dot{N}_b^j = \epsilon^j \dot{w}_b^j, \tag{C.7}$$

where $S_L^j = \frac{T^j F_2^j L^j}{Q^j}$ is the labor share of output at firm j, $s_a^j = \frac{L_1^j N_a^j}{L^j}$ is the labor share of type a workers at firm j, $\sigma^j = \frac{F_1^j F_2^j}{F_{12}^j F^j}$ is the elasticity of substitution between capital K^j and labor L^j and $\phi^j = \frac{L_1^j L_2^j}{L^j L_{12}^j}$ is the elasticity of substitution between type a and b workers at firm j. These equations can be used to solve for $\dot{w}_i^j$. Note that the when the payroll tax is asymmetric and there is a marginal increase only in τ_a, the only equation that changes is the totally differentiated FOC for type b workers (C.33). It now reads

$$\dot{P}^j - (1 - S_L^j)\frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) + s_a^j \frac{1}{\phi^j}(\dot{N}_a^j - \dot{N}_b^j) = \dot{w}_b^j,$$

where now $\dot{X}^j = \frac{\partial \ln X^j}{\partial \ln(1+\tau_a)}$. All other equations remain the same and the system can be solved for $\dot{w}_i^j$.

The exposition of the previous paragraph also reveals why the legal incidence does not affect the economic incidence in the model. Suppose that the legal incidence is instead on workers, so that a workers after-payroll tax wage is $(1 - \tau_i)w_i^j$. Consider a marginal increase to a uniform payroll tax rate τ. The system of equations is now given by:

$$\dot{P}^j + S_L^j \frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) = 0, \tag{C.8}$$

$$\dot{P}^j - (1 - S_L^j)\frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) - (1 - s_a^j)\frac{1}{\phi^j}(\dot{N}_a^j - \dot{N}_b^j) = \dot{w}_a^j, \tag{C.9}$$

$$\dot{P}^j - (1 - S_L^j)\frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) + s_a^j \frac{1}{\phi^j}(\dot{N}_a^j - \dot{N}_b^j) = \dot{w}_b^j, \tag{C.10}$$

$$\dot{Q}^j = (1 - S_L^j)\dot{K}^j + S_L^j(s_a^j \dot{N}_a^j + (1 - s_a^j)\dot{N}_b^j), \tag{C.11}$$

$$\dot{Q}^j = -\beta^j \dot{P}^j, \tag{C.12}$$

$$\dot{N}_a^j = \epsilon^j(\dot{w}_a^j - 1), \tag{C.13}$$

$$\dot{N}_b^j = \epsilon^j(\dot{w}_b^j - 1), \tag{C.14}$$

where now $\dot{X}^j = \frac{\partial \ln X^j}{\partial \ln(1-\tau)}$. Substitute (C.13) and (C.14) in (C.9) and (C.10) as well as (C.36) and (C.37) in (C.32) and (C.33) and notice that the resulting system is the same.

C.1.2 Changes in the Outside Option

I begin by deriving expressions for the effect that a change in the outside option has on a worker's wage. Consider an exogenous change in the outside option of worker type a, $\bar{w}_a$. Let $\dot{X}^j = \frac{\partial \ln X^j}{\partial \ln \bar{w}_i}$ The outcomes of interest are $\dot{w}_i^j$. The response of the endogenous variables are determined by the following seven equations:

$$\dot{P}^j + S_L^j \frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) = 0, \tag{C.15}$$

$$\dot{P}^j - (1 - S_L^j)\frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) - (1 - s_a^j)\frac{1}{\phi^j}(\dot{N}_a^j - \dot{N}_b^j) = \dot{w}_a^j, \tag{C.16}$$

$$\dot{P}^j - (1 - S_L^j)\frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) + s_a^j \frac{1}{\phi^j}(\dot{N}_a^j - \dot{N}_b^j) = \dot{w}_b^j, \tag{C.17}$$

$$\dot{Q}^j = (1 - S_L^j)\dot{K}^j + S_L^j(s_a^j \dot{N}_a^j + (1 - s_a^j)\dot{N}_b^j), \tag{C.18}$$

$$\dot{Q}^j = -\beta^j \dot{P}^j, \tag{C.19}$$

$$\dot{N}_a^j = \dot{\Omega}_a^j + \epsilon^j \dot{w}_a^j, \tag{C.20}$$

$$\dot{N}_b^j = \epsilon^j \dot{w}_b^j, \tag{C.21}$$

where $\dot{\Omega}_a^j$ captures how the appeal of the firm changes when the outside option increases. I assume that $\dot{\Omega}_a < 0$ so that an increase in the outside option leads to a decrease in the labor supply to the firm. Solving the system yields the following expression:

$$\dot{w}_a^j = -\dot{\Omega}_a^j \Big[\frac{S_L^j\beta^j + (1-S_L^j)\sigma^j}{S_L^j\beta^j + (1-S_L^j)\sigma^j + \epsilon^j}\Big]\frac{1-s_a^j}{\phi^j + \epsilon^j} - \dot{\Omega}_a^j \frac{\epsilon^j + \phi^j s_a^j}{\epsilon^j + \phi^j}\frac{1}{S_L^j\beta^j + (1-S_L^j)\sigma^j + \epsilon^j} \tag{C.22}$$

$$\dot{w}_b^j = \dot{\Omega}_a^j \Big[\frac{S_L^j\beta^j + (1-S_L^j)\sigma^j}{S_L^j\beta^j + (1-S_L^j)\sigma^j + \epsilon^j}\Big]\frac{s_a^j}{\phi^j + \epsilon^j} - \dot{\Omega}_a^j \frac{\phi^j s_a^j}{\epsilon^j + \phi^j}\frac{1}{S_L^j\beta^j + (1-S_L^j)\sigma^j + \epsilon^j} \tag{C.23}$$

The equations illustrate that an increase in the outside option of type a workers has an unambiguously positive effect on the wages of type a workers and an ambiguous effect on type b workers. Similar expressions can be derived for a change in the outside option of type b workers.

Consider a marginal increase to a uniform payroll tax rate τ. The outside option now responds to the change in the payroll tax rate. Let $\Delta\bar{w}_i = \frac{\partial \ln \bar{w}_i}{\partial \ln(1+\tau)}$. Let $\dot{X}^j = \frac{\partial \ln X^j}{\partial \ln(1+\tau)}$. Taking the logarithm of the model's seven equations and totally differentiating with respect to $\ln(1+\tau)$ yields:

$$\dot{P}^j + S_L^j \frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) = 0, \tag{C.24}$$

$$\dot{P}^j - (1-S_L^j)\frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) - (1-s_a^j)\frac{1}{\phi^j}(\dot{N}_a^j - \dot{N}_b^j) = 1 + \dot{w}_a^j, \tag{C.25}$$

$$\dot{P}^j - (1-S_L^j)\frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) + s_a^j\frac{1}{\phi^j}(\dot{N}_a^j - \dot{N}_b^j) = 1 + \dot{w}_b^j, \tag{C.26}$$

$$\dot{Q}^j = (1-S_L^j)\dot{K}^j + S_L^j(s_a^j\dot{N}_a^j + (1-s_a^j)\dot{N}_b^j), \tag{C.27}$$

$$\dot{Q}^j = -\beta^j\dot{P}^j, \tag{C.28}$$

$$\dot{N}_a^j = \dot{\Omega}_a^j\Delta\bar{w}_a + \epsilon^j\dot{w}_a^j, \tag{C.29}$$

$$\dot{N}_b^j = \dot{\Omega}_a^j\Delta\bar{w}_b + \epsilon^j\dot{w}_b^j, \tag{C.30}$$

Solving this system and taking into account the expressions for the wage responses to changes in the outside option yield (3.2.2). A similar exercise can be applied to arrive at wage responses with a non-constant outside option and a non-uniform payroll tax change.

C.1.3 Different Elasticities

The key difference when allowing the elasticities of labor supply to vary by worker type is that now when the amount of labor used in production changes, the cost minimizing ratio of two types of workers changes. The labor supply of type i to firm j is now given by

$$N_i^j(w_i^j) = \Omega_i^j (w_i^j)^{\epsilon_i^j}.$$

Consider a marginal increase to a uniform payroll tax rate τ. Let $\dot{X}^j = \frac{\partial \ln X^j}{\partial \ln(1+\tau)}$. Taking the logarithm of the model's seven equations and totally differentiating with respect to $\ln(1+\tau)$ yields:

$$\dot{P}^j + S_L^j \frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) = 0, \tag{C.31}$$

$$\dot{P}^j - (1 - S_L^j)\frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) - (1 - s_a^j)\frac{1}{\phi^j}(\dot{N}_a^j - \dot{N}_b^j) = 1 + \dot{w}_a^j, \tag{C.32}$$

$$\dot{P}^j - (1 - S_L^j)\frac{1}{\sigma}(\dot{L}^j - \dot{K}^j) + s_a^j \frac{1}{\phi^j}(\dot{N}_a^j - \dot{N}_b^j) = 1 + \dot{w}_b^j, \tag{C.33}$$

$$\dot{Q}^j = (1 - S_L^j)\dot{K}^j + S_L^j(s_a^j \dot{N}_a^j + (1 - s_a^j)\dot{N}_b^j), \tag{C.34}$$

$$\dot{Q}^j = -\beta^j \dot{P}^j, \tag{C.35}$$

$$\dot{N}_a^j = \epsilon_a^j \dot{w}_a^j, \tag{C.36}$$

$$\dot{N}_b^j = \epsilon_b^j \dot{w}_b^j, \tag{C.37}$$

Solving the system for the endogenous responses yields the following expression for the wage response of a worker of type i at firm j:

$$\dot{w}_i^j = -\Big[\frac{[S_L^j \beta^j + (1 - S_L^j)\sigma^j][\phi^j + \epsilon_{-i}^j]}{[S_L^j \beta^j + (1 - S_L^j)\sigma^j][\phi^j + (\epsilon_b^j s_a^j + (1 - s_a^j)\epsilon_a^j)] + \phi^j[s_a^j \epsilon_a^j + (1 - s_a^j)\epsilon_b^j] + \epsilon_a^j \epsilon_b^j}\Big]$$

The increase in the payroll tax rate increases the cost of labor which increases production costs and increases the relative cost of labor and capital. The reason the expression varies from (3.2.1) is that the decrease in the amount of labor used also has implications for the cost minimizing ratio of type a and b workers. When the elasticity is equal, the cost minimizing ratio of type a and b workers did not change. How the cost minimizing ratio changes depends on both elasticities, ϵ_a^j and ϵ_b^j, as well as the labor share of type a workers, s_a^j. The same steps yield the wage responses in the case of a non-uniform rate increase.

C.2 Empirical Appendix

C.2.1 Data

The dataset is constructed in three steps. First, a set of firms is selected from the IAB Establishment Panel for the years 2000-2008. The IAB Establishment Panel contains survey results for a stratified sample of approximately 16,000 firms each year.[1] The number of linked firms in the data vary between 5,000 and 6,000, depending on the year. Next, all workers who were employed at one of the firms at any point between 1999 and 2009 are selected. The last step retrieves the labor market biographies of these workers for the years 1993-2010. The data allow the exact workforce composition of sample firms at any point in time between 1999 and 2009 and workers to be followed both before and after they work at a sample firm. Civil servants and self-employed workers are not included in the data as they are exempt from the social security system. For more information on the LIAB LM 9310 see Klosterhuber et al. (2013).

C.2.2 Private Health Insurance System

Overview The health care system is divided into a public and private health insurance system. Access to the private insurance system is restricted for workers who are not civil servants or self-employed. A workers is an obligatory member of the public insurance system unless their gross yearly earnings exceeds a threshold. In the years around 2000, the threshold corresponded to the health insurance payroll tax cap. Once the yearly earnings exceed the threshold for one year workers become voluntary members of the public insurance system and have the option of switching to the private system. The main difference between the public and private insurance system is that the contributions in the former are increasing in earnings up to the payroll tax cap whereas contributions to the latter system are determined by the age and health of the worker as well as the coverage of the insurance plan.[2] A worker who chooses to become a member of the private insurance system does not continue to contribute to the public system. The firm is required to contribute to a worker's private plan. The contribution corresponds to the minimum of half of the total premium or half of the payroll tax liability which would arise were the worker a member of the public insurance system. Unfortunately, the data do not contain

[1]The survey unit is an establishment. A firm potentially consists of multiple establishments if its production facilities are spread across multiple municipalities. Throughout the paper I refer to establishments as firms.

[2]A minimum amount of coverage is required by law. However, workers can reduce their premiums by increasing their co-pay.

information on a worker's insurance status.

Effect of Reform The effect of the reform on a privately insured workers differs from a publicly insured worker. While the reform does not change the total insurance premium a privately insured worker is charged for her plan, the firm's contribution towards the plan weakly increases and the worker's contribution weakly decreases. In particular, if prior to the reform half of the insurance premium exceeded the firm's share of payroll taxes for health and LT insurance which would be due were the worker a member of the public insurance system, the firm's contribution increases. The increase in the firm's contribution is bounded below by zero and bounded above by the increase in the payroll tax liability which would arise were the worker a member of the public insurance system. Since the total insurance premium does not respond to the reform, the worker's contribution to the insurance premium weakly decreases. The lower and upper bound on the absolute change in the worker's contribution correspond to the lower and upper bound for the firm. A worker who in a year prior to 2001 decided to become a member of the private insurance system and whose yearly earnings in 2001 were below the new cap was allowed to choose between staying in the private insurance system and switching back into the public system.

Appendix Tables

	Treated	Not Treated 1	Not Treated 2
Age			
20-30	0.05	0.08	0.11
30-40	0.31	0.36	0.34
40-50	0.46	0.41	0.38
50-60	0.17	0.15	0.17
>60	0.01	0.00	0.00
Gender			
Female	0.27	0.21	0.24
Male	0.73	0.79	0.76
Occupation			
Blue Collar	0.54	0.77	0.85
White Collar	0.46	0.23	0.15
Skill			
High	0.28	0.06	0.02
Medium	0.71	0.91	0.92
Low	0.01	0.03	0.06
N	7089	17209	14708

Table C.1: Summary statistics as of 2000 — workers

Note: The Table reports summary statistics for workers as of 2000. Column 1 reports the statistics for Treated workers and Columns 2 and 3 report the statistics for not treated workers. A worker is Treated if her total earnings in 2000 were in the range [EUR32.6k, EUR40.6k], where the lower bound corresponds to the pre-reform payroll tax cap for health and long-term care insurance. A worker is Not Treated 1 if her total earnings in 2000 were in the range [EUR24.6k, EUR32.6k]. A worker is Not Treated 2 if her total earnings in 2000 were in the range [EUR16.6k, EUR24.2k]. Based on her 2000 earnings, the reform did not increase the payroll tax liability of a not treated worker, but did increase the payroll tax liability of a treated worker. Workers are required to have been full-time employed at any firm prior to 2000 and to remain full-time employed at their 2000 employer throughout 2002. Each entry corresponds to the share of a given type of worker (e.g. 5% of Treated workers are between 20 and 30 years old in 2000).

	Share
Size	
<20	0.18
20-50	0.27
50-100	0.20
100-500	0.30
>500	0.05
Sector	
Manufacturing	0.45
Construction	0.13
Traffic	0.10
Trade Services	0.21
Mining, Agriculture, Energy	0.12
State	
East Berlin	0.08
Brandenburg	0.17
Mecklenburg-Vorpommern	0.15
Saxony	0.18
Saxony-Anhalt	0.18
Thuringia	0.24
Age	
<6	0.06
6-9	0.31
>9	0.63
Profit	
Good	0.37
Satisfactory	0.34
Low	0.26
Union	
Yes - Sector	0.46
Yes - Firm	0.12
No	0.41
N	631

Table C.2: Summary statistics as of 2000 — firms

Note: The Table reports summary statistics for firms as of 2000. Firms are required to employ at least one treated worker in 2000 and to operate between 1996 and 2002. Each entry corresponds to the share of a given type of worker (e.g. 8% of firms are in East Berlin).

	Treated	Not Treated 1	Not Treated 2
Age			
20-30	2.6	1.75	0.91
	(1.01)	(0.92)	(0.73)
	[0.14, 6.02]	[0.00, 6.42]	[0.00, 4.89]
30-40	2.31	1.68	0.84
	(1.07)	(0.96)	(0.70)
	[0.01, 6.22]	[0.01, 6.49]	[0.00, 5.33]
40-50	2.3	1.71	0.88
	(1.13)	(0.99)	(0.73)
	[0.00, 6.42]	[0.00, 6.00]	[0.00, 5.49]
50-60	2.13	1.61	0.87
	(1.15)	(1.03)	(0.73)
	[0.00, 6.42]	[0.00, 6.35]	[0.00, 6.02]
>60	1.5	1.34	0.69
	(1.07)	(1.07)	(0.55)
	[0.01, 4.48]	[0.012, 6.00]	[0.03, 2.49]
Gender			
Female	2.53	1.95	0.95
	(1.24)	(1.16)	(0.95)
	[0.01, 6.42]	[0.01, 6.00]	[0.00, 5.49]
Male	2.2	1.61	0.84
	(1.01)	(0.92)	(0.84)
	[0.02, 6.42]	[0.00, 6.42]	[0.00, 6.02]
Occupation			
Blue Collar	2.37	1.67	0.85
	(1.04)	(0.94)	(0.70)
	[0.01, 6.42]	[0.00, 6.00]	[0.00, 5.49]
White Collar	2.19	1.74	0.95
	(1.19)	(1.10)	(0.83)
	[0.00, 6.42]	[0.00, 6.42]	[0.00, 6.02]
Skill			
High	2.04	1.32	0.93
	(1.06)	(0.88)	(0.90)
	[0.00, 6.42]	[0.01, 5.51]	[0.00, 4.89]
Medium	2.39	1.7	0.87
	(1.12)	(0.99)	(0.72)
	[0.00, 6.42]	[0.00, 6.42]	[0.00, 6.02]
Low	2.4	1.82	0.79
	(1.07)	(0.96)	(0.70)
	[0.20, 3.79]	[0.03, 4.89]	[0.01, 4.48]

Table C.3: Covariation between $\Delta\mathcal{T}$ and worker characteristics as of 2000

Note: The Table reports statistics on the covariation of the exposure measure, $\Delta\mathcal{T}$, and worker characteristics. Each cell contains the mean exposure measure amongst workers with a given characteristic and earnings in 2000, the standard deviation, as well as the min and max. For example, the mean exposure amongst 20-30 year old Treated workers is 2.6, the standard deviation 1.01, the min 0.14 and the max 6.02.

	Mean	Std. Deviation	[min, max]
Size			
<20	1.41	1.38	[0.00, 6.40]
20-50	1.16	1.14	[0.00, 6.35]
50-100	1.11	1.19	[0.01, 6.22]
100-500	1.19	1.17	[0.01, 6.42]
>500	1.72	1.3	[0.03, 4.89]
Sector			
Manufacturing	1.21	1.15	[0.00, 6.42]
Construction	0.85	0.7	[0.01, 3.02]
Traffic	1.12	1.22	[0.01, 6.35]
Trade Services	1.65	1.58	[0.01, 6.40]
Mining, Agriculture, Energy	1.12	0.97	[0.00, 4.51]
State			
East Berlin	1.91	1.85	[0.01, 6.40]
Brandenburg	1.15	0.93	[0.01, 4.39]
Mecklenburg-Vorpommern	1.12	1.07	[0.01, 6.00]
Saxony	1.22	1.27	[0.00, 6.35]
Saxony-Anhalt	1.18	1.07	[0.05, 5.51]
Thuringia	1.21	1.25	[0.00, 6.42]
Age			
<6	1.51	1.48	[0.10, 6.40]
6-9	1.14	1.16	[0.00, 6.42]
>9	1.26	1.22	[0.00, 6.42]
Profit			
Good	1.41	1.4	[0.00, 6.42]
Satisfactory	1.12	1.14	[0.00, 6.00]
Low	1.2	1.01	[0.01, 5.33]
Union			
Yes - Sector	1.44	1.28	[0.00, 6.42]
Yes - Firm	1.31	1.31	[0.01, 6.40]
No	0.99	1.08	[0.00, 6.35]

Table C.4: Covariation between $\Delta\mathcal{T}$ and firm characteristics as of 2000

Note: The Table reports statistics on the covariation of the exposure measure, $\Delta\mathcal{T}$, and firm characteristics. Each row contains the mean exposure measure amongst firms with a given characteristic, the standard deviation, as well as the min and max. For example, the mean exposure amongst manufacturing firms is 1.21, the standard deviation 1.15, the min 0.00 and the max 6.42.

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