

Knaisch, Jonas; Pöschel, Carla

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Jonas Knaisch, Carla Pöschel

**Corporate Income Tax and Wages:
A Meta-Regression Analysis**

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Corporate Tax Incidence and Wages: A Meta-Regression Analysis*

Jonas Knaisch

Carla Pöschel

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Abstract

We apply meta-regression analysis to quantitatively review the growing empirical tax incidence literature that indicates a substantial shift of the corporate income tax burden on employees. While most studies report large negative elasticities, our findings suggest that estimates with positive values are published less often than they should. After accounting for publication bias, we estimate an average wage elasticity to corporate taxes of -0.024. Our average estimate of the semi-elasticity suggests that the tax incidence effect on wages is economically small: A 1%-point increase in CIT rates is associated with a decline in wages of only 0.110% to 0.235%. Moreover, we find that the data coverage, the temporal focus and the average sample year drive the heterogeneity among estimates. Additional analyses indicate that a robust true negative association only exists for within-country studies focusing on the rent-sharing mechanism to identify the corporate tax incidence.

Keywords: Tax incidence, Wages, Corporate income tax, Elasticity, Semi-elasticity, Meta-regression analysis

* Jonas Knaisch, Otto-von-Guericke-University Magdeburg, Germany, e-mail address: jonas.knaisch@ovgu.de; Carla Pöschel, Freie University Berlin, Germany, e-mail address: carla.poeschel@fu-berlin.de. We thank Bruce Calvert, Nadja Dwenger, Sebastian Eichfelder, Shafik Hebous, Jochen Hundsdoerfer, Alexander Klemm, Ruud de Mooij, Viktor Steiner, and participants at the IMF Tax Policy Brown Bag Seminar 2021 for their helpful comments and suggestions. All errors remain our own.

1. Introduction

While the corporate income tax (CIT) is legally levied on firms, views on who ultimately bears the actual tax burden differ widely. One may assume that the entire CIT burden falls on capital owners, since those are the legal targets of the CIT. However, the tax burden can also be passed on to consumers through higher prices for products and services, or employees in form of lower wages. In its Final Report on Action 11, the Organization for Economic Co-operation and Development (OECD) concludes that the “*economic incidence, particularly of the CIT in a global economy, is still an unresolved issue for economists*” (OECD 2015, p. 116). Accordingly, advances on the issue are of considerable interest, since the tax incidence is a key parameter for policy makers due to its implications for the progressivity and the distributive fairness of a tax system (Auerbach 2006).

The academic tax incidence discussion dates back at least to Harberger (1962). Given a set of restrictive assumptions (in particular, a closed economy with a fixed stock of labor and capital), the CIT burden entirely falls on capital in the long-run. The simple Harberger (1962) model abstracts from several important determinants, such as imperfect competition or international capital flows. Therefore, extensions assume an open economy where capital is perfectly mobile across countries, while labor is not (e.g., Harberger 1995; Harberger 2008a, 2008b; Gravelle & Smetters 2006; Randolph 2006). As a result, the taxation of capital induces capital flows abroad and a decline in the marginal labor productivity, leading to lower wages and labor bearing a substantial share of the CIT.

Building on these theoretical models, a growing empirical literature assesses the CIT incidence on wage rates using advanced estimation techniques. A first stream of studies provides either country or state level estimates of wage effects by using variations in CIT rates across countries or states over time. Those contributions identify the ‘indirect’ incidence effect via the capital reallocation among countries, i.e. the open-economy general equilibrium

mechanism. The first empirical study in this vein of Hassett and Mathur (2006) uses aggregate wage data for 72 countries between 1981 and 2002. The authors apply regressions of five-year average manufacturing hourly wages on three measures of the CIT burden (i.e., statutory tax rate, effective marginal and average tax rate). They estimate an extremely large wage elasticity, as their findings imply that a 1% increase in the CIT rate is associated with a decrease in hourly wages of roughly 1%.¹

In contrast, an emerging second strand of literature focuses on the ‘direct’ incidence of corporate taxes on wages. Arulampalam, Devereux and Maffini (2012) argue that the company and its employees collectively bargain over the firms’ economic after-tax profits that are paid out in wages. The empirical strategy to measure the impact of corporate taxes on wages through this rent-sharing mechanism is by controlling for labor productivity (e.g., value-added of workers). Using firm level data on 55,082 European firms from 9 countries for the years 1996–2003, they estimate that a 1% increase in the CIT payment per worker results in a decrease of wages by 0.09% in the long-run, which is rather small in comparison to general equilibrium estimates.

However, the majority of empirical studies corroborates evidence for labor bearing a substantial share of the CIT, since estimates suggest a tax incidence of 30–100% on employees (e.g., Hassett & Mathur 2006; Felix 2007; Arulampalam, Devereux & Maffini 2012; Dwenger, Rattenhuber & Steiner 2017; Fuest, Peichl & Siegloch 2018). Thus, the precise magnitude of the tax incidence on wages still remains controversial (many studies point to this, see e.g., Gentry and College 2007, p. 13; Harris 2009, p. 2; Dwenger, Rattenhuber & Steiner 2017, p. 1). Why do these huge divergences emerge in the reported

¹ The studies by Felix (2007), Gravelle and Hungerford (2007) and Clausing (2012) replicate the model specifications of Hassett and Mathur (2006) and estimate wage elasticities with magnitudes which are half as large. Gravelle and Hungerford (2007) note that their results are sensitive to alternative exchange rate conversions and the lag structure. More recent versions of the paper report smaller elasticity estimates (see, Hassett & Mathur 2010; Hassett & Mathur 2015).

results? It is widely acknowledged among the reviews on the empirical tax incidence literature that the inconsistent magnitude of the wage elasticity is driven by differences in design aspects (e.g., data and methodological issues) of the studies (see, Gentry & Collage 2007; Gravelle 2011 & Clausing 2012, 2013).

Given the diversity in findings and the vast amount of methodological choices, this paper contributes to the literature by using meta-regression analysis (MRA) to quantitatively combine the corporate tax incidence literature. First, we test for the presence of publication bias in the underlying literature. Second, we compute how much of the CIT burden is borne by employees on average. Third, we investigate to what extent the reported estimates are driven by differences in methodological aspects (e.g., datasets, specifications and estimation methods) across studies in order to shed light on the sources of heterogeneity. We systematically review the latest empirical studies on the effect of the CIT on wages by focusing on the methodological aspects of the studies and go beyond a qualitative literature review by deriving an average wage semi-elasticity with respect to corporate taxes. To the best of our knowledge, we provide the first MRA to explain the inconclusiveness of empirical studies on the CIT incidence on wages.

We find robust evidence that the primary literature suffers from substantial publication selection in favor of negative elasticity estimates. The presence of publication selectivity is more pronounced among studies of authors that are affiliated with political advisory councils or private institutes, think tanks and corporations than for those that are exclusively linked to universities or publicly financed research institutes. After correcting for the presence of publication bias, we estimate a negative but small average wage elasticity of -0.024. While Hassett and Mathur (2006) report a wage elasticity of roughly -1.0, our results for the semi-elasticity suggests that the tax incidence effect on wages is economically small: A 1%-point increase in CIT rates is associated with a decline in wages of only 0.110% to 0.235%.

Moreover, in further MRA, we explain the variation in estimates by adding several moderator variables regarding the underlying tax burden measures, datasets, estimation methods and specifications. This allows us to detect significant sources of heterogeneity (or bias) in the primary literature. Our findings reveal that the use of cross-country data instead of within-country data yields smaller (i.e., less negative) wage elasticities on average. Additional subsample analyses show that a robust average true negative effect beyond publication bias only exists for studies that rely on within-country data. Hence, prior studies do not seem to be able to empirically identify the open-economy general equilibrium tax incidence mechanism cleanly. Our results suggest that their estimates are somewhat biased downward due to an over-control bias (e.g., controlling for labor productivity). Additionally, long-term effects are larger than short-term or static effects, as the tax incidence effect occurs over time rather than immediately. Lastly, the incidence on wages trends upwards over time. This might be either consistent with better data availability or an increasing capital mobility during the last decades. Our results can guide future empirical studies, as the MRA uncovers potential sources of heterogeneity in the underlying literature and underlines the importance of carefully and transparently establishing specification choices.

Finally, our findings also contribute to recent debates about raising the CIT rate and its implications for wages. Considering an allegedly ‘conservative’ wage elasticity range of -0.16 to -0.33, the Council of Economic Advisers (CEA) forecasted in 2017 that reducing the CIT rate to 20% would raise the average household income by more than \$4,000 annually in the US (CEA 2017), thereby providing a strong argument in favor of the Tax Cuts and Jobs Act under the Trump administration.² More recently, president Joe Biden’s tax plan includes raising the CIT rate to 28%, which brings the tax incidence issue in focus again (Biden Harris Campaign 2020). The Tax Foundation assumes in its evaluation of Biden’s tax plan a tax

² Summers (2017) and Furman (2017) have criticized the CEA’s estimates as implausible because they imply an increase in wages of 3 to 5 times the tax revenue loss.

incidence of roughly 50% falling on employees (Tax Foundation 2021). Our estimates indicate that the degree to which the burden of the CIT is shifted to employees is relatively low in comparison to prior findings of, for example, Hassett and Mathur (2006) and Fuest, Peichl and Siegloch (2018). Accordingly, the appealing argument of a large tax incidence effect on wages for advocates of low CIT rates should be taken with care.

The remainder of this paper is structured as follows. Section 2 describes the literature selection approach and presents our meta-sample. Section 3 explains the meta-regression model and discusses our moderator variables along with their descriptive statistics. Section 4 displays the MRA results. Finally, Section 5 concludes by summarizing and discussing the implications of the results.

2. Meta-Sample

2.1 Wage Elasticity to Corporate Taxes

MRA results are only meaningful if the estimates are comparable across primary studies (Stanley 2001). The wage elasticity to corporate taxes is commonly estimated according to the following baseline equation (1):

$$\log(wage) = \alpha + \beta \log(CIT) + \gamma X + \varepsilon \quad (1),$$

where *wage* is the wage rate, *CIT* denotes the CIT rate (see Section III for further descriptions), and the vector *X* typically contains various country-, individual- and/or firm-level controls.³ ε is the error term. The main coefficient β (expected to be negative) captures the wage elasticity to taxes and is the outcome variable of our MRA. The wage elasticity determines the percentage change in wages when the CIT rate increases by 1% (i.e.,

³ The underlying studies use nominal or real wage rates, while these are measured either hourly, weekly, monthly or annually. Some studies also use average wage rates over the past five or three years (e.g., Gravelle and Hungerford 2007; Clausing 2012; Hassett & Mathur 2015; McKenzie & Ferde 2017). Due to correlation between various moderators, we cannot control for these differences in our MRA. Fuest, Peichl and Siegloch (2018) use the net-of-tax rate, we therefore multiply their elasticity estimates by -1.

$\partial \log(wage) / \partial \log(CIT)$). Some studies use other versions of the baseline equation (1) than a log-log specification (e.g., linear or semi-log). Since most studies focus on wage elasticities, we ensure comparability among studies by consistently transforming the initial coefficient estimates from publications into elasticities using the sample mean value of the CIT rate or wage rate variables (i.e., $semi_elasticity \times mean(CIT)$ or $coefficient \times mean(CIT) / mean(wage)$).

However, the magnitude of an elasticity estimate depends on the level of the CIT rate in the initial sample, such that the elasticity itself is difficult to interpret. We therefore transform the initial elasticity estimates into semi-elasticities by dividing the elasticity by the sample mean value of the CIT rate variable.⁴ The semi-elasticity measures the percentage change in wages when the CIT rate increases by 1%-point (i.e., $\partial \log(wage) / \partial CIT$). The use of semi-elasticities will serve as a robustness check for our baseline elasticity results, while simultaneously enabling us to estimate an average true effect beyond publication bias that does not depend on the variable means of single studies.

2.2 Selection of Literature

We primarily used the IDEAS database and Google Scholar to locate appropriate studies. We employed the following keywords (and combinations of them): *tax incidence*, *wages*, *elasticity* and *corporate income tax*. Additionally, we scanned literature reviews (Gentry & College 2007; Gravelle 2011; Clausing 2012, 2013) and references of the identified studies. We completed our search process in December 2020.

We screened the abstracts of the studies and eliminated those that are not empirically investigating the association between the corporate income tax and wage rates. Our final meta-sample is determined by two selection criteria: First, the study must report results from

⁴ This leaves us with a sample of 110 observations, because Clausing (2012), Gravelle and Hungerford (2007) and McKenzie and Ferede (2017) do not provide descriptive statistics (for all) of their variables.

estimating a variant of the baseline equation (1) (inclusion criterion 1).⁵ Second, the study must provide standard errors or *t*-statistics and in some cases the sample mean values of the CIT and wage rate variables to be able to derive elasticity values (inclusion criterion 2).⁶ We only consider the latest version of a study to avoid autocorrelation among elasticity estimates. Appendix A presents in Figure A.1 a PRISMA flow chart which illustrates the selection steps of the literature and our inclusion criteria (Moher et al. 2009).

Since selecting a single estimate per primary study is quite subjective and results in a small sample size and less heterogeneity among estimates, we include multiple estimates from each primary study, as long as there is a substantial difference regarding the variables, specifications, estimation strategies or samples. We remove estimates for specific subsamples of observations (e.g., firm size, country size or union membership) from our meta-sample.

Table A.1 in Appendix A outlines the full meta-sample of studies with an overview of the underlying tax burden measures, data coverage and elasticities (i.e. number, mean, minimum, maximum and standard deviation). Our full meta-sample contains 17 primary studies comprising 140 estimates published between 2007 and 2018. The mean value of the wage elasticity of our full meta-sample equals -0.188, with a standard deviation of 0.210. The most extreme values (minimum and maximum) are -0.836 and 0.160 for Gravelle and Hungerford (2007) and Clausing (2012), respectively. The number of estimates from each primary study ranges from 2 (Goodspeed 2012; Liu & Altshuler 2013) to 15 (Hassett & Mathur 2015). The means of the estimates per primary study are negative (as expected) and range from -0.001 for Goodspeed (2012) to -0.573 for Hassett and Mathur (2015), indicating a substantial dispersion

⁵ Felix and Hines (2009), for example, examine the incidence of the CIT on union wage rates by using interaction terms. Desai, Foley and Hines Jr. (2007) constrain the total shares of labor and capital to one to investigate the relative burden of the CIT. Suárez Serrato and Zidar (2016) define in their reduced-form specification the dependent variable as wage growth. As their regression equations are not comparable to the baseline eq. (1), we exclude those studies from our meta-sample. See Table A.2 in Appendix A for more explanations.

⁶ Felix (2009) reports neither standard errors nor *t*-statistics and Gyourko and Tracy (1989) and Ljungqvist and Smolyansky (2016) do not provide sample mean values of their CIT rate variables.

of primary estimates. We exclude the extreme estimates of Goodspeed (2012). Moreover, we drop four outliers referring to the publication of Ebrahimi and Vaillancourt (2016), as those estimates are far to the left of the remainder of the funnel plot in Figure 2 due to their extreme low standard errors and, thus, can be seen as outliers (Stanley & Doucouliagos 2012). Since the meta-sample is rather small (140 estimates), our results are quite sensitive to those estimates. After this adjustment, our final sample comprises 134 observations.

(insert Table 1 about here)

(insert Figure 1 about here)

Table 1 and Figure 1 provide an overview of the distribution of the elasticity and semi-elasticity estimates in our final meta-sample; the mean value of the wage elasticity equals -0.190, with a standard deviation of 0.213, while the median corresponds to -0.125. The histogram in subfigure (a) illustrates the left-skewed distribution of the elasticity estimates due to some large negative estimates. As the mean level of the CIT rate variables differ substantially across our studies (e.g., from 0.045 to 8.782 due to different CIT variables, countries, and sample periods), the magnitude of the values of the semi-elasticity scatter much wider compared to those of the elasticity. The mean of the tax semi-elasticity to wages in subfigure (b) is -0.638, i.e. the wage rate decreases by 0.638% if the CIT rate variable increases by 1%-point on average. Thus, the mean of the semi-elasticity is more than 3.4 times larger in absolute terms than the mean elasticity value in subfigure (a).

3. Meta-Regression Methodology

The outlined large dispersion among primary studies cannot be explained descriptively since the literature differs regarding various methodological choices. Meta-regression analysis therefore quantitatively combines estimates of the primary literature to investigate the

heterogeneity among estimates formally (Stanley 2001). We code several moderator variables which capture the issue of publication bias, diverse study characteristics and methodological choices (datasets, estimation methods and specifications). Table 2 summarizes the full set of moderators, along with a description and their summary statistics. The mean of the moderators can be interpreted in percentage terms. At first glance, the summary statistics point to heterogeneous estimates, since the means vary sharply between the moderators.

(insert Table 2 about here)

Publication bias: A publication bias arises if statistically insignificant or supposedly counterintuitive estimates are not published in a journal or do not even appear in a working paper. To visually test for the presence of publication bias, a funnel plot is commonly used to map the estimates (in our case, the wage elasticity to taxes) against their precision (the inverse of the standard error) (Sutton et al. 2000). While imprecise estimates scatter widely at the bottom of the diagram, more precise estimates are distributed at the top. In the absence of publication bias, the estimates should be symmetrically spread around the average true effect.

(insert Figure 2 about here)

(insert Figure 3 about here)

The funnel plot of the wage elasticities in Figure 2 shows an elongated left tail with a missing right side, since the vast majority of the estimates vary between -0.8 and 0, while positive wage elasticities are almost absent. The peak of the funnel plot is composed of the most precise estimates, scattered around -0.05 and 0. The asymmetric shape of the funnel plot is more consistent with the presence of a publication selectivity regarding the sign of the estimates than for statistical significance, because the funnel is thick rather than hollow (Havránek et al. 2018). Accordingly, elasticities with positive values are selected for publication less often than they should in the underlying literature. The scatter plot of the

elasticity estimates by significance in subfigure (a) of Figure 3 illustrates this as well: the figure shows that 81 estimates are statistically significant (at least at the 5% level) and 53 estimates are not statistically significant, while 128 estimates exert a negative sign and only 6 elasticity estimates are positive. Since positive wage elasticity estimates are contradictory to the theoretical arguments, authors may consider these positive results as a signal for model misspecifications and adjust their models accordingly.

We also address the issue of publication selection more formally by including the standard error (*Standard error*) of the corresponding estimate as an explanatory variable:

$$\hat{\epsilon}_{is} = \alpha + \beta \cdot \widehat{SE}_{is} + \gamma X_{is} + \epsilon_{is} \quad (2),$$

where the dependent variable is the estimate $\hat{\epsilon}_{is}$ (*Elasticity estimate*) and \widehat{SE}_{is} is the standard error of regression $i = 1, \dots, I$ referring to primary study $s = 1, \dots, S$, X_{is} is a vector of the moderator variables (see the descriptions in the next subsections), α specifies the constant and ϵ_{is} is the error term.

The funnel asymmetry test of the coefficient on *Standard error*, β , indicates the presence of publication bias (Egger et al. 1997). The underlying intuition is simple: A correlation between the variables *Elasticity estimate* and *Standard error* appears because of publication bias, i.e. authors may have to search longer for statistical significance by testing various methods or model specifications under the given conditions (e.g., small sample), resulting in larger values of estimates or lower values of standard errors. The precision effect test on the constant (α) assesses whether an average ‘true’ effect exists beyond publication bias (Stanley 2008).

The residuals are clearly heteroscedastic since the wage elasticity estimates exhibit different precisions (as shown by their different standard errors). To correct for

heteroscedasticity, equation (2) is weighted by the inverse variances of the estimates ($1/\widehat{SE}_{is}^2$) as analytical weights (i.e., unrestricted weighted least squares (WLS) meta-regression) (Stanley & Doucouliagos 2015). Beyond correcting for heteroscedasticity, weighting by the inverse variances corrects for low-quality estimates, since imprecise coefficients are given less weight in the MRA. Lastly, we consider multiple estimates from each primary study in our meta-sample, which bears the risk of within-study dependency (i.e., autocorrelation). We allow for autocorrelation between estimates per primary study due to unobserved study-level heterogeneity and cluster standard errors at the study level (Stanley & Doucouliagos 2012).

Measuring the corporate income tax rate: Measuring the corporate tax burden is not straightforward and the underlying studies use various measures to capture the impact of corporate taxes on wages. More than half of the selected estimates use the top statutory corporate tax rate (STR) as main explanatory variable (62%). The STR is a relatively rough measure of the CIT that a firm actually pays, since it neglects tax provisions that affect the tax base (e.g., such as tax deductions, income exemptions and tax incentives). Thus, using STRs to proxy for corporate taxes can be associated with a measurement error. In order to overcome these issues, ex-ante (“forward-looking”) tax rates can be applied, which relate the tax rate to the corresponding tax base. Generally, ex-ante rates assess the tax burden on a prospective investment or the incentive to invest by assuming a mix of assets (e.g. intangibles, buildings, machinery, financial assets and inventories), financing sources (e.g. debt, equity or retained earnings) and fixed rates of interest, inflation, and depreciation. While effective marginal tax rates (EMTRs) reflect tax incentives for marginal investments (intensive-margin response), effective average tax rates (EATRs) model tax incentives for profitable investments and thus,

apply to discrete location decisions of firms (extensive-margin response).⁷ These tax rate variables, however, often rely on a set of simplifying assumptions and overlook corporate tax planning strategies or tax reliefs provided by tax authorities across countries. Since many firms use tax planning activities to lower their tax liabilities (see, e.g. Dyreng, Hanlon & Maydew 2008; Hanlon & Heitzman 2010), ex-ante tax rates do not solve all the issues of the STR, despite being a more reasonable approximation for investment tax incentives and tax base effects. Lastly, a number of underlying studies use ex-post (“backward-looking”) average tax rates (ATRs), which represent the corporate taxes that are actually paid as a share of taxable income. These types of tax measures capture both, tax provisions and tax planning activities (e.g. nominal rates, depreciation allowances, treatment of losses, tax credits, profit shifting and tax reliefs). However, omitted variables and shocks may impact the profitability (and loss carryforwards) of firms which in turn may affect ATRs as well as wage rates (Devereux & Griffith 1998).

Hence, the choice of a tax rate measure should always depend on the particular study setting and research question and is not universally appropriate. Due to the described implications of the different measures, the wage elasticity might be sensitive to the applied CIT rate variable. We therefore group various rates instead of adding moderator variables for each type of tax measure and distinguish only between STRs (62%), ex-ante (10%) and ex-post effective tax rates (28%) to ensure enough variation in each moderator variable. Subfigure (b) in Figure 3 illustrates the association between the elasticity estimates and their standard errors separately for each tax rate variable. The estimates that rely on the STR scatter much wider (-0.836 to 0.063) than those that use ex-ante rates (-0.643 to -0.037) or ex-post rates (-0.247 to 0.16); the latter estimates exhibit the lowest variability. Given this initial

⁷ Among the studies using effective corporate tax rates, 10 estimates refer to ex-ante rates (EMTRs or EATRs) along the lines of Devereux and Griffith (1999). The two estimates of Liu and Altshuler (2013) and one estimate of McKenzie and Ferede (2017) refer to a different measurement.

evidence for substantial variation of the estimates across tax rate measures, we formally test whether the CIT rate variable explains the heterogeneity across estimates in Section 4.

Theoretical mechanism: According to the general equilibrium theory of corporate tax incidence, an increase in the CIT lowers the after-tax return on capital, which leads to an outflow of capital to low-tax locations and lower capital investment. This depresses labor productivity and wages in the high-tax country, since labor is far less mobile than capital. In contrast, Arulampalam, Devereux and Maffini (2012) argue that a higher CIT lowers the firms' economic after-tax profits over which the firm and its employees bargain (e.g., rent-sharing mechanism), while the final corporate tax incidence is driven by the bargaining power and outside options of both parties. According to Arulampalam, Devereux and Maffini (2012), an empirical strategy to measure the impact of corporate taxes on wages through wage bargaining is by controlling for labor productivity in order to account for the general equilibrium effect. Some of the studies do not directly address the channel they seek to consider: for example, Hassett and Mathur (2015) and Carroll (2009) claim that they focus on the whole general equilibrium effect, even though they control for labor productivity. Thus, they shut down the impact of variations in the capital intensity on wages and effectively measure the 'direct' incidence effect. To disentangle the 'direct' incidence effect, we introduce the moderator variable *Unconditioned* that is equal to 1 if a study does not control for labor productivity (e.g., value added per worker or GDP per worker), and 0 otherwise.

In addition to the choice of control variables, the exploration of either the 'direct' or 'indirect' mechanism is also associated to the underlying dataset used. A first stream of studies uses aggregate wage data and exploits CIT rate variations across countries as identification strategy to identify the open-economy general equilibrium mechanism (see the fifth column in Table A.1 in Appendix A, e.g., Felix 2007; Gravelle & Hungerford 2007 and Hassett & Mathur 2015). These publications rely on less complete data and thus, their

estimates may be driven by (unobservable) differences in economic or political conditions across countries (e.g., omitted variable bias). Carroll (2009) therefore employs aggregate data for 50 states within the US for the period 1970–2007 and finds that a 1% increase in the CIT rate decreases real wages by 0.014%, which is a rather small estimate in comparison to earlier findings.

In contrast, studies that seek to identify the rent-sharing mechanism focus on regional, industry or individual CIT variations within a single country. Fuest, Peichl and Siegloch (2018) exploit variations in local business tax rates across municipalities in Germany using establishment level data. Their results suggest that a 1% decrease in the net-of-business tax rate (reflecting an increase in the tax rate) reduces wages by 0.39%. Since primary studies differ regarding the exploration of either cross-country (e.g., worldwide or across OECD countries) or within-country variation of the CIT rate, we add the moderator variable *Cross-country data* to our MRA.⁸

Estimation methods: The majority of primary studies exploit the panel structure of their datasets and estimate variations in wages over time, while controlling for unobserved but fixed heterogeneity between countries, states or individuals to address endogeneity issues (coded as *FE estimator*, 55%). We use *FE Estimator* as our base category and introduce two moderator variables indicating deviating methods: *OLS* and *GMM*.⁹ We expect a bias for simple OLS estimates because of the influence of confounders in comparison to other estimates which rely, e.g., on IV, FE or GMM specifications.

Temporal dynamics: Auerbach (2006) points to the importance of dynamics over time, i.e. the point in time at which the CIT burden is distributed among individuals: The incidence

⁸ Due to fewer observations for European countries, we consider the estimates of Arulampalam, Devereux and Maffini (2012) and Becker, Fuest and Riedel (2012) in the subsample of studies covering OECD countries.

⁹ We omit the moderator variable *IV* due to collinearity with *Ex-ante rate* (see Table B.1 in Appendix B) and the moderator variable *RE estimator* due to low variability.

effect via the adjustment of factor prices and capital allocation, that follows after a change of the CIT, is likely to occur over time, rather than immediately. Consequently, the long-term effect is likely to be larger than the short-term effect. Most studies, however, provide single-point estimates by using static rather than dynamic models and are not able to cleanly separate long- and short-term effects. Additionally, the exact distinction between short- and long-term estimates is ambiguous in some studies (see, e.g., Carroll 2009 and Hassett & Mathur 2015).¹⁰ We therefore only mark an estimate as *Long-term effect* or *Short-term effect* when authors explicitly provide long-term or short-term elasticity estimates. All other estimates are designated as *Static effect*, although some studies use lags of the CIT rate.

Time trend: In a global economy, the corporate tax incidence is strongly driven by cross-border effects and the mobility of capital among countries. Given an increasing capital mobility during the last decades (see, e.g. Altshuler, Grubert & Newlon 2001; De Mooij & Ederveen 2008; Feld & Heckemeyer 2011), the corporate tax incidence on wages is likely to trend upward over time. As Clausing (2012, p. 467, 2013, p. 171) notes, however, the growing importance of corporate tax avoidance may mitigate the increasing capital mobility effect, since the CIT deploys a smaller effect when firms can shift profits to low-tax countries without corresponding real activity adjustments (e.g., investment and employment). According to the findings of Arulampalam, Devereux & Maffini (2012), multinational enterprises (MNEs) and domestic firms shift a similar share of the tax burden to employees. Dyreng et al. (2020) show empirically, that the tax incidence on employees and tax avoidance of firms are negatively correlated. The findings for the capital mobility vs. tax avoidance imply that the time trend effect on the CIT incidence is not clearly predictable and ultimately an empirical issue. We therefore code the moderator variable *Average sample year*, which is

¹⁰ Hassett and Mathur (2015) justify their use of the five-year average wage by noting that the economic effects of CIT rate changes show up over longer time periods due to capital adjustment costs. Carroll (2009) uses a five year lag structure but does not report these estimates; even so, we label their estimates as *Static effect*.

normalized between zero and one by setting the oldest average sample year (1988.5) to zero and the latest (2005.5) to one.

4. Meta-Regression Results

4.1 Testing Publication Bias

Table 3 reports our results for testing the presence of publication bias by including the variable *Standard error*. In subpanel (a), we check whether our result in column (1), where unrestricted WLS is used to correct for heteroscedasticity, is robust to alternative weights and models. We use a simple Ordinary Least Squares (OLS) regression in column (2) and the inverse of the number of estimates per study as an alternative weighting factor in column (3), because the number of estimates varies considerably among publications. Weighting by the number of estimates treats studies similar and thus, corrects for the over- and under-representation of studies in the sample. Since some estimation methods may inherently produce larger elasticity estimates and corresponding standard errors, we use the inverse of the square root of the number of observations as an instrument variable (IV) for the *Standard error* in column (4) (Havranek et al. 2015).¹¹ The sample in column (4) comprises only 124 estimates because Gravelle and Hungerford (2007) do not report the number of observations in their regression tables. The parameter estimates on *Standard error* are negative ($\beta < 0$) and statistically significant across all columns. Hence, we find robust evidence for the presence of publication bias in the corporate tax incidence literature, suggesting a substantial selectivity in the reported wage elasticity estimates.

(insert Table 3 about here)

¹¹ The exclusion restriction is satisfied by the argument that the number of observations of the corresponding estimate relates to its standard error but not to the underlying estimation method (Havranek et al. 2015).

In subpanel (b), we examine which publications drive the bias in our sample by dividing the estimates in several subsamples. If publication selectivity is present, we should find a higher extent of publication bias for studies that are published in peer-reviewed journals than for other publications (e.g. working papers, policy reports etc.), because researchers may be incentivized to report statistically significant and/or expected estimates to increase the chance of publication. The parameter estimate on *Standard error* in column (1) is indeed larger in absolute terms than in column (2), confirming this prediction.

Furthermore, a large tax incidence effect is an appealing argument for cutting the CIT rate. Thus, another aspect of publication bias is that political or economic interests may determine which estimation approaches are pursued and how results are presented in a study. We test whether the affiliation of the authors and therefore the degree of their independence moderates the extent of the bias. Studies that are more likely to follow an agenda are in turn more prone to report larger magnitudes of estimates (regardless of its particular direction). In order to ensure objectivity as far as possible, we designate authors to be independent if they are solely part of an university or an (publicly financed) independent research institute (e.g., Ifo Institut für Wirtschaftsforschung, RWI – Leibniz-Institut für Wirtschaftsforschung, National Bureau of Economic Research), while labeling the affiliation with political advisory councils or privately financed institutes, think tanks and corporations (e.g., Council of Economic Advisors, Fraser Institute, IZA – Institute of Labor Economics, Center for Economic Policy Research, Ernst & Young) as dependent.¹² The parameter estimate for the variable *Standard Error* in column (3) is about twice as large as in column (4), suggesting that the results of ‘dependent’ authors seem to be adapted to certain expectations. However, testing for publication bias in a univariate regression may produce biased estimates if important moderators are omitted. Therefore, we further test for the issue after accounting for other sources of heterogeneity in the next subsection.

¹² We collected the information on the authors’ affiliations from the primary studies and affiliation websites.

Despite publication bias, we find evidence for a true negative wage elasticity to taxes, since the constants in Table 3 are negative and statistically significant across most of our specifications in subpanels (a) and (b). According to our preferred model in column (1) of subpanel (a), the true average wage elasticity equals -0.024 after correcting for publication bias, which is much smaller than the unweighted mean value of -0.190.

(insert Table 4 about here)

The average true effect can be interpreted more usefully when semi-elasticities are used since those do not depend on the underlying sample means of a particular study. In Table 4, the dependent variable is the *Semi-elasticity estimate*, and its corresponding *Standard error* is the explanatory variable. A complication of using semi-elasticities, however, is that the estimates are not directly comparable when the units of measurement of the independent variable vary. Therefore, we exclude the estimates of Arulampalam, Devereux and Maffini (2012) and aus dem Moore (2014) in column (2) because both studies use the corporate tax liability per employee (i.e., absolute unit) as the main explanatory variable, rather than a traditional CIT rate measure (i.e., percentage unit). We use the variance, *Squared standard error*, instead of the standard error as explanatory variable in column (3), since, according to simulations, a linear publication bias correction is likely to underestimate the average true effect (Moreno et al., 2009; Stanley & Doucouliagos, 2012). The average semi-elasticity in columns (2) and (3) is significantly negative, but it turns out that the magnitude in column (2) is indeed smaller than in column (3). Our average estimates of the semi-elasticity suggest that the tax incidence effect on wages is economically small: A 1%-point increase in CIT rates is associated with a decline in wages of only 0.110% to 0.235%, thereby confirming the notion that the average true effect seems to be only a fraction of the unweighted estimate means of prior studies.

4.2 Sources of Heterogeneity

Our MRA in Table 5 investigates the degree to which estimates vary with various methodological aspects and over time. Instead of adding the full set of moderator variables simultaneously, we include them step by step to address multicollinearity concerns. The baseline model in column (1) considers only two moderators which refer to the underlying CIT rate variable (*Ex-ante rate* and *Ex-post rate*) and the variable *Standard error* for the publication bias correction. We use the variable *STR* as the benchmark category, because most of the included estimates rely on the *STR* as the main explanatory variable. The coefficients on the variable *Ex-post rate* are positive and change their sign on the variable *Ex-ante rate* across the columns. As the coefficients are not statistically significant for both moderators, we find no evidence that the CIT variable drives the heterogeneity among estimates systematically. However, the graphical evidence in Figure 3 suggests notable differences between estimates that rely on different CIT rate variables. We therefore address this issue in a subsample analysis in the next subsection.

(insert Table 5 about here)

A further aim of our MRA is to differentiate between the ‘direct’ and ‘indirect’ tax incidence effect on wages. Hence, we add the variables *Unconditioned* and *Cross-country data* in column (2). The regression coefficients on *Unconditioned* are negative but not statistically significant throughout our models. Accordingly, we cannot infer a systematical difference between studies that control for labor productivity (e.g., value added per worker or GDP per worker) and those not controlling for labor productivity. As the estimates on the variable *Cross-country data* are significantly positive, the elasticity estimates seem to be smaller (i.e., less negative) when a primary study uses cross-country instead of within-country data on average. Following the traditional open-economy point of view, however, cross-country studies should produce more negative tax elasticity estimates, since labor is arguably

less mobile across countries than within countries and thus, cannot escape the CIT burden so easily. Therefore, we interact the two moderators *Unconditioned* and *Cross-country data* in column (5). The negative and statistically significant parameter estimate suggests that cross-country studies suffer from over-control bias (e.g., as they control for labor productivity) and thus, report downward biased estimates.

Column (3) includes the moderator variables regarding the estimation method (*OLS* and *GMM*) and *Long-term effect*. Surprisingly, our results suggest that the estimation method is not a source of heterogeneity in estimates, since the coefficients on *OLS* and *GMM* are both negative but not statistically significant. In contrast, the estimates for *Long-term effect* confirm our prediction in Section 3. On average, the long-term effect is larger (by -0.057% to -0.132%) than the short-term or static effect.

Finally, our most comprehensive model in column (4) adds the variable *Average sample year*. Its coefficient is negative and statistically significant at the 5% level. Accordingly, the magnitude of the reported wage elasticities tends to increase over time. There are at least two possible drivers: The larger wage responsiveness in later years can indicate a rising capital mobility from the 1990s onwards, but we consider it as more likely that the increase over time is a result of a better data availability which produces more accurate estimates of the wage elasticity to taxes (see e.g., the significantly positive moderator *Cross-country data*).

The most notable finding is that the coefficients for the variable *Standard error* remain negative and statistically significant at the 1% level across all models. Adding the full set of moderator variables in column (4) depresses the magnitude of the coefficient on *Standard error*, but the evidence on publication bias survives. It should be noted that the values of the constant in Table 5 are conditioned on the included moderator variables when they take zero values (i.e., depend on the reference categories) and thus, cannot be interpreted as an average tax incidence effect as in Table 3 and Table 4.

4.3 Robustness Results: Subsample Analysis

In the last part of our analysis, we perform a set of subsample MRAs in Table 6 to evaluate the sensitivity and implications of our results in Table 5. In subpanel (a), we split the full meta-sample according to the various CIT rate variables (*STR*, *Ex-ante rate* and *Ex-post rate*). The operational choice how to capture the tax burden has direct impacts on the capability of the econometric specification to incorporate tax incentives and effects on capital and wages accurately. Our finding in column (1) is consistent with the discussion in the literature: The *STR* is seen as an inaccurate indicator for the tax burden because of the omission of tax base effects (e.g. tax deductions, income exemptions, tax credits or formula apportionment). By contrast, the average effect for ex-ante or ex-post rates ranges from -0.050 to -0.016. It is ambiguous in the literature, which effective corporate tax rate is the most appropriate. Swenson (1994) proposes to apply ATRs, since ex-ante tax rates do not capture tax law complexities and are sensitive to the underlying assumptions. Subpanel (a) indeed shows that the coefficient on *Standard Error* becomes statistically insignificant in column (3), indicating that studies that use ATRs might be less plagued by publication bias. However, ATRs give rise to possible endogeneity concerns (see, e.g. Devereux & Griffith 1998; Carroll 2009). We cannot infer from our analysis whether a certain tax rate measure is ‘better’ or ‘worse’ conclusively. However, considering our results and the described advantages and disadvantages of each CIT measure, it would be advisable for future empirical studies to conduct their analyses by using all possibilities (*STRs*, ex-ante and ex-post effective tax rates) to validate their results.

(insert Table 6 about here)

Finally, we estimate additional subsample MRAs for cross- and within-country studies in subpanel (b) of Table 6, because studies in our meta-sample are quite heterogeneous with respect to their geographical coverage (e.g., worldwide, OECD countries or single countries,

like the US, Canada or Germany). We include the variable *Unconditioned* and it turns out that the parameter estimates are negative and statistically significant in most columns. We can now calculate the true effect while accounting for this heterogeneity factor: we multiply the moderator *Unconditioned* by its subsample maxima.

As labor is likely to be less mobile across countries than within countries, studies using firm or industry level data within a country to identify the rent-sharing mechanism should find less negative (direct) elasticities in comparison to cross-country studies. A part of the underlying primary literature is ambiguous regarding the theoretical justifications for their econometric specification: for example, Carroll (2009) or Hassett and Mathur (2015) control for labor productivity, and therefore, their model should not be designated as appropriate to identify the open-economy general equilibrium mechanism. The averaged true effect for cross-country studies unconditioned on labor productivity in column (1) (-0.182) confirms this, as the effect is indeed larger in absolute terms than the average true effect conditional on labor productivity in column (5) (-0.017). As shown in column (3), the average true effect in column (1) is driven by the 6 studies that cover OECD countries. The subsample in column (3) contains the study of Arulampalam, Devereux and Maffini (2012) that focuses on the rent-sharing mechanism, even though cross-country data is used. Therefore, we remove the elasticity estimates of Arulampalam, Devereux and Maffini (2012) in column (4), which diminishes the magnitude of the constant for the conditioned estimates towards zero. The finding supplements that it is notorious hard to identify the open-economy general equilibrium mechanism empirically in cross-country settings, which can be due to the unobservable omitted variables.

With respect to the subsample of within-country studies, we exclude the estimates from the study of Carroll (2012) and McKenzie and Ferede (2017) in column (6), because the authors use aggregate rather than disaggregate within-country data and thus, focus on the general

equilibrium mechanism. The magnitude of the true wage elasticity conditional on labor productivity slightly increases and is still statistically significant, indicating the presence of a true wage elasticity beyond publication bias for studies using disaggregate within-country data.

In columns (8) to (10) we estimate WLS regressions without clustering standard errors at the study level, because the number of study clusters per subsample is too small. The results, however, should be interpreted with caution because the number of observations per subsample is rather low. We find evidence for a weak country heterogeneity: the conditioned average true effects for columns (8) and (10) of subpanel (b) show that the average elasticity to taxes is slightly smaller for US than German studies (i.e., -0.008 vs. -0.011, respectively). As the subsample for Canadian estimates contains only estimates that do not control for labor productivity (i.e., general equilibrium mechanism), the average true effect for Canada is not comparable to the two other effects. Overall, our robustness results confirm that the data used and the choice of the explanatory and control variables can make a large difference in the magnitude of the estimated effect, and therefore, studies on the tax incidence should be more transparent and coherent in their specification choices.

5. Concluding Discussion

In the past 20 years, several empirical studies have provided evidence for labor bearing a substantial share of the CIT through lower wages. As these findings crucially hinge on the underlying study design, however, the exact magnitude of the tax incidence on employees is still inconclusive. In particular, the underlying studies differ considerably in operational choices to measure the CIT burden, the data coverage and specifications. Since all empirical tax incidence studies essentially try to investigate a similar effect, it is important to uncover the potential reasons and implications of these variations. This paper therefore sets up a

comprehensive meta-sample containing 134 estimates from 16 primary studies on the corporate tax incidence in order to investigate: (1) How large is the consensus CIT incidence falling on employees via lower wages? and (2) What are the sources of heterogeneity among estimates? We contribute to the literature by providing an average estimate for the wage elasticity and by evaluating the impact of heterogeneity of diverse study characteristics to shed light on the puzzling inconclusiveness of the underlying literature.

While the meta-sample mean of the wage elasticity corresponds to -0.190 , we find robust evidence for a substantial publication bias in favor of negative estimates rather than statistical significance. After correcting for publication selection, the averaged true wage elasticity amounts to only -0.024 , which is roughly an eighth of the unconditioned mean of elasticity estimates. Our semi-elasticity estimates suggest that a 1%-point increase in CIT rates is associated with a decline in wages of only 0.110% to 0.235%.

Moreover, the reported size of the wage responses depends on the data coverage, the temporal focus and the average sample year. The estimates are smaller (i.e., less negative) for cross-country than within-country studies and the degree of shifting is much larger in the long-run and trends upwards over time. Furthermore, the applied CIT rate measure seems to make a difference regarding the precision and magnitude of an estimate. Studies that use ex-post average tax rates are not plagued by a publication bias in comparison to those that use statutory or ex-ante rates. Moreover, our results point to the difficulty of the underlying studies to cleanly distinguish between the general equilibrium ('total' or 'indirect') and rent-sharing ('direct') mechanism econometrically. Most notably, even after accounting for heterogeneity in estimates, the evidence for substantial publication bias and a very small consensus estimate survives.

Given these findings, it is advisable for future empirical studies to transparently report specification choices and in particular to provide extensive robustness checks by altering the

operational constructs used (CIT measure, temporal dynamic assumptions and data sample criteria). In addition, the underlying theoretical model should be evolved coherently whenever a distinction between direct and indirect effects is made. Justifying control settings and specification choices more transparently can help to gain a deeper inside with respect to the policy implications of studies' incidence results.

We also acknowledge some limitations of our MRA. First, our meta-sample naturally covers estimates from different studies. As it is rather small (134 estimates), we are not able to control for all study characteristics. Second, the underlying tax incidence mechanism is very complex. The economic interactions of various factors, such as the size of an economy (e.g., Hassett & Mathur 2015), the firms' tax planning behavior (e.g., OECD 2015, Dyreng et al. 2020) or the degree of tax competition (e.g., Liu & Altshuler 2013), may have an impact on the tax incidence on wages. Thus, a part of the conclusions of the literature remains unexplored in our MRA. Our results should therefore be interpreted with conventional caution but are a good starting point for future empirical research.

Finally, our most important result of a very small 'true' average tax incidence has direct implications for current policy debates about raising CIT rates. For advocates of lower corporate taxes, a large corporate tax incidence on wages is a very appealing argument. In line with that, the CEA used a 'conservative' range of elasticity estimates of -0.16 to -0.33 in 2017 to calculate the average advantage for employees when the CIT is lowered. At that time, this was a strong argument in favor of Donald Trump's Tax Cuts and Jobs Act which introduced a flat corporate tax rate of 21% in the US. More recently, the Tax Foundation claims in its analysis of president-elect Joe Biden's corporate tax plan which proposes the reversal of the Tax Cuts and Jobs Act and raising the CIT rate to 28% that "*studies examining corporate income taxes [...] support the idea that workers bear a large portion of the corporate income tax through lower wages.*" (Tax Foundation 2021, p. 11). The Tax Foundation assumes in its

analysis that 50% of the CIT burden is borne by workers.¹³ As this study shows, the assumed incidence by the Tax Foundation exceeds the weighted mean of reported semi-elasticity estimates (i.e., -0.110 to -0.235) by more than 100%. Further considering the robust evidence for substantial publication bias and the very small consensus estimate of this MRA, the proposed incidence (counter-)argument should be taken with care and seems to be ideologically driven to some extent.

¹³ The evaluation was conducted by Garrett Watson and William McBride on behalf of the Tax Foundation in February 2021. The only reference that they explicitly rely on for the CIT incidence is the study of Fuest et al. (2018), while not taking into account the general inconclusiveness of the literature and implications of different datasets and institutional settings.

Table 1

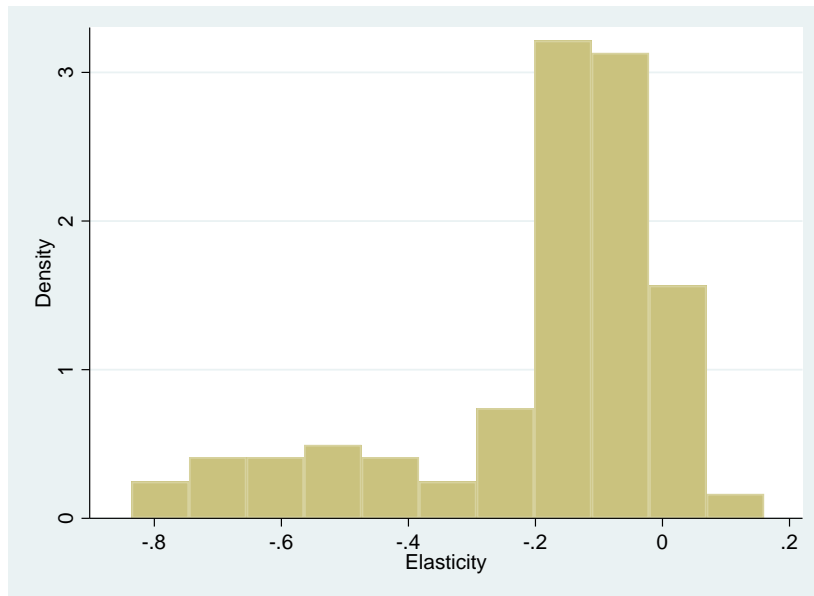
Distribution of the elasticity and semi-elasticity estimates

	Min	1 st Quantile	Median	3 rd Quantile	Max	Mean	Std. Dev.
<i>Elasticity estimate</i>	-0.836	-0.229	-0.125	-0.030	0.160	-0.190	0.213
<i>Semi-elasticity estimate</i>	-2.394	-1.166	-0.328	-0.109	0.151	-0.638	0.723

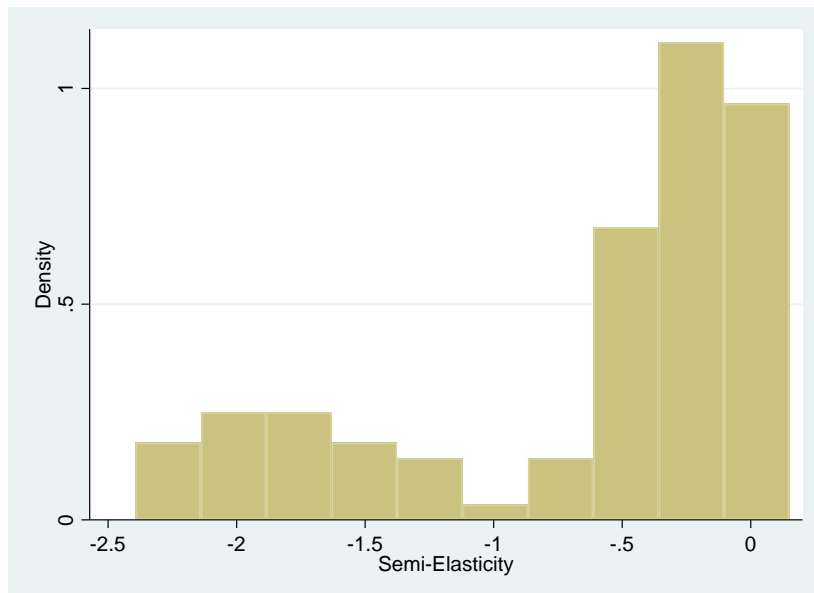
Notes: We dropped four outliers of Ebrahimi and Vaillancourt (2016) and the two extreme estimates of Goodspeed (2012).

Figure 1

Histogram



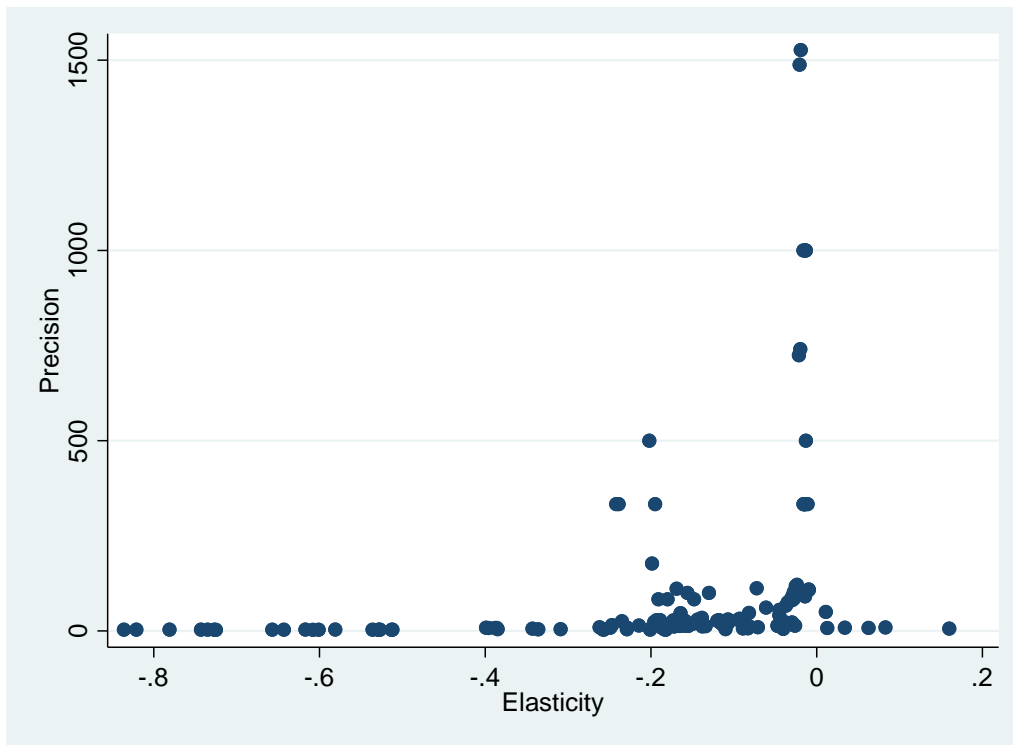
(a) *Elasticity estimates*



(b) *Semi-elasticity estimates*

Figure 2

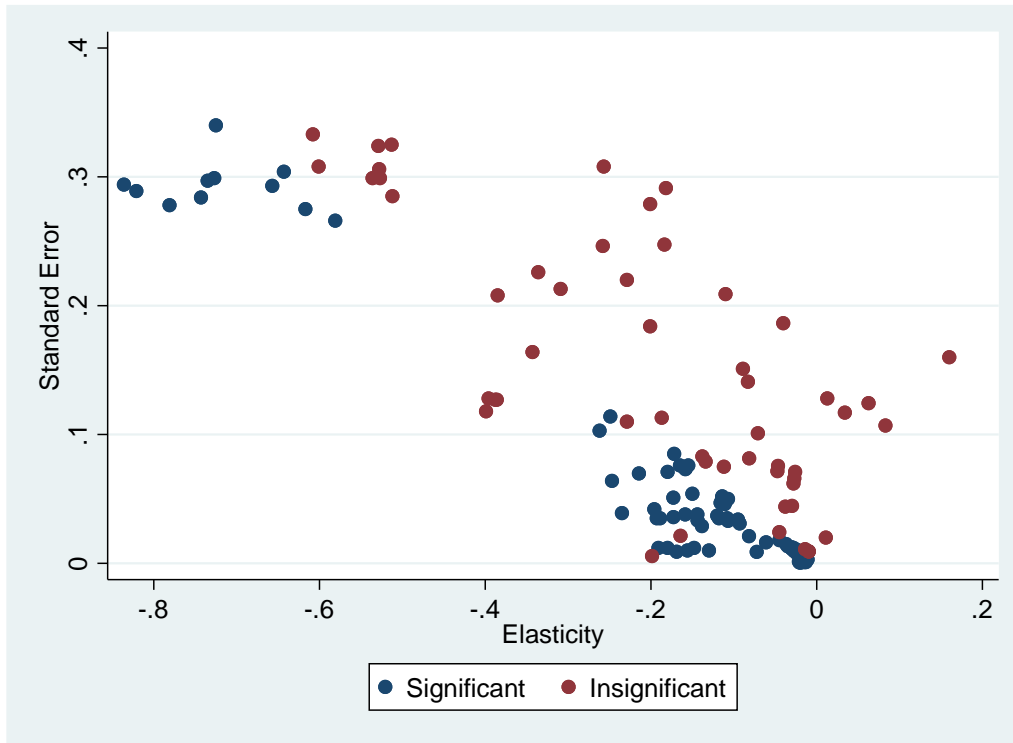
Funnel plot of the elasticity estimates



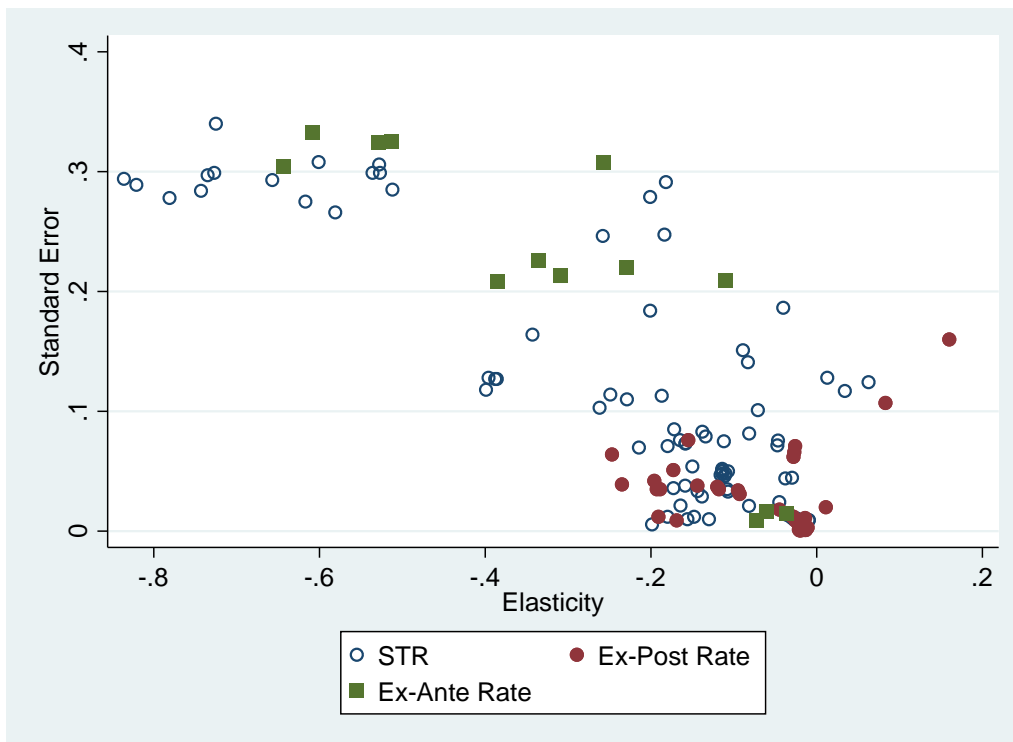
Notes: We dropped the two extreme estimates of Goodspeed (2012).

Figure 3

Scatter plot of the elasticity estimates



(a) Significance



(b) CIT rate variable

Notes: We dropped four outliers of Ebrahimi and Vaillancourt (2016) and the two extreme estimates of Goodspeed (2012).

Table 2*Description and summary statistics of moderator variables*

Moderator variables	Description	Summary statistics (N = 134)	
		Mean	Std. dev.
Publication bias			
<i>Standard error</i>	Standard errors of the elasticity estimate	0.100	0.105
CIT rate			
<i>STR*</i>	= 1 if the elasticity estimate relies on the STR as the explanatory variable, and 0 otherwise	0.619	0.487
<i>Ex-ante rate</i>	= 1 if the elasticity estimate relies on the METR or AETR as the explanatory variable, and 0 otherwise	0.097	0.297
<i>Ex-post rate</i>	= 1 if the elasticity estimate relies on the ATR as the explanatory variable, and 0 otherwise	0.284	0.452
Dataset			
<i>Cross-country data</i>	= 1 if the elasticity estimate relies on cross-country data, and zero otherwise	0.530	0.501
<i>Within-country data*</i>	= 1 if the elasticity estimate relies on within-country data, and zero otherwise	0.470	0.501
<i>Average sample year</i>	= average sample year of the respective elasticity estimate, normalized between 0 and 1	0.451	0.294
Control variables			
<i>Conditioned*</i>	= 1 if the elasticity estimate relies on a regression controlling for labor productivity, and 0 otherwise	0.672	0.471
<i>Unconditioned</i>	= 1 if the elasticity estimate relies on a regression not controlling for labor productivity, and 0 otherwise	0.328	0.471
Estimation method			
<i>OLS</i>	= 1 if the elasticity estimate relies on (pooled) OLS estimator, and 0 otherwise	0.127	0.334
<i>IV*</i>	= 1 if the elasticity estimate relies on IV, and 0 otherwise	0.187	0.391
<i>GMM</i>	= 1 if the elasticity estimate relies on GMM estimator, and 0 otherwise	0.090	0.287
<i>FE estimator*</i>	= 1 if the elasticity estimate relies on fixed effects estimator, and 0 otherwise	0.552	0.499
<i>RE estimator*</i>	= 1 if the elasticity estimate relies on random effects estimator or IV, and 0 otherwise	0.045	0.208
Temporal dynamics			
<i>Long-term effect</i>	= 1 if a long-term elasticity estimate, and 0 otherwise	0.119	0.325
<i>Short-term effect*</i>	= 1 if a short-term elasticity estimate, and 0 otherwise	0.172	0.378
<i>Static effect*</i>	= 1 if a static elasticity estimate, and 0 otherwise	0.709	0.456

Notes: We dropped four outliers of Ebrahimi and Vaillancourt (2016) and the two extreme estimates of Goodspeed (2012). The superscript * marks the benchmark category of the respective study characteristic.

Table 3
Testing for publication bias

(a) Alternative models

Moderator variables	WLS (1)	OLS (2)	Number of estimates (3)	IV (4)
Publication bias				
<i>Standard error</i>	-1.659*** (0.221)	-1.635*** (0.240)	-1.472*** (0.276)	-1.188** (0.522)
Constant (true effect)	-0.024*** (0.006)	-0.026 (0.020)	-0.044** (0.018)	-0.067** (0.031)
Number of observations	134	134	134	124
Number of primary studies	16	16	16	15
Adj. R-squared	0.532	0.648	0.593	0.586
VIF	1	1	1	1

Notes: The dependent variable is the *Elasticity estimate*. Weighted least squares (WLS) meta-regression with the inverse of the squared *Standard error* as analytical weights is used in column (1), ordinary least squares (OLS) meta-regression is used in column (2), weighted least squares (WLS) meta-regression with the inverse of the *Number of Estimates* as analytical weights is used in column (3), and the inverse of the square root of the *Number of observations* as an instrument variable (IV) for the *Standard error* is used in column (4). ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively. Standard errors are in parentheses and clustered at the study level to control for autocorrelation (within-study dependency).

(b) Subsamples

Moderator variables	Published (1)	Others (2)	Dependence (3)	Independence (4)
Publication bias				
<i>Standard error</i>	-2.786*** (0.672)	-2.093** (0.768)	-3.325** (1.268)	-2.041*** (0.398)
Constant (true effect)	-0.014*** (0.003)	-0.017*** (0.001)	-0.011*** (0.001)	-0.019*** (0.001)
Number of observations	65	69	50	84
Number of primary studies	8	8	6	10
Adj. R-squared	0.218	0.301	0.319	0.214
VIF	1	1	1	1

Notes: The dependent variable is the *Elasticity estimate*. Weighted least squares (WLS) meta-regression with the inverse of the squared *Standard error* as analytical weights is used. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively. Standard errors are in parentheses and clustered at the study level to control for autocorrelation (within-study dependency).

Table 4
Semi-elasticity estimates

Moderator variables	WLS (1)	WO A/D/M & M (2)	Squared standard error (3)
Publication bias			
<i>Standard error</i>	-3.773*** (0.176)	-2.671*** (0.452)	
<i>Squared standard error</i>			-1.460 (0.979)
Constant (true effect)	-0.008 (0.005)	-0.110*** (0.034)	-0.235*** (0.016)
Number of observations	110	91	91
Number of primary studies	14	12	12
Adj. R-squared	0.741	0.481	0.073
VIF	1	1	1

Notes: The dependent variable is the *Semi-elasticity estimate*. Weighted least squares (WLS) meta-regression with the inverse of the squared *Standard error* as analytical weights is used. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively. Standard errors are in parentheses and clustered at the study level to control for autocorrelation (within-study dependency).

Table 5
Sources of heterogeneity

Moderator variables	(1)	(2)	(3)	(4)	(5)
CIT rate					
<i>Ex-ante rate</i>	-0.006 (0.023)	0.028 (0.045)	-0.005 (0.039)	0.001 (0.032)	0.000 (0.032)
<i>Ex-post rate</i>	0.016 (0.021)	0.010 (0.011)	0.005 (0.009)	0.015 (0.012)	0.014 (0.011)
Theoretical mechanism					
<i>Unconditioned</i>		-0.043 (0.039)	-0.021 (0.030)	-0.029 (0.027)	-0.028 (0.027)
<i>Cross-country data</i>		0.002 (0.003)	0.004*** (0.001)	0.003*** (0.001)	0.004*** (0.001)
<i>Unconditioned x Cross-country data</i>					-0.085** (0.029)
Estimation method					
<i>OLS</i>			-0.002 (0.001)	-0.000 (0.001)	-0.000 (0.001)
<i>GMM</i>			-0.034 (0.033)	-0.032 (0.033)	-0.031 (0.000)
Temporal dynamics					
<i>Long-term effect</i>			-0.133*** (0.039)	-0.126*** (0.033)	-0.057* (0.031)
Time trend					
<i>Average sample year</i>				-0.023** (0.010)	-0.023** (0.010)
Publication bias					
<i>Standard error</i>	-2.090*** (0.458)	-1.728*** (0.562)	-1.302*** (0.326)	-1.314*** (0.392)	-1.496*** (0.376)
Constant	-0.031 (0.022)	-0.026** (0.012)	-0.022** (0.010)	-0.016*** (0.004)	-0.014*** (0.004)
Number of observations	134	134	134	134	134
Number of primary studies	16	16	16	16	16
Adj. R-squared	0.262	0.376	0.603	0.671	0.684
VIF	1.13	1.16	1.22	1.27	2.25

Notes: The dependent variable is the *Elasticity estimate*. Weighted least squares (WLS) meta-regression with the inverse of the squared *Standard error* as analytical weights is used. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively. Standard errors are in parentheses and clustered at the study level to control for autocorrelation (within-study dependency).

Table 6
Robustness results

(a) *CIT rate variables*

Moderator variables	CIT variables		
	<i>STR</i>	<i>Ex-ante rate</i>	<i>Ex-post rate</i>
	(1)	(2)	(3)
Publication bias			
<i>Standard error</i>	-2.102*** (0.568)	-1.137** (0.242)	-2.394 (1.261)
Constant (true effect)	-0.031 (0.023)	-0.050** (0.012)	-0.016*** (0.003)
Number of observations	83	13	38
Number of primary studies	12	4	6
Adj. R-squared	0.159	0.546	0.184
VIF	1	1	1

Notes: The dependent variable is the *Elasticity estimate*. Weighted least squares (WLS) meta-regression with the inverse of the squared *Standard error* as analytical weights is used. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively. Standard errors are in parentheses and clustered at the study level to control for autocorrelation (within-study dependency).

(b) *Theoretical mechanism*

Moderator variables	Theoretical mechanism								
	<i>Cross-country data</i>				<i>Within-country data</i>				
	<i>Total</i>	<i>Worldwide</i>	<i>OECD</i>	<i>WO A/D/M</i>	<i>Total</i>	<i>WO C & M/F</i>	<i>US</i>	<i>Canada</i>	<i>Germany</i>
(1)	(2)	(3)	(4)	(5)	(6)	(8)	(9)	(10)	
Publication bias									
<i>Standard error</i>	-1.676** (0.584)	-2.230* (0.280)	-1.575 (0.967)	-1.290** (0.449)	-2.255*** (0.448)	-1.414*** (0.326)	-2.160*** (0.668)	1.055 (0.991)	-1.708*** (0.348)
Theoretical mechanism									
<i>Unconditioned</i>	-0.169*** (0.004)		-0.170*** (0.007)	-0.178*** (0.008)	-0.017 (0.027)	-0.093** (0.028)	-0.001 (0.005)		-0.109*** (0.016)
Constant	-0.013*** (0.001)	0.089 (0.138)	-0.014*** (0.001)	-0.008 (0.010)	-0.017*** (0.002)	-0.019*** (0.000)	-0.008** (0.003)	-0.146*** (0.019)	-0.011** (0.005)
True effect (<i>Conditioned</i>)	-0.013*** (0.001)	0.089 (0.138)	-0.014*** (0.001)	-0.008 (0.010)	-0.017*** (0.002)	-0.019*** (0.000)	-0.008** (0.003)		-0.011** (0.005)
True effect (<i>Unconditioned</i>)	-0.182*** (0.005)		-0.183*** (0.008)	-0.185*** (0.004)	-0.034 (0.027)	-0.112** (0.028)	-0.010 (0.006)	-0.146*** (0.019)	-0.112*** (0.019)
Number of observations	71	25	46	60	63	38	14	17	24
Number of primary studies	8	2	6	7	8	6	2	2	3
Adj. R-squared	0.687	0.523	0.671	0.820	0.413	0.869	0.413	0.008	0.932
VIF	1.02	1.00	1.04	1.07	1.22	1.46	1.03	1.00	2.52

Notes: The dependent variable is the *Elasticity estimate*. Weighted least squares (WLS) meta-regression with the inverse of the squared *Standard error* as analytical weights is used. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively. Standard errors are in parentheses and clustered at the study level to control for autocorrelation (within-study dependency) except for columns (8) to (10). The variable *Unconditioned* equals 0 for all estimates in column (2) and 1 for all estimates in column (9) and is therefore omitted.

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Appendices

Appendix A

Studies Included in the Meta-Sample

Figure A.1

PRISMA flow chart: Selection of the literature and inclusion criteria

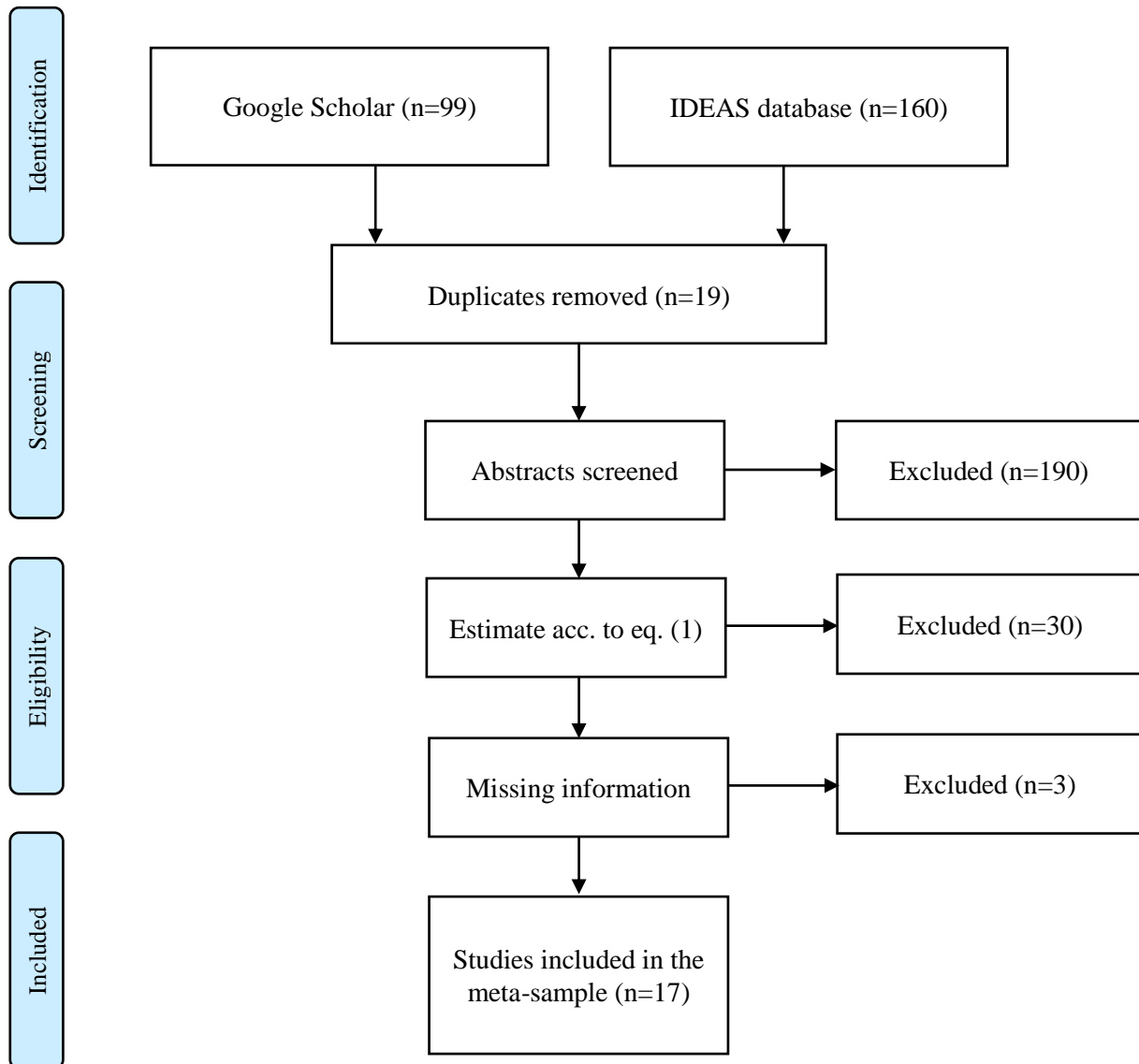


Table A.1*Primary studies on the tax incidence on wages*

#	Authors and publication year	CIT rate	Period	Dataset	Elasticity estimate			Std. Dev.	
					N	Mean	Min		Max
1	Arulampalam, Devereux and Maffini (2012)	ATR	1996-2005	disaggregate cross-country	11	-0.076	-0.191	0.011	0.070
2	Aus dem Moore (2014)	ATR	1994-2010	disaggregate within-country	8	-0.024	-0.030	-0.019	0.004
3	Azémar and Hubbard (2015)	STR	1980-2004	aggregate cross-country	11	-0.065	-0.199	0.063	0.082
4	Bauer, Kasten and Siemers (2017)	STR	1995-2004	disaggregate within-country	10	-0.054	-0.173	-0.024	0.047
5	Becker, Fuest and Riedel (2012)	STR	2000-2006	disaggregate cross-country	3	-0.172	-0.215	-0.144	0.037
6	Carroll (2009)	STR, ATR	1970-2007	aggregate within-country	12	-0.069	-0.172	-0.011	0.068
7	Clausing (2012)	STR, ATR	1981-2009	aggregate cross-country	12	-0.151	-0.725	0.160	0.268
8	Dwenger, Rattenhuber and Steiner (2017)	ATR	1998-2006	disaggregate within-country	8	-0.192	-0.247	-0.144	0.036
9	Ebrahimi and Vaillancourt (2016)	STR	1998-2013	disaggregate within-country	8	-0.186	-0.242	-0.130	0.041
10	Exbrayat and Geys (2016)	STR	1982-2007	aggregate cross-country	3	-0.053	-0.139	-0.009	0.074
11	Felix (2007)	STR	1979-2002	aggregate cross-country	6	-0.190	-0.258	-0.158	0.037
12	Fuest, Peichl and Siegloch (2018)	STR	1989-2008	disaggregate within-country	6	-0.357	-0.399	-0.229	0.066
13	Goodspeed (2012)	STR	2003	disaggregate within-country	2	-0.001	-0.001	-0.000	0.000
14	Gravelle and Hungerford (2007)	STR, METR, AETR	1981-2002	aggregate cross-country	10	-0.436	-0.836	-0.110	0.246
15	Hassett and Mathur (2015)	STR, METR, AETR	1981-2005	aggregate cross-country	15	-0.573	-0.821	-0.249	0.166
16	Liu and Altshuler (2013)	METR	1982-1997	disaggregate within-country	2	-0.067	-0.072	-0.061	0.008
17	McKenzie and Ferde (2017)	STR, METR, ATR	1981-2014	aggregate within-country	13	-0.104	-0.150	-0.037	0.030
	Full meta-sample	-	1970-2013	-	140	-0.188	-0.836	0.160	0.210

Notes: The estimates of Fuest, Peichl and Siegloch (2018) are multiplied by -1 (net-of-tax rate estimates).

Table A.2*Excluded studies on the tax incidence on wages (selection)*

#	Authors and publication year	Reasons for exclusion
1	Agrawal and Chakraborty (2019)	Underlying estimation eq. is not comparable to eq. (1): The total tax incidence (of labor and capital) is constrained to one.
2	Felix (2009)	Missing information regarding the standard errors and <i>t</i> -statistics.
3	Felix and Hines (2009)	Underlying estimation eq. is not comparable to eq. (1): Interaction term.
4	Kakpo (2018)	Underlying estimation eq. is not comparable to eq. (1): Difference-in-Differences approach using 2005 Ohio tax reform.
5	Aus dem Moore, Kasten and Schmidt (2014)	Underlying estimation eq. is not comparable to eq. (1): Difference-in-Differences approach using 2000 German tax reform.
6	Clausing (2013)	Underlying estimation eq. is not comparable to eq. (1): Vector auto-regression model.
7	Desai, Goley and Hines (2007)	Underlying estimation eq. is not comparable to eq. (1): The total tax incidence (of labor and capital) is constrained to one.
8	Gyourko and Tracy (1989)	Missing information regarding the mean value of the CIT variable.
9	Hotei (2018)	Underlying estimation eq. is not comparable to eq. (1): Vector auto-regression model.
10	Ljungqvist and Smolyansky (2016)	Missing information regarding the mean value of the CIT variable.
11	Suárez Serrato and Zidar (2016)	Underlying estimation eq. is not comparable to eq. (1): Wage growth as dependent variable.
12	Giroud and Rauh (2019)	Underlying estimation eq. is not comparable to eq. (1): Interaction term.

Appendix B

Supplementary Statistics

Table B.2
Correlation matrix of the moderator variables

Moderator variables	<i>Standard error</i>	<i>STR</i>	<i>Ex-ante rate</i>	<i>Ex-post rate</i>	<i>Cross-country data</i>	<i>Within-country data</i>	<i>OLS</i>	<i>IV</i>	<i>GMM</i>	<i>FE estimator</i>	<i>RE estimator</i>	<i>Long-term effect</i>	<i>Short-term effect</i>	<i>Static effect</i>	<i>Conditioned</i>	<i>Unconditioned</i>	<i>Average sample year</i>
<i>Standard error</i>	1.000																
<i>STR</i>	0.138	1.000															
<i>Ex-ante rate</i>	-0.004	-0.019	1.000														
<i>Ex-post rate</i>	0.938	-0.031	-0.029	1.000													
<i>Cross-country data</i>	0.546	-0.019	-0.021	0.446	1.000												
<i>Within-country data</i>	0.772	0.052	-0.019	0.880	-0.027	1.000											
<i>OLS</i>	0.440	-0.017	-0.020	0.315	0.448	0.116	1.000										
<i>IV</i>	-0.024	-0.022	0.899	-0.047	-0.034	-0.033	-0.032	1.000									
<i>GMM</i>	-0.001	-0.034	-0.032	-0.045	-0.032	-0.036	-0.036	-0.052	1.000								
<i>FE estimator</i>	0.840	0.046	-0.024	0.938	0.308	0.889	-0.028	-0.040	-0.045	1.000							
<i>RE estimator</i>	-0.050	-0.019	-0.019	-0.031	-0.022	-0.025	-0.021	-0.031	-0.034	-0.026	1.000						
<i>Long-term effect</i>	0.020	0.110	-0.015	-0.024	0.001	-0.019	0.009	-0.017	-0.026	-0.021	-0.016	1.000					
<i>Short-term effect</i>	0.926	-0.029	-0.027	0.998	0.448	0.877	0.317	-0.045	-0.042	0.935	-0.029	-0.023	1.000				
<i>Static effect</i>	0.229	0.699	0.009	-0.004	-0.037	0.069	-0.035	-0.007	-0.058	0.060	-0.033	-0.027	-0.049	1.000			
<i>Conditioned</i>	0.945	0.037	-0.029	0.997	0.445	0.883	0.314	-0.048	-0.046	0.940	-0.032	-0.025	0.996	0.036	1.000		
<i>Unconditioned</i>	0.080	0.021	0.100	-0.006	-0.020	0.006	-0.017	0.083	-0.038	0.002	-0.022	0.255	-0.033	0.283	-0.036	1.000	
<i>Average sample year</i>	0.918	-0.028	-0.026	0.997	0.404	0.898	0.300	-0.044	-0.042	0.940	-0.029	-0.021	0.999	-0.049	0.995	-0.031	1.000

Notes: This matrix shows the correlation of moderator variables using the full meta-sample without the four outliers of Ebrahimi and Vaillancourt (2016) and the two extreme estimates of Goodspeed (2012). The moderator variables are weighted by the inverse of squared *Standard error*. Table 1 provides descriptions of the moderator variables.

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Vorstand: Prof. Dr. Ralf Maiterth (Vorsitzender),
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Heiko Müller, Jens Müller, Rainer Niemann,
Deborah Schanz, Sebastian Schanz, Caren Sureth-
Sloane, Corinna Treisch

Kontaktadresse:

Prof. Dr. Caren Sureth-Sloane, Universität Paderborn,
Fakultät für Wirtschaftswissenschaften,
Warburger Str. 100, 33098 Paderborn,
www.arqus.info, Email: info@arqus.info

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