

aus dem Moore, Nils; Kasten, Tanja; Schmidt, Christoph M.

**Working Paper**

## Do Wages Rise when Corporate Taxes Fall? - Evidence from Germany's Tax Reform 2000

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Nils aus dem Moore  
Tanja Kasten  
Christoph M. Schmidt

**Do Wages Rise when Corporate  
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**Evidence from Germany's Tax Reform 2000**

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Technische Universität Dortmund, Department of Economic and Social Sciences  
Vogelpothsweg 87, 44227 Dortmund, Germany

Universität Duisburg-Essen, Department of Economics  
Universitätsstr. 12, 45117 Essen, Germany

Rheinisch-Westfälisches Institut für Wirtschaftsforschung (RWI)  
Hohenzollernstr. 1-3, 45128 Essen, Germany

## Editors

Prof. Dr. Thomas K. Bauer  
RUB, Department of Economics, Empirical Economics  
Phone: +49 (0) 234/3 22 83 41, e-mail: [thomas.bauer@rub.de](mailto:thomas.bauer@rub.de)

Prof. Dr. Wolfgang Leininger  
Technische Universität Dortmund, Department of Economic and Social Sciences  
Economics – Microeconomics  
Phone: +49 (0) 231/7 55-3297, e-mail: [W.Leininger@wiso.uni-dortmund.de](mailto:W.Leininger@wiso.uni-dortmund.de)

Prof. Dr. Volker Clausen  
University of Duisburg-Essen, Department of Economics  
International Economics  
Phone: +49 (0) 201/1 83-3655, e-mail: [vclausen@vwl.uni-due.de](mailto:vclausen@vwl.uni-due.de)

Prof. Dr. Roland Döhrn, Prof. Dr. Manuel Frondel, Prof. Dr. Jochen Kluge  
RWI, Phone: +49 (0) 201/81 49-213, e-mail: [presse@rwi-essen.de](mailto:presse@rwi-essen.de)

## Editorial Office

Sabine Weiler  
RWI, Phone: +49 (0) 201/81 49-213, e-mail: [sabine.weiler@rwi-essen.de](mailto:sabine.weiler@rwi-essen.de)

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Nils aus dem Moore, Tanja Kasten, and Christoph M. Schmidt<sup>1</sup>

# Do Wages Rise when Corporate Taxes Fall?

Evidence from Germany's Tax Reform 2000

## Abstract

*We contribute to the empirical literature on the effective incidence of corporate income taxation by using the German Business Tax Reform of the year 2000 (GBTR 2000) as a natural experiment. Its effect on wages in the manufacturing sector is identified by means of a difference-in-differences analysis that uses French firms as comparison group. We provide evidence that GBTR 2000 led to a significant and sizeable wage effect. For 2001, the first year after GBTR 2000 took effect, we estimate a short-run effect that implies a wage increase of 7.9 percent. Due to the dynamic nature of the empirical model used, the incidence effect grows gradually over time during the evaluation period.*

*JEL Classification: H22, H25, J31, J38*

*Keywords: Corporate income taxation; tax reform; tax incidence; profit taxation; wages; difference-in-differences*

*December 2014*

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<sup>1</sup> Nils aus dem Moore and Tanja Kasten, RWI - Berlin Office; Christoph M. Schmidt, RWI and Ruhr University Bochum - The authors are grateful to Thomas K. Bauer, Robert S. Chirinka, Nadja Dwenger, Wolfgang Eggert, Frank Fossen, Clemens Fuest, Michael Kvasnicka, Li Liu and Jack Mintz for helpful comments and suggestions. We also gratefully acknowledge the comments and suggestions of discussants and participants at the 65th Congress of the International Institute of Public Finance (IIPF) in Cape Town and at the 2011 AEA Annual Meeting in Denver. The usual disclaimer applies. - This paper corresponds to chapter 2 of the dissertation „Myths and Measurement - Firm-level Effects of Corporate Income Taxation“ at the Faculty of Management and Economics at Ruhr-Universität Bochum, available online at <http://www-brs.ub.ruhr-uni-bochum.de/netatml/HSS/Diss/MooreNilsausdem/>. - All correspondence to Nils aus dem Moore, RWI - Berlin Office, Invalidenstr. 112, 10115 Berlin, Germany, e-mail: [Nils.ausdemMoore@rwi-essen.de](mailto:Nils.ausdemMoore@rwi-essen.de)

# 1 Introduction

The last three decades saw considerable reforms to corporate income taxes in industrialized countries. A downward trend was most pronounced between the early 1980s and the end of the 1990s. In this period, statutory corporate income taxes in OECD countries fell from an average of 48 percent to 35 percent (Devereux, Griffith and Klemm, 2002).<sup>1</sup> Germany was one of the last countries to jump on the corporate-tax-cut bandwagon. Not until 2001, it abandoned its split-rate system of 40 percent for retained and 30 percent for distributed profits in favor of a single uniform corporate tax rate of 25 percent.

We use this German Business Tax Reform 2000 (in the following: GBTR 2000) as a quasi-experiment to shed light on the empirical question of the effective incidence of the corporate income tax on wages. We use a wage bargaining model as the basis for a counterfactual design that enables us to answer our research question “Do wages rise when corporate taxes fall?” Concretely, we identify the direct incidence of the GBTR 2000 on wages in the German manufacturing industry through a difference-in-differences approach (DiD) that uses manufacturing companies in France as the control group. We find evidence that GBTR 2000 led to a positive, significant and sizeable wage effect. Hence, our results suggest that GBTR 2000 did not only benefit affected corporations but also their employees.

The question of how the presumptive benefits from a trend of ever declining corporate income tax rates are shared among the share- and stakeholders of corporations is of high importance for policy makers. This holds true especially for the question of the effective incidence of the corporate income tax on the factor labor, since many theoretical models and empirical studies suggest that the immobile workforce may ultimately be the victim of tax competition (Sinn, 2003, 21).<sup>2</sup>

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<sup>1</sup>A downward trend, albeit less pronounced, is also observed for effective tax rates that account for modifications in the provisions related to the corporate tax base, at least with respect to projects that earn positive economic profits, see Devereux et al. (2002, 460-468).

<sup>2</sup>The underlying assumption for this claim is the conjecture that labour taxes might be needed to fill the gap that is caused by the erosion of corporate taxes and the corresponding loss of revenue, see Sinn (2003, 20). However, as is documented inter alia by Becker and Elsayyad (2009, 108-109), the commonly supposed shift from corporate tax reliance to taxes on less mobile personal income, i.e. mainly labour, did not materialize, obviously because there was not gap that had to be filled: Despite reduced corporate

Our paper links the vast literature on corporate tax competition to the small but growing literature on the effective incidence of corporate income taxation. The theoretical part of the latter is characterized by two starkly contradicting views that depend on whether one assumes a closed economy or an open economy setting. The first view dates back to Arnold C. Harberger (1962) who came to the conclusion that the tax is borne entirely by owners of capital. The second strand of the literature gives up the crucial assumption of a closed economy. It assumes instead that capital is perfectly mobile between countries, but labor is not. In this setting, a (higher) tax on corporate income tends to shift capital to the rest of the world. This outflow of capital reduces the return to labor and the home country labor force effectively bears the entire burden of the tax (cf. Bradford 1978, Gordon 1986, Kotlikoff and Summers 1987).

A number of more recent contributions have developed general equilibrium models of the long-run incidence of taxes on corporate income in an open economy (see Randolph 2006, Gravelle and Smetters 2006, Gravelle 2010, and Harberger 1995, 2006). Incorporating more detailed assumptions about the economy, such as the extent of factor mobility, supply elasticities, the relative capital intensities of the different sectors and differentiating between perfect versus imperfect competition scenarios, these models arrive at intermediate predictions concerning the distribution of the corporate tax burden among the factors of production.

Against this backdrop of conflicting theoretical results that depend heavily on the assumptions made, a nascent empirical literature uses international data on corporate taxes and wages to estimate the burden of the corporate income tax (see *aus dem Moore* 2014 for a comprehensive overview and *Clausing* 2012 for a thorough review). Instead of trying to measure how corporate taxes affect rates of return on investment, which was the usual approach of the early empirical endeavours on the subject (cf. *Krzyzaniak and Musgrave* 1963, and *Cragg, Harberger and Mieszkowski* 1967), these papers concentrate on whether the imposition of corporate taxes reduces wages.<sup>3</sup> Despite many methodological

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tax rates, corporate tax revenue in OECD countries remained high or has even been increased. Possible solutions to this “corporate tax rate revenue puzzle” are offered by *Auerbach* (2007) and *de Mooij and Nicodème* (2008).

<sup>3</sup>For our evaluation of GBTR 2000, the following contributions possess the greatest relevance (here

differences across the studies, most of them come to the conclusion that labor bears a substantial burden of the corporate tax.

We extend the literature by using the wage bargaining approach of Arulampalam et al. (2012) in a regression formulation of the difference-in-differences (DiD) model. We find evidence that GBTR 2000 led to a significant and sizeable wage effect. In 2001, the first year after GBTR 2000 took effect, we observe a significant short-run effect that implies a wage increase of 7.9 percent. Due to the dynamic nature of our empirical model, the incidence effect grows gradually over time in the subsequent years. At the end of the evaluation period in the year 2005, the cumulative effect reaches a value of 13.3 percent. The remainder proceeds as follows: Section 2 presents an outline of the GBTR 2000, section 3 discusses the research design, section 4 presents our empirical analysis and section 5 concludes.

## 2 The German Business Tax Reform 2000

GBTR 2000 was motivated by concerns about the international competitiveness of corporate income taxation in Germany. One of its goals was to make Germany a more attractive location for international investment.<sup>4</sup> At the same time, GBTR 2000 was part of a wider overhaul of the German tax system. The overarching “Tax Relief Act” comprised several modifications and reforms concerning different types of taxes, implemented stepwise between the years 1998 and 2001. With respect to corporations, all defining dimensions of the previous tax system were modified with effect from January 1, 2001, namely the (i) corporate tax system, (ii) the corporation tax rate, and (iii) the corporate tax base.<sup>5</sup>

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in alphabetical order): Arulampalam et al. (2012), Bauer, Kasten and Siemers (2012), Desai, Foley and Hines (2007), Dwenger, Rattenhuber and Steiner (2011), Felix (2007), Felix and Hines Jr. (2009b,a), Fuest, Peichl and Siegloch (2013), Hassett and Mathur (2010), and Liu and Altshuler (2013).

<sup>4</sup>As the German Finance minister at the time, Hans Eichel, stated in a press conference in December 2009 on the announcement of the tax reform package: “An overall good place for business also needs to be a good place for business with respect to tax laws.” (see Herz, 2005, our translation).

<sup>5</sup>The brief description of GBTR 2000 and the surrounding Tax Relief Act in the following paragraphs is based on European Commission (2003b, 102) and Sørensen (2007, 358-359).

First, concerning the corporate tax system, the full imputation method that had been in force since 1977 was abolished. Instead, a shareholder relief system has been introduced. Under the new system, only one half of the dividends received by a private shareholder are subject to personal income tax. At the same time, all deductions connected with dividend income from the income tax base are halved. However, other elements of private capital income, such as interest receipts, continue to be taxed at the full rate.

Second, the changes in the corporation tax rate covered both the structure and the level. The split-rate that distinguished between retained (40 percent) and distributed profits (30 percent) was abolished and a single uniform tax rate of 25 percent was introduced.

Third, the base of the corporate income tax was broadened through cut-backs in the depreciation allowances both for tangible fixed assets and for buildings. The maximum declining balance rate for tangible fixed assets was reduced from 30 percent to 20 percent. For buildings, the straight-line depreciation rate was reduced from 4 percent to 3 percent.

With respect to the question of corporate tax incidence, the most important element of the GBTR 2000 is the harmonization and considerable reduction of the split corporation tax rate to a uniform rate of 25 percent. This motivates the question whether this tax rate cut led to a significant wage effect.

Finally, in parallel to the GBTR 2000, the Tax Relief Act has reduced the personal income tax rates that also apply to all non-corporate businesses in Germany in a substantial way, albeit with a phase-in period of several years. The top marginal personal income tax rate was lowered from 53 percent (55.92 percent including the so-called “solidarity surcharge” of 5.5 percent) in three successive steps, leading to a rate of 42 percent in 2005 (44.31 percent including the solidarity levy). The top marginal tax rate begins to bite at a taxable income of 52,152 Euro. During the phase-in period, the top marginal rate had been set at 48.5 percent for the years 2001 to 2002, and at 47 percent for the years 2003 and 2004.

## 3 Research Design

Our analysis is based on the wage bargaining channel for the direct incidence of the corporate income tax as proposed by Arulampalam et al. (2012). We identify the direct wage effect of the GBTR 2000 corporate tax rate cut by using a modified bargaining model in a regression formulation of the difference-in-differences approach (henceforth: regression DiD). In subsection 3.1 we give a brief outline of the underlying theoretical framework.<sup>6</sup> Subsection 3.2 delineates our procedure for the choice of an adequate comparison country to model the necessary counterfactual within the regression DiD. Subsection 3.3 describes the data used.

### 3.1 Theoretical Framework

Historically, the theoretical literature has established two channels of indirect incidence of the corporate income tax on wages: while the capital stock channel is caused by responses of investment to the tax-induced alteration of factor returns, the other is induced by the alteration of output prices. These channels both affect the level of pre-tax profit. The capital stock channel is also responsible for the inevitable deadweight loss of the corporate income tax that results from tax-induced changes in the behavior of the affected firms (cf. Kotlikoff and Summers 1987, Fullerton and Metcalf 2002).

The model of a direct incidence of the corporate income tax on wages, derived by Arulampalam et al. (2012) and generalized by Fuest et al. (2013), establishes an additional mechanism by which corporate taxes may be passed on in the form of lower wages. This new wage bargaining channel arises from rent-sharing between workers and firms in a context of imperfect competition: For a given pre-tax profit of a firm, a higher (lower) tax liability will directly reduce (enlarge) the quasi-rent over which workers and firms can bargain.

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<sup>6</sup>See the accompanying paper aus dem Moore (2014) for a more comprehensive exposition of the wage bargaining approach to the effective incidence of corporate income taxation.

This conceptual framework leads Arulampalam et al. (2012) to an empirical wage specification of the form

$$(1) \quad w = w(f, \mu, \bar{w}, \bar{\phi})$$

where  $f$  is the value added per employee,  $\mu$  represents the relative union bargaining power,  $\bar{w}$  the outside option for workers, and  $\bar{\phi}$  contains variables to capture the tax liability of the firm.

Including the value added captures the effects of capital adjustments and output price changes. In so doing, these indirect incidence effects of corporate income taxation are separated from the direct effect that results from the wage bargaining channel. The possibility to empirically detect the direct wage effect by estimating it on the basis of firm-level accounting data provides us with the opportunity to use this framework as the basis for our regression DID approach. However, this approach would not be very meaningful for our purpose if we had to assume that the entirety of the corporate tax incidence is dominated by sizeable indirect effects and that the direct incidence accounts only for a minor part of the overall incidence. But recent empirical studies convincingly point to the fact that the opposite seems to be true: in the contribution of Fuest et al. (2013), which identifies separately the direct wage effect through the wage bargain and the conventional indirect wage effect through reduced investment, the bulk of the overall incidence results undoubtedly from the direct effect that arises through the wage-bargain.

Against this backdrop, we are confident that our DiD approach which is based on the wage bargaining channel captures the better part of the incidence effect that was caused by the GBTR 2000.

### 3.2 Selection of a Comparison Country

Due to the temporal parallelity between GBTR 2000 and the personal income tax reforms of the Tax Relief Act, the corporate sector and the non-corporate sector were concurrently affected by substantial income tax reforms. This fact precludes the implementation of a

DiD design based on a comparison within Germany, i.e. between incorporated companies on the one side which are liable to corporate income taxation and non-incorporated firms on the other side which are not directly affected by changes of corporate tax rates. Hence, to identify the wage incidence effects of the German business tax reform, we need to find a suitable comparison country.

The ideal comparison country on economic grounds would possess a corporate tax system without any relevant changes during our period of observation, should be quite similar to Germany in key aspects of the economic structure and the macroeconomic situation, and should display a steady evolution of wages in manufacturing devoid of significant jumps (that might result from economic policy measures like a labor market reform). Moreover, from a pragmatic perspective, it would allow for a disaggregated firm-level analysis on the basis of high quality data, ideally in the form of a balanced dataset that contains long spells without gaps for all relevant variables. Given that it is possible, to some degree at least, to control for structural and business cycle differences between different countries by means of adequate control variables, the required stability of the respective corporate tax system served as the preeminent economic selection criterion. Hence, we started our selection procedure of a suitable comparison country with an analysis of the evolution of corporate tax rates in potential comparison countries within Europe during a reasonable time span around the effective date of GBTR 2000.

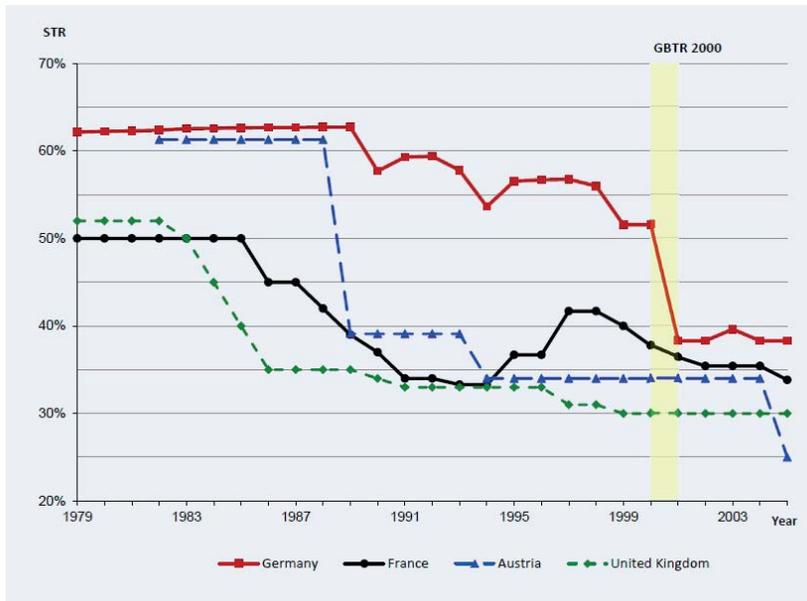
For our purpose, two tax rate measures are relevant that capture different aspects of the respective corporate tax system: The statutory tax rate (STR) is the benchmark rate from tax law that applies to pre-tax profits and dominates political debates about corporate tax reform. However, with respect to business decisions in already established firms its economic importance is limited since it abstracts from tax base effects. In contrast, the effective marginal tax rate (EMTR) integrates relevant features of the tax base, notably it accounts for depreciation allowances. Therefore, the EMTR is the relevant measure with regard to investment decisions at the intensive margin, i.e. decisions about investments in already existing production facilities.<sup>7</sup>

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<sup>7</sup>In contrast, the effective average tax rate (EATR) displays the tax burden that is relevant for corporate decisions at the extensive margin, i.e. concerning the location choice for a new production facility.

Figures 1 and 2 show the evolution of the statutory tax rate and the effective marginal tax rate of the corporate income tax in Germany, France, Austria and the United Kingdom in the time period from 1979 to 2005. The yellow shaded bar highlights the effective date of GBTR 2000 in January 2001.

Figure 1: Statutory Tax Rates, 1979-2005, for Germany, France, Austria and the United Kingdom



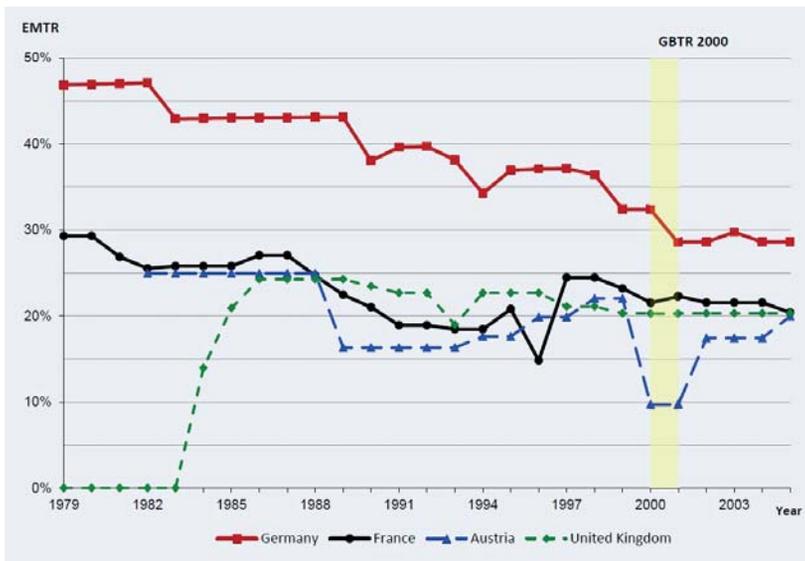
Source: Devereux et al. (2002) for the data (Update 2005), own illustration.

The case of Austria shows why it is not sufficient to consider only the statutory tax rate when choosing a comparison country: while in Figure 1 Austria looks like a near-to-perfect comparison country in the relevant time span from 1996 until 2005 except for the drop in the last year, this picture changes if one considers the effective marginal tax rates instead. This can be seen in Figure 2. Simultaneously to the introduction of GBTR 2000 in Germany, the Austrian EMTR exhibits a decline (caused by legal alterations of the tax

Since the present analysis is focused on incidence effects in firms that were established already several years before the GBTR 2000 took place, the EATR was not relevant for the choice of our comparison country. We report its evolution only for the sake of completeness in Figure 4 of the appendix.

base) which is even larger than the fall of the EMTR in Germany due to GBTR 2000. Hence, Austria is obviously no reasonable choice for providing the required comparison sample of corporations. In contrast, France and the UK both exhibit a fairly constant evolution of the respective statutory and effective marginal rates of the corporate income tax with a smooth decline in France, mostly before the year 2001, and an almost flat evolution in the UK.

Figure 2: Effective Marginal Tax Rates, 1979-2005, for Germany, France, Austria and the United Kingdom



Source: Devereux et al. (2002) for the data (Update 2005), own illustration.

Table 1 contains variables with respect to size (population, nominal GDP), economic structure (nominal GDP per capita, output share of manufacturing, GDP share of trade), features of the labor market (wage bargaining system, union density, collective bargaining coverage, labor compensation per hour, hours worked) and the macroeconomic situation (unemployment rate, growth in nominal GDP) in Germany and the three possible comparison countries.

Table 1: Country profiles for Germany, France, Austria and the United Kingdom (1996-2005)

|  | Germany |                         |        | France             |             |       | United Kingdom <sup>a</sup> |         |       | Austria |              |       |
|--|---------|-------------------------|--------|--------------------|-------------|-------|-----------------------------|---------|-------|---------|--------------|-------|
|  | 1996    | 2005                    | Δ      | 1996               | 2005        | Δ     | 1996                        | 2005    | Δ     | 1996    | 2005         | Δ     |
| Population ( <i>millions</i> )   | 81.92   | 82.47                   | 0.7%   | 58.03              | 61.00       | 5.1%  | 58.16                       | 60.24   | 3.6%  | 18.31   | 20.40        | 11.4% |
| GDP ( <i>nominal, billion USD</i> )                                    | 1,892   | 2,588                   | 36.8%  | 1,243              | 1,869       | 50.4% | 1,220                       | 1,969   | 61.4% | 412     | 697          | 69.0% |
| GDP per capita ( <i>nominal, USD</i> )                                 | 23,098  | 31,380                  | 35.9%  | 20,845             | 29,759      | 42.8% | 20,977                      | 32,695  | 55.9% | 21,600  | 32,586       | 50.9% |
| Output share of manufacturing <sup>b</sup>                             | 32.5%   | 35.2%                   | 2.7pp  | 28.4%              | 25.6%       | 2.8pp | 27.9%                       | 19.1%   | 8.8pp | 28.0%   | 28.9%        | 0.9pp |
| GDP share of trade   | 24.4%   | 38.4%                   | 14.0pp | 22.4%              | 26.5%       | 4.1pp | 29.3%                       | 28.1%   | 1.2pp | 19.3%   | 21.0%        | 1.7pp |
| Unemployment rate  | 8.7%    | 10.6%                   | 1.9pp  | 11.5%              | 9.3%        | 2.2pp | 7.9%                        | 4.8%    | 3.1pp | 8.2%    | 5.1%         | 3.1pp |
| Hourly compensation in manufacturing <sup>b</sup> ( <i>nom., EUR</i> ) | 24.60   | 30.00                   | 22.0%  | 20.50              | 27.29       | 33.1% | 16.96                       | 26.81   | 36.7% | 17.52   | 23.76        | 35.6% |
| Average hours worked, all sectors                                      | 1518    | 1435                    | 5.5%   | 1655               | 1559        | 5.8%  | 1742                        | 1676    | 3.8%  | 1799    | 1732         | 3.7%  |
| Dominant level(s) of wage bargaining <sup>c</sup>                      |         | Sector                  |        | Sector and Company |             |       |                             | Company |       |         | Sector       |       |
| Union density <sup>d</sup>   | 27.8%   | 22.6% (2003)            | 5.2pp  | 8.3%               | 8.3% (2003) | -     | 31.7%                       | 29.3%   | 2.4pp | 40.1%   | 35.4% (2002) | 4.7pp |
| Collective bargaining coverage <sup>d</sup>                            |         | 70% (West) / 54% (East) |        | 90%                |             |       |                             | <40%    |       |         | ≈98%         |       |

Notes: Unless indicated otherwise, all information is taken from the *OECD.Stat* database.

(a) Monetary values for the UK are converted with the average annual foreign exchange rate from the year 2005.

(b) Source: EU KLEMS Growth and Productivity Accounts, November 2009.

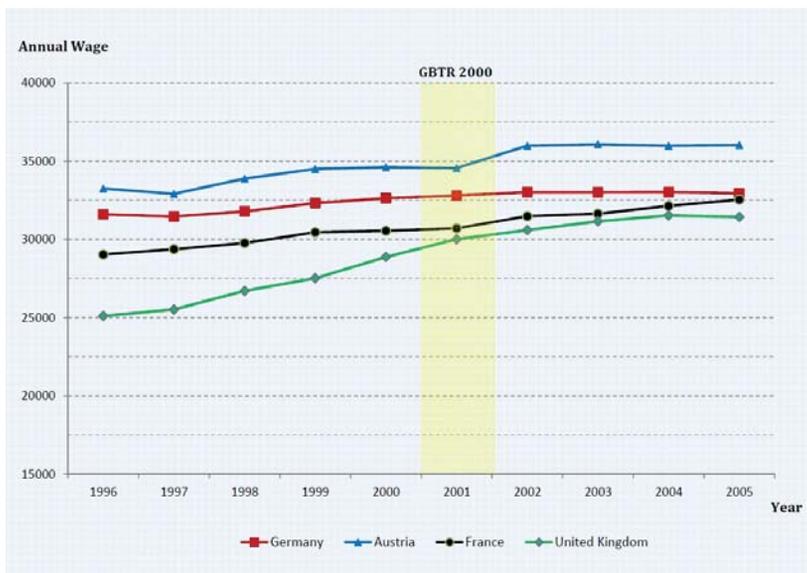
(c) Source: EIRC (2005), "Changes in national collective bargaining systems since 1990," Dublin.

(d) Source: Visser (2006), "Union membership statistics in 24 countries," Monthly Labor Review (January).

Table 1 shows that there is no clear “winner” who comes close to the ideal of being an economic twin of Germany. Every “candidate” looks quite good in some dimensions and rather bad in others. However, as said before, not all dimensions have the same critical importance in the given context: in our regression DiD approach, we should be able to control for deviations in the macroeconomic evolution over time through country-specific and time-varying controls like GDP or the unemployment rate.

If we focus on the more structural attributes and their change over time (displayed in the respective  $\Delta$  columns either as a percentage change for base values in levels or as a change in percentage points, if base values are already percentages), France and Austria seem to be better suited as a comparison country than the United Kingdom.

Figure 3: Average Wages in Manufacturing, 1996-2005, for Germany, France, Austria and the United Kingdom



*Source:* OECD.Stat for the data, own illustration.

*Note:* Average wages are in national currencies and 2011 constant prices.

In the third step of our selection procedure, we took a closer look at the evolution of average manufacturing sector wages in Austria, France and the UK to gauge their respec-

tive suitability as comparison country. Figure 3 shows a noticeable increase in Austria from the year 2001 to 2002, i.e. in parallel to the coming into effect of GBTR 2000, thereby rendering the country less suitable as a possible source for the required counterfactual company sample. France and the UK both display a virtually constant upward trend. Since the gradient is much greater in the UK, France appears to be a more suitable comparison country in this respect.

Synthesizing the insights from the first three steps of our selection procedure, we choose France as our comparison country: first, the important features of its corporate tax system remain fairly constant during the period of study; second, it is similar to Germany in many relevant aspects; and third, the evolution of the average wage in the manufacturing sector seems to be steady enough, with a constant trend before and after the coming into effect of GBTR 2000. Overall, the country should fulfill the necessary conditions to serve as an adequate source for the required counterfactual firm sample. Last but not least, the data quality for France is among the best in the Amadeus accounting data base. Nevertheless, as part of our robustness checks, we also performed a DiD analysis with companies from the UK serving as the comparison group.

### 3.3 Data

Our empirical analysis is based on the pan-European database AMADEUS compiled by the Bureau van Dijk (BvD). The dataset contains detailed accounting information on more than 10 million companies from 41 countries, including the EU countries and Eastern Europe. A standard company report includes 24 balance sheet items, 26 ratios, 25 profit and loss items and descriptive information including trade descriptions and activity codes.

Our empirical approach requires a large number of observations for at least four subsequent years in both periods, i.e., before and after the implementation of GBTR 2000. Trying to fill existing gaps in individual updates of the Amadeus database as far as possible, we merged the updates #136 (January 2006) and #168 (January 2008) to cover consistently the time span from 1996 to 2005. We limit our sample to the manufacturing industry to improve the comparability between the firms from Germany and France. Furthermore,

we only consider quoted companies since they exhibit more comprehensive and complete information than non-quoted firms, especially in the case of Germany.<sup>8</sup>

Following Arulampalam et al. (2012), we only selected companies that are not defined as “micro” by European Commission (2003a), that is companies with at least two subsequent years of recorded total assets bigger than two million Euros and at least one employee. As usual, observations with obvious errors (like negative values for fixed assets, turnover or total costs of employees) were dropped. Finally, all observations in the first and 99th percentile of the distribution for the main variables have been removed. The resulting sample contains information on 208 firms in Germany and 201 firms in France. Table 2 displays the observations per year in the manufacturing sectors of Germany and France.

Table 2: Observations per Year

| Year  | Germany | France | Total |
|-------|---------|--------|-------|
| 1996  | 208     | 199    | 407   |
| 1997  | 207     | 200    | 407   |
| 1998  | 206     | 199    | 405   |
| 1999  | 205     | 199    | 404   |
| 2000  | 206     | 200    | 406   |
| 2001  | 205     | 200    | 405   |
| 2002  | 206     | 201    | 407   |
| 2003  | 203     | 201    | 404   |
| 2004  | 203     | 199    | 402   |
| 2005  | 204     | 200    | 404   |
| Total | 2,053   | 1,998  | 4,041 |

*Note:* Data from Amadeus (BvD),  
Updates #136 and #168.

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<sup>8</sup>In France, publication requirements in the form of a yearly balance sheet submission at a public registry also apply to unlisted companies of moderate size, whereas in Germany, comparable mandatory reporting requirements only apply to quoted corporations.

Turning to the variables used in our regression framework, we proxy the annual **wage**  $w_{it}$  by the annual average company wage, calculated by dividing the Amadeus variable “costs of employees” by the “**number of employees**”. A potential distortion of using this very rough proxy measure for the wages actually paid is that a layoff of workers might, given unchanged overhead costs in the short-run, lead to an artificial rise of our wage rate.<sup>9</sup>

In addition, the base variable “Total costs of employees” for the wage rate also includes social security contributions that could be affected by legal changes independent of GBTR2000 and thereby invalidate our identification strategy. Unfortunately, the Amadeus database does not provide a wage measure that is disaggregated further into its single components. To interpret our later regression results in a strict sense as a pure wage effect, we thus have to assume that social security contributions in both countries remained fairly constant over time.<sup>10</sup>

The **value added per employee** is calculated by dividing the Amadeus variable “Value added” by the total number of employees. The macroeconomic variables **national rate of unemployment** and **national rate of GDP growth** are taken from the OECD database, likewise the harmonised consumer price index that was used to deflate all monetary variables with 2005 being the base year.

Table 3 provides an overview of the summary statistics of our final dataset for Germany and France, including additional information on the **tax liability per employee**, calculated by dividing the tax item recorded in the profit and loss statement by the number of employees, as well as on the **operating profit per employee** and **operating turnover per employee** that are constructed in the same way.

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<sup>9</sup>To control at least in a rudimentary way for the possibility of such an employment effect, we included the annual number of employees in a later robustness check of our benchmark specification, but its estimated coefficient was very small and not significant.

<sup>10</sup>In fact, data from the OECD tax database suggests that this assumption is not too farfetched for the time period under consideration, see OECD (2013).

Table 3: Descriptive Statistics for Germany and France, Manufacturing Sector

|                                 | Germany |        |        | France |        |        |
|---------------------------------|---------|--------|--------|--------|--------|--------|
|                                 | Mean    | Median | S.D.   | Mean   | Median | S.D.   |
| Wage Rate                       | 48.82   | 48.71  | 15.70  | 42.54  | 40.01  | 16.31  |
| Number of Employees             | 13,704  | 1,020  | 52,984 | 3,278  | 311    | 13,588 |
| Value added per Employee        | 73.82   | 67.29  | 61.19  | 67.43  | 55.93  | 62.81  |
| Tax Liability per Employee      | 5.17    | 2.77   | 10.92  | 5.17   | 2.21   | 11.21  |
| Operating Profit per Employee   | 12.44   | 8.99   | 51.65  | 14.95  | 9.01   | 34.95  |
| Operating Turnover per Employee | 237.73  | 181.78 | 279.61 | 208.39 | 162.85 | 176.27 |
| National Rate of Unemployment   | 8.87    | 8.70   | 0.92   | 9.77   | 9.20   | 1.13   |
| National Rate of GDP Growth     | 1.29    | 1.40   | 1.02   | 2.22   | 1.90   | 1.02   |

*Notes:* Data from Amadeus (BvD), Updates #136 and #168. All monetary values in EUR, deflated to year 2005.

## 4 Empirical Analysis

The following subsections introduce the econometric model (4.1), present the results of our baseline DiD regression (4.2) and discuss issues of robustness (4.3).

### 4.1 Econometric Model

Since we want to evaluate the direct incidence effect of GBTR 2000 as a policy intervention by means of a regression DiD approach, the annual tax liability  $\bar{\phi}$  (see equation 1) from the original framework drops out of this reduced form policy evaluation.<sup>11</sup> Building upon the basic specification from aus dem Moore (2014), our preferred specification for the implementation of our regression DID approach reads as follows:

$$\begin{aligned} \ln w_{it} &= \beta_{10} \ln w_{i,t-1} + \beta_{11} \ln w_{i,t-2} \\ &+ \beta_{20} \ln av_{i,t} + \beta_{21} \ln av_{i,t-1} + \beta_{22} \ln av_{i,t-2} \\ (2) \quad &+ \sigma_{01} unempl_{j,t} + \sigma_{02} gdp_{j,t} \\ &+ \gamma_{01} treat_j + \gamma_{02} period_t + \delta DiD_{j,t} \\ &+ \lambda_{01} year_t + \lambda_{02} \mu_i + \epsilon_{i,t} \end{aligned}$$

where  $i$ ,  $j$  and  $t$  index companies, countries and years respectively and  $\ln w_{it}$  is the log wage rate. By including the per capita level of value added in logs ( $av_{i,t}$ ) in equation 2, we not only control for changes in the business situation of the firm, but also for possible indirect incidence effects of corporate income taxation that would materialize in the value added.

Under the assumption that firms earn economic rents over which employers and employees may bargain, the extent of the tax effect on the wage rate essentially depends on the bargaining power of both negotiating partners. By adding a country-specific and time-

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<sup>11</sup>Concretely, our aim here is not to estimate the corporate tax elasticity of wages in the manufacturing sector as it was done in aus dem Moore (2014) by regressing annual wages on annual tax liabilities at the firm-level, but to analyze whether GBTR 2000 as an important corporate tax policy reform had a seizable effect on wages in the manufacturing sector. Obviously, the identification of a presumptive wage effect of GBTR 2000 requires that annual tax liabilities are not explicitly modelled in the DiD regression.

varying unemployment rate ( $unempl_{j,t}$ ) as well as the country-specific and time-varying rate of GDP growth ( $gdp_{j,t}$ ) in our estimation model, we try to capture important factors for the bargaining power of employers and employees at least to some extent. In the context of the bargaining model, we assume that higher GDP growth provides for more leeway for wage gains, but that the assertiveness of the union declines with higher unemployment rates.

The DiD approach is implemented in the fourth row of equation 2:  $Germany_j$  is a dummy variable that indicates the treatment group in our quasi-experimental setting. Thus,  $Germany_j$  equals “1” if the firm is located in Germany and “0” for French companies. The time period dummy  $After2000_t$  is an auxiliary variable that is “1” if the respective observation falls in the post reform period (2001-2005) and “0” if it is located in the time span before GBTR 2000 (1996-2000). We follow the standard DiD approach by defining  $DiD_{j,t}$  as the product of  $Germany$  and  $After2000$ :  $DiD_{j,t} = Germany_j \times After2000_t$ . Therefore,  $DiD_{j,t}$  is “1” for German companies in the post reform period and “0” otherwise. By using year dummies  $year_t$  we account for general time effects.<sup>12</sup> Additionally, we include the vector  $\mu_i$  to capture the company-specific time-invariant effects. The overall error term is represented by  $\epsilon_{i,t}$ . In order to adjust for inflation, all monetary variables in equation 2 are deflated to year 2005 prices by using the harmonised country- and year-specific consumer price indices provided by the OECD.

In our research design, the correct interpretation of  $\delta$ , the tax reform evaluation parameter of interest, is not trivial due to the integration of the DiD technique into a dynamic panel model. When considered individually, the estimate of the coefficient  $\delta$  states the direct incidence effect on wages of GBTR 2000 only with respect to the first year after the reform, i.e. in 2001, but not for the entire evaluation period from 2001 to 2005. Table 4 shows the evolution of the cumulative effect over time. The formulae for the respective cumulative incidence effects in the years from 2001 to 2005, displayed in the notation of equation 2, result as the difference of the predicted values for Germany and France in the respective years (See the appendix for the explicit derivation of these formulae).

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<sup>12</sup>Due to the inclusion of year dummies, the  $After2000$  dummy will drop out of the later estimations for reasons of multicollinearity. The interaction term  $DiD_{j,t}$  is unimpaired from this technical effect.

Table 4: Formulae for Dynamic DiD Effects from 2001 to 2005

| Year | Effect Formula   |
|------|--|
| 2001 | $\delta$   |
| 2002 | $\delta (1 + \beta_{10})$  |
| 2003 | $\delta (1 + \beta_{10} + \beta_{10}^2 + \beta_{11})$  |
| 2004 | $\delta (1 + \beta_{10} + \beta_{10}^2 + \beta_{10}^3 + \beta_{11} + \beta_{11}^2)$  |
| 2005 | $\delta (1 + \beta_{10} + \beta_{10}^2 + \beta_{10}^3 + \beta_{10}^4 + \beta_{11} + \beta_{11}^2 + \beta_{11} + \beta_{11}^3)$ |

## 4.2 Estimation Results

We estimate our preferred dynamic specification (2) by means of four different estimation techniques: We run pooled Ordinary Least Squares (OLS) and Fixed Effects (FE) as well as the “Difference” and “System” versions of the Generalized Methods of Moments (GMM) estimator. As commonly known, OLS and FE estimates are inconsistent in a dynamic context with a lagged dependent variable,<sup>13</sup> but they enable us to check the validity of the GMM estimations and the plausibility of our results: Where OLS overestimates the coefficients of the lagged dependent variable  $\ln w_{i,t-1}$ , FE leads to underestimation. This implies that credible GMM results should fall between these bounds (cf. Bond 2002, 3-5). Table 5 summarizes the results of our benchmark estimations.

The results in table 5 show that only the “System” GMM estimation leads to an estimate of the coefficient of the lagged dependent variable  $\ln w_{i,t-1}$  of 0.39 that lies between the upward-biased OLS estimate (0.52) and the downward-biased FE estimate (0.11). The diagnostic test results for this estimation are also satisfying: The AR(1)-test for first order serial correlation is rejected, but the AR(2)-test for second order serial correlation is not. This means that a crucial condition for the applicability of the GMM estimator, namely the absence of serial correlation in the errors of the levels equation, is fulfilled. Likewise, Hansen’s  $\chi^2$ -test for over-identification is clearly not rejected. We can therefore concentrate our interpretation on the “System” GMM results.

<sup>13</sup>See the discussion in aus dem Moore (2014) and the references given therein.

Table 5: DiD Analysis of GBTR 2000, Benchmark Results

| Dependent Variable:             | OLS                    | FE                    | GMM                   | GMM                    |
|---------------------------------|------------------------|-----------------------|-----------------------|------------------------|
| Wage (in logs)                  |                        |                       | (Difference)          | (System)               |
| Wage ( <i>first lag</i> )       | 0.524***<br>(0.0430)   | 0.106**<br>(0.0510)   | 0.101<br>(0.0705)     | 0.388***<br>(0.0530)   |
| ( <i>second lag</i> )           | 0.212***<br>(0.0345)   | -0.168***<br>(0.0537) | -0.102*<br>(0.0589)   | 0.0604<br>(0.0418)     |
| Value added                     | 0.428***<br>(0.0606)   | 0.411***<br>(0.0551)  | 0.268***<br>(0.0876)  | 0.428***<br>(0.0922)   |
| ( <i>first lag</i> )            | -0.199***<br>(0.0503)  | -0.0567<br>(0.0476)   | -0.0791<br>(0.0519)   | -0.158***<br>(0.0554)  |
| ( <i>second lag</i> )           | -0.0767***<br>(0.0248) | 0.0643*<br>(0.0345)   | 0.0439<br>(0.0326)    | -0.00978<br>(0.0232)   |
| treat ( <i>Dummy</i> )          | -0.0719<br>(0.0498)    |                       |                       | -0.0441<br>(0.0351)    |
| DiD                             | 0.0555<br>(0.0427)     | 0.0666*<br>(0.0389)   | 0.0553*<br>(0.0327)   | 0.0790***<br>(0.0305)  |
| Unemployment Rate               | -0.0276*<br>(0.0151)   | -0.0291**<br>(0.0145) | -0.0378**<br>(0.0163) | -0.0340***<br>(0.0117) |
| GDP Growth Rate                 | -0.0397*<br>(0.0216)   | -0.0289<br>(0.0211)   | -0.00907<br>(0.0210)  | -0.0250<br>(0.0172)    |
| $R^2$                           | 0.766                  | 0.403                 |                       |                        |
| Observations                    | 1,468                  | 1,468                 | 1,147                 | 1,468                  |
| Groups                          |                        | 285                   | 262                   | 285                    |
| Instruments                     |                        |                       | 80                    | 124                    |
| AR(1)-test $\tilde{U}$ p-value  |                        |                       | 0.000                 | 0.000                  |
| AR(2)-test $\tilde{U}$ p-value  |                        |                       | 0.407                 | 0.173                  |
| Hansen $\chi^2$ -test - p-value |                        |                       | 0.557                 | 0.382                  |

*Notes:* (i) Year dummies and a constant term are included in all estimates. (ii) The standard errors are reported in parentheses. (iii) \* Significant at 10%, \*\* at 5%, \*\*\* at 1%. (vi) Columns (3) and (4) show results of two-step estimators with finite sample correction.

As expected, the coefficients for the lagged wage rate and for value added per worker are highly significant at the one percent level, and of considerable but not implausible size (0.39 and 0.44 respectively). Although it is only measured at the country-level, the unemployment rate has a significant and, as expected, negative effect on the wage rate (-0.03). In contrast, the GDP growth rate is not significant (p-value of 0.148). Given that the business cycle situation should be captured to a certain degree by the inclusion of year effects and, even more important, be reflected in the firm-specific value added,

the non-significance of GDP is not surprising. Overall, our parsimonious model seems to capture quite well the most important factors that influence wages at the firm-level.

Turning to our DiD framework, we note the non-significance of the country dummy *Germany* (p-value of 0.21). Hence, after the inclusion of the present covariates and control variables at firm- and country-level, there were obviously no discernible differences left between German and French companies that showed up in a statistically significant manner over the time-span from 1996 to 2005.

Finally, the coefficient value of 0.079 for the interaction term  $DiD_{j,t}$  is highly significant at the 1 percent level. Following the formulae depicted in table 4, the results for the cumulative effects in the years 2002 to 2005 were calculated as a nonlinear combination of estimates according to the delta method.<sup>14</sup> The summarized results in table 6 show how the size of the respective cumulative effect gradually builds up over time, with larger increments in the first three years (2001 to 2003) and considerably smaller increments in the years four and five (2004 and 2005). In economic terms, our DiD evaluation of GBTR 2000 implies a short-run effect, i.e., in year one after the reform, of increasing wages by 7.9 percent and a long-run effect, i.e. in year five after the reform, of increasing wages by 13.3 percent.

Table 6: Cumulative DiD-Effects, estimates over time from 2001 to 2005

| Cumulative DiD Effect       | Coefficient | Std. Err. | z    | P> z  | [95% Conf. Interval] |
|-----------------------------|-------------|-----------|------|-------|----------------------|
| Dependent Variable:<br>Wage |             |           |      |       |                      |
| Effect 2001                 | .0789599    | .0304766  | 2.59 | 0.010 | .019227 .1386929     |
| Cum. Effect 2002            | .1095838    | .0427296  | 2.56 | 0.010 | .0258354 .1933322    |
| Cum. Effect 2003            | .1262273    | .0488513  | 2.58 | 0.010 | .0304806 .221974     |
| Cum. Effect 2004            | .1311215    | .0510607  | 2.57 | 0.010 | .0310443 .2311986    |
| Cum. Effect 2005            | .1329254    | .0519837  | 2.56 | 0.011 | .0310392 .2348117    |

<sup>14</sup>We used the Stata command *nlcom* to implement the delta method.

### 4.3 Robustness

To check the sensitivity of our baseline result, we first experimented with alternatives to the dynamic structure and set of control variables of our empirical model. Secondly, we tested different instrument sets in the implementation of the “System” GMM technique. Finally, we also applied the simple aggregation and averaging procedure proposed by Bertrand, Duflo and Mullainathan (2004).

First, we inspected the robustness of our baseline results by implementing a more parsimonious specification, i.e., with only one lagged value of wage rate and value added. The ensuing results for the “System” GMM estimation are displayed in column (1) of table 7.<sup>15</sup> The central variables still display significant coefficients that are in most cases of the expected sign, with the notable exception of, as before in our baseline estimation, the first lag of value added. The policy evaluation parameter of interest, DiD, has now a coefficient size that corresponds to an immediate wage effect of 5.5 percent. However, its significance dropped from the one percent to the ten percent level.

The second column of table 7 shows the results from a specification without any lags for the value added that we estimated for two reasons: First, we do not need the respective coefficients of the lags of value added for the calculation of the direct incidence effect over time (see above). Second, the dubious negative sign of the first lag in our baseline results and in column one of table 7 suggests that a closer inspection of the way in which the value added is included in the estimation is warranted. The results in column (2) confirm, especially when compared to column (1), that the lags of value added are not the decisive driver of our results for the parameter DiD. Its coefficient size now implies a rise of the wage rate of 6.1 percent. The significance is unchanged at the ten percent level.

Finally, in column three of table 7, we document the result of a specification that differs from the baseline model only by the absence of the GDP growth rate that turned out to be insignificant in table 5. As expected, the impact of dropping the GDP growth rate is quite small, but nevertheless visible: The coefficient size of DiD is reduced from 7.9 percent in the benchmark specification to 7.0 percent, its level of significance drops

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<sup>15</sup>The full results including OLS, FE and “Difference” GMM are available upon request.

from the one to the five percent level. We also carried out estimations that included further control variables which one might assume to have an impact on the tax liability of a firm, like its debt ratio (as a proxy for the tax-shield effect from deductible costs of debt financing) or its capital intensity (as a proxy for likely tax savings due to allowances for capital investments), but none of them was found to be significant.<sup>16</sup> Overall, the three robustness checks that are incorporated in table 7 suggest that the existence of a positive wage effect of GBTR 2000 via the bargaining channel is qualitatively quite robust, but quantitatively rather sensitive to alternative specifications.

One explanation for the differences in the coefficient size and significance of DiD seems to be that alternatives in the dynamic structure lead to visible differences in the number of firms and observations that are included in the respective estimation. As a specification choice gets more demanding with respect to the number of spells without gaps, the sample of firms that fulfill this standard becomes ever smaller. Our benchmark estimation is based on 1,468 observations from 285 different firms, whereas the specification with only one lagged value of wage rate and value added in column (1) of table 7 uses 1,874 observations from 326 firms. Obviously, we face a trade-off between quantity and quality of the data included in the DiD estimations. If sample size is reduced as a side effect of a more demanding specification, the coefficient size and the precision of its estimation grow. An economic explanation for this observation might be that in our dataset, companies with fewer or no gaps in their spells are at the same time more profitable, and hence provide the basis for a larger tax incidence effect via the wage bargaining channel.

A potential caveat in our study (as in almost every implementation of the “System” GMM technique) is the choice of the instrumental variables, especially the risk of obtaining significant results as a consequence of instrument proliferation (see Roodman 2009b). However, in the case of our benchmark specification the imperative to reduce the size or the composition of the instrument set seems not to be present: given a favorable ratio of 124 instruments used in an estimation with 285 groups (firms), our results should not be driven by instrument inflation. Nevertheless, we checked the respective robustness of our

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<sup>16</sup>Results are available upon request.

Table 7: Alternative DiD Specifications

|                                 | (1)                   | (2)                      | (3)                   |
|---------------------------------|-----------------------|--------------------------|-----------------------|
| Dependent Variable:             | “One lag”             | “No lags of value added” | “No GDP”              |
| Wage (in logs)                  | GMM<br>(System)       | GMM<br>(System)          | GMM<br>(System)       |
| Wage ( <i>first lag</i> )       | 0.339***<br>(0.0567)  | 0.276***<br>(0.0609)     | 0.388***<br>(0.0535)  |
| ( <i>second lag</i> )           |                       | 0.0622**<br>(0.0306)     | 0.0590<br>(0.0414)    |
| Value added                     | 0.292***<br>(0.0917)  | 0.248***<br>(0.0662)     | 0.433***<br>(0.0910)  |
| ( <i>first lag</i> )            | -0.0812*<br>(0.0463)  |                          | -0.159***<br>(0.0545) |
| ( <i>second lag</i> )           |                       |                          | -0.00906<br>(0.0236)  |
| treat ( <i>Dummy</i> )          | 0.0211<br>(0.0369)    | -0.0321<br>(0.0391)      | -0.00676<br>(0.0308)  |
| period ( <i>Dummy</i> )         | -0.0246<br>(0.0345)   |                          | -0.0242<br>(0.0198)   |
| DiD                             | 0.0554*<br>(0.0294)   | 0.0606*<br>(0.0314)      | 0.0698**<br>(0.0320)  |
| Unemployment Rate               | -0.0210*<br>(0.0115)  | -0.0302**<br>(0.0122)    | -0.0311**<br>(0.0122) |
| GDP Growth Rate                 | -0.00573<br>(0.00941) | -0.0307<br>(0.0188)      |                       |
| Observations                    | 1874                  | 1571                     | 1468                  |
| Groups                          | 326                   | 302                      | 285                   |
| Instruments                     | 85                    | 124                      | 123                   |
| AR(1)-test $\tilde{U}$ p-value  | 0.000                 | 0.001                    | 0.000                 |
| AR(2)-test $\tilde{U}$ p-value  | 0.450                 | 0.198                    | 0.174                 |
| Hansen $\chi^2$ -test - p-value | 0.287                 | 0.322                    | 0.390                 |

*Notes:* (i) Year dummies and a constant term are included in all estimates. (ii) The standard errors are reported in parentheses. (iii) \* Significant at 10%, \*\* at 5%, \*\*\* at 1%. (vi) Columns (3) and (4) show results of two-step estimators with finite sample correction.

baseline results through a stepwise reduction in the number of instruments used. Table 8 documents the results obtained.

For ease of comparability, column (1) repeats our benchmark specification that contained the second and all available higher lags of the wage rate and value added as GMM instru-

ments.<sup>17</sup> In column (2), the instrument count is reduced to 104 by the restriction to use only the second and third lags as GMM instruments (2 3). With respect to the results, two changes are noteworthy: First, the size of the DiD coefficient is reduced to 6.6 percent and its significance drops to the 5 percent level. Second, the previously insignificant coefficient of the GDP growth rate is now significant at the 10 percent level, but with the “wrong” sign: there is no economic reasoning that provides an explanation why the wage rate should decline in times of higher GDP growth rates.

The specification reported in column (3) pushes the reduction of the instrument set one step further by restricting it only to the second lag (2 2). The number of instruments drops slightly from 104 to 94, but the estimation results stay virtually unchanged in comparison to column (2). Finally, we enforce the use of a drastically reduced number of instruments by collapsing the instrument matrix. While the numerical size of the DiD coefficient in column (4) remains virtually unchanged compared to column (3), its significance now vanishes completely. At the same time, the wrongly signed first lag of value added loses its significance in contrast to all previous estimations. On the other hand, the absolute size of the negatively signed GDP coefficient grows considerably from -0.027 to -0.046.

To sum up, we note that our benchmark estimation is quantitatively and qualitatively very robust to a considerable reduction of the number of instruments from 124 to 94, but that collapsing the instrument matrix leads not only to a very small instrument count of 24 in an estimation with 285 groups, but also to the disappearance of the significance of the coefficient estimate for the policy evaluation variable DiD. However, we believe that the “collapse” option replaces one evil with another, i.e., while lowering the risk of using too many instruments, it enforces the use of too few.

If we ignore the time-series information completely by splitting and averaging the data in two periods, before and after the coming into effect of GBTR 2000, and then run a static OLS regression on the resulting panel of length two as advocated by Bertrand et al. (2004, 267), all coefficients except the one for value added turn insignificant. This result does of

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<sup>17</sup>Abbreviated here with the respective notation from the Stata command *xtabond2* as (2 .) in the row “Instrument lag structure”: The leading (2) denotes that the second lag is used as the first instrument, the following (.) implies that all higher lags are, if available in the data, used to generate further instruments, see Roodman 2009a for details.

Table 8: Alternative Specifications of GMM Instruments

|                                 | (1)                    | (2)                   | (3)                   | (4)                   |
|---------------------------------|------------------------|-----------------------|-----------------------|-----------------------|
| Instrument lag structure        | GMM Lags (2.)          | GMM Lags (2 3)        | GMM-Lags (2 2)        | “Collapse”            |
| Dependent Variable:             | GMM                    | GMM                   | GMM                   | GMM                   |
| Wage                            | (System)               | (System)              | (System)              | (System)              |
| Wage ( <i>first Lag</i> )       | 0.388***<br>(0.0530)   | 0.381***<br>(0.0550)  | 0.387***<br>(0.0554)  | 0.241***<br>(0.0921)  |
| ( <i>second Lag</i> )           | 0.0604<br>(0.0418)     | 0.0665<br>(0.0420)    | 0.0661<br>(0.0427)    | -0.0199<br>(0.0655)   |
| Value added                     | 0.428***<br>(0.0922)   | 0.418***<br>(0.0884)  | 0.422***<br>(0.0953)  | 0.480**<br>(0.211)    |
| ( <i>first Lag</i> )            | -0.158***<br>(0.0554)  | -0.148***<br>(0.0549) | -0.150***<br>(0.0579) | -0.131<br>(0.0932)    |
| ( <i>second Lag</i> )           | -0.00978<br>(0.0232)   | -0.00822<br>(0.0241)  | -0.00633<br>(0.0240)  | 0.0247<br>(0.0396)    |
| treat ( <i>Dummy</i> )          | -0.0441<br>(0.0351)    | -0.0380<br>(0.0353)   | -0.0358<br>(0.0354)   | -0.0471<br>(0.0500)   |
| DiD                             | 0.0790***<br>(0.0305)  | 0.0658**<br>(0.0293)  | 0.0650**<br>(0.0281)  | 0.0652<br>(0.0435)    |
| Unemployment Rate               | -0.0340***<br>(0.0117) | -0.0266**<br>(0.0116) | -0.0254**<br>(0.0113) | -0.0368**<br>(0.0165) |
| GDP Growth Rate                 | -0.0250<br>(0.0172)    | -0.0308*<br>(0.0157)  | -0.0274*<br>(0.0150)  | -0.0460**<br>(0.0214) |
| Observations                    | 1468                   | 1468                  | 1468                  | 1468                  |
| Groups                          | 285                    | 285                   | 285                   | 285                   |
| Instruments                     | 124                    | 104                   | 94                    | 34                    |
| AR(1)-test $\tilde{U}$ p-value  | 0.000                  | 0.000                 | 0.000                 | 0.000                 |
| AR(2)-test $\tilde{U}$ p-value  | 0.173                  | 0.152                 | 0.154                 | 0.255                 |
| Hansen $\chi^2$ -test - p-value | 0.382                  | 0.325                 | 0.383                 | 0.662                 |

Notes: (i) Year dummies and a constant term are included in all estimates. (ii) The standard errors are reported in parentheses. (iii) \* Significant at 10%, \*\* at 5%, \*\*\* at 1%. (vi) Columns (3) and (4) show results of two-step estimators with finite sample correction.

course not support the claim of a significant wage effect of GBTR 2000, but it does not preclude it either since the time-series information is obviously an important information in the dynamic context of a wage bargaining model. Moreover, the fact that simple aggregate means of the central variables in the two periods before and after GBTR 2000 do not reveal by themselves a significant DiD effect, as already shown by figure 3, was the reason to set up a regression DiD framework in the first place.

## 5 Conclusion

We use the bargaining model for the direct incidence of the corporate income tax proposed by Arulampalam et al. (2012) as the basis for a difference-in-differences regression model to identify the wage effect of the German Business Tax Reform 2000 (GBTR 2000) for workers in the German manufacturing sector. We use manufacturing companies in France as the comparison group to construct the required counterfactual and estimate a dynamic model to account for adjustment lags with OLS, Fixed Effects and GMM techniques.

Our preferred “System” GMM specification suggests that GBTR 2000 led to a significant and sizeable wage effect. In 2001, the first year with GBTR 2000 in effect, we observe a significant short-run effect that implies a wage increase of 7.9 percent. Due to the dynamic nature of our model, the incidence effect grows gradually over time. At the end of the evaluation period in the year 2005, the cumulative effect reaches a value of 13.3 percent.

Our findings prove to be quite robust with respect to changes in the dynamic structure of the empirical model and to the instrument set used in the “System” GMM estimation. The effect size of our result seems quite plausible against the backdrop of recent contributions to the empirical literature on corporate tax incidence, notably Fuest et al. (2013), that underline the dominance of the direct incidence through the wage bargain over indirect effects via capital stock adjustments.

However, we are aware of the fact that to obtain these results, we had to invoke several rather strong assumptions with the respect to the firm-level data we use and the choice of France as the comparison country that would threaten the validity of our identification strategy if refuted. In the following, we briefly explore these issues.

Firstly, we had to use consolidated accounts of quoted companies to ensure the necessary density of observations for the German companies that is needed for the estimation of a dynamic model, especially if one has to employ the data consuming GMM estimation techniques. As a result, the information contained in these accounts does not only result from business activities in Germany but in case of the multinational groups in our dataset also from their international subsidiaries. However, the resulting composition effect does

not invalidate the use of the bargaining model from Arulampalam et al. (2012) as our conceptual framework, since the empirical studies of Budd and Slaughter (2004) and Budd, Konings and Slaughter (2005) provide convincing evidence that the domestic labor forces of multinational enterprises bargain over the aggregate profits at group level, not only over the domestic share of these profits. Nonetheless, it would have been a desirable refinement of our analysis to distinguish purely national from multinational companies, if the accounting data quality for Germany had allowed for an analysis on a broader basis, i.e., including also unlisted firms.<sup>18</sup>

With respect to the validity of the comparison between manufacturing companies from Germany and France, the ideal *ceteris paribus* condition of no relevant policy changes over time except for GBTR 2000 was of course not completely fulfilled in the real world. There were a number of reforms and policy changes in both countries that possibly had an impact on the evolution of wages in the manufacturing sector. Most noteworthy are, in France, the stepwise introduction of the 35h-workweek in February 2000 (for large firms) and January 2001 (for smaller companies)<sup>19</sup>, and in Germany, the enacting of the so called “Hartz” reforms of the labor market in December 2002 and December 2003 that, amongst other measures, consolidated the previously separated institutions of unemployment aid and social welfare in a single system.<sup>20</sup>

However, both reforms should principally reduce the size of the incidence effect that we obtain in our DiD regression, since the introduction of the 35h workweek tends to increase the wage sum at the level of the individual firm level in France *ceteris paribus* (due to increased overtime pay or the remuneration of additional workers), whereas the “Hartz” reforms supposedly depressed wages, at least in the low-pay sector, in Germany.<sup>21</sup>

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<sup>18</sup>However, in their multi-country analysis, Arulampalam et al. explicitly distinguish firms operating only in one country from those that are part of a multinational enterprise, but find no significant difference between the two groups with respect to the direct corporate tax incidence via the wage bargaining channel, cf. Arulampalam et al. (2012, 1050-1052).

<sup>19</sup>See Estevao and S. (2008) for details.

<sup>20</sup>See Fertig and Kluve (2004) and Jacobi and Kluve (April 2006) for details.

<sup>21</sup>Moreover, the most profound part of the comprehensive Hartz reform package, called “Hartz IV”, came not into effect until January 2005, i.e. only shortly before the end of our evaluation period and four years after the effective date of the GBTR 2000.

It therefore seems to be virtually impossible that our supposed incidence effect is a mere artefact of two reforms that have nothing to do with the GBTR 2000.

In light of these limitations, our results should be received with due caution. Nevertheless, our answer to the research question “Do wages rise if corporate taxes fall?” is a tentative “Yes, they do.”

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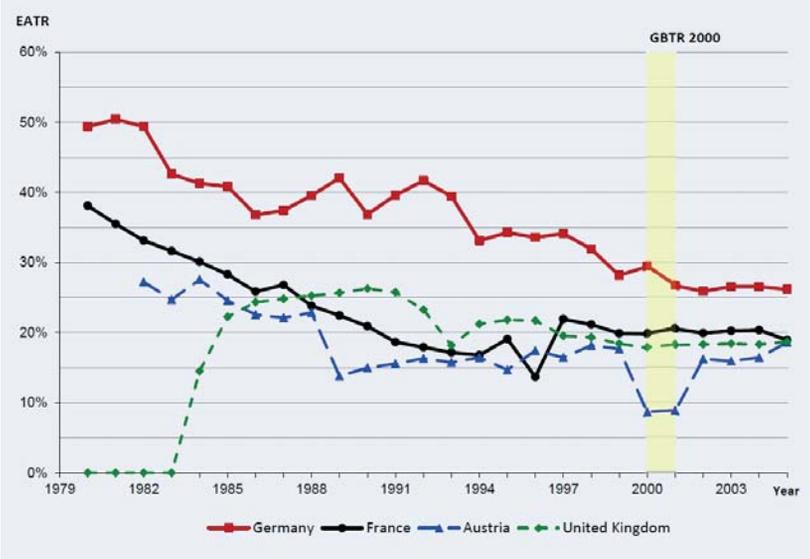
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# Appendix

Figure 4: Effective Average Tax Rates, 1979-2005, for Germany, France, Austria and the United Kingdom



Source: Devereux et al. (2002) for the data (Update 2005), own illustration.

## Calculation of Dynamic DiD Effects

Following the notation of equation 2 and ignoring the macroeconomic control variables GDP growth and unemployment rate (without any effect on the results), one obtains as the predicted value of the wage rate in the year 2001 for the treatment group from Germany (i.e.,  $treat_j = 1$ ):

$$\begin{aligned}
 E(\ln w_{2001} \mid treat_j = 1) &= \beta_{10} \ln w_{2000} + \beta_{11} \ln w_{1999} \\
 &+ \beta_{20} \ln av_{2001} + \beta_{21} \ln av_{2000} + \beta_{22} \ln av_{1999} \\
 &+ \gamma_{01} treat_j + \gamma_{02} period_t + \delta DiD_{j,t} \\
 &+ \lambda_{01} year_t + \lambda_{02} \mu_i + \epsilon_{i,t}
 \end{aligned}
 \tag{3}$$

The predicted value of the wage rate in the year 2001 for the comparison group from France (i.e.  $treat_j = 0$ ) reads as follows:

$$\begin{aligned}
 E(\ln w_{2001} \mid treat_j = 0) &= \beta_{10} \ln w_{2000} + \beta_{11} \ln w_{1999} \\
 &+ \beta_{20} \ln av_{2001} + \beta_{21} \ln av_{2000} + \beta_{22} \ln av_{1999} \\
 &+ \gamma_{01} treat_j + \gamma_{02} period_t + \delta DiD_{j,t} \\
 &+ \lambda_{01} year_t + \lambda_{02} \mu_i + \epsilon_{i,t}
 \end{aligned}
 \tag{4}$$

The difference-in-difference between the two predicted values is simply the coefficient  $\delta$  of the policy evaluation variable  $DiD$ , as illustrated (in the style of Wooldridge 2006, 459) by the following table 9.

Table 9: Illustration of DiD Estimator

|                  | Before 2000   | After 2000                           | After - Before         |
|------------------|---------------|--------------------------------------|------------------------|
| France           | 0             | $\gamma_{02}$                        | $\gamma_{02}$          |
| Germany          | $\gamma_{01}$ | $\gamma_{01} + \gamma_{02} + \delta$ | $\gamma_{02} + \delta$ |
| Germany - France | $\gamma_{01}$ | $\gamma_{01} + \delta$               | $\delta$               |

Likewise, the respective DiD effects for subsequent years of the evaluation period are obtained by subtracting the respective predicted value for France from the respective predicted value for Germany. The dynamic propagation of the initial effect  $\delta$  takes place via  $\beta_{10}$  and  $\beta_{11}$  (see table 4).