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Chapter 1

Introduction

> *"Labour was the first price, the original purchase – money that was paid for all things. It was not by gold or by silver, but by labour, that all wealth of the world was originally purchased."*
>
> Adam Smith, 1776, *The Wealth of Nations*, p. 34

According to Adam Smith, labor is at the root of all economic activity, the offspring of all consumption and the original source of all wealth that mankind has accumulated. Today, almost 250 years later, Smith's assessment is still true: the labor market is still pivotal in the lives of a vast majority of people. Although the labor share of total income has been declining over the past 30 years, it is still mostly labor that enables individuals to purchase goods and/or to invest in capital: labor earnings still make up 65%-80% of total income (Gollin, 2002; Karabarbounis and Neiman, 2012). The importance of labor income goes beyond pure purchasing power. The distribution of earnings significantly affects people's lives as well. Individuals are "creatures of comparison" (Oswald, 2010). They compare themselves and their incomes to people in their peer group (see, e.g., Luttmer, 2005; Clark, Frijters, and Shields, 2008); they form preferences for a fair overall distribution of incomes (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000); and they often take strong normative views on distributional issues (Atkinson and Bourguignon, 2000). It is therefore not surprising that the rise in top incomes over the past years (see, e.g., Atkinson and Piketty, 2007) has spurred on political movements like "Occupy Wallstreet" and has initiated general debates about the adequacy of the pay of top earners.

Besides the price of labor and its distribution, people also care about the quantity of labor, thus employment. Individuals devote a significant share of their lifetime to working (Robinson and Godbey, 1997). In Western countries a large majority of the employable population works: while men have always worked full-time, female labor force participation has increased strongly over the past decades (Blundell, Bozio, and Laroque, 2011). Acknowledging the central role of employment, it is not surprising that being unemployed and thus excluded from the labor market has a negative effect on individuals' well-being. In fact, this negative effect has been found to even exceed the costs of foregone earnings (Winkelmann and Winkelmann, 1998; Di Tella, MacCulloch, and Oswald, 2001; Clark, 2003). This additional burden is often attributed to the implicit costs of social exclusion. Moreover, unemployment has been found to have a significant and persistent scarring effect (Arulampalam, 2001), implying that longer spells of past unemployment are associated with longer spells of future joblessness. This effect is usually explained by stigma in the sense that firms interpret past unemployment as a signal of lower productivity of a worker (see, e.g., Gibbons and Katz, 1991; Omori, 1997; Oberholzer-Gee, 2008).

Given the apparent importance of labor, it is crucial to understand how decisions of policymakers affect labor markets. Governments take an active role in shaping the labor market outcomes and thus have an important impact on the lives of their citizens. Sound policymaking can improve sub-optimal outcomes by correcting market failures or taking back wrong policy decisions of the past. On the contrary, poor policymaking either due to ignorance or the non-benevolence of policymakers (Nordhaus, 1975) can have large negative economic – and in the long-run even democratic – consequences.

Against this backdrop, this dissertation aims at improving the understanding of how public policies affect labor markets by evaluating five specific government interventions and looking at their labor market outcomes.

1.1 Chapters and research questions

In one way or another most policy evaluation studies investigate the optimality of a policy in terms of efficiency and/or equity. This broad characterization also applies to the five specific studies that are compiled in this dissertation. In the following I will introduce the topics of the studies and state the central research questions raised.

Chapter 2: Employment effects of a workfare reform[1]

Chapter 2 deals with the effects of workfare, a policy to abolish the disincentive effects induced by high welfare benefits. In the standard neoclassical model high welfare benefits lead to disincentive labor supply effects because people with a low wage will not participate in the labor market and prefer to accept welfare benefits instead. This is especially true, as the net income through benefits comes with a maximum amount of leisure and thus a very high implicit hourly wage. Thus, the presence of welfare benefits undermines the consumption/leisure trade-off, which ensures an efficient allocation of labor in the absence of any government intervention. This problem has motivated a policy proposal called "workfare" that makes welfare benefit receipt conditional on a work requirement (e.g. doing community service). In Chapter 2, we raise the research question: *What is the employment effect of introducing workfare in Germany?* In answering this question, we also make a methodological contribution by extending standard structural labor supply models to account for labor demand effects and thereby identifying the partial labor market equilibrium.

Chapter 3: Wage effects of corporate taxation[2]

In Chapter 3 we look at the wage effects of corporate profit taxes. Companies, being legal constructs, can be taxed but cannot bear the burden of corporate taxation. Eventually this burden will be passed on to real individuals. It is possible that capitalists/shareholders receive lower interests/dividends if corporate tax rates increase; consumers might bear (parts of) the burden through higher product prices; employees might receive lower wages. In this chapter we focus on the last channel and address the question: *Do higher local corporate taxes reduce wages?* The answer to this question has very important implications for public policy in general and the political debate in particular since proponents of higher corporate taxation often base their postulation on distributive arguments: firm owners who are usually regarded as relatively rich compared

[1] This chapter has been published as "Accounting for Labor Demand Effects in Structural Labor Supply Models" (joint with Andreas Peichl, see Peichl and Siegloch, 2012).

[2] This chapter is based on a (so far unpublished) article entitled "Do Higher Corporate Taxes Reduce Wages? Micro Evidence from Germany" (joint with Clemens Fuest and Andreas Peichl, see Fuest et al., 2013). An earlier version circulates as "Which Workers Bear the Burden of Corporate Taxation and Which Firms Can Pass It On? Micro Evidence from Germany" (Fuest et al., 2012)

to the average citizen should contribute (more) to overall tax revenues. If, however, these allegedly rich firm owners are able to pass on the corporate tax burden to their workers, calls for corporate tax increases based on redistributive motives are misled.

Chapter 4: Employment effects of corporate taxation[3]

In Chapter 4 I analyze the same tax, i.e. a corporate profit tax, but look at its employment effects, thus at a different labor market outcome. The research question is: *How do changes in corporate taxation affect employment?* Similarly to the argument made above it is crucial for the overall evaluation of corporate taxation to know how employment reacts to higher profit tax rates. Moreover, this study adds to the understanding of an old question in economics, namely the mobility of production factors in response to corporate taxes (Harberger, 1962). In an international context labor has been found to be quite immobile yielding negligible employment effects of corporate taxation. This has led to the stylized fact that workers bear a substantial share of the corporate tax burden through lower wages (Auerbach, 2005). Chapter 4, however, focuses on the effects of *municipal* corporate taxation on employment *within* a country. In such a local context labor should be much more mobile and thus employment effects of corporate taxation should differ markedly from the ones obtained in an international setting.

Chapter 5: Well-being effects of income taxation[4]

Chapter 5 looks at the direct effect of labor income taxation on individual well-being. Well-being is the concept individuals should eventually seek to maximize. Thus, the well-being of a society, which is equal to a (weighted) sum of citizens' individual utilities, should be the ultimate interest of a benevolent government. Nevertheless, it is virtually impossible for a politician to design a policy that directly maximizes welfare based on observable labor market outcomes such as income, since he cannot know the underlying preferences of each individual. If, for instance, an individual has a high preference for leisure and is willing to sacrifice most of his income for more leisure, he is

[3] This chapter is based on a (so far unpublished) single-authored manuscript with the title "Employment Effects of Local Corporate Taxes" (see Siegloch, 2013).

[4] This chapter circulates as "Happy Taxpayers? Income Taxation and Well-Being" (joint with Alpaslan Akay, Olivier Bargain, Mathias Dolls, Dirk Neumann and Andreas Peichl, see Akay et al., 2012).

poor but does not necessarily have a low level of utility. This problem directly translates to welfare assessments of public policy since the researcher – just as the politician – will necessarily have difficulties to uncover the true underlying preferences of the citizens.[5] Chapter 5 evades the problem of individual preferences heterogeneity by directly looking at subjective well-being (a measure of individual utility) and analyzing how it is affected by taxation. Thus, our research question is: *What is the effect of labor taxation on well-being?* Clearly, this question is at the core of policy evaluation since taxation is arguably the most important instrument in the hands of a benevolent government that is trying to maximize the *society's* well-being. Thus, the findings of Chapter 5 are meaningful for the optimal calibration of income taxes.

Chapter 6: Stabilizing effects of work sharing policies[6]

In Chapter 6 we analyze the German labor market adjustments during the Great Recession in 2008-09. Public policy is especially important in times of economic crises since sound policymaking can prevent sharp declines in output, stabilize aggregate demand and thereby cushion the impact of the crisis. Given the high labor income share mentioned above, it is particularly important to stabilize the labor market. One promising way to do this is work sharing, i.e. keeping people in employment by reducing working hours. Germany was especially successful in this respect during the Great Recession. In contrast to other countries like the U.S., employment levels remained practically unchanged despite a particularly sizeable output loss. Instead the sharp drop in output materialized almost exclusively at the intensive margin, thus in reduced working hours. This "German labor market miracle" has received considerable attention both in academia and the wider public (Krugman, 2009). In Chapter 6 we take a closer look at this miracle by focusing on the potential amenities and detriments of the intensive margin labor demand adjustments for the income distribution. Our research question is: *What were the distributional consequences of the peculiar German labor market adjustments during the Great Recession?* The insights obtained from the analysis are

[5] Bargain, Decoster, Dolls, Neumann, Peichl, and Siegloch (2013) show the importance of respecting preferences heterogeneity when it comes to cross-country welfare comparisons.

[6] This chapter has been published as "Distributional Consequences of Labor-demand Shocks: The 2008-09 Recession in Germany" (joint with Olivier Bargain, Herwig Immervoll and Andreas Peichl, see Bargain et al., 2012).

relevant for policymakers since a good understanding of the German “miracle” can help other governments in designing preventive policies to guard against the next economic downturn.

1.2 A typology

In the following section, I set up a typology to classify the particular chapters. The typology is meant to guide the reader through the dissertation by making similarities and differences among the particular studies apparent. The typology consists of four dimensions: (1) the *labor market outcome* affected, (2) the *policy type* under study, (3) the underlying *government principle*, and (4) the *evaluation technique* used.

Labor market outcome. Looking at the standard microeconomic labor market model an individual's utility U depends positively on consumption c and leisure l: $U(c,l)$. Though being simplistic this representation provides a useful approximation to the relevant labor market outcomes to be investigated when evaluating public policies. The first labor market outcome is *employment*, which is determined by the labor supply, i.e. the inverse of leisure, of an individual and the labor demand of firms. This labor market outcome will be studied in Chapter 2 where the employment effects of workfare will be addressed by explicitly modeling the interaction of labor supply and demand. Moreover, employment is also the outcome under investigation in Chapter 4, which shows how corporate profit taxes affect employment. Finally, Chapter 6 looks at the margin of labor market reactions (employment vs. working hours) during economic crises and the consequences for the income distribution.

The other argument of the utility function, namely consumption, is determined by the *wage* an individual receives and the individual employment decision. Wage effects, being the second labor market outcome, will be the main focus in Chapter 3, where the incidence of corporate taxes on wages is analyzed. Finally, *well-being*, described by the theoretical concept of utility U, is by itself a labor market outcome. It is studied in Chapter 5, where the effects of income taxation on subjective well-being are investigated.

Public policies. Governments can dispose of numerous specific policies to affect labor market outcomes. I broadly distinguish between *taxation* and *labor market institutions* (LMI).[7] In this dissertation I will cover two specific taxes, corporate profit taxes (more precisely the German local business tax) in Chapters 3 and 4 and the German personal income tax system in Chapters 5.

Labor market institutions is a collective term that subsumes many different policies and regulations. As stressed above, I analyze the workfare principle, a work requirement for welfare recipients, in Chapter 2. Moreover, collective bargaining agreements and their interaction with profit taxation play an important role in Chapter 3. Last labor market institutions facilitating work sharing, such as the German short-term working scheme and working time accounts, can explain parts of the German labor market response to the financial crisis in 2008-09 as shown in Chapter 6.

Government principles. With regard to the government principle, I make use of the well-established taxonomy by Musgrave (1959) to categorize the studies of this dissertation. Musgrave differentiates between three branches of government activity: allocation, redistribution and stabilization. While the *allocation* branch aims at optimizing the factor allocation in the economy and thus at increasing efficiency, the *redistribution* branch is concerned with the distribution of resources within the economy. As for *stabilization* the goal is to ensure macroeconomic stability, in particular through stable prices and high employment.[8] It is important to note that, depending on the specific reform, the principles can be rivaling or complementary.

In terms of allocation, employment is of central interest. In Chapter 2 high welfare benefits induce disincentive to work and thus distort the optimal allocation of employment. Policies trying to mitigate these disincentive effects adhere to the *allocative* government principle. Clearly, Chapter 4 analyzing the employment effects of corporate taxation also regards the allocation branch.

The redistributive principle implies a fair distribution of labor income and the pre-

[7] Note that in some classifications taxation is subsumed under labor market institutions. In this thesis taxation is, however, treated as an independent category.

[8] Musgrave's underlying theoretical fiction is that each branch is run by a "manager" who is only interested in his own department. In some cases a government's policy only aims at improving the situation in one field of activity. In others governments might have two or even all three managers working simultaneously on a project.

vention of high levels of inequality.[9] The central instrument of redistribution and thus inequality reduction are tax-benefit systems. Thus, Chapter 5 analyzing income tax effects on well-being is assigned to the redistributive branch. Moreover, we show in Chapter 3 that higher corporate taxes reduce wages (in the presence of labor unions). Given that profit taxes are often intended to target allegedly rich firm owners, this finding also has important redistributive implications.

The stabilization branch is especially important in times of crises. During a recession, short-term government policies such as fiscal stimulus packages aim at stabilizing aggregate demand and/or keeping people in employment. Furthermore, tax-benefit systems fulfill not only a redistributive but also a stabilizing function by cushioning the net income losses induced by gross income shocks when entering unemployment (Dolls, Fuest, and Peichl, 2012). These mechanisms will be analyzed in Chapter 6 when looking at the German labor market experience during the Great Recession in 2008-09.[10]

Evaluation techniques. The general goal of all evaluation studies is to establish a causal relationship between the policy and the labor market outcome. Ideally, the identification strategy mimics the research design of laboratory experiments with two randomly drawn groups (treatment and control) and an exogenous policy intervention. While such designs can quite easily be set up in the laboratory, it is very challenging and often impossible to find such situations in the real world. In particular, two general challenges, exogeneity and generalizability, have to be met in order to pin down average policy effects in a quasi-experimental setting. Those challenges subsume most – if not all – of the very specific pitfalls of policy evaluation studies.

In terms of concrete evaluation techniques I distinguish between two broad classes: *ex post* and *ex ante* evaluation. Clearly, the classes differ in the points in time at which a policy effect is evaluated, but they can also be aligned with the mentioned challenges, exogeneity and generalizability. In ex post studies exogeneity is necessary to identify the true policy effect. It must be ruled out that policy and labor market outcome are simultaneously correlated with unobserved variables, otherwise estimates are biased due

[9] There is no consensus on the definition of a "fair" distribution. Fairness perception and inequality aversion differ across individuals. In fact, average inequality aversion also varies across countries as shown by Bargain, Dolls, Neumann, Peichl, and Siegloch (2013).

[10] In the context of the current European debt crisis, Bargain, Dolls, Fuest, Neumann, Peichl, Pestel, and Siegloch (2013) look at the stabilizing effects of moving Europe closer to a fiscal union.

to endogeneity. In Chapters 3 and 4 the effects of municipal corporate taxes on wages and employment are analyzed. It is likely that unobserved firm and municipal characteristics (e.g. manager ability or geographical location) affect both the policy variable and outcomes. Thus, we suggest estimators to account for these sources of endogeneity. Moreover, potential feedback effects from the outcome variable on the policy have to be eliminated. This second source of endogeneity is referred to as reverse causality. In Chapter 5, for example, our identification strategy has to rule out that taxation is itself a function of individual well-being.

The generalizability of the policy effect is important for the external validity of the findings. Due to the strict exogeneity requirements mentioned above, ex post studies are often only able to identify a local effect by showing how a specific subgroup of the population is affected by a certain policy. Policymakers are, however, usually interested in an overall, average effect of a policy. Hence, they want to generalize the findings to the whole population. Ex ante evaluation studies are a viable extension to address the problem of generalizability. A typical ex ante evaluation starts with a thorough and precise estimation of the status quo and its underlying mechanisms.[11] Based on the replication of the real world, a policy reform is introduced by changing parameters in the structural model of the "status quo", letting the modeled mechanisms work to arrive at a counterfactual outcome of the policy intervention. Thus, in terms of external validity ex ante analyses go one step further: while the estimated policy effects in ex post studies are often implicitly (and sometimes wrongly) regarded as generalizable, ex ante analyses explicitly make out-of-sample predictions. On the one hand, this is more demanding since more structure has to be imposed in order to model the status quo. On the other hand, if the status quo is modeled correctly, ex ante analyses do not face the problem of generalizability and are by construction externally valid. A standard technique used in ex ante evaluation, in particular in the context of tax-benefit systems and labor supply, is microsimulation (Creedy and Kalb, 2006). Microsimulation models replicate the real world tax-benefit schedule of a country to derive disposable incomes and estimate the actual consumption/leisure preferences of the population. With these two ingredients at hand, income and labor supply effects of hypothetical policy reforms can be simulated. In Chapters 2 and 6 of this thesis, I will make use of $IZA\Psi MOD$, a behavioral tax-benefit microsimulation model for Germany (see Peichl, Schneider, and

[11] Here, the same exogeneity requirements mentioned in the context of ex post analyses have to be met.

Siegloch, 2010, for a documentation).

Table 1.2.1: Chapter classification

	Chap. 2	Chap. 3	Chap. 4	Chap. 5	Chap. 6
Labor market outcome					
employment	x		x		x
wages		x			
well-being				x	
Policy type					
taxation		x	x	x	
labor market institutions	x	x			x
Government principle					
allocation	x		x		
redistribution		x		x	
stabilization					x
Evaluation technique					
ex post		x	x	x	
ex ante	x				x

Classification. Given the four dimensions of the typology, I classify the five studies compiled in this dissertation according to Table 1.2.1. Within each dimension, I only assign the chapters to the one dominant element that reflects the main focus of the respective study. On the one hand, this does not do justice to the full scope of the analyses. For instance, the insights obtained from looking at the stabilization effects of work sharing policies in Chapter 6 also have important redistributive implications since not all socio-economic groups are affected similarly. Likewise, the assignment of Chapters 2 and 6 to the *ex ante* evaluation technique is ambiguous given that both studies also rely on estimates derived from *ex post* analyses. On the other hand, the classification is intended to provide some guidance, which makes simplification inevitable. Yet, I do make one exception to this strict assignment rule: as the interaction of corporate taxes and collective bargaining agreements is crucial for the wage effect found in Chapter 3, I assign this chapter to both policy types.

1.3 Summary of the main findings

In this section I summarize the main findings of the five chapters presented in this dissertation. Moreover, I briefly discuss how the insights obtained can be informative for other studies analyzing labor market effects of public policies.

Chapter findings. In Chapter 2 we find that workfare improves the efficiency of the labor market, which is in line with the theoretical prediction. Introducing workfare in Germany would yield large and positive employment effects of about one million full-time equivalents, increasing the government budget by more than 10 billion euros per year. On the one hand, it is often argued that generous welfare benefits lead to high unemployment rates, since some individuals might not see the financial necessity to pick up a job. On the other hand, these benefits clearly have a distributional purpose, which might be more important to policymakers than efficiency losses. Hence, there is an inherent conflict between the allocation and the redistribution branch of government when it comes to welfare benefits. The findings of Chapter 2 suggest, however, that a policy like workfare could reduce inefficiencies while making an acceptable adjustment to the redistributive nature of welfare benefits, which still has the support of society.[12] Thus, government policy can mitigate detrimental and unintended side effects of other policies. This suggests that well combined policy packages can potentially overcome trade-offs between a society's efficiency and equity concerns.

The interaction of different policies also explains the empirical findings of Chapter 3. We find that the wage effect of corporate municipal taxes crucially depends on whether firms are under a collective bargaining agreement or not. In line with theory, we show a sizeable negative wage effect of corporate taxes for firms where wages are determined by collective bargaining. In these cases a one euro increase in the annual tax liabilities yields a 67 cent decrease in the annual wage bill. The reason for the negative wage effect is that corporate profit taxes decrease the rents which are shared between firm owners and workers through bargaining. Interestingly, we find that general equilibrium wage effects of corporate taxation are negligible. This is in sharp contrast to standard theoretical predictions and to previous empirical findings which show a negative wage

[12] Falk and Huffman (2007) show that the workfare principle is perceived as fair in laboratory experiments.

effect.

The explanation for this negligible general equilibrium wage effect is given in Chapter 4 where I find a strong negative effect of local corporate taxes on employment. A 1% increase in the effective statutory tax rate leads to a drop in employment of 1.3%. The effect is particularly pronounced for medium-sized firms, which have been found to bear the highest relative corporate tax burden in Germany. The negative employment effect is a novel result in public finance. So far, corporate tax effects on production factors have been studied in an international context, where labor is arguably immobile and employment does not respond to tax changes. In contrast, Chapter 4 focuses on the effects of municipal corporate taxation *within* a country, where I find that labor is quite mobile within labor market regions. This insight underscores the importance of adjusting standard models to the specific context, i.e. to the economic and legal environment, when evaluating the impact of public policies.

Chapter 5 analyzes how income taxation affects individual well-being. In line with intuition, the effect of income taxes on well-being is negative when we control for gross income. This result supports previous findings that income has a positive effect on well-being (Clark et al., 2008), since, conditional on gross income, every increase in taxation decreases net income. However, when controlling for net income, we find a positive effect of income taxation on subjective well-being. In other words, if we keep living standards constant, higher taxation is associated with higher levels of well-being. This result is consistent with several possible channels through which taxes affect welfare including public goods, insurance, redistributive taste and tax morale. In fact, we find particular support for the hypothesis that the positive tax effect is driven by the redistributive taste of individuals, which is in line with the findings of Corneo and Grüner (2002). On a more general note Chapter 5 touches upon a fundamental question of public policy: what is the effect of taxation on a society's well-being. Clearly, income is an insufficient measure of well-being to answer this question. Instead more direct measures such as subjective well-being data are indispensable.

In Chapter 6 we assess the distributional and fiscal implications of the peculiar German labor market response during the Great Recession in 2008-09. Unlike in the U.S. or other Western countries, Germany did not experience mass layoffs, but labor demand adjustments materialized almost exclusively at the intensive margin. Our counterfactual simulations reveal that these adjustments – via reductions in working hours – exhibit less severe effects on the income distribution. In addition, working hours reductions are also preferable from a fiscal point of view. In this context, we discuss the cushioning effect of the tax-benefit system and the conditions under which German-style work sharing policies can be successful in other countries. While the short-term working scheme received much attention, other labor market institutions such as opening clauses to collective agreements or working time accounts were at least equally important to facilitate the German crisis response. Moreover, conditions for working hours reductions in Germany were ideal, as the output drop mostly occurred in the export-oriented sectors, where motivation to hoard skilled labor was high and firms had the necessary financial resources to do so. We, therefore, argue that whether German work sharing policies can be successful in other countries or not crucially depends on initial conditions (especially the structure of the economy and labor market institutions) as well as on the specificities of the output shock.

General results. The five studies of this dissertation show that public policy has an important impact on labor market outcomes. On the whole, the specific findings allow drawing three rather broad conclusions which apply to policy evaluation analyses in general.

First, the interaction of policies, in particular of taxation and labor market institutions, is crucial for understanding the labor market effects of policy reforms. As demonstrated in Chapter 2, the combination of different instruments – in this case workfare and welfare benefits – creates opportunities for governments to overcome potential trade-offs a single policy would entail. Moreover, it is perfectly possible that a certain policy only has an effect in a specific institutional environment (see Chapter 3). In this regard the role of labor market institutions has so far received relatively little attention in empirical microeconometric work. Unlike tax rates, national LMI such as employment protection or union status do not change often and are usually not measurable on a continuous scale. Thus, there is little within-country variation in LMI, which poses econometric problems. Nevertheless, researcher should bear the importance of LMI in mind and come up with research designs that overcome the econometric challenges. Quasi-experimental designs exploiting discontinuities in legislative rules or legal exemptions for certain groups of workers or firms seem to be a promising avenue in this respect (cf. Chapters 3 and 4). In terms of data, detailed and preferably administrative micro data are necessary. In particular, the emergence of linked employer-employee data, as used in Chapters 2, 3, 4 and 6, has opened up splendid opportunities for researchers to study the interaction of different labor market policies on both the worker and the firm level (Abowd and Kramarz, 1999; Hamermesh, 1999).

Second, Chapters 3 and 4 have shown that orthodox economic truths rely on assumptions which are often only valid in very specific economic contexts. The same model applied in a slightly different economic environment can lead to different effects and conclusions: corporate taxes on the municipal level affect factor inputs and prices quite differently than national taxes. In this regard, it is also important to note that the exportability of successful national policies to other countries crucially depends on whether the countries are comparable in key economic and institutional features as discussed in Chapter 6.

Third, alternative measures of economic performance are necessary for the advancement of empirical economic research. The rise of subjective well-being data in the past decades (Clark et al., 2008) and the strive to go "beyond GDP" and thus beyond purely money based performance measures has created ample new opportunities for researchers (Fleurbaey, 2009). Especially with regard to public policy evaluation, these new performance measures – if used properly – enable researchers to reconsider many of the established findings and re-address core questions of economic analysis (see Chapter 5). This will help the profession to meet one of its biggest challenges, which is "to incorporate the findings of modern psychology while retaining the rigor of the cost-benefit framework which is the strength and glory of [public economics]" (Layard, 2006).

Chapter 2

Employment effects of a workfare reform*

2.1 Introduction

Labor supply elasticities are important ingredients for policy evaluation (see, e.g., Blundell, Duncan, McCrae, and Meghir, 2000 for a partial equilibrium application and Bovenberg, Graafland, and de Mooij, 2000 for a general equilibrium model). Furthermore, they crucially affect the optimal design of tax systems (see, e.g., Saez, 2001, Immervoll, Kleven, Kreiner, and Saez, 2007 and Blundell, Brewer, Haan, and Shephard, 2009). The elasticities are usually derived using some sort of (structural or reduced form) labor supply model (see, e.g., Aaberge, Dagsvik, and Strøm, 1995; Hoynes, 1996; Aaberge, Colombino, and Strøm, 1999; Aaberge, Colombino, and Strøm, 2000; Eissa and Hoynes, 2004; Heim, 2007, 2009). All these studies have in common that they focus only on the supply side implicitly assuming perfectly elastic labor demand. Only in this case labor supply effects equal eventual employment effects. However, as the extensive empirical evidence suggests, labor demand is usually somewhat elastic (Hamermesh, 1993). Hence, labor market estimates stemming from pure labor supply models are almost surely biased and inference based on them is consequently flawed.

In this paper, we develop a straightforward approach to extend random utility models

* The following chapter has been published as "Accounting for Labor Demand Effects in Structural Labor Supply Models" (joint with Andreas Peichl, see Peichl and Siegloch, 2012).

of labor supply to explicitly take into account demand effects. In terms of labor supply modeling, no generally agreed-upon standard estimation approach exists. Recent practice has mostly relied on natural experiments based on tax reforms to identify responses to exogenous variations in net wages (see Blundell and MaCurdy, 1999 and Bargain, Peichl, and Orsini, 2011 for surveys). While these approaches address the microeconometric identification issues especially with respect to the endogeneity of wages, they are less robust with respect to general equilibrium effects on the labor market.[1] For this reason we use structural labor supply and demand models and iterate them until the partial labor market equilibrium is reached. Our approach is related to the work of Creedy and Duncan (2005) as well as Haan and Steiner (2006) who also employ discrete choice labor supply modeling. In both studies information on labor demand is used to calculate wage adjustments after some kind of labor supply shift. The authors of the former study employ the concept of aggregate labor supply to determine the effects of proportional wage changes. In contrast, Haan and Steiner (2006) model labor supply responses and wage adjustments at the individual level.

We augment the original methods in several ways. First, instead of relying on labor demand elasticities from the literature, we estimate own labor demand functions for different types of workers, based on rich, linked administrative employer employee data. By doing that, we remain at the microdata level as the detailed administrative firm dataset allows the identification of precise labor demand reactions to wage changes for different labor inputs (i.e. household type/skill cells). In addition, our iteration process guarantees that households individually face possible demand restrictions depending on their characteristics. Hence, we capture the full heterogeneity of the microdata sample. Finally, neither Creedy and Duncan (2005) nor Haan and Steiner (2006) provide much evidence on how the interaction of supply and demand side functions. We open the black box and give detailed insight on both the iteration process itself and its theoretical plausibility.

We also see several advantages of our approach compared to alternative methods of

[1] That is, the natural experiment approach works well provided that control groups are well defined and not affected by the policy change. However, if reforms affect large numbers of people, changes in supply and demand of the treatment group can have feedback effects on the behavior of the control group, which cannot be captured in this approach. In a recent paper, Chetty, Friedman, Olsen, and Pistaferri (2011) stress the importance of structural modeling by showing that quasi-experimental evidence ignores firm responses and labor market frictions.

incorporating labor demand effects in labor supply estimations, such as computable general equilibrium (CGE) models (see Peichl, 2009 for an overview) or models integrating demand side restrictions via probabilities (cf. Blundell, Ham, and Meghir, 1987). Our model is slender and parsimonious, since it focuses only on the labor market. At the same time, we can introduce much more heterogeneity, as both supply and demand sides are estimated using microdata. Moreover, we explicitly model the interaction of demand and supply, taking firm behavior into account and separating it from labor supply effects.

In order to demonstrate the performance of our newly developed demand-supply link, we depart from a standard, discrete choice, structural labor supply model following van Soest (1995) and Blundell et al. (2000). We estimate the model with the 2009 wave of the German Socio-Economic Panel Study (SOEP), a representative, microdata, household panel study, using the IZA tax benefit calculator IZAΨMOD to transform gross income to net income. As a counterfactual policy reform, we introduce a workfare concept (see Besley and Coate, 1992; Moffitt, 2002). Every employable individual living in a household that receives government benefits has to fulfill a work requirement equivalent to a full-time job. We choose this specific counterfactual mainly because it is expected to have a substantive positive labor supply effect and because it is often criticized for ignoring demand side restrictions. Furthermore, the effect on the government budget is expected to be positive, making the reform feasible from a fiscal point of view.

Our simulation results show that demand effects do indeed play an important role. They offset the positive labor supply reaction of the workfare reform by 25 percent (equivalent to $380,000$ full-time jobs). Thus, labor demand works as a stabilizer to labor supply shifts. To check the robustness of our results, we also simulate different counterfactuals. We find demand effects of comparable sizes in relative terms. Moreover, the stabilizing effect also works in the other direction, that is, if a reform reduces labor supply, the incorporation of labor demand effects countervails the negative supply effects, making the overall employment effect less negative. Further sensitivity tests show that, in line with theory, the higher the demand elasticity, the smaller the demand adjustments.

The paper is structured as follows. Section 2.2 compares our method to the literature. In Section 2.3, we set up a standard labor supply model. Section 2.4 describes the labor demand model. Section 2.5 demonstrates the linkage of labor supply and demand.

Empirical results are presented and discussed in Section 2.6 and Section 2.7 concludes.

2.2 Related literature

There are other approaches to account for demand effects in labor supply models which are naturally related to ours. One common method, particularly in the field of ex-ante policy evaluation, is linking labor supply models with computable general equilibrium (CGE) models (see Bourguignon, Robilliard, and Robinson, 2003; Bovenberg et al., 2000; Boeters, Feil, and Gürtzgen, 2005; Arntz, Boeters, Gürtzgen, and Schubert, 2008; Boeters and Feil, 2009; Hérault, 2010). The advantage of our approach is that we overcome possible aggregation and linking problems in micro macro models.[2] Our analysis remains on the micro-level, as both the supply and demand sides are estimated using microdata. This allows us to introduce much more heterogeneity into the analysis, since we do not rely on just a few representative agents, as is the case in CGE models. Moreover, we do not have to model further markets and impose assumptions on how, for example, a decline in consumption translates into a reduction in output. Instead we adopt a partial framework and focus solely on the labor market.[3] As a consequence, our method abstracts from intertemporal adjustments and optimization behavior. Temporary labor demand shocks could potentially delay but do not alter the adjustment process to the new labor market equilibrium.[4]

Another cluster of studies tries to extend structural labor supply models by introduc-

[2] When conducting such a micro macro linkage, several potential problems arise (see Peichl, 2009). The main problem is the lack of theoretical and empirical consistency between the micro and macro components, which can give rise to biased results. To be able to successfully link microsimulation and CGE models, there have to be some common variables through which the two models can exchange information. Although CGE models are based on the microeconomic general equilibrium theory, they usually use aggregated macrodata for the analysis. Hence, it is necessary to aggregate or disaggregate these variables in order to make them comparable with the variables in the other model. Furthermore, it has to be checked whether the same variable in both models represents the same population (e.g. household consumption in the micro-model vs. aggregated total consumption, including the government's in the macro-model).

[3] On the other hand, our slender approach is not able to take into account general equilibrium effects (other than wage and employment changes). In particular, we ignore changes in consumption and consumer prices. Hence, if these responses are important, our approach is not able to capture the full effects of a policy change (but it still performs better than a pure labor supply model).

[4] Bargain et al. (2012) use a model similar to ours based on the same dataset to estimate the labor demand effects of the Great Recession for Germany, taking into account that wages were quite sticky in the short-run (see Chapter 6).

ing probabilities which account for possible demand side frictions. Within this line of literature, there is a whole range of different models which can be broadly divided into three subgroups. Firstly, there are Double Hurdle Models that assume a two-tier decision making process (see Blundell et al., 1987; Hogan, 2004 and Bargain, Caliendo, Haan, and Orsini, 2010 for a recent empirical implementation for Germany). In the first stage, the individual decides whether to participate in the labor market or be inactive. The second hurdle is the probability of being involuntary unemployed, conditional on having chosen to work. This probability can be interpreted as a demand side restriction.

The second group of studies extends labor supply models to take classical non-employment into account. Meyer and Wise (1983a,b) model the effects of a minimum wage on youth employment by introducing the probability that a worker is not productive enough to be hired. Laroque and Salanié (2002) extend this framework and include the probability of being involuntarily unemployed due to frictional or business-cycle related unemployment.

The third probability-based approach to integrate labor demand constraints is to restrict the set of hours which can be chosen by individuals. In those models, working hours generally stem from some sort of offer distribution (see Moffitt, 1982; van Soest, Woittiez, and Kapteyn, 1990; Aaberge et al., 1995). Tummers and Woittiez (1991) extend those models by allowing the wage rate to vary with the offered hours. Bloemen (2000) generalizes hour offers to job offers, which consist of both an hour and a wage component.

These probability based approaches rely on pure labor supply models, which are extended by a demand side restriction. In contrast, we employ two separate structural models. In particular, we recover structural parameters from the demand side of the labor market and thus account directly for firm behavior. We then explicitly model the interplay of demand and supply which takes place through wage adjustments. In that sense our approach is related to the work of Bingley and Lanot (2002) whose model also allows the equilibrium wage to react to changes in income taxes. From a more general perspective, our structural demand estimates account for the various demand side restrictions assumed in the different probability based models. Furthermore, the structural nature of our approach enables us to conduct counterfactual policy simulations affecting both sides of the labor market.

2.3 Labor supply model

We construct a discrete choice, random utility model to estimate the labor supply behavior of individuals, based on a structural specification of preferences. The main advantage of this model over continuous ones is the possibility to account for non-linearities and non-convexities in the budget set, which is important for identification.[5] Those kinds of models have become quite standard in the last 15 years (see Aaberge et al., 1995; van Soest, 1995; Blundell et al., 2000), and so we only present the underlying assumptions we made to arrive at our model specification. Following van Soest (1995), we rely on a translog specification of utility. The (deterministic) utility of a couple household i for each discrete choice $j = 1, ..., J$ can be written as:

$$\begin{aligned} U_{ij} \; = \; & \alpha_{ci} \ln c_{ij} + \alpha_{h_f i} \ln h^f_{ij} + \alpha_{h_m i} \ln h^m_{ij} + \alpha_{h_{ff}} \left(\ln h^f_{ij}\right)^2 + \\ & \alpha_{h_{mm}} \left(\ln h^m_{ij}\right)^2 + \alpha_{cc} \left(\ln c_{ij}\right)^2 + \alpha_{ch_f} \ln c_{ij} \ln h^f_{ij} + \\ & \alpha_{ch_m} \ln c_{ij} \ln h^m_{ij} + \alpha_{h_m h_f} \ln h^f_{ij} \ln h^m_{ij} + \beta_f D^f_{ij} + \beta_m D^m_{ij} \end{aligned} \qquad (2.3.1)$$

with household consumption c_{ij} and spouses' worked hours h^f_{ij} (female) and h^m_{ij} (male) and $D^{m/f}_{ij}$ being part-time dummies representing fixed costs of work. We assume seven discrete hour categories: $0, 10, 20, 30, 40, 50$ and 60 h for each individual. Hence, the $J = 49$ choices in a couple correspond to all combinations of the spouses' working-time categories. Coefficients on consumption and worked hours vary linearly with several taste-shifters (for instance age, age squared, presence of children, region).

The direct utility function is estimated using McFadden's conditional logit model (McFadden, 1973), maximizing the probability that the household chooses the observed working-hour category, given its characteristics and its calculated consumption. In

[5] In general, correct identification of the preference parameters of the structural model is crucial for the subsequent analysis. As unobserved characteristics can influence both wages and work preferences, estimates obtained from cross-sectional wage variation are potentially biased. Fully accounting for tax-benefit policies creates variation in net wages between individuals with the same gross wage. That is, individuals face different effective marginal tax rates because of their circumstances (marital status, age, family compositions, home-ownership status, disability status) or different levels of non-labor income. Furthermore, we benefit from spatial variation that can produce additional exogenous variation in net wages. In Germany, housing benefits vary at the municipality level, taking into account local differences in housing costs. Social assistance levels as well as church taxes and social insurance contributions vary between states. In addition, we use predicted wages for all individuals (rather than for the unemployed only), in order to further reduce the bias.

addition to this deterministic part, the household's random utility level depends on a stochastic error term. We calibrate the random part of the utility function by drawing error terms from the Extreme Value Type-I distribution in order to guarantee that the observed choices yield the maximum random utility (see Duncan and Weeks, 1998; Creedy and Kalb, 2005).[6]

The model is estimated on the German Socio-Economic Panel Study (SOEP), which is a representative microdata household panel study (Wagner, Frick, and Schupp, 2007). We select the 2010 wave of SOEP, which contains information about the year 2009. We observe around $25,000$ individuals in more than $12,000$ households. Among others, we draw the following data: gross wage, job type, government transfers, working time, composition of household as well as age and education of household members. The latter information is particularly important for the demand extension, since we are able to assign different skill levels to individuals: high-skilled individuals hold a university, polytechnical or college degree; medium-skilled workers have either completed vocational training or obtained the highest German high school diploma, called the *Abitur;* low-skilled workers have neither finished vocational training nor obtained the *Abitur*.

In order to translate gross earnings into net income, we use the IZA tax benefit calculator, called IZAΨMOD (see Peichl et al., 2010, for an overview). IZAΨMOD comprises all relevant features of the German tax and benefit system, such as income taxation and social insurance contribution rules, as well as unemployment, housing and child benefits. We apply the rules as of January 2009. Our calculations are made representative for Germany by using the SOEP population weights. For the labor supply estimation (and the eventual demand extension), we assume that certain individuals do not supply labor or have an inelastic labor supply (such as pensioners, people in education, civil-servants or the self-employed). By assumption, those groups do not adjust their labor market behavior due to a policy reform; they are nonetheless part of the sample (for the analysis of fiscal or distributional effects).

[6] It should be noted that the choice of the discrete labor supply model is irrelevant for the demand extension proposed later on. The eventual labor supply labor demand link proposed in this paper is very general and does not depend on the derivation of the error terms. We obtain similar results when using other approaches, such as the analytical derivation of error terms proposed by Bonin and Schneider (2006a) or using the conventional frequency method (Aaberge et al., 1995; van Soest, 1995). Moreover, our results are robust with respect to different discretizations and specifications of the utility function.

2.4 Labor demand model

2.4.1 Empirical model and estimation

For the demand model, we follow standard practice by adopting the dual approach and minimizing costs given a constant output (Hamermesh, 1993). We select a translog cost function, as proposed by Christensen, Jorgenson, and Lau (1973), which is a linear, second-order approximation to an arbitrary cost function. The translog cost function belongs to the class of flexible cost functions, which do not restrict the substitution elasticities of input factors, and is therefore preferable to Cobb Douglas or Constant Elasticity of Substitution functions.[7]

We follow the concrete specification proposed by Diewert and Wales (1987) and calculate the costs C of a firm, given a certain output Y, as follows[8]:

$$\begin{aligned}\ln C(w_i, Y) = \alpha_0 + \sum_{i=1}^{n} \alpha_i \ln w_i + 0.5 \sum_{i=1}^{n} \sum_{j=1}^{n} \alpha_{ij} \ln w_i \ln w_j + \\ \beta_Y \ln Y + \sum_{i=1}^{n} \beta_{iY} \ln w_i \ln Y + 0.5 \beta_{YY} (\ln Y)^2 \\ \delta_t t + \sum_{i=1}^{n} \delta_{it} t \ln w_i + 0.5 \delta_{tt} t^2 + \delta_{tY} t \ln Y \end{aligned} \quad (2.4.1)$$

where $w_i, i = 1, .., I$ denotes unit costs (i.e. the wage) of the i^{th} labor input and t is a time index.[9] Besides the condition $a_{ij} = a_{ji}$, several other restrictions on the parameters hold, ensuring linear homogeneity in factor prices and allowing for non-constant returns to scale:

$$\sum_{i=1}^{n} \alpha_i = 1 \quad \sum_{i=1}^{n} \alpha_{ij} = \sum_{j=1}^{n} \alpha_{ij} = 0 \quad \sum_{i=1}^{n} \beta_{iY} = 0 \quad \sum_{i=1}^{n} \delta_{it} = 0 \quad (2.4.2)$$

By Shephard's lemma (see Shephard, 1970) the first derivative of the cost function with respect to a specific factor price yields the demand for this input, $X_i = \frac{\partial C}{\partial w_i}$. Exploiting the fact that the cost function is logarithmized and thus that $\frac{\partial \ln C}{\partial \ln w_i} = \frac{\partial C}{\partial w_i} \frac{w_i}{C}$, we

[7] See Lichter, Peichl, and Siegloch, 2013 for more details on the choice of cost functions.
[8] Time and firm indices have not been included for increased clarity.
[9] As there is no direct measure of capital in the firm data, we assume perfect separability between labor and capital. In fact, robustness checks have shown that the inclusion of capital, approximated by investments in the preceding year, hardly changes the estimated elasticities.

derive the cost shares:

$$S_i = \frac{w_i X_i}{C} = \frac{\partial \ln C(w_i, Y)}{\partial \ln w_i} = \alpha_i + \sum_{j=1}^{n} \alpha_{ij} \ln w_j + \beta_{iY} \ln Y + \delta_{it} t \tag{2.4.3}$$

It is straightforward to calculate labor demand elasticities from the cost share. Own-wage elasticities are defined as:

$$\overline{\mu}_{ii}^{TL} = \frac{\alpha_{ii} - \widehat{S}_i + \widehat{S}_i \widehat{S}_i}{\widehat{S}_i} \tag{2.4.4}$$

and cross-wage elasticities yield:

$$\overline{\mu}_{ij}^{TL} = \frac{\alpha_{ij} + \widehat{S}_i \widehat{S}_j}{\widehat{S}_i} \tag{2.4.5}$$

To each of the I share functions a disturbance term ε_i is added. It is assumed that the resulting disturbance vector $\varepsilon = \{\varepsilon_1, ..., \varepsilon_I\}$ is multivariate and normally distributed, with mean vector zero and constant covariance matrix. Since the share functions add up to unity, one equation is dropped by using the restrictions (2.4.2) and the relation $S_i = 1 - \sum_{j \neq i} S_j$.

Assuming three different types of labor inputs (subindex 1 for high-skilled, 2 for medium-skilled and 3 for low-skilled labor), we arrive at the system of share equations to be estimated:

$$\begin{aligned} S_2 &= \alpha_2 + \alpha_{22} \ln\left(\frac{w_2}{w_1}\right) + \alpha_{23} \ln\left(\frac{w_3}{w_1}\right) + \beta_{2Y} \ln Y + \delta_{2t} t + \varepsilon_2 \\ S_3 &= \alpha_3 + \alpha_{32} \ln\left(\frac{w_2}{w_1}\right) + \alpha_{33} \ln\left(\frac{w_3}{w_1}\right) + \beta_{3Y} \ln Y + \delta_{3t} t + \varepsilon_3 \end{aligned} \tag{2.4.6}$$

We estimate the equation system by Seemingly Unrelated Regression (SUR) as developed by Zellner (1962). As it is likely that the error terms are correlated within firms over cost shares, SUR is more efficient than estimating the equations separately with ordinary least squares (OLS). At the first stage, SUR uses equation-by-equation OLS to obtain the covariance matrix of the error terms, Ω. Then a feasible generalized least squares (FGLS) estimation is performed on the system of equations, conditional on Ω.

As the summing-up condition necessitates one equation to be dropped, we iterate the FGLS estimation until the changes in the estimated parameters and in Ω become arbitrarily small. This guarantees that the results do not depend on the choice of the cost share to be discarded. Note that the results are equivalent to the maximum likelihood estimator fitted to SUR.

Berndt (1991) suggests several tests to ensure that the estimated demand model is in line with the underlying theoretical assumptions. First, as the cost functions must be monotonically increasing in prices, all cost shares S_i should be positive at each observation. Second, in order for C to be quasi-concave in input prices the $I \times I$ matrix of Hicks-Allan substitution elasticities has to be negative semi-definite at each observation.[10] The third test is a simple adding-up condition ensuring that $\sum_{j=1}^{n} \overline{\mu}_{ij} = 0, \forall i.$ Our model passes all three tests as indicated in Table 2.8.1 in the appendix (Section 2.8).

Finally, note that the standard structural labor demand model assumes wages and thus labor supply to be fixed, just as the labor supply model assumes labor demand (and wages) to be fixed. The simultaneity of both sides of the labor market is introduced through the iteration process described in Section 2.5.

2.4.2 Data

We use linked employer-employee data (LEED) to estimate the demand for differently skilled labor. The use of LEED is essential for our micro-level approach since it enables us to observe both individual skill-specific wages and firm-related information, such as output. The data is taken from the linked employer-employee dataset (LIAB) provided by the Institute of Employment Research (IAB) in Nuremberg, Germany (see Alda, Bender, and Gartner, 2005, for more information on the dataset).

The employee data are a sample of the administrative employment statistics of the German Federal Employment Agency (*Bundesagentur für Arbeit*), called the German employment register, which covers all employees paying social security contributions or receiving unemployment benefits (Bender, Haas, and Klose, 2000, see). The public sector is excluded, as civil servants are rarely observed in the social security data. Employee information recorded in the data includes wages, age, seniority, qualification, occupation, employment type (full-time, part-time or irregular employment), industry

[10] With the substitution elasticities being defined as $\overline{\sigma}_{ii}^{TL} = \frac{\alpha_{ii} - \widehat{S}_i + \widehat{S}_i \widehat{S}_i}{\widehat{S}_i \widehat{S}_i}$ and $\overline{\sigma}_{ij}^{TL} = \frac{\alpha_{ij} + \widehat{S}_i \widehat{S}_j}{\widehat{S}_i \widehat{S}_j}$.

and region. We use the same skill definition as in the supply part of the model, differentiating between high, medium and low-skilled workers. Since we are interested in labor demand dependent on the skill level, individuals with missing information on qualification are excluded.

The firm component of the LIAB is the IAB Establishment Panel (cf. Kölling, 2000). The term "establishment"refers to the fact that the observation unit is the individual plant, not the firm; there can be several plants per company. The Establishment Panel is a representative, stratified, random sample containing annual information on establishment structure and personnel decisions from 1993 onward. It includes establishments with at least one worker for whom social contributions were paid, covering 16 industries and establishments from both the former West and East Germany.

We exclude the mining, agricultural, financial as well as the public sector, since turnover is measured differently in these industries.[11] Output is adjusted for inflation, using the German consumer price index obtained from the German Federal Statistical Office. Plants with missing information on output are excluded, as well as establishments with fewer than three workers in one of the three skill categories. Finally, we use survey weights provided in the LIAB to make the establishment sample representative for the whole population of German establishments. The final panel comprises 12 years (from 1996 to 2007) and $4,073$ establishments, which are, on average, observed 3.3 times during the period studied. This results in $13,451$ establishment-year observations and between 1.6 and 2.0 million workers per year.

2.4.3 Labor demand elasticities

The own- and cross-wage labor demand elasticities for the three skill types are calculated according to formulas 2.4.4 and 2.4.5, respectively, by inserting the parameter estimates of the structural model.[12] Table 2.4.1 presents the results. Looking at own-wage elasticities, we find the highest elasticity for low-skilled workers at -1.05, followed by high-skilled at -0.56, and medium-skilled at -0.37. These results mirror the findings

[11] The exclusion of the public sector can also be justified by the fact that labor supply (in terms of hours) is not very flexible in the public sector, as most employees are civil servants or on heavily regulated contracts with fixed hours. Therefore, in IZAΨMOD labor supply for public sector workers is fixed, an assumption made in most microsimulation models – at least for Germany.

[12] Estimation results can be found Table 2.8.1 in the appendix (Section 2.8). For a more thorough analysis and discussion of the effects of different specifications, see Lichter et al. (2013).

of previous studies on labor demand in Germany (see Falk and Koebel, 2001, 2004; Addison, Bellmann, Schank, and Teixeira, 2008; Bauer, Kluve, Schaffner, and Schmidt, 2009; Freier and Steiner, 2010). Firstly, all elasticities are negative and finite, as postulated by theory, corroborating the claim that employment effects cannot be solely determined by labor supply shifts. Secondly, the absolute value of the own-wage elasticity of the low-skilled is higher than the elasticity of the medium-skilled. Higher elasticities of low-skilled workers are normally explained by globalization and international competition from low-wage countries, which destroy jobs for unskilled workers in industrial countries. As for the relationship between the high and medium-skilled, the empirical picture is somewhat ambiguous. In about half of the studies on Germany, the absolute value of high-skilled elasticities is greater than the value of the medium-skilled; in the other half, it is smaller. However, as far as the magnitude is concerned, most elasticities lie in the interval from -0.05 to -1.0.

Table 2.4.1: Labor demand elasticities

	Demand		
Wages	High-skilled	Medium-skilled	Low-skilled
High-skilled	**-0.56**	0.60	-0.04
Medium-skilled	0.13	**-0.37**	0.24
Low-skilled	-0.04	1.09	**-1.05**

Source: Own estimates based on LIAB.

The cross-wage elasticities reveal that medium-skilled workers are substitutes for both low and high-skilled workers. If, for example, the wages of low-skilled go up by 1 percent, demand for medium-skilled workers increases by 1.09 percent. In contrast, the demand for low-skilled reacts much less to a change in the medium-skilled wages; and there is hardly any reaction to wages of high-skilled workers.

In principle, the labor demand model would allow to further differentiate elasticities by, for instance, industry or socio-demographic characteristics. There is, however, clearly a trade-off between labor input disaggregation and the empirical tractability of the model due to too small sample sizes (see Bargain et al., 2012 for a further discussion of this issue). As elasticities between industries vary only slightly, we choose a slender approach to guarantee sufficient numbers of observations per cell, differentiating only between three skill groups. In addition, we include industry dummies in equation system 2.4.6.

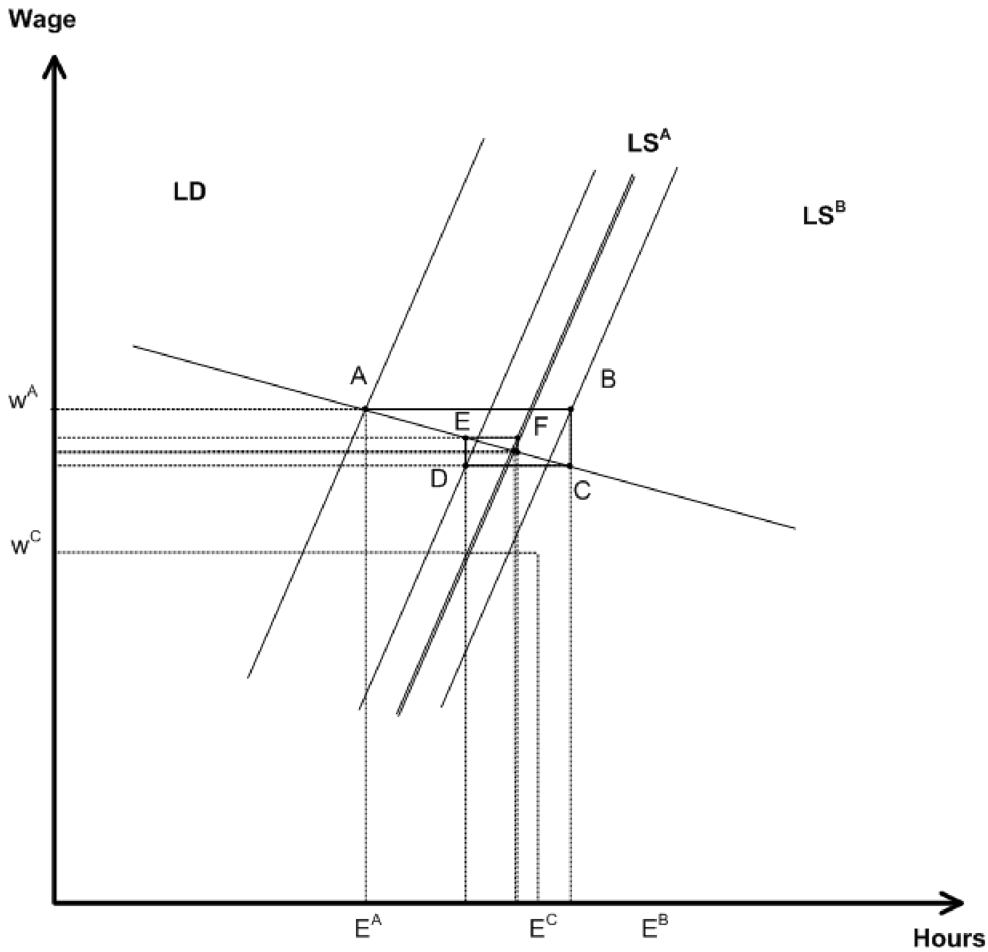
2.5 Demand-supply link

We now extend the labor supply model described in Section 2.3 to take into account labor demand adjustments based on the model described in Section 2.4. Figure 2.5.1 portrays the operating mode of the demand-supply link. Point A depicts the labor market equilibrium. A policy reform shifts the labor supply to the right (LS^B).[13] Without a demand module, implicitly assuming perfectly elastic labor demand, the resulting employment would rise to E^B. Assuming a downward-sloped labor demand curve, however, it is trivial to see that this cannot be the equilibrium of the labor market under perfect competition, since supply does not equal demand. Due to the wage elasticity, the rise in employment $\Delta E^1 = E^B - E^A$ yields a decrease in the wage, $\Delta w^1 = w^C - w^A$. We thus calculate Δw^1 using ΔE^1 and the demand elasticity. We feed the new wage, w^C, into the supply model and recalculate the net income. The change in net income will again have an effect on labor supply, which is simulated using the behavioral labor supply module. Assuming a positive labor supply elasticity, the labor supply shifts to the left (from LS^B to LS^D), reducing the initially positive employment effect. Once again using demand elasticities, this reduction of employment, $\Delta E^2 = E^D - E^B$, will lead to an increase in the wage, $\Delta w^2 = w^E - w^C$, shifting the supply curve to the right (LS^D to LS^F). This procedure is iterated until the employment shifts and thus the wage shifts become arbitrarily small and the model converges.[14] At this point supply equals demand and the new market equilibrium is at point Z.

[13] Note that we do not distinguish between compensated and uncompensated labor supply responses in our analysis. Estimated income elasticities are negligibly small and thus uncompensated and compensated responses are almost identical. In addition, policymakers tend to believe that working (more) is beneficial. As a consequence elasticities which do not compensate for foregone leisure seem to be the more appropriate measure when conducting a policy simulation.

[14] We consider a relative change in working hours of less than 0.1 percent to be sufficiently small. Depending on the size of the household-type/skill cell, this corresponds to between 400 and 7,000 full-time equivalents.

Figure 2.5.1: Supply and demand adjustments



Source: Own presentation.

As seen, different skill groups have different labor demand elasticities yielding different wage changes due to labor supply reactions. We consequently apply the iteration algorithm separately for each household type and skill group. To sum up, the iteration algorithm for every skill/household type combination is defined as follows:

1. The change in net income due to the tax reform is calculated.
2. The labor supply effect is simulated, given the new net income.
3. The gross wage adjusts according to the supply effect and the labor demand function.

4. The labor supply effect is re-simulated given the new wage.
5. If the relative change in working hours is greater than 0.1 percent, repeat steps 3 and 4.

Some restrictive assumptions have to be made to justify the iteration algorithm. Most notably, the demand elasticity must be constant at any point of the demand curve.[15] Furthermore, the assumptions of a perfectly competitive market must be fulfilled so that we are not faced with wage rigidities whatsoever.[16] Last, as we want to demonstrate the importance of taking labor demand restrictions into account, we choose a pure labor supply side reform as a policy application. The demand side is assumed not to react to the policy change, so that the labor demand curve does not shift and the labor demand elasticities do not change. In principle, it is straightforward to extend the method presented here to allow for shifts in the labor demand curve as a reaction to policy changes.

Finally, when linking two different models, a potential problem is that there is no guarantee of coherence (see Peichl, 2009 for a general discussion). It is a priori not possible to specify restrictions that globally ensure convergence in theory when using common flexible functional forms for preferences and technology. In practice, however, convergence is usually achieved when working with standard, well-behaved functions.

2.6 Empirical application

2.6.1 Modeling workfare

In order to demonstrate the effects of the labor supply-demand link, we simulate the effects of a counterfactual reform introducing the workfare concept for Germany. In general, the workfare principle requires everybody who receives social benefits to work full-time (see Besley and Coate, 1992; Torfing, 1999; Moffitt, 2002; Ljungqvist, 1999, 2010)). Workfare concepts have been implemented in several countries, such as Denmark (cf. Torfing, 1999), the Netherlands, the UK and, under the label "Wisconsin

[15] Similarly, we arrive at the aggregate labor supply by aggregating the individual labor supply responses evaluated at the observed labor supply choice. This leads to a "labor supply response schedule" which approximates, but differs from, the aggregate labor supply function discussed in the macroeconomics literature (see the discussion in Creedy and Duncan, 2005 and Creedy and Kalb, 2006).

[16] Note that this assumption is not crucial for the general method, which could be embedded into a different labor market model (e.g. union wage bargaining or efficiency wages).

Works" the U.S. (cf. Ochel, 2005, for a survey). In Germany, workfare has been widely discussed as an alternative to the current, generous social assistance system (see German Council of Economic Experts, 2005; Bonin and Schneider, 2006b).

We choose the workfare concept as a counterfactual for several reasons. Firstly, there is very little evidence on the effects of workfare on labor supply and demand. Secondly, theory predicts an unambiguously positive effect on labor supply, as the choice of non-participation (and dependency on unemployment benefits) no longer agrees with the maximum amount of leisure. As the people in workfare have to work in a full-time community job to receive government transfers, they have the incentive to take up a regular job, which generally yields a higher income. Thirdly, due to the expected positive labor supply effects, positive fiscal effects are likely, which makes the counterfactual a viable reform proposal from a fiscal point of view. Finally, workfare concepts are often criticized for ignoring the possibility of demand restrictions. If the excess labor supply induced by the work requirement does not translate into regular employment, because the respective private sector labor demand does not exist, the intended reform effect does not materialize and the fiscal costs might increase substantially (see e.g. Peck and Theodore, 2000).[17] This critique makes the reform a very appropriate counterfactual to illustrate the importance of taking labor demand effects into account.

In our application, we implement workfare as follows (cf. German Council of Economic Experts, 2005): every employable individual who lives in a household that receives government benefits has to enroll in full-time community work for 40 hours per week.[18] If the recipient is regularly employed but still relies to some extent on government transfers, the hours of mandatory community work is the difference between 40 hours and the regular weekly hours stated in the work contract. All other rules of the tax and benefit system remain unchanged.

[17] More normative arguments against workfare attack the concept from ethical points of view and on the grounds of fairness (see e.g. Peck and Theodore, 2000). Yet, recent behavioral experiments suggest that such regulations are perceived as fair in a Rawlsian state of the world (see Falk and Huffman, 2007).

[18] In Germany, there are two types of unemployment benefits: unemployment benefit I, which come from an insurance and unemployment benefits II, i.e. social assistance. Additionally, there are housing and subsidiary child benefits (*Wohngeld* and *Kinderzuschlag*), which can be substitutes for social assistance and are consequently subject to the workfare rules as well. For details on the German benefit system, see Peichl et al. (2010).

2.6.2 Simulation results

The reform scenario is simulated with and without taking labor demand effects into account. Table 2.6.1 summarizes labor supply and labor demand effects of the reform by household type, skill level and gender. The first column of the table presents full-time equivalents (FTE) in the status quo. Column 2 reports the isolated labor supply effect without demand adjustments. The reform yields a substantial and positive labor supply effect of about 1.5 million full-time equivalents (which is in line with previous findings for Germany). As expected, labor supply responses are unambiguously positive across all household types and skill subgroups. Column 3 shows the effects of taking labor demand restrictions into account: the total increase in FTE is about $377,000$ lower than in the situation of pure labor supply adjustments. The overall offsetting effect of the labor demand restrictions (relative to the labor supply increase) is 25 percent (column 4). Labor demand effects countervail the positive labor supply effects for all household types, skill and gender groups, explaining the negative sign of the ratio between labor demand and labor supply, reported in last column of Table 2.6.1.

Table 2.6.1: Labor demand effects by household-type, skill and gender

Groups	FTE base	LS effect	LD effect	LD/LS effect (%)
Single men	5192.6	422.6	-103.6	-24.51
Single women	4904.2	311.2	-74.8	-24.02
Men in couples	12188.9	161.5	-19.9	-12.29
Women in couples	8075.9	595.7	-179.2	-30.09
High-skilled	8789.9	14.7	-0.6	-3.81
Medium-skilled	19186.0	876.2	-272.1	-31.05
Low-skilled	2385.7	600.2	-104.8	-17.46
Men	17381.4	584.2	-123.4	-21.13
Women	12980.0	906.9	-254.0	-28.01
Overall	**30361.5**	**1491.1**	**-377.4**	**-25.31**

Source: Own calculations based on IZAΨMOD. *Notes:* Full-time equivalents (FTE) in 1000s. The LS effect measures the difference in FTE to the status quo, whereas the LD effect measures the difference to the LS effect.

Hence, labor demand works as a stabilizer to employment shifts, as suggested by Figure 2.5.1. The magnitude of demand effects differs across household types, gender and skill groups but is substantive except for high-skilled workers. The higher relative offsetting effect for the medium-skilled is explained by two factors. Firstly, the pure labor supply effect of workfare on the low-skilled is relatively higher than the effect for the medium-skilled (25% increase vs. 5%). Secondly, the labor demand elasticity of the low-skilled in absolute terms is higher, implying a smaller wage decrease for a given increase in employment. Looking at gender differences, Table 2.6.1 shows that countervailing effects of labor demand are stronger for women (−28% vs. −21%). As the skill distribution over gender is comparable, the difference is due to different labor supply elasticities. Women, especially couples, have higher supply elasticities and decrease their labor supply to a larger extent when wages decrease as a consequence of the demand effect.

In order to open the black box, Table 2.6.2 demonstrates the iteration of hours changes and wage adjustments for all skill/household combinations. The numerical results accurately mirror the graphical representation of Figure 2.5.1.[19] Wage and hour changes are alternating in sign, due to the negative demand elasticities, and changes become smaller as the models converge. Furthermore, the table reveals different convergence velocities; for males in couples, for instance, the model converges after three iteration steps, whereas for medium-skilled females in couples the model iterates seven times until convergence is achieved. The table also illustrates the role of different elasticities. Although the initial percentage changes in hours are much larger for low-skilled women than for medium-skilled (25% vs. 7%), the model converges much more quickly due to the higher demand elasticities for the low-skilled, since a given hour change can be induced by a relatively smaller wage change. Thus, the less elastic the labor demand, the slower the convergence of the model.

[19] Taking the example of high-skilled single males, the first hours change of 0.11% corresponds to the distance $\overrightarrow{AB}$ in Figure 2.5.1. The first wage change (−0.20%) equals the distance $\overrightarrow{BC}$. The hours and wage changes in iteration stage 2 correspond to $\overrightarrow{CD}$ and $\overrightarrow{DE}$ respectively. Note that changes are reported in percent of the previous total hours or wages.

Table 2.6.2: Iteration process

#	High-skilled		Medium-skilled		Low-skilled	
	ΔHours	ΔWage	ΔHours	ΔWage	ΔHours	ΔWage
	Single men					
1	0.11	-0.20	5.12	-13.84	49.68	-47.32
2	-0.03	0.05	-1.65	4.47	-8.21	7.82
3	-	-	0.50	-1.35	0.52	-0.49
4	-	-	-0.15	0.41	-0.03	0.03
5	-	-	0.05	-0.13	-	-
	Single women					
1	-5.26	9.39	8.50	-22.96	30.56	-29.10
2	1.00	-1.79	-3.07	8.29	-1.85	1.77
3	-0.16	0.28	0.95	-2.57	0.15	-0.15
4	0.04	-0.07	-0.35	0.94	0.00	0.00
5	-	-	0.12	-0.31	-	-
6	-	-	-0.03	0.09	-	-
	Men in couples					
1	0.41	-0.74	1.07	-2.90	7.54	-7.18
2	-0.14	0.25	-0.18	0.48	-0.93	0.88
3	0.08	-0.15	0.03	-0.08	0.10	-0.09
	Women in couples					
1	3.21	-5.74	7.29	-19.71	24.96	-23.77
2	-0.77	1.37	-3.63	9.80	-4.08	3.89
3	0.17	-0.30	1.86	-5.04	0.47	-0.45
4	-0.05	0.08	-0.98	2.64	-0.08	0.08
5	-	-	0.42	-1.15	-	-
6	-	-	-0.15	0.41	-	-
7	-	-	0.03	-0.07	-	-

Source: Own calculations based on IZAΨMOD. *Notes:* All changes in percent.

As far as fiscal effects are concerned, Table 2.6.3 shows that the workfare reform does indeed increase the government budget – due to the unambiguously positive labor supply effect and the resulting increases in tax and social insurance contributions, combined with decreases in benefit payments. The table shows that the government budget increases by 31.7 billion euros, which – for the population sample – corresponds to approximately 9.4 percent. The countervailing demand effect, of course, reduces this positive budget effect to 27.7 billion euros (8.2%) compared to the status quo. In addition, the government has to finance the community-jobs for those people who remain dependent on government transfers. There are no clear estimates on how much these jobs would cost. Fuest and Peichl (2008) calculate annual administrative costs of about 4,200 euros per job, referring to estimations of the German Federal Employment Agency. The simulation results suggest that after labor supply adjustments, 3.8 million people would receive benefits and be required to work in a community job.[20] If we take into account the demand model the number increases to 4.1 million people employed in the workfare program. Hence, using a pure labor supply model, the net effect on government budget yields approximately 15.7 billion euros. When taking demand restrictions into account, the positive budget effect shrinks to 10.5 billion euros.

Table 2.6.3: Fiscal effects

	After LS Changes in billion €		After LS and LD Changes in billion €	
Tax revenue	1.2	0.9	0.9	0.7
Social insurance contributions	15.4	5.2	13.4	4.5
Benefit payments	15.1	17.0	13.4	15.2
Budget effect	31.7	9.4	27.7	8.2
Persons in workfare (in millions)	3.8	-	4.1	-
Costs of workfare	16.0	-	17.2	-
Total effect	15.7	-	10.5	-

Source: Own calculations based on IZAΨMOD.

Thus, the workfare reform increases government revenues and is feasible from a fiscal point of view. This is even more true, as the positive budget effect is a conservative estimate for mainly three reasons. Firstly, we overestimate the number of benefit

[20] Note that in the baseline the number of hypothetical individuals in workfare, i.e. the employable people receiving some kind of government transfers, is 5.4 million.

recipients, as we are not able to model benefit take-up rates in a reliable way. Secondly, there might be some people choosing not to take up community work (foregoing benefit payments). Thirdly, the full-time equivalent occupation does not necessarily have to be a community job. The work requirement could also be fulfilled by participating in a training program or by more actively applying for new jobs. Consequently, it is very likely that the number of workfare jobs is substantially smaller, making the reform even more feasible when accounting for demand effects.

2.6.3 Robustness checks

In order to test both the theoretical and empirical reliability of our approach, we perform several robustness checks. Firstly, we check the plausibility of the model with respect to different labor demand elasticities. We compare the baseline scenario with a low and a high-elasticity scenario. In the high (low) scenario, we increase (decrease) the wage elasticities presented in Table 2.4.1 by 20 percent. Table 2.6.4 summarizes the results.

It becomes evident that the higher the elasticities are in absolute terms, the smaller the offsetting demand effect (LS/LD). This finding confirms the insight obtained from examining the convergence pattern by skill type presented in Section 2.6.2. As expected, Table 2.6.4 shows that the model converges more quickly for all household types if the absolute value of the elasticity is higher.

Table 2.6.4: Elasticity sensitivity

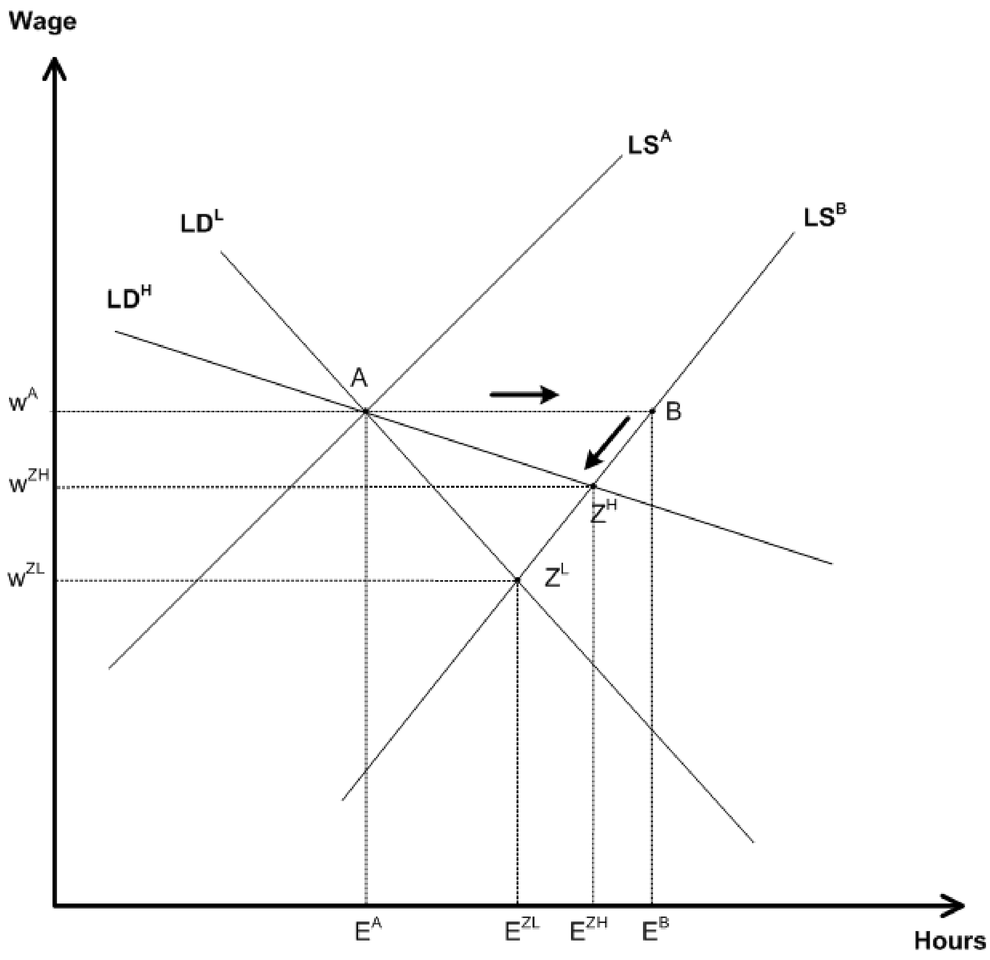
	Low-elasticity scen.		Baseline scen.		High-elasticity scen.	
Household type	LS/LD effect	Iter.	LS/LD effect	Iter.	LS/LD effect	Iter.
Single men	-29.84	5	-24.51	5	-20.89	4
Single women	-29.70	6	-24.02	6	-19.46	5
Men in couples	-13.73	4	-12.29	3	-10.40	3
Women in couples	-38.64	9	-30.09	7	-25.44	6
Overall	**-31.58**		**-25.31**		**-21.27**	

Source: Own calculations based on IZAΨMOD. *Notes:* Low/high-elasticity scenarios refer to elasticities in absolute terms.

This result is in line with theory and can be best explained graphically. Figure 2.6.1 is a simplification of the iteration process described in Figure 2.5.1 and shows the effect of a tax reform in presence of a low and high-elasticity demand curve (LD^L

and LD^H). In the LD^L-case, the wage reduction and the countervailing labor demand effect is higher than in the LD^H-case. The rationale behind this graphical finding is the following: if labor demand is more elastic, a given change in working hours can be achieved with a smaller change in the wage. Let us assume a fixed rise in working hours due to a tax reform. The higher the absolute value of the elasticity, the smaller is the wage decrease necessary to induce such a change in working hours. With the iteration process described above, this implies that the wage reactions and thus the effects of the demand module are smaller. As a result, the model converges more quickly.

Figure 2.6.1: The role of different elasticities



Source: Own presentation.

As a second robustness check, we simulated several other reform scenarios (for instance, different versions of flat tax reforms both increasing and decreasing government budgets as well as revenue neutral scenarios). In all cases, labor demand works as a stabilizer for the supply response, also when the initial tax reform reduces labor supply. In other words, if labor supply falls due to a reform, demand adjustments soften this effect, so that the resulting employment effect is less negative than the initial labor supply reaction. Moreover, we find that the magnitude of the offsetting labor demand effects is relatively stable and lies in the range of 15 to 35 percent, depending on the specific reform simulated and the size of the labor demand elasticities.

Third, we check the sensitivity of our results with respect to the specification of the labor supply and the labor demand model. We find that the (qualitative) results are independent of the concrete specification of the utility function. Also, the number of discrete labor supply choices and the method employed to calculate the residuals of the random utility model only marginally affect the quantitative results. As for the demand model, labor demand elasticities are robust with respect to the underlying cost function, the returns to scale and the inclusion of capital treated as a quasi-fixed input factor.

2.7 Conclusions

Structural labor supply models are important tools for the evaluation of policy reforms. Yet, most of the models ignore the demand side of the labor market. They either assume a perfectly elastic labor demand curve, so that labor supply effects are assumed to be equal to the employment effect, or simply restrict the analysis to the supply side. Employing a newly developed demand model based on detailed, linked employer-employee data for Germany, we show that, in line with earlier findings, labor demand is not at all perfectly elastic, but demand elasticities are finite, ranging from -0.37 to -1.05. It immediately follows that labor market estimates obtained from pure labor supply models are biased. Ex-ante policy evaluation studies must account for demand effects in order to produce reliable results.

In this paper, we propose a straightforward method to meet this necessity. We build a demand-supply link that iterates labor supply and demand adjustments until the partial labor market equilibrium is achieved. We make use of the estimated labor demand elasticities and calculate how a labor supply shift affects the gross wage. We then re-estimate labor supply, given the adjusted wages. This loop is repeated until the model converges.

In order to demonstrate the performance of our supply-demand link, we introduce a workfare reform for Germany, a counterfactual that yields unambiguously positive labor supply effects, while increasing the government budget. We find that labor demand plays a crucial role for the assessment of the policy reform. On average, labor demand adjustments offset the positive labor supply effect of the reform by 25 percent. Instead of the pure labor supply effect of around 1.5 million full-time jobs, the reform would yield roughly 1.1 million new jobs. Even after accounting for labor demand, the reform still increases government budget, making the reform proposal feasible from a fiscal point of view.

The new demand-supply link is an important extension for structural labor supply models. It makes employment predictions more accurate and consequently ex-ante policy recommendations more reliable. Moreover, the proposed method has several advantages compared to existing approaches that account for demand restrictions, such as Double Hurdle or CGE models. Our approach is general enough to be used in combination with any labor supply model; it is parsimonious as we restrict the analysis to the labor market; it accounts for heterogeneity, as we remain at the micro-level, enabling us to identify the precise adjustment process.

Nevertheless, there are shortcomings to the approach, which have to be addressed in future research. It would be desirable to not only attach a demand extension to an existing labor supply model but also to assume an integrated and comprehensive labor market model. This would enable us to relax the relatively strong assumption of a perfectly competitive labor market and to impose rigidities caused by, for example, efficiency wages, labor unions or search frictions. When assuming an integrated labor market, it would also be possible to allow labor demand to react to a tax reform and the induced supply changes.[21] In such a framework, the demand curve would shift and these shifts would have to be part of the iteration process as well. In addition, we have neglected dynamic as well as general equilibrium effects. For instance, the large shift in labor supply due to the workfare reform and the increase in the government budget can have intertemporal labor supply effects. Future taxes could be lower and

[21] At the moment, this kind of simultaneity is, however, hard to achieve due to practical reasons, as the usage of the LIAB dataset is only possible via remote access, making an iteration process for the labor demand prohibitively time consuming and unviable from a programmer's point of view.

due to these expected income effects, an individual might choose to work less today. In addition, the increased labor supply of women with children has implications for childcare arrangements which again might have (macroeconomic) feedback effects that are not taken into account in the present application. We leave all this for future research.

2.8 Appendix

Table 2.8.1: Estimation statistics of demand model

Model statistics		
Observations	11472	
Cost share S_2: chi^2 (p-value)	3274.5 (0.00)	
Cost share S_3: chi^2 (p-value)	7855.5 (0.00)	
Theoretical fit		
% predicted cost shares < 0	0	
% strict quasi-concavity	100	
% adding-up condition met	100	
Estimates	Coefficient	Standard Errors
α_2	-0.5180659	0.222145
α_3	4.100664	0.2055328
α_{22}	-0.0435001	0.0028518
α_{23}	0.0581057	0.0020848
α_{32}	0.0581057	0.0020848
α_{33}	-0.0301589	0.0020759
β_{2Y}	0.0050241	0.0002953
β_{3Y}	-0.0067779	0.0002734
δ_{2t}	0.0005713	0.0001111
δ_{3t}	-0.0019055	0.0001028
$d_{2IConstruction}$	0.0107481	0.001305
$d_{3IConstruction}$	-0.0135479	0.0012088
$d_{2ITrafficComm}$	0.071256	0.001942
$d_{3ITrafficComm}$	-0.0612755	0.0018033
$d_{2IServices}$	0.0201532	0.0009461
$d_{3IServices}$	-0.0640076	0.0008706

Source: Own estimations based on LIAB.

Chapter 3

Wage effects of corporate taxation*

3.1 Introduction

The debate about who bears the burden of corporate taxation has recently shifted from the theoretical to the empirical arena. A large number of theoretical contributions inspired by Harberger (1962)'s seminal paper suggest that labor bears a substantial share of the corporate tax burden.[1] However, there are only few empirical studies on the wage incidence of corporate taxation. The main reason for this lack of empirical evidence is that measuring the effect of corporate taxation on wages raises a number of difficult conceptual and econometric issues. First, conceptually, it is important to distinguish between different channels through which corporate taxes can affect wages. For instance, most theoretical studies emphasize that corporate taxes reduce wages because they reduce investment. But taxes may also affect the wage setting process, depending on the

* This chapter is based on a (so far unpublished) article entitled "Do Higher Corporate Taxes Reduce Wages? Micro Evidence from Germany" (joint with Clemens Fuest and Andreas Peichl, see Fuest et al., 2013). An earlier version circulates as "Which Workers Bear the Burden of Corporate Taxation and Which Firms Can Pass It On? Micro Evidence from Germany" (Fuest et al., 2012)

[1] The literature following Harberger (1962) extended the model to the open economy case (Diamond and Mirrlees, 1971; Bradford, 1978; Kotlikoff and Summers, 1987; Harberger, 1995), incorporated more sectors (Shoven, 1976) and introduced uncertainty (Ratti, 1977). Surveys are provided by Auerbach (2005) and Harberger (2006). Recent computational general equilibrium (CGE) models find that labor bears a substantial share of the corporate tax burden under reasonable assumptions (see Gravelle, 2013, for an overview).

way in which wages are determined. Then there may be general equilibrium effects of tax changes on wages and prices. Second, there needs to be sufficient exogenous variation in corporate tax rates. Third, the analysis must control appropriately for the economic environment in which the tax changes occur. Clearly, the latter two conditions are necessary to establish a quasi-experimental setting which is crucial for identification.

It is difficult to find a research design dealing with all of these issues in a satisfactory manner. One avenue is to use cross-country data (Hassett and Mahur, 2006; Felix, 2007; Desai, Foley, and Hines, 2007), which is helpful to capture general equilibrium effects. Yet, cross-country studies on the wage incidence of corporate taxes usually exploit differentials in country-specific tax rates over time and therefore often have troubles to defend the common trend assumption: in general, it is not likely that differences in the wage growth paths of e.g. Germany and the U.S. can be purely attributed to changes in national corporate tax policies.

An alternative to using cross-country data is to look at a single country and to exploit regional and/or industry-specific variation in corporate taxes to identify the wage incidence (Dwenger, Rattenhuber, and Steiner, 2011; Liu and Altshuler, 2013). Here, the common trend assumption is arguably more credible, while the variation in tax rates is often not as clear as in the first group of studies. The influential paper by Arulampalam, Devereux, and Maffini (2012) acknowledges this problem by exploiting both cross-firm and cross-country variation in tax *burdens*. Nevertheless, as the authors admit, studies using cross-country firm data are normally not able to capture the general equilibrium effects of national corporate taxes. Therefore the authors focus on what they call the direct wage effect, which arises in a collective bargaining setting. The analysis leaves aside the indirect wage effect through investment as well as other general equilibrium effects on prices and other relevant variables.

In this paper, we exploit the specific institutional setting of the German local business tax to achieve a clean identification of the full corporate tax incidence on wages. From 1998 to 2008, on average 8% of the 11,441 German municipalities adjusted their local business tax rates per year. These municipalities face the same overall economic conditions and are therefore comparable so that the necessary common trend assumption is likely to hold.[2] Moreover, since municipalities can be interpreted as small open

[2] A similar set-up is used by Felix and Hines Jr. (2009) who exploit the variation in corporate tax rates among U.S. states. Yet, their results are based on a single cross-section, which makes it impossible to

economies within the highly integrated German national economy, with close to perfect mobility of capital and labor across (municipal) borders, we are confident to measure the full incidence of corporate tax changes, not just the direct effect. The reason is that general equilibrium effects on interest rates and other variables that might affect wages will be negligible in this setting.

We set up a theoretical model that allows us to study the incidence of a local corporate tax in an economy with different types of labor (skilled and unskilled) and where wages in some firms are determined through collective bargaining. In the model the full incidence of corporate taxation can be decomposed into a direct effect related to rent division in collective bargaining and an indirect effect through the adjustment of investment and employment. We then test the theoretical model combining administrative panel data on the universe of the German municipalities with rich administrative linked employer-employee microdata taken from German social security records.

Our empirical findings are as follows: First, in line with the theoretical model, we find parametric and non-parametric evidence for a sizeable direct wage effect of corporate taxation. We estimate a direct wage elasticity with respect to the effective corporate tax rate of −0.35. In money terms this implies that a one euro increase in annual tax liabilities yields a 50 cent decrease of the annual wage bill, which is in line with findings of other recent empirical studies (Arulampalam et al., 2012; Liu and Altshuler, 2013). The incidence increases to −67 cents when including one lag of the corporate tax rate. Second, we find only a small indirect effect on wages. This is consistent with labor being mobile across municipalities.[3] In sum, we estimate a full incidence, consisting of the direct and the indirect effect, of −77 cents, which means that raising one euro of corporate tax revenue reduces local wages in unionized firms by 77 cents, roughly three quarters of the revenue raised.

The negative direct wage effect is larger if collective bargaining takes place at the firm level rather than at the sectoral level, which is in line with the theoretical model. High and medium-skilled workers experience relatively higher wage losses than low-skilled workers if corporate tax rates increase. One explanation would be that medium and high-skilled workers have more bargaining power and thus capture a higher share of

control for potential state fixed effects.

[3] Intra-regional employment effects of the German local business tax are analyzed in a companion paper (Siegloch, 2013), see Chapter 4.

the rent generated by the firm so that they also lose more if taxes diminish this rent. We run several tests to show that our estimates are well-identified. Reassuringly, including job or labor market region time trends does not render our estimates, which makes it unlikely that our estimates are biased by unobserved, time-variant confounders. Moreover, a placebo test on non-liable firms shows a zero effect of the local business tax on wages.

Our study adds to the existing literature in four dimensions. First and most importantly, this is the first study on the wage incidence of corporate taxes using linked employer-employee data. In addition to administrative wage data, exploiting firm information is crucial to (i) take into account that only certain types of firms pay the local business tax while other types of firms are exempt by law, (ii) identify the wage setting and collective bargaining status of the firm, (iii) differentiate between corporate and non-corporate firms. Moreover, we are able to estimate heterogeneous worker and firm effects and look at the channels of how the tax burden is passed on to workers, which proves to be important in the empirical analysis. Second, this study is the first to exploit compelling variation in tax rates induced by numerous local tax reforms in order to cleanly identify the tax incidence on wages, while keeping the overall economic environment constant.[4] Third, the particular institutional setting of the German business tax allows us to estimate the full wage incidence taking into account the direct and the indirect effect of corporate tax changes. Finally, in our theoretical analysis, we extend the model of Arulampalam et al. (2012) by allowing for different skill levels and by distinguishing between firm and sector level bargaining.

The rest of the paper is structured as follows. In Section 3.2 we describe the German corporate tax legislation with a focus on the German local business tax and give a short overview of wage bargaining arrangements in Germany. In Section 3.3 we set up a theoretical wage bargaining model that features the particular German setting to

[4] The only other study with a similar set-up is Bauer, Kasten, and Siemers (2012), which was conducted simultaneously and independently of our study (cf. the earliest version of this study Fuest et al., 2011). A closer look at Bauer et al. (2012) shows, however, that they do not have information on the municipality (*Gemeinde*) in their data. Hence, they run the analysis on the more aggregate county (*Kreis*) level (there are roughly 11,400 municipalities vs. 400 counties). This makes the variation imprecise since annual tax changes occurring in only 8% of the municipalities lead to variation of average tax rates in 65-75% of the counties. Thus, firms (and wages) in unaffected municipalities are wrongly exposed to the county's average changes. Moreover, the authors are lacking relevant firm information since they are not using linked employer-employee data.

demonstrate how municipal corporate taxes affect the wages of heterogeneous workers. Section 3.4 presents the datasets. Empirical results are shown and discussed in Section 3.5, while Section 3.6 concludes.

3.2 Institutional background

3.2.1 Corporate taxation in Germany

Corporate firms (*Kapitalgesellschaften*) face two profit taxes in Germany, the local business tax (LBT) and the corporate tax (CT) levied by the federal government. The local business tax applies to both corporate and non-corporate firms (*Personengesellschaften*)[5], while most firms in the agricultural and public sector are not liable to the LBT. Moreover, certain liberal professions such as journalists, physicians or lawyers are exempt. The tax base, Y, is the same for both LBT and CT and essentially consists of operating profits since 1998.[6] Importantly, until 2007 the LBT due could be deducted from the tax base. The tax rate of the local business tax, τ_{LBT}, consists of two components: the basic federal rate (*Steuermesszahl*), τ_{fed}, which is set at the national level, and the collection rate (*Hebesatz*), cr, set at the local level. Thus $\tau_{LBT} = \tau_{fed} \cdot cr$. τ_{fed} was at 5.0% from 1998 to 2007 and decreased to 3.5% in 2008. The collection rate usually varied between 250% and 450% in the period from 1998 to 2008 (5th and 95th percentiles). The collection rates for year t are passed by the municipal councils during the budgeting for t, which usually take places in the last three months of year $t-1$. It is important to note that a municipality can only adjust the collection rate which applies to all (liable) firms in the municipality; it can neither change the tax base nor liability criteria, which are both set at the federal level.

The CT rate, τ_{CT}, has undergone several changes in recent years. Until 2000 an imputation system existed in Germany, where retained profits where subject to a tax rate of 45% in 1998 and 40% in 1999 and 2000 – dividends were taxed at a rate of 30% from 1998 to 2000. As of 2001 retained and distributed profits were equally taxed at 25% (26.5% in 2003). In 2008 τ_{CT} was lowered to 15%. In all years, a so-called

[5] Taxation of non-corporate firms will be discussed in Section 3.5.6.

[6] From 1998 to 2007 half of the long-term debt service was added to Y. As of 2008 the long-term debt services was replaced by 25% of all interest payments exceeding 100,000 euros.

solidary surcharge, *soli*, of 5.5% of the corporate tax rate is added.

In order to calculate the total effective tax rate for corporate firms, first, LBT and CT rates are added. Second, the deduction of the LBT liabilities from the tax base has to be taken into account. The effective (statutory) marginal tax rate[7] for corporate firms, τ_{EMTR}^{corp}, from 1998 to 2007, is

$$\tau_{EMTR}^{corp} = \frac{\tau_{CT} \cdot (1 + soli) + \tau_{fed} \cdot cr}{1 + \tau_{fed} \cdot cr}.$$

Since 2008 the denominator is 1, as the LBT cannot be deducted from the tax base anymore. Assuming a collection rate of 350%, the average EMTR decreased from 0.55 in 1998 to 0.28 in 2008 with an average value of 0.41 over the whole sample period. For a collection rate of 250% (450%) the average τ_{EMTR}^{corp} is 0.38 (0.43).

3.2.2 Wage bargaining in Germany

As our theoretical and empirical analysis takes into account collective bargaining, we briefly sketch the situation of labor unions in Germany. Traditionally, German labor unions have been influential. Collective bargaining agreements (CBAs) at the industry-level are the most important bargaining mechanism for wage determination. Nevertheless, there has been a significant decline in bargaining coverage. In West (East) Germany, the total proportion of employees covered by CBA decreased from 76% (63%) in 1998 to 65% (51%) in 2009; the share of workers covered by sectoral agreements fell from 68% (52%) to 56% (38%) (Ellguth, Gerner, and Stegmaier, 2012). Firms may pay wages in excess of wages stipulated in CBAs (*Günstigkeitsprinzip*). If they want to pay lower wages, in contrast, opening clauses negotiated between unions and firms are required. Effectively, there is a number of cases where bargaining takes place at the firm level. This applies in the case of wage payments in excess of CBAs, opening clauses or company agreements (*Firmentarifvertrag*). Some employers are not at all covered by a CBA and can completely rely on individual contracts with each employee. Note that with a few exceptions there is no legal minimum wage in Germany. However, the social security and welfare system provides an implicit minimum wage and CBAs ensure that

[7] Note that this is an effective *statutory* marginal tax rate, as opposed to more conventional measures of the effective marginal tax rate which include tax base parameters.

wages are above a certain level (*Lohnabstandsgebot*). The average duration of a CBA increased from 12 months in 1991 to 22 months in 2011.[8] Usually, negotiations take place in the first half of a year.

3.3 Theoretical framework

Consider an economy which consists of n jurisdictions. There are many firms in each jurisdiction. Firms use the following factors of production: capital (K), which is homogeneous, and workers with differing skill levels. There are 2 skill levels. Labor of skill type k, $k = 1, 2$, is denoted by L^k. The production function $F(K, L^1, L^2)$ may differ across firms but is assumed to have the usual neoclassical properties and exhibits declining returns to scale in capital and labor, i.e. there is an implicit fourth factor, which can be thought of as a location specific rent. Capital and both types of labor are mobile across municipal borders. This is a strong assumption when it comes to labor mobility within a large country. However, within a labor market region that may include a fairly large number of jurisdictions (in Germany there are on average 44 municipalities per labor market region), it is plausible that employees are highly mobile – at least at the margin. Firms are immobile, due to the location specific rent. Note that in our data we observe only few firms changing municipality.

Firm profits are taxed by the individual jurisdictions and by the central government. The rate of the local profit tax in jurisdiction i is denoted by t_i; the rate of the profit tax levied by the central government is denoted by T. Both taxes have the same base, apart from the fact that the local tax is deductible from the base of the profit tax levied by the central government. The after tax profit of the representative firm located in jurisdiction i is given by

$$P_i = [F_i(K_i, L_i^1, L_i^2) - \sum_{k=1}^{2} w_i^k L_i^k](1 - \tau_i) - (1 - \alpha\tau_i)rK_i, \qquad (3.3.1)$$

where w_i^k is the wage for labor of skill type k, $\tau_i = T + t_i(1 - T)$ is the effective statutory

[8] In the chemical industry, for instance, the CBA was renewed in 2002, 2003, 2004, 2005, 2007, 2008, 2010, 2011, while in the steel industry new collective agreements were negotiated in 2002, 2003, 2005, 2006, 2008, 2009, 2010. The firm-level CBA of Volkswagen was renewed in 2002, 2004, 2006, 2007, 2009, 2010, 2011. See the WSI Collective Agreement Archive for more information http://www.boeckler.de/wsi-tarifarchiv_39335.htm.

tax rate on profits and r is the non-tax cost of capital.[9] The variable α is the share of the capital costs which can be deducted from the tax base. In line with most existing tax systems, we assume $0 < \alpha < 1$, which implies that capital costs are partly but not fully deductible.

Firms operate under conditions of perfect competition in output and input markets, with the exception of the labor market. There is a dual labor market with two types of firms. In the first type, workers are represented by trade unions and wages are set via bargaining. In the second type, no unions exist and wage setting is competitive. To simplify notation we normalize the number of firms per type and jurisdiction to unity. In unionized firms wages are set according to a standard efficient bargaining model, where unions and firms bargain over wages and employment.[10] Each skill type is represented by one trade union.[11] Bargaining takes place either at the firm level or at the sector level.

3.3.1 Firm level bargaining

We start by assuming that bargaining takes place at the firm level. Each firm negotiates with all unions simultaneously (Barth and Zweimüller, 1995). The objective function of the union representing the workers of skill type k in firm i is given by

$$Z_i^k = L_i^k w_i^k.$$

The reservation wage for workers in unionized firms is $\overline{w}^k$, the wage rate of skill group k in the competitive labor market. Therefore the fall back utility of the union is $\overline{Z}_i^k = L_i^k \overline{w}^k$. We assume that the reservation profit of the firm is equal to zero.[12]

After wages and employment levels are determined, firms set K_i to maximize profits,

[9] If firms operate in more than one jurisdiction the local tax in Germany is determined through formula apportionment. This has implications for the incidence of the tax. We analyze this case theoretically in Appendix A (Section 3.7). In our empirical analysis of incidence effects we find no significant differences between multi and single-establishment firms.

[10] It is straightforward to show that our key results regarding the incidence of the profit tax on wages would be very similar if we used a model where unions and firms only bargain over wages (see, e.g., Oswald, 1993).

[11] Assuming that there is one union which represents all skill levels would lead to the same qualitative results regarding the impact of corporate tax changes.

[12] An alternative assumption would be that the other skill groups work and receive their wages and that output and investment would be lower than in the case of agreement. This would add notation without changing the signs of the results derived below.

which implies

$$\frac{\partial F_i(K_i, L_i^1, L_i^2)}{\partial K_i} = R_i, \tag{3.3.2}$$

where R_i denotes the cost of capital, which is given by

$$R_i = r\frac{(1-\alpha\tau_i)}{(1-\tau_i)}.$$

The outcome of the wage bargaining process is given by

$$w_i^{k*}, L_i^{k*} = \arg\max_{w_i^k, L_i^k} \Omega_i^k,$$

where

$$\Omega_i^k = \beta^k \ln(Z_i^k - \overline{Z}_i^k) + (1-\beta^k)\ln P_i.$$

The parameter $\beta^k \in (0,1)$ stands for the relative bargaining power of skill type k union. The first order conditions of the bargaining problem can be rearranged to yield

$$w_i^{k*} = \overline{w}^k + \frac{\beta^k}{(1-\beta^k)}\frac{P_i}{L_i^k(1-\tau_i)} \tag{3.3.3}$$

and

$$\frac{\partial F(K_i, L_i^1, L_i^2)}{\partial L_i^k} = \overline{w}^k \quad k = 1,2. \tag{3.3.4}$$

The equilibrium wage rate in unionized firms is equal to the skill specific reservation wage plus a share of the firm's profit per worker. The size of this share depends on the bargaining power of the trade union. Employment is set so that the marginal productivity of labor is equal to the reservation wage.

The focus of our analysis is how changes in local profit taxes affect wages. We distinguish between two channels through which taxes affect wages. Following Arulampalam et al. (2012), we refer to the first channel as the direct impact. Through wage bargaining workers receive part of the profits generated by the firm. If higher taxes reduce these profits, this will affect wages for given levels of capital and labor inputs. The second channel, referred to as the indirect channel, is the change in wages caused by the adjustment of *other* input factors or input prices as a reaction to the change in tax. To define the two effects more precisely in our model, we can solve equation (3.3.3) for the equilibrium wage rate of skill type k:

$$w_i^{k*} = \frac{1}{(1-\beta^k\beta^j)L_i^k}[\overline{w}_i^k(1-\beta^k)L_i^k - \overline{w}_i^j(1-\beta^j)\beta^k L_i^j$$
$$+ (1-\beta^j)\beta^k\left(F_i(K_i, L_i^1, L_i^2) - R_i K_i\right)], \quad k \neq j. \tag{3.3.5}$$

Equation (3.3.5) is the focus of our interest. Note first that equations (3.3.2) and (3.3.4) define the factor demand equations as functions of the skill specific reservation wage and the cost of capital, $K_i(R_i, \overline{w}_i^1, \overline{w}_i^2)$, $L_i^1(R_i, \overline{w}_i^1, \overline{w}_i^2)$, $L_i^2(R_i, \overline{w}_i^1, \overline{w}_i^2)$. Given this, equation (3.3.5) defines the wage rate of skill group k as a function of the cost of capital, the firm's factor inputs and a number of other variables like relative bargaining power, i.e. $w_i^{k*} = w_i^{k*}(\tau_i, K_i(R_i, \overline{w}_i^1, \overline{w}_i^2), L_i^k(R_i, \overline{w}_i^1, \overline{w}_i^2), L_i^j(R_i, \overline{w}_i^1, \overline{w}_i^2), ...)$. Differentiating (3.3.5) with respect to τ_i allows us to express the overall effect of a change in τ_i on w_i^{k*} as

$$\frac{\partial w_i^{k*}}{\partial \tau_i} = -\frac{(1-\beta^j)\beta^k}{(1-\beta^k\beta^j)}\frac{K_i}{L_i^k}\frac{\partial R_i}{\partial \tau_i} - \frac{(w_i^{k*} - \overline{w}^k)}{L_i^k}\frac{\partial L_i^k}{\partial R_i}\frac{\partial R_i}{\partial \tau_i}$$
$$- \frac{\overline{w}_i^j(1-\beta^j)\beta^k}{(1-\beta^k\beta^j)L_i^k}\frac{\partial L_i^j}{\partial R_i}\frac{\partial R_i}{\partial \tau_i}, \quad k \neq j \tag{3.3.6}$$

This overall effect can be expressed as the sum of a direct effect and an indirect effect:

$$\frac{\partial w_i^{k*}}{\partial \tau_i} = \textit{direct effect} + \textit{indirect effect}$$

where

$$\textit{direct effect} \equiv -\frac{(1-\beta^j)\beta^k}{(1-\beta^k\beta^j)}\frac{K_i}{L_i^k}\frac{\partial R_i}{\partial \tau_i} < 0; \tag{3.3.7}$$

$$\textit{indirect effect} \equiv -\left(\frac{(w_i^{k*} - \overline{w}^k)}{L_i^k}\frac{\partial L_i^k}{\partial R_i} + \frac{\overline{w}_i^j(1-\beta^j)\beta^k}{(1-\beta^k\beta^j)L_i^k}\frac{\partial L_i^j}{\partial R_i}\right)\frac{\partial R_i}{\partial \tau_i} \gtreqless 0, \tag{3.3.8}$$

with

$$\frac{\partial R_i}{\partial \tau_i} = r\frac{(1-\alpha)}{(1-\tau_i)^2} > 0.$$

The direct effect is unambiguously negative. The reason is that an increase in the tax rate reduces the quasi rent generated by the firm. Since unions capture part of this rent, workers lose if this rent becomes smaller. Note also that the magnitude of the direct effect is increasing in the bargaining power of the skill group β^k. This means that skill groups with a lot of bargaining power are more likely to bear a larger burden of profit taxes. Again the explanation is that these groups capture a higher share of the rent in the first place.

The sign of the indirect effect is in general ambiguous. Interestingly, the change in investment caused by the tax change has no (immediate) effect on the wage. The explanation is straightforward. It follows from equations (3.3.2) and (3.3.5) that, if the firm is at its profit maximizing investment level, $\frac{\partial w_i^{k*}}{\partial K_i} = 0$. A marginal change in investment does not change the profit generated by the firm so that the equilibrium wage, which is a share of this rent, does not change either. However, a change in investment will affect employment. Likewise a marginal change in employment does not change the overall rent either, but it does affect the rent per worker. Therefore the sign of the indirect effect in this model only depends on how a change in corporate taxes affects the number of workers in the skill group. How exactly the number of high and low-skilled workers is affected depends on the properties of the production function including the complementarity between high and low-skilled labor and capital, as we show in Appendix A (Section 3.7).[13] For our empirical analysis one should bear in mind that, according to the theory, the indirect effect is transmitted through adjustment in employment, not investment.

These findings may be summarized as:

Result 1: *Direct effect*: For given factor input levels, an increase in the corporate tax rate reduces the wage rate of all skill groups in unionized firms.

Result 2: *Indirect effect*: The indirect effect of a corporate tax change on wages may be positive or negative.

3.3.2 Sector level bargaining

It is an important aspect of our analysis that the effects of corporate tax changes on wages may not only differ across skill groups but also across firms with different characteristics. One important difference between firms is that wage bargaining institutions may differ. So far we have assumed that bargaining takes place at the firm level. In

[13] For instance, if all factors of production are complements, i.e. $\frac{\partial^2 F}{\partial L^k \partial K}, \frac{\partial^2 F}{\partial L^k \partial L^j} > 0$, $k = 1, 2, k \neq j$, an increase in the corporate tax rate unambiguously reduces demand for both types of labor. But if some factors are substitutes results may be different.

many countries including Germany, wage bargaining can also take place at the sectoral level. In this case, if firms in a sector are located in many different municipalities, one would expect that the impact of a change in the local corporate tax in one municipality has a small or possibly a negligible effect on the wage rate.

We model wage negotiations at the sector level as follows. Assume that $m < n$ unionized firms in the economy belong to one sector. Wages for each skill group are identical in all firms, and the objective function of the union is given by $\sum_{i=1}^{m} Z_i^k = \sum_{i=1}^{m} L_i^k w^k$. It is not quite clear what bargaining over employment means at the sector level. In the following we assume that employer associations and unions bargain over employment in each individual firm. This means that the result of the bargaining process is a uniform wage for all workers of skill level k in a sector and a vector of employment levels $L_1^k, ..., L_m^k$.[14] Firms pursue the objective to maximize the sum of their profits $\sum_{i=1}^{m} P_i$. The derivation of equilibrium employment levels and wages for the two skill groups is equivalent to the derivation described in the preceding section. The equilibrium wage rate for skill group k is now given by

$$w^{ks*} = \frac{\Theta}{(1-\beta^k\beta^j)\sum_{i=1}^{m}[L_i^k(1-\tau_i)]} \qquad k \neq j, \tag{3.3.9}$$

where

$$\Theta = \overline{w}^k(1-\beta^k)\sum_{i=1}^{m}[L_i^k(1-\tau_i)] - \overline{w}^j(1-\beta^j)\beta^k\sum_{i=1}^{m}[L_i^j(1-\tau_i)] + (1-\beta^j)\beta^k\left(\sum_{i=1}^{m}[(F_i(K_i, L_i^1, L_i^2)(1-\tau_i) - (1-\alpha\tau_i)rK_i)]\right) \tag{3.3.10}$$

It is straightforward to show that, as soon as at least two firms in a sector are located in different jurisdictions, the direct effect of a tax change on union wages in the jurisdiction where the tax change takes place is smaller in magnitude than in the case of firm level bargaining. We give the formal proof in Appendix A (Section 3.7). The result is intuitive - wage bargaining will be influenced by taxes in all jurisdictions where the

[14] An earlier version of this paper used the seniority model of wage bargaining (Oswald, 1993), where unions are dominated by workers who are interested in higher wages but not in the level of employment. In this framework the issue of bargaining over employment at the sector level does not arise. This model yields similar results in terms of the level of collective bargaining.

sector has a presence so that a tax change in just one jurisdiction has a limited impact. The indirect effect is not necessarily smaller because different firms may have different production functions so that their input demand may react differently to corporate tax changes. This can be summarized as

Result 3: The direct effect of a change in the local corporate tax rate on wages in unionized firms is smaller under sector level bargaining, compared to firm level bargaining.

Note that Results 1 and 2 still hold under sector level bargaining.

3.3.3 Corporate tax incidence in the competitive sector

Consider finally the firms in the competitive sector. Since capital and both types of labor are mobile across jurisdictional borders, competitive wages are determined in the national labor market. Since each municipality is small, relative to the economy as a whole, a change in the local tax rate will leave wages in the competitive sector unchanged. The tax change will affect factor inputs but not factor prices (see Siegloch, 2013, and Chapter 4). This may be stated as

Result 4: In firms without union bargaining, the direct and indirect effect of a change in the local corporate tax rate on wages is equal to zero.

Clearly, this result is specific to local corporate tax incidence. Given that labor mobility across national borders is typically low, the incidence of a nationwide corporate tax would be different.

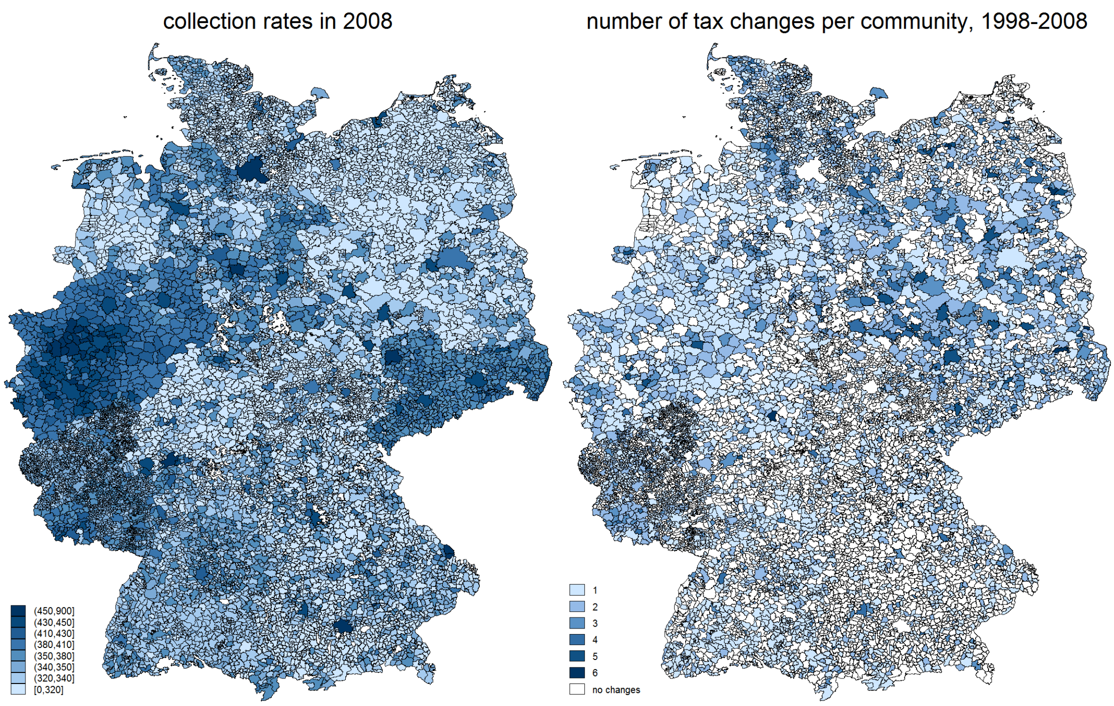
3.4 Data

For our analysis we combine two distinct data sources: first, administrative data on the universe of German municipalities containing information on their fiscal and budgetary situation (Section 3.4.1), and, second, detailed administrative linked employer-employee data from social security records (Section 3.4.2).

3.4.1 Municipality data

As far as municipal data are concerned, we make use of statistics provided by the Statistical Offices of the 16 German federal states (*Statistische Landesämter*). The states collect information on the fiscal and budgetary situation of the municipalities. We combine and harmonize the annual state specific datasets and construct a panel on the universe of municipalities from 1998 to 2008 covering roughly 125,000 data points – i.e. municipality-years. Most importantly, the dataset contains information on the local collection rate, but also on the population size and municipal expenses and revenues. Moreover, we add data from the German federal employment agency on regional unemployment rates on the more aggregate county (*Kreis*) level to control for local labor market conditions. As all these regional factors have been found to affect local business tax rates (Büttner, 2003), it is important to control for them in the empirical analysis below.

Figure 3.4.1: Cross-sectional and time variation in collection rates



Source: Statistical Offices of the Länder

Figure 3.4.1 depicts Germany's 11,441 municipalities and visualizes the substantial cross-sectional and time variation in collection rates. While the left panel of the figure shows the cross-sectional variation in local tax rates in 2008 with darker colors showing higher tax rates, the right panel shows the number of tax changes a municipality has experienced during the observation period 1998-2008 with darker colors indicating more changes.

Table 3.4.1: Share of communities with changing collection rates (in %)

	$\Delta\tau \neq 0$	$\Delta\tau > 0$	$\Delta\tau < 0$
Total	8.1	7.2	0.9
	by Year		
1999	5.4	4.3	1.1
2000	8.4	7.4	1.0
2001	12.7	11.5	1.3
2002	8.6	7.9	0.7
2003	9.8	9.1	0.8
2004	8.8	8.2	0.6
2005	11.0	10.4	0.7
2006	7.8	7.0	0.8
2007	4.4	3.7	0.8
2008	4.0	3.2	0.8

Source: Statistical Offices of the Länder. *Note:* The average change is 21 points (corresponding to an increase of 1.3% in the effective statutory tax rate). N=11,441 per year.

We now take a closer look at the within-municipality time variation of the collection rates, which is later used to identify the tax effect on wages. Table 3.4.1 shows that on average 8% of the municipalities (i.e. about 1,000 municipalities) change their collection rate per year. The average change amounts to 21 points, which corresponds to an increase of the effective statutory tax rate of 1.3% for a corporate firm during that period. Most municipalities increase collection rates over time and most of the increases in collection rates occurred between 2000 and 2006. Next, Table 3.4.2 shows that the changes in collection rates are not concentrated among a few communities but rather widespread. More than half of the communities have changed their tax rates at least once during the observation period. Furthermore, one third of the communities were affected at least by one big tax change, defined as a change equal to or greater than the mean change of 21 points.

Table 3.4.2: Number of tax changes per community, 1998-2008

	any change		big change	
# changes	# municipalities	in %	# municipalities	in %
0	4977	43.50	7575	66.21
1	4376	38.25	3376	29.51
2	1552	13.57	430	3.76
3	402	3.51	57	0.50
4	96	0.84	2	0.02
5	32	0.28	1	0.01
6	6	0.05	0	0.00

Source: Statistical Offices of the Länder. *Note:* The average change is 21 points (corresponding to an increase of 1.3% in the effective statutory tax rate). A big change is defined as an increase in more than 20 points. The average big change is 31 points (equal to an EMTR increase of 2.1%).

3.4.2 Worker and firm data

We combine the administrative municipal data with the linked employer-employee dataset (LIAB) provided by the Institute of Employment Research (IAB) in Nuremberg, Germany (Alda et al., 2005). The employee data are a 2% sample of the administrative employment statistics of the German Federal Employment Agency (*Bundesagentur für Arbeit*), called the German employment register, which covers all employees paying social security contributions or receiving unemployment benefits (Bender et al., 2000). Note that civil servants are typically not observed in the social security data. The employee information are recorded on June 30th of each year and include information on wages, age, tenure, occupation, employment type (full-time or part-time employment) and qualification. Individuals with missing information are excluded. Our worker panel consists of between 1.6 and 2.0 million workers annually observed from 1998 to 2008.

Importantly, wages are right censored at the ceiling for the social security contributions. Although the ceiling is quite high with annual labor earnings of 63,400 euros in 2008 for Western Germany, more than 10% of the observations are censored. In principle, there are two ways to deal with this problem: impute the censored wages or exclude the observations. We opt for the latter alternative and exclude all workers from the baseline sample which have at least once earned a wage above the contribution ceiling during the observation period. There are two reasons for this rigorous treatment, which particularly affects high-skilled workers.[15] First, as we will argue below, the estimated wage effect is a lower bound. Second, given that the imputation method cannot replicate the true data generating process, imputing parts of the wages creates an artificial variation in the left-hand side variable, which might lead to biased conclusions. In fact, if corporate taxes do affect wages, one must control for them in the imputation stage and would create endogeneity per definition. We check the sensitivity of our results with respect to the treatment of censored wages and find large differences in the results for high-skilled workers.

The firm component of the LIAB is the IAB Establishment Panel (Kölling, 2000), which is a stratified random sample of the universe of all German establishments. The term establishment refers to the fact that the observational unit is the individual plant, not the firm; there can be several plants per firm.[16] The employer data covers establishments with at least one worker for whom social insurance contributions were paid. We extract the following variables: value added, investment, number of employees, industry, total wage bill, legal form, union wage status (industry, firm or no collective agreement), self-rated profitability[17], presence of a work council.

Sample selection and descriptive statistics. We restrict our baseline sample to full-time workers in corporate firms in the manufacturing industry liable to the local business tax. We exclude part-time and marginally employed workers to rule out adjustments at different margins (notably hours of work) and solely focus on the wage effect. As stated above, we exclude all workers that have at least once earned a wage above the contribution ceiling during the observation period. As far as firm characteristics are concerned, the choice of focusing on firms that are liable to the LBT is obvious – yet we use non-liable firms for a placebo test below. We further narrow the baseline sample to corporate firms since the effective statutory marginal tax rate for non-corporate firms cannot be calculated given the information in the LIAB. Nevertheless, we estimate the wage incidence for non-corporate firms as a sensitivity check, making assumptions on

[15] We differentiate between three skill groups: high-skilled workers have obtained a college/university degree; medium-skilled have either completed a vocational training or obtained the highest high school diploma (*Abitur*); low-skilled have neither completed a vocational training nor obtained the *Abitur*.

[16] The LBT base of firms with multiple establishments is divided between municipalities according to formula apportionment based on the wage bill of the individual establishments.

[17] The survey question asks for a self-assessment of the profit situation on a five-point scale ranging from very good to unsatisfactory. We dichotomize the variable into good and poor profitability.

the personal income tax rate. Last, we focus on manufacturing firms, which are the backbone of the German economy and make up the largest share of the corporate firm sample (66%). Again, we present effects for other industries as an extension.

Table 3.4.3: Descriptive statistics, baseline sample, LIAB 1998-2008

	mean	sd	min	max	N
monthly wage	3168	812	421	5510	4086275
high-skilled wage	3733	868	441	5509	145593
medium-skilled wage	3210	819	421	5509	3113870
low-skilled wage	2911	686	466	5510	826812
age	41	10	16	64	4086275
tenure	11	8	0	34	4086275
share: male	0.81	0.39	0	1	4086275
share: blue collar	0.83	0.37	0	1	4086275
share: white collar	0.17	0.37	0	1	4086275
employees (fulltime)	327	1446	1	47695	14724
annual value added (in 1000)	38834	225593	9	9066228	14724
annual investments (in 1000)	4159	29702	0	1755000	14724
share: sector union contract	0.46	0.50	0	1	14724
share: firm union contract	0.11	0.31	0	1	14724
share: no union contract	0.44	0.50	0	1	14724
share: stand alone plant	0.71	0.45	0	1	14724
share: part of multi-plant firm	0.29	0.45	0	1	14724
collection rate (in %)	348	42	150	490	6894
population (in 1000)	26.31	99.87	0	3425	6894
local unemp. rate	0.12	0.06	0	0	6894
municipal revenues (in millions)	48.61	202.63	4	4416	6894
municipal expenses (in millions)	43.46	194.37	4	5971	6893

Source: LIAB and Statistical Offices of the Länder. *Note:* All money variables in 2008 euros.

Table 3.4.3 shows descriptive statistics of the baseline estimation sample. The average monthly wage in our sample is 3,168 euros (all money variables are in 2008 euros). Wages are increasing in qualification. The average age is 41, the average firm specific tenure 11 years. Men are clearly over-represented. The share of high-skilled workers is very low due to the strict treatment of censored wages (the share in the whole sample is 14%). At the same time, low-skilled are over-represented compared to the full sample (20% vs. 14%). The average firm in the sample has 327 employees with an annual value added of 38.8 million euros. 46% (11%) of the firms have a sector (firm) level collective bargaining agreement in place, while 29% of the plants are part of a multi-establishment company. The average plant is located in a municipality with 26,000 inhabitants, a regional unemployment rate of 12% and collection rate of 348%.

3.5 Empirical results

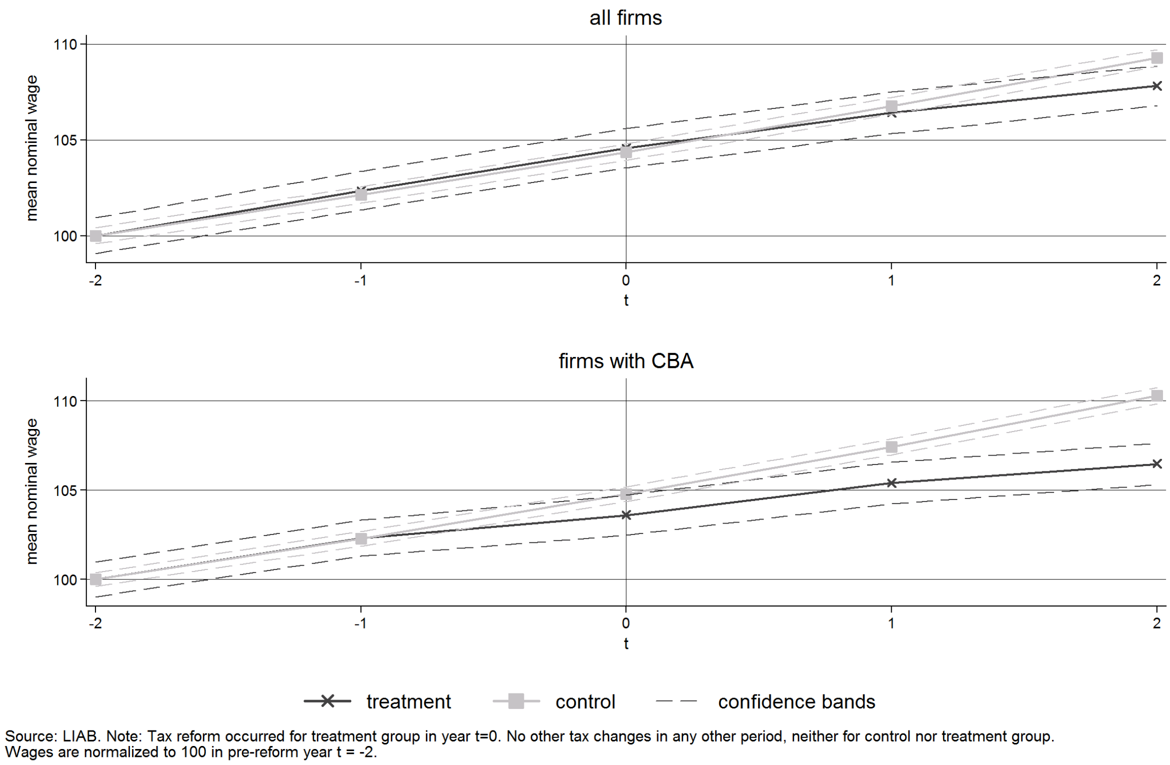
In the following section, we estimate the incidence of corporate taxation on wages. We start off by providing non-parametric evidence of the corporate tax incidence on wages using an event study design (Section 3.5.1). Section 3.5.2 presents the regression model. In Section 3.5.3 we analyze the direct wage effect and address potential identification challenges. In Section 3.5.4 we estimate the full wage effect including the indirect effect and find that the latter is negligible in the context of the German local business tax. Consequently, we further explore the direct effect, testing for heterogeneous worker (Section 3.5.5) and firm effects (Section 3.5.6).

3.5.1 Event study

As a first check we provide a non-parametric test whether corporate taxes affect wages using an event study design. We look at differences in the wage growth rates between municipalities that have changed the tax rate and municipalities that have not. We assign municipalities to treatment and control group according to the following criteria: a municipality is *treated* if a tax *increase* occurred at the beginning of year t and no tax changes happened in years $t-2, t-1, t+1, t+2$. Conversely, a municipality is assigned to the *control* group if no tax change occurred from year $t-2$ to $t+2$. We thus analyze five-year spells of nominal wage growth around a change in the local business tax occurring in year t. In the upper panel of Figure 3.5.1 all firm types are used to calculate average wages; in the lower part only firms with collective bargaining agreements are considered.

The figure clearly shows that there is a negative effect of profit taxation on wages, but – in line with the theory – we only find a significant effect for firms with collective bargaining agreements (confidence intervals are bootstrapped using 200 draws). In t, i.e. the year of the tax change, the wage growth path of treated firms with CBAs becomes flatter, while the path for the control group is not affected. In period $t + 1$ the growth path for the treated lies significantly below the one for the control group. Note that in both panels of Figure 3.5.1 wage growth paths are identical until period $t - 1$, which indicates that the common trend assumption holds. Translating the graphical evidence to numbers, we find that the wage growth for CBA firms from year $t - 1$ to year $t + 1$ in the control (treatment) group is 6.14% (3.09%). The average increase in the EMTR in period t is 1.3%. Taking average values for pre-tax profits and the total wage bill, this implies a wage bill incidence of -77 cents for a 1 euro increase in the tax bill.

Figure 3.5.1: Mean change in wages in %



3.5.2 Empirical model

We estimate a Mincerian wage equation using the log monthly wage of individual i in firm f, municipality m and year t, $\ln(w_{ifm,t})$, as dependent variable. The independent variable of interest is the collection rate of municipality m, $\ln(cr_{m,t})$. We further include three sets of control variables on the worker, firm and municipality level. Controls on the worker level are captured by vector $\mathbf{X}_{i,t}$ and include age and firm specific tenure (both in quadratic forms). On the firm level, vector $\mathbf{Y}_{f,t}$ controls for the number of employees, value added, investment, full-time hours (all in logs), skill shares of the workforce, and a work council dummy. Municipality controls are denoted by $\mathbf{Z}_{m,t}$ and comprise the population size, the property tax rate, total revenues and expenses (in logs) as well as the local unemployment rate (on the county level). In addition, we control for a large set of potential confounding wage trends by including skill-year, occupation-year (blue/white collar), firm size-year, collective agreement type-year as well as state-year fixed effects (all trends are summarized in vector $\mathbf{T}_{ifm,t}$). Furthermore, we include four kinds of fixed effects: person, firm, municipal and year ($\mu_i, \mu_f, \mu_m, \mu_t$). The baseline model thus reads

$$\begin{aligned} \ln w_{ifm,t} = \quad & \alpha_{t-l} \ln \tau_{m,t-l} + \boldsymbol{\beta}\mathbf{X}'_{i,t} + \boldsymbol{\gamma}\mathbf{Y}'_{f,t} + \boldsymbol{\lambda}\mathbf{Z}'_{m,t} + \\ & \boldsymbol{T_{imf,t}} + \mu_i + \mu_f + \mu_m + \mu_t + \varepsilon_{ifm,t}, \end{aligned} \tag{3.5.1}$$

where the error term $\varepsilon_{ifm,t}$ is clustered at the county level.

The year fixed effects account for changes in the tax base and for changes in the federal rates of the local business tax and the corporate tax. By including person, firm and municipal fixed effects, we wipe out any time-invariant confounding factor on these levels, such as unobserved worker or management ability, or geographical location. Technically, we apply the spell fixed-effects estimator suggested by Andrews, Schank, and Upward (2006) by time-demeaning within each unique worker-firm-municipality combination.[18] We manually add the time-trend dummies included in vector $\mathbf{T}_{ifm,t}$.

While the regression equation (3.5.1) could imply that the simultaneity between wages and tax rates biases our estimates, a closer look at the timing of events reveals that reverse causality should not be an issue. The city council of municipality m usually sets the collection rate for year t in the last three months of year $t-1$. In the LIAB, wages are measured as of June 30th of year t. So when regressing wages in t on collection rates in t, there is already a lag of 7 to 9 months, which should mitigate reverse causality concerns. Nevertheless, our estimates might suffer from endogeneity if there are unobserved local shocks that affect both tax rates and wages. We will address this issue below using

[18] Note that only a few establishments change location, so in practice the firm dummies are collinear with the municipality dummies and the model is almost identical to a two-way fixed effects model.

different approaches.

3.5.3 Direct wage effect

Table 3.5.1 presents the baseline results for the direct wage effect. We thus regress log wages on log collection rates conditional on the number of employees, investment and output. In the first specification we include all firm types – with and without a CBA in place. We find a significant and negative coefficient: an increase in the local collection rate by 1%, leads to a decrease of the average wage in a firm by 0.076%. As this estimate is not easy to interpret, we translate the log-log coefficient into two more intuitive measures, a wage elasticity and an incidence measure. While the wage elasticity measures the percent change in wages of a one percent increase of the (statutory) EMTR, the incidence reports the euro change of the annual wage bill – for given employment levels – as a response to a one euro increase of the annual tax liabilities. We report the two measures at the bottom of Table 3.5.1. For specification (1) we find a wage elasticity of -0.31 and an incidence of -44 cents. Note that results are similar when using the collection rate in levels instead of logs (see Table 3.8.1 in Appendix B, Section 3.8). In the second and third specification we estimate our model separately for firms with and without a CBA. We only find a direct wage effect for workers in firms with a CBA, which makes sense as the direct effect arises due to a shock to the rents in a wage bargaining framework. Theoretically there cannot be a direct effect in firms without a CBA. We thus exclude firms without a CBA from the sample when further investigating the direct wage effect and refer to specification (2) as our baseline estimates.

Interacting the collection rate with dummies for sector level vs. firm level bargaining reveals that there is hardly any difference when it comes to point estimates and wage elasticities (model (4)). Despite this similarity there is quite a large difference in terms of the euro incidence between firms with the two CBA-types. In line with theory, we find that the wage incidence is 27% higher (13 cents) in firms where the bargaining takes place at the firm level compared to firms with a sector level CBA.

Next, we check the timing of the corporate tax effect on wages. As stated above, the implicit lag of the collection rate in the baseline specification (regressing $\ln(w_{ifm,t})$ on $\ln(cr_{m,t})$) is 7 to 9 months. Nevertheless, it might be the case that some firms are not able to adjust wages that quickly given that the average duration of a collective bargaining

Table 3.5.1: Direct effect on log wages: baseline results

Model	(1)	(2)	(3)	(4)	(5)
Firm type	All	With CBA	Without CBA	With CBA	
log collection rate$_t$	-0.076**	-0.093**	0.024		-0.098**
	(0.036)	(0.045)	(0.031)		(0.048)
log collection rate$_t$: sector level				-0.092**	
				(0.045)	
log collection rate$_t$: firm level				-0.094*	
				(0.055)	
log collection rate$_{t-1}$					-0.019
					(0.034)
log value added	0.004***	0.004**	0.008***	0.004**	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
log investment	0.000	-0.000	0.004***	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
log employees	0.038***	0.032***	0.073***	0.032***	0.033***
	(0.008)	(0.009)	(0.009)	(0.009)	(0.010)
log full-time hours	0.031	0.033	0.013	0.033	0.035
	(0.037)	(0.040)	(0.071)	(0.040)	(0.040)
work council	0.006**	0.006**	0.002	0.006**	0.007*
	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
share high-skilled employees	-0.000	0.002	-0.051	0.002	0.001
	(0.051)	(0.058)	(0.045)	(0.058)	(0.061)
share medium-skilled employees	-0.008	-0.020	-0.020	-0.020	-0.023
	(0.036)	(0.048)	(0.020)	(0.048)	(0.052)
local unemp. rate	-0.061	-0.026	-0.106	-0.026	0.043
	(0.124)	(0.145)	(0.116)	(0.145)	(0.155)
community population	0.083	0.106*	-0.119	0.106*	0.137**
	(0.053)	(0.062)	(0.088)	(0.062)	(0.066)
log expenses	-0.010*	-0.013**	0.008	-0.013**	-0.013*
	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)
log revenues	0.013**	0.017**	0.003	0.017**	0.019***
	(0.006)	(0.007)	(0.004)	(0.007)	(0.007)
local property tax rate	-0.060*	-0.079**	0.068***	-0.079**	-0.087**
	(0.032)	(0.037)	(0.025)	(0.037)	(0.038)
Adjusted R^2	0.186	0.198	0.142	0.198	0.181
Observations	4016476	3512491	503985	3512491	3204780
Groups	1240030	1085873	210230	1085873	1014992
Clusters	405	395	351	395	395
Long run effect					-0.118**
Wage elasticity	-0.31	-0.38	0.09		-0.46
Wage elasticity: sector level				-0.39	
Wage elasticity: firm level				-0.35	
Euro incidence	-0.44	-0.53	0.12		-0.67
Euro incidence: sector level				-0.49	
Euro incidence: firm level				-0.62	

Note: All specifications include person, firm, municipal and year fixed effects as well as: skill-year, occupation-year, firm size-year, CBA type-year, state-year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

agreement in Germany is between one and two years (cf. Section 3.2). As a consequence we estimate a distributed lag model adding the collection rates in $t-1$. Thus, this model shows how CBA firms react to profit tax changes within 19 to 21 months. Results are shown in specification (5) of Table 3.5.1. Note that we report the long-run effect, which is calculated by adding the two coefficients for periods t and $t-1$ and testing the joint significance at the bottom of the table; the wage elasticity and incidence measure for specification (5) rely on this long-run effect. Intuitively, elasticities and incidence rise in absolute terms when comparing model (5) to model (3). Yet, the increase is rather small, which suggests that most of the wage adjustments take place within the first 9 months. Adding the collection rate of period $t-2$ does not change results (not reported).

Identification. As mentioned before, we interpret the German institutional setting as many quasi-experiments in a small open economy setting at the municipal level. Nonetheless, our analysis might be prone to endogeneity issues. In terms of reverse causality from wages to tax rates, we have argued above that we are safe because of the timing of the events. In terms of omitted variable bias, it is impossible to find an instrument varying on the municipal level which would not be affected by the same unobserved local shock that affects both tax rates and wages.

Yet, we still have to rule out that our estimates are biased due to unobserved time-variant confounders.[19] There are two likely sources for this kind of confounding variation. First, any shock hitting the local labor market might affect simultaneously the budget of municipalities and the (wage-setting) behavior of firms. Thus, we add quadratic labor market region-trends to our baseline model to account for such potentially unobserved local labor market shocks.[20] Second, there might be shocks to certain industries and occupations, which should clearly affect the wages. As many industries (and thus occupations) are regionally clustered (Sternberg and Litzenberger, 2004), those shocks

[19] Table 3.8.2 in Appendix B (Section 3.8) shows the importance of accounting for time-invariant confounders as omitting person fixed effects renders the tax coefficient insignificant.

[20] Labor market regions delineate independent economic areas around an economic center where the appendant areas are defined on commuter flows. In this paper, we follow the rather narrow labor market region definition of the Federal Institute for Research on Building, Urban Affairs and Spatial Development (*Bundesinstitut für Bau-, Stadt- und Raumforschung*), which differentiates between 258 regions (see Eckey, Kosfeld, and Türck, 2006). Note that, due to 3.5 million observations (and remote access to the data), it is computationally not feasible to estimate the model with $258 \cdot 11$ region-year fixed effects. Hence we have to rely on a quadratic trend specification.

might also affect local taxation. While our baseline estimates already focus on the manufacturing industry, we go one step further and include job-year fixed effects to our model.[21] If industries and jobs are regionally clustered, the inclusion of these job-year fixed effects should take care of any such endogeneity problems.

Table 3.5.2: Effects on log wages: exogeneity tests

Model	(1)	(2)	(3)	(4)
Sample		Baseline		Placebo
log collection rate	-0.093**	-0.080*	-0.081*	-0.033
	(0.045)	(0.047)	(0.043)	(0.030)
log value added	0.004**	0.004*	0.004***	0.001
	(0.002)	(0.002)	(0.001)	(0.001)
log investment	-0.000	0.001	-0.000	0.001***
	(0.001)	(0.001)	(0.001)	(0.000)
log employees	0.032***	0.021**	0.031***	0.000
	(0.009)	(0.008)	(0.008)	(0.004)
log full-time hours	0.033	-0.027	0.037	0.040
	(0.040)	(0.038)	(0.041)	(0.044)
work council	0.006**	0.004	0.006**	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)
share high-skilled employees	0.002	0.091*	-0.015	0.041
	(0.058)	(0.049)	(0.054)	(0.041)
share medium-skilled employees	-0.020	0.073*	-0.008	0.021
	(0.048)	(0.039)	(0.044)	(0.039)
local unemp. rate	-0.026	-0.185	-0.065	-0.159**
	(0.145)	(0.151)	(0.136)	(0.075)
community population	0.106*	-0.037	0.103*	0.114*
	(0.062)	(0.088)	(0.057)	(0.062)
log expenses	-0.013**	-0.011*	-0.012*	0.003
	(0.007)	(0.006)	(0.006)	(0.005)
log revenues	0.017**	0.010	0.016**	-0.006
	(0.007)	(0.007)	(0.006)	(0.005)
local property tax rate	-0.079**	-0.033	-0.072**	0.069***
	(0.037)	(0.035)	(0.036)	(0.025)
labor market region trends	No	Yes	No	No
occupation-year FE	No	No	Yes	No
Adjusted R^2	0.198	0.226	0.210	0.397
Observations	3512491	3512491	3512491	287206
Groups	1085873	1085873	1085873	113810
Clusters	395	395	395	368
Elasticity	-0.38	-0.33	-0.33	
Incidence	-0.53	-0.46	-0.46	

Note: All specifications include person, firm, municipal and year fixed effects as well as: skill-year, occupation-year, firm size-year, CBA type-year, state-year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Table 3.5.2 shows how adding quadratic labor market region-trends and job-year fixed effects influences the baseline estimates, which are shown in specification (1).

[21] We differentiate between 33 different jobs.

Reassuringly, controlling for labor market region or job shocks does not change the direct wage effect much. Estimates decreases slightly but remain significant at the 10% level. Of course, statistical significance decreases as we take out more variation with these specifications. We now find a direct wage elasticity of -0.33 and an incidence of -0.46 cents. Overall, our results seem to be very robust to time-varying confounders since it is unlikely that a potential confounder is neither correlated with labor market region nor with job trends.

We provide a further identification test by running a placebo regression exploiting the fact that certain firms are not liable to the local business tax. Most firms in the public sector are not liable as well as firms in the agricultural or mining industry. Moreover, there are special exemptions within the manufacturing sector and in other industries. Specification (4) of Table 3.5.2 presents the result of a placebo test confirming that the effect of the local business tax on the wages in non-liable firms is zero.

3.5.4 The indirect wage effect

So far we have estimated the direct effect of corporate taxation on wages (*conditional* on output, investment and employment), which arises due to shocks to the overall rents in a collective bargaining context. The theoretical model in Section 3.3 also highlights an indirect effect of the local business tax, which affects wages through lower investment and the complementarity between labor and capital. We have argued above that this classical Harberger-type general equilibrium effect should be small in the context of the German local business tax due to the regional mobility of labor.

We now take a closer look at this indirect wage effect by estimating the full wage effect of corporate taxation with the indirect effect being the difference between the full and the direct effect. Given that factor input responses might take some time to unfold, we add the collection rates of period $t-1$ to the model. Specification (1) of Table 3.5.3 replicates the findings made above (specification (5) of Table 3.5.1): the direct wage incidence for firms with a CBA is 67 cents. In the following four specifications we estimate the full wage incidence of the local business tax without conditioning on employment, investment and/or output. The absolute incidence increases slightly to 77 cents (specification (5)). Note that the estimate is identical to the one derived from the event study presented in Section 3.5.1.

Table 3.5.3: General equilibrium effects on log wages

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Firm type	with CBA					without CBA				
log collection rate$_t$	-0.098**	-0.097**	-0.098**	-0.103**	-0.100**	0.016	0.017	0.014	0.005	0.003
	(0.048)	(0.047)	(0.048)	(0.048)	(0.047)	(0.033)	(0.033)	(0.033)	(0.036)	(0.037)
log collection rate$_{t-1}$	-0.019	-0.027	-0.019	-0.024	-0.035	0.038	0.038	0.043	0.004	0.005
	(0.034)	(0.035)	(0.034)	(0.034)	(0.036)	(0.030)	(0.031)	(0.031)	(0.031)	(0.033)
log value added	0.004**		0.004**	0.005***		0.008***		0.008***	0.011***	
	(0.002)		(0.002)	(0.001)		(0.002)		(0.002)	(0.002)	
log investment	-0.000	0.000		0.000		0.004***	0.004***		0.005***	
	(0.001)	(0.001)		(0.001)		(0.001)	(0.001)		(0.001)	
log employees	0.033***	0.036***	0.033***			0.073***	0.078***	0.076***		
	(0.010)	(0.009)	(0.009)			(0.009)	(0.009)	(0.009)		
log full-time hours	0.035	0.055	0.034			0.001	-0.003	-0.003		
	(0.040)	(0.036)	(0.039)			(0.074)	(0.073)	(0.075)		
work council	0.007*	0.008**	0.007*	0.004	0.005	0.003	0.003	0.002	0.004	0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
share high-skilled employees	0.001	0.012	-0.000	0.032	0.053	-0.037	-0.032	-0.035	-0.088*	-0.086*
	(0.061)	(0.061)	(0.061)	(0.060)	(0.065)	(0.048)	(0.049)	(0.048)	(0.049)	(0.050)
share medium-skilled employees	-0.023	-0.028	-0.023	-0.010	-0.020	-0.020	-0.016	-0.019	-0.049**	-0.043**
	(0.052)	(0.054)	(0.052)	(0.049)	(0.053)	(0.020)	(0.021)	(0.020)	(0.023)	(0.022)
local unemp. rate	0.043	0.024	0.041			-0.154	-0.145	-0.159		
	(0.155)	(0.159)	(0.149)			(0.114)	(0.116)	(0.114)		
community population	0.137**	0.129*	0.138**	0.134**	0.121*	-0.096	-0.102	-0.101	-0.101	-0.116
	(0.066)	(0.066)	(0.068)	(0.067)	(0.069)	(0.093)	(0.094)	(0.096)	(0.107)	(0.111)
log expenses	-0.013*	-0.013*	-0.013*	-0.012*	-0.012*	0.007	0.007	0.006	0.009	0.008
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)
log revenues	0.019***	0.019***	0.019***	0.021***	0.021***	0.001	0.001	0.003	0.004	0.006
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
local property tax rate	-0.087**	-0.088**	-0.087**	-0.100***	-0.102***	0.063**	0.067***	0.065**	0.059**	0.066**
	(0.038)	(0.038)	(0.038)	(0.038)	(0.037)	(0.026)	(0.026)	(0.026)	(0.027)	(0.028)
firm-size year FE	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No	No
Adjusted R^2	0.181	0.180	0.181	0.177	0.176	0.129	0.128	0.128	0.114	0.108
Observations	3204780	3204780	3204780	3204780	3204780	484767	484767	484767	484767	484767
Groups	1014992	1014992	1014992	1014992	1014992	202613	202613	202613	202613	202613
Clusters	395	395	395	395	395	350	350	350	350	350
Elasticity	-0.46	-0.49	-0.46	-0.50	-0.53	0.21	0.21	0.22	0.04	0.03
Incidence	-0.67	-0.71	-0.67	-0.72	-0.77	0.29	0.29	0.30	0.05	0.04

Note: All specifications include person, firm, municipal and year fixed effects as well as: skill-year, occupation-year, firm size-year, CBA type-year, state-year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

The higher incidence implies an indirect wage effect of roughly 10 cents. This increase is too small to be statistically significant. Thus we cannot state that the direct effect is different from the full effect. In line with the theoretical model and given the very local nature of the corporate tax, the small indirect effect may not be surprising. Workers are mobile within labor market regions, which prevents wages from falling when investment decreases. In fact, the companion paper by Siegloch (2013) shows that there is a strong and negative employment effect of the local business tax, which can be attributed to local labor mobility, see Chapter 4.

Specifications (6) to (10) of Table 3.5.3 show further evidence of a negligible indirect effect for firms without a collective bargaining agreement. Model (6) confirms that there is no direct effect for non-CBA firms. In specifications (7) to (10), we estimate the full, unconditional wage incidence model and find that there seems to be no indirect effect.

3.5.5 Heterogeneous worker effects

In this section we use the rich LIAB data to test whether different worker groups are affected differently by corporate taxation. Given the small indirect wage effect, we focus on the direct effect, excluding firms without a CBA and conditioning on employment, value added and investment in the estimation.

We start by testing one specific feature of our theoretical model, namely heterogeneous skill effects. As described in Section 3.4.2, we differentiate between three skill groups (high, medium and low). In order to test for heterogeneous worker effects, we interact the log collection rate with skill dummy variables. Specification (1) of Table 3.5.4 shows that the wage effect of corporate taxes is driven by medium-skilled workers. We neither find a significantly negative effect for high nor for low-skilled wages. Yet, it is possible that there are different adjustment speeds for different skill types. Hence, we also add interacted collection rates of period $t-1$ to the model (specification (2)). It turns out that corporate taxes also have a negative and significant long-run effect (the sum of the two coefficients for periods t and $t-1$) on high-skilled wages. In fact, the high-skilled long-run wage elasticity is more negative than the one for medium-skilled (-0.54 vs. -0.48). The effect on low-skilled workers remains small and not significantly distinguishable from zero. This suggests that the wage incidence is increasing in skill: the higher the qualification, the stronger the negative wage effect.

Table 3.5.4: Effects on log wages: by skill

Model	(1)	(2)
log collection rate$_t$ x high skilled	-0.099	-0.081
	(0.073)	(0.058)
log collection rate$_t$ x medium skilled	-0.097**	-0.096**
	(0.045)	(0.046)
log collection rate$_t$ x low skilled	-0.061	-0.121
	(0.056)	(0.075)
log collection rate$_{t-1}$ x high skilled		-0.062
		(0.042)
log collection rate$_{t-1}$ x medium skilled		-0.027
		(0.033)
log collection rate$_{t-1}$ x low skilled		0.041
		(0.059)
log value added	0.004**	0.004**
	(0.002)	(0.002)
log investment	-0.000	-0.000
	(0.001)	(0.001)
log employees	0.032***	0.033***
	(0.009)	(0.010)
log full-time hours	0.032	0.034
	(0.041)	(0.040)
work council	0.006**	0.007*
	(0.003)	(0.004)
share high-skilled employees	0.002	0.001
	(0.057)	(0.060)
share medium-skilled employees	-0.020	-0.023
	(0.048)	(0.051)
local unemp. rate	-0.026	0.043
	(0.146)	(0.155)
community population	0.107*	0.138**
	(0.062)	(0.066)
log expenses	-0.013**	-0.013*
	(0.007)	(0.007)
log revenues	0.017**	0.019***
	(0.007)	(0.007)
local property tax rate	-0.080**	-0.087**
	(0.037)	(0.038)
Adjusted R^2	0.198	0.181
Observations	3512491	3204780
Groups	1085873	1014992
Clusters	395	395
Long run effect: high skilled	-0.099	-0.142*
Long run effect: medium skilled	-0.097**	-0.123**
Long run effect: low skilled	-0.061	-0.080
Wage elasticity: high skilled	-0.39	-0.54
Wage elasticity: medium skilled	-0.40	-0.48
Wage elasticity: low skilled	-0.26	-0.33
Euro incidence: high skilled	-0.12	-0.17
Euro incidence: medium skilled	-0.43	-0.56
Euro incidence: low skilled	-0.06	-0.08

Note: All specifications include person, firm, municipal and year fixed effects as well as: skill-year, occupation-year, firm size-year, CBA type-year, state-year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

The skill pattern is in line with intuition since it is more difficult to reduce the rents of low-skilled workers whose wage rates are already close to the implicit minimum wage, which is determined by unemployment benefits and social assistance. A tax increase in the bargaining model decreases the overall rents to be shared between the firm and its workers. Worker groups that generally do not receive much of the rents do not have much to lose from the rent shock: bargaining power is self-defeating as shown in Section 3.3. In terms of the incidence, firms reduce the wage bill of the medium-skilled most, which is, of course, a pure size effect, given that 75% of the workers are medium-skilled.

As stated above, we make a rather rigorous choice in treating censored wages. By dropping all workers that have once had a censored wage during the observation period, we exclude many high-skilled workers and supposedly keep a negatively selected group of the highly qualified. In the following we test the sensitivity of our results with respect to this choice. While specification (1) of Table 3.5.5 shows the baseline results, we relax the rigorous treatment of censored wages slowly when moving to the right: in model (2) we only drop the workers in years where wages are actually censored (as opposed to dropping all workers with once censored wages). In specification (3), we do not drop any observations but use the contribution ceiling as the wage when observations are censored. In the last model, we impute censored wages using the Tobit-procedure provided by the IAB (Gartner, 2005).

The results show that the treatment of censored wages affects skill groups differently. The coefficients for the medium- and low-skilled become slightly more negative, but expectedly do not change much as most of these workers earn wages below the contribution ceiling. In contrast, the coefficient on the high-skilled is very sensitive: it changes size and sign across specifications. Based on these insights, we argue that our rigorous sample selection with respect to the wage censoring is the only reliable way. This, however, might come at the cost that we are likely to face a negatively selected group of high-skilled workers. If this is the case, the true direct wage incidence for the high-skilled is probably even more negative, given that higher ability high-skilled are likely to extract more rents than lower ability high-skilled. Hence, our baseline estimates arguably provide a lower bound for high-skilled workers.

Table 3.5.5: Effects on log wages: Sensitivity w.r.t wage censoring

Model	(1)	(2)	(3)	(4)
Wage treatment	person never censored	not censored in t	ceiling	imputed
log collection rate x high skilled	-0.099	-0.045	0.019	-0.017
	(0.073)	(0.074)	(0.049)	(0.057)
log collection rate x medium skilled	-0.097**	-0.105**	-0.107***	-0.124***
	(0.045)	(0.045)	(0.039)	(0.044)
log collection rate x low skilled	-0.061	-0.068	-0.072	-0.091
	(0.056)	(0.056)	(0.049)	(0.058)
log value added	0.004**	0.004**	0.004***	0.003**
	(0.002)	(0.002)	(0.001)	(0.002)
log investment	-0.000	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
log employees	0.032***	0.032***	0.023***	0.025***
	(0.009)	(0.009)	(0.008)	(0.009)
log full-time hours	0.032	0.031	0.031	0.026
	(0.041)	(0.042)	(0.038)	(0.042)
work council	0.006**	0.005*	0.004	0.003
	(0.003)	(0.003)	(0.003)	(0.003)
share high-skilled employees	0.002	0.012	0.017	0.023
	(0.057)	(0.055)	(0.043)	(0.048)
share medium-skilled employees	-0.020	-0.025	-0.027	-0.041
	(0.048)	(0.047)	(0.041)	(0.045)
local unemp. rate	-0.026	-0.006	-0.044	0.001
	(0.146)	(0.146)	(0.128)	(0.142)
community population	0.107*	0.104*	0.074	0.118**
	(0.062)	(0.061)	(0.053)	(0.059)
log expenses	-0.013**	-0.013*	-0.013**	-0.015**
	(0.007)	(0.007)	(0.006)	(0.007)
log revenues	0.017**	0.016**	0.015**	0.019***
	(0.007)	(0.007)	(0.006)	(0.007)
local property tax rate	-0.080**	-0.082**	-0.071**	-0.084**
	(0.037)	(0.037)	(0.030)	(0.034)
Adjusted R^2	0.198	0.208	0.221	0.140
Observations	3512491	3820751	4592096	4592096
Groups	1085873	1197097	1373324	1373324
Clusters	395	395	395	395
Wage elasticity: high skilled	-0.39	-0.18	0.08	-0.07
Wage elasticity: medium skilled	-0.40	-0.43	-0.43	-0.51
Wage elasticity: low skilled	-0.26	-0.29	-0.31	-0.39
Euro incidence: high skilled	-0.12	-0.06	0.02	-0.02
Euro incidence: medium skilled	-0.43	-0.46	-0.46	-0.53
Euro incidence: low skilled	-0.06	-0.07	-0.07	-0.09

Note: All specifications include person, firm, municipal and year fixed effects as well as: skill-year, occupation-year, firm size-year, CBA type-year, state-year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Table 3.5.6: Heterogenous worker effects on log wages

Model	(1)	(2)	(3)	(4)
Group	firm tenure	age	collar type	mobility
log collection rate	-0.091*	-0.094**	-0.089*	-0.103**
	(0.047)	(0.046)	(0.046)	(0.046)
log collection rate ∗ medium	-0.004			
	(0.016)			
log collection rate ∗ high	0.007			
	(0.022)			
log collection rate ∗ medium		0.003		
		(0.010)		
log collection rate ∗ old		0.003		
		(0.019)		
log collection rate ∗ white collar			-0.027	
			(0.025)	
log collection rate ∗ mobile workers				0.214***
				(0.070)
log value added	0.004**	0.004**	0.004**	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)
log investment	-0.000	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
log employees	0.032***	0.032***	0.032***	0.032***
	(0.009)	(0.009)	(0.009)	(0.009)
log full-time hours	0.033	0.033	0.033	0.032
	(0.041)	(0.040)	(0.041)	(0.041)
work council	0.006**	0.006**	0.006**	0.005*
	(0.003)	(0.003)	(0.003)	(0.003)
share high-skilled employees	0.001	0.002	0.002	0.002
	(0.057)	(0.058)	(0.058)	(0.058)
share medium-skilled employees	-0.018	-0.019	-0.020	-0.019
	(0.048)	(0.048)	(0.048)	(0.048)
local unemp. rate	-0.025	-0.027	-0.026	-0.024
	(0.145)	(0.146)	(0.145)	(0.145)
community population	0.101	0.106*	0.107*	0.105*
	(0.062)	(0.062)	(0.062)	(0.062)
log expenses	-0.013**	-0.013**	-0.013**	-0.012*
	(0.006)	(0.007)	(0.007)	(0.006)
log revenues	0.016**	0.017**	0.017**	0.016**
	(0.007)	(0.007)	(0.007)	(0.007)
local property tax rate	-0.079**	-0.080**	-0.079**	-0.079**
Adjusted R^2	0.199	0.199	0.198	0.199
Observations	3512491	3512491	3512491	3512491
Groups	1085873	1085873	1085873	1085873
Clusters	395	395	395	395

Note: All specifications include person, firm, municipal and year fixed effects as well as: skill-year, occupation-year, firm size-year, CBA type-year, state-year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Next, we test for other heterogeneous worker effects (see Table 3.5.6) by interacting the log collection rate with various worker type dummy variables.[22] Specifications (1) and (2) show that there are neither significant differences by tenure nor by age groups. Moreover, we do not find different wage effects for blue and white-collar workers. Last in model (4), we differentiate between workers who switch firms and workers who stay in the same plant during our period of observation. The latter group is apparently less mobile and might therefore bear a higher corporate tax burden. In fact, as Table 3.5.6 suggests, job stayers show a much higher negative wage effect, whereas more mobile workers who change firms do not seem to be affected at all by the burden shifting of firms (the point estimate for job switchers is positive but not statistically significantly so).

3.5.6 Heterogeneous firm effects

Finally, we use the establishment part of the LIAB to check for heterogeneous firm effects. As done in Section 3.5.5, we focus on the direct wage effect for CBA firms. We start off by testing differences between corporate and non-corporate firms (*Personengesellschaften*). As noted above, the tax treatment of the two legal types is quite different. Non-corporate firms are not subject to the corporate tax but the personal income tax (on operating profits assigned to the proprietor), which is progressive and where marginal rates consequently depend on the taxable income. As for the local business tax the definition of the base also differs compared to corporate firms.[23] As firm characteristics also differ strongly between corporate and non-corporate firms – notably in terms of size –, we split the sample and estimate the baseline model separately for both legal types. Despite the stark differences, specification (1) and (2) of Table 3.5.7 shows that point estimates and therefore wage elasticities are quite similar between corporate

[22] The base effects of the dummy variables are included but not shown in the table.

[23] Non-corporate firms have an allowance of 24,500 euros. In addition, a share of the business tax liabilities can be deducted from the personal income tax base: $1.8 \cdot \tau_{fed} \cdot Y$ from 2001 to 2007 and $3.8 \cdot \tau_{fed} \cdot Y$ since 2008 onwards. Moreover, there was a reduced τ_{fed} for small non-corporate firms prior to 2008: for every 12,000 euros exceeding the allowance of 24,500 euros, τ_{fed} was raised by one percentage point so that the full basic federal rate of 5.0% had to be paid with a taxable income starting from 72,500 euros. Assuming that profits of the firms are so high that companies are in the highest PIT bracket and face the top marginal tax rate, τ_{PIT}^{top}, the effective marginal tax rate for a non-corporate firms $\tau_{EMTR}^{non-corp}$ from 1998 to 2007, is $\tau_{EMTR}^{non-corp} = \frac{\tau_{PIT}^{top} \cdot (1+soli)+\tau_{fed} \cdot cr}{1+\tau_{fed} \cdot 1.8}$. Since 2008 the denominator of the fraction is set to $1 + \tau_{fed} \cdot 3.8$.

and non-corporate firms. Yet, the negative wage effect for non-corporate firms is not statistically significant at conventional levels (p-value of 0.11). The wage incidence of non-corporate firms is only 30 cents as compared to 53 cents for corporate firms (specification (2)). This might be explained by lower bargaining power of workers in non-corporate firms where owners are more dominant and workers less organized.

Table 3.5.7: Effects on log wages - by legal form and industry

Model	(1)	(2)	(3)	(4)	(5)
Sample	Legal type		Industry		
	corporate	non-norporate	manufacturing	traffic	services
log collection rate	-0.093**	-0.102	-0.093**	-0.061	-0.023
	(0.045)	(0.064)	(0.045)	(0.045)	(0.066)
log value added	0.004**	0.007***	0.004**	-0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
log investment	-0.000	-0.003*	-0.000	0.006*	0.001
	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)
log employees	0.032***	0.049***	0.032***	-0.040***	0.021***
	(0.009)	(0.010)	(0.009)	(0.015)	(0.007)
log full-time hours	0.033	0.064	0.033	0.098	0.021
	(0.040)	(0.051)	(0.040)	(0.063)	(0.034)
work council	0.006**	0.009	0.006**	0.020*	0.004
	(0.003)	(0.010)	(0.003)	(0.011)	(0.006)
share high-skilled employees	0.002	-0.076	0.002	0.013	0.154***
	(0.058)	(0.090)	(0.058)	(0.146)	(0.042)
share medium-skilled employees	-0.020	-0.056	-0.020	0.137*	-0.011
	(0.048)	(0.035)	(0.048)	(0.070)	(0.023)
local unemp. rate	-0.026	0.004	-0.026	-0.418	-0.095
	(0.145)	(0.257)	(0.145)	(0.360)	(0.173)
community population	0.106*	-0.026	0.106*	0.041	0.184***
	(0.062)	(0.092)	(0.062)	(0.104)	(0.069)
log expenses	-0.013**	0.001	-0.013**	0.004	0.021***
	(0.007)	(0.008)	(0.007)	(0.022)	(0.007)
log revenues	0.017**	0.009	0.017**	-0.012	-0.008
	(0.007)	(0.008)	(0.007)	(0.029)	(0.009)
local property tax rate	-0.079**	0.008	-0.079**	0.068	-0.025
	(0.037)	(0.038)	(0.037)	(0.057)	(0.029)
Adjusted R^2	0.198	0.203	0.198	0.145	0.113
Observations	3512491	201603	3512491	339154	467551
Groups	1085873	92557	1085873	98385	212523
Clusters	395	316	395	167	323
Elasticity	-0.38	-0.39	-0.38	-0.25	-0.09
Incidence	-0.53	-0.30	-0.53	-0.44	-0.02

Note: All specifications include person, firm, municipal and year fixed effects as well as: skill-year, occupation-year, firm size-year, CBA type-year, state-year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Recall that our baseline sample is restricted to firms in the manufacturing sector. We estimate the baseline model also for the traffic and the service sector. Interestingly, we do not find significantly negative effects for the two other sectors as indicated by specifications (4) and (5) of Table 3.5.7, which could also be due to too small sample sizes or lower bargaining power of workers – especially in the service sector (where wages are closer to the implicit minimum wage).

Next, we run several interaction models to test for further firm heterogeneity. Table 3.5.8 shows the results. In model (1) we test whether there are differences between single and multi-establishment firms following the theoretical analysis of the tax incidence under formula apportionment shown in Appendix A (Section 3.7). The interaction term is insignificant suggesting that the wage incidence does not differ between single and multi-establishment firms, which is not surprising given that the formula apportionment is based on the wage bill of the respective establishment.

Looking at the effects by firm size in specification (2), we find an interesting pattern: there are strong negative effects on wages for small firms with less than 50 employees and for larger firms with more than 250 workers. Medium-sized firms with a workforce of 50-250 do not seem to cut wages after increases of the local business tax. Specifications (3) and (4) show that there are no differences in terms of the firm's profitability or between firms with and without a work council. Last, we use a survey question of the LIAB asking "whether local taxation was important for the initial location decision of the firm". Intuitively, we only find significantly negative tax effects for firms that cared about local taxation when choosing their location and thus already demonstrated their tax sensitivity.

Table 3.5.8: Heterogenous firm effects on log wages

Model Group	(1) establishment type	(2) firm size	(3) profitability	(4) work council	(5) tax salience
log collection rate	-0.115**	-0.139**	-0.097**	-0.080*	-0.071
	(0.056)	(0.066)	(0.047)	(0.048)	(0.072)
log collection rate ∗ stand alone	0.025				
	(0.030)				
log collection rate ∗ 50-250 employees		0.117**			
		(0.051)			
log collection rate ∗ 250-1000 employees		0.035			
		(0.062)			
log collection rate ∗ >1000 employees		0.027			
		(0.078)			
log collection rate ∗ poor			0.014		
			(0.015)		
log collection rate ∗ work council				-0.013	
				(0.022)	
log collection rate ∗ local tax relevant.					-0.047
					(0.089)
log value added	0.004***	0.004**	0.004**	0.004**	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
log investment	-0.000	-0.000	-0.000	-0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
log employees	0.031***	0.032***	0.032***	0.032***	0.036***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)
log full-time hours	0.034	0.034	0.036	0.033	0.021
	(0.041)	(0.041)	(0.040)	(0.040)	(0.039)
work council	0.007**	0.006**	0.006**	0.024	0.003
	(0.003)	(0.003)	(0.003)	(0.030)	(0.005)
share high-skilled employees	-0.000	0.002	0.000	0.002	0.040
	(0.056)	(0.057)	(0.058)	(0.058)	(0.065)
share medium-skilled employees	-0.020	-0.019	-0.023	-0.020	0.002
	(0.048)	(0.048)	(0.048)	(0.048)	(0.053)
local unemp. rate	-0.029	-0.025	-0.024	-0.027	-0.133
	(0.146)	(0.145)	(0.145)	(0.145)	(0.141)
community population	0.113*	0.107*	0.106*	0.106*	0.106
	(0.063)	(0.061)	(0.061)	(0.062)	(0.067)
log expenses	-0.011	-0.013*	-0.014**	-0.013**	-0.010
	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)
log revenues	0.017***	0.017**	0.017**	0.017**	0.020**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
local property tax rate	-0.073*	-0.078**	-0.081**	-0.079**	-0.040
	(0.039)	(0.036)	(0.037)	(0.037)	(0.033)
Adjusted R^2	0.199	0.199	0.199	0.198	0.213
Observations	3495591	3512491	3512491	3512491	2551316
Groups	1080893	1085873	1085873	1085873	647658
Clusters	394	395	395	395	364

Note: All specifications include person, firm, municipal and year fixed effects as well as: skill-year, occupation-year, firm size-year, CBA type-year, state-yea r fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

3.6 Conclusions

How much of the corporate tax burden is borne by workers? While this question has been heavily discussed ever since Harberger (1962)'s seminal work, compelling empirical evidence is scarce due to tough requirements that have to be met in order to identify the full wage effect. In this paper, we use the setting of German corporate tax legislation that provides a nearly ideal laboratory to answer this question. On average 8% of the 11,441 German municipalities change their corporate tax rates per year. We link administrative information on the universe of the German municipalities from 1998 to 2008 to high-quality administrative linked employer-employee data to estimate the effect of corporate taxation on individual wages. Moreover, the local corporate tax legislation allows us to treat the German municipalities as many small open economies and thereby gauge the full incidence of corporate taxation on wages, including often neglected indirect effects related to the adjustment of investment and other input factors.

We find that a 1% increase in the effective marginal corporate tax rate leads to a 0.3% decrease in wages. This implies that for every additional tax euro a firm has to pay, the wage bill is decreased by 50 to 77 cents. We decompose the full incidence into a direct effect arising from wage bargaining and an indirect effect driven by the adjustment of other inputs. In line with our theoretical model, we find that the direct effect that arises in a wage bargaining context is only found for firms with a collective bargaining agreement. We also find that the negative direct wage incidence is increasing in skill. High-skilled workers who are likely to extract the highest rents in a wage bargaining context, experience the highest relative wage decreases if corporate taxes rise. As for the indirect effect we find rather small estimates. This could be due to the high regional mobility of labor (see Siegloch, 2013, and Chapter 4).

Our theoretical and empirical findings have important policy implications. In the public and political debate arguments in favor of (higher) corporate taxes are often based on redistributive motives: allegedly rich firm owners shall contribute to financing public goods and social safety nets by paying their fair share of taxes.[24] Opponents of high

[24] See, for example, the recent debate in the United Kingdom about big multinational firms like the online retailer Amazon or the coffee chain Starbucks that have paid small amounts of corporate taxes despite large revenues in the past. While this specific debate rather focuses on loopholes in the tax base through tax avoidance possibilities of multinationals, it shows that many people expect firms to pay more taxes to contribute to the public good (see http://www.bbc.co.uk/news/business-19967397).

corporate taxes often claim that eventually capital will not bear the tax burden, but instead the burden is shifted to labor, being immobile in an international context.

The findings presented in this paper shed new light on this debate and show that the shifting of the corporate tax burden is more complex. We find that the incidence depends (i) on the relevant labor market institutions and (ii) on the jurisdictional level at which the tax is set and thus the relative mobility of production factors. We find that labor bears a substantial share of the corporate tax burden when collective bargaining agreements are in place and workers extract rents. Put differently this result suggests that if rents are predominantly extracted by capitalists and firm owners, corporate taxation is less harmful for workers. Our findings of a negligible indirect effect suggest that an increase in *local* corporate taxation in one jurisdiction, holding constant taxes in all other jurisdictions, might fall on firm owners since regionally mobile labor prevents wages from falling – especially in a competitive labor market. Moreover, within the workforce, we find that the low-skilled are less affected by corporate tax increases than workers with higher skills. From a redistributive perspective, this might be seen as a desirable feature of corporate taxation.

In addition, as Bradford (1978) shows, labor as whole can still lose despite the negligible indirect effect on local wages. Repelled labor leads to many small but in sum substantial decreases in the marginal products (and thus in wages) in the labor absorbing low-tax municipalities. Moreover, the effects of tax competition, which are quite complex in the setting of the German local business tax (Janeba and Osterloh, 2012), have to be taken into account when discussing the effectiveness of local corporate taxes as an instrument to tax firm owners.

Last, one should bear in mind that these findings refer to local corporate tax changes. Whether they would apply to a nationwide corporate tax is far from clear. For small open economies with international labor mobility, this might be the case. But if labor mobility is low, the incidence on wages may be different.

3.7 Appendix A

In this appendix we briefly discuss some additional aspects of the corporate tax incidence on wages implied by the wage bargaining model developed in Section 3.3.

Indirect effect First, consider the impact of changes in the corporate tax rate on demand for inputs. Standard comparative analysis of equations (3.3.2) and (3.3.4) shows that the impact of corporate tax changes on demand for labor of skill type $k \neq j$ is

$$\frac{\partial L_i^k(R_i,\overline{w}_i^1,\overline{w}_i^2)}{\partial R_i} = \frac{1}{\Delta}\left(\frac{\partial^2 F(K_i,L_i^1,L_i^2)}{\partial L_i^k \partial L_i^j}\frac{\partial^2 F(K_i,L_i^1,L_i^2)}{\partial L_i^j \partial K_i} - \frac{\partial^2 F(K_i,L_i^1,L_i^2)}{\partial L_i^j \partial L_i^j}\frac{\partial^2 F(K_i,L_i^1,L_i^2)}{\partial L_i^k \partial K_i}\right)$$

where $\Delta < 0$ is the determinant of the Hessian matrix of the production function. The concavity of the production function implies $\Delta < 0$. As stated in the text, the equation shows that a sufficient condition for $\frac{\partial L_i^k(R_i,\overline{w}_i^1,\overline{w}_i^2)}{\partial R_i} < 0$ is that both skill types are complements for each other and for capital in the production function $\frac{\partial^2 F(K_i,L_i^1,L_i^2)}{\partial L_i^k \partial L_i^j}, \frac{\partial^2 F(K_i,L_i^1,L_i^2)}{\partial L_i^j \partial K_i}, \frac{\partial^2 F(K_i,L_i^1,L_i^2)}{\partial L_i^k \partial K_i} > 0$. But in general the sign of $\frac{\partial L_i^k(R_i,\overline{w}_i^1,\overline{w}_i^2)}{\partial R_i}$ may be positive or negative.

Sector level bargaining Next we provide the proof of result 3, which states that the magnitude of the effect of a tax change in jurisdiction i on the wage is larger under firm level bargaining than under sector level bargaining. Differentiating the wage equation (3.3.9) with respect to τ_i and using equation (3.3.10) yields

$$\frac{dw^{ks*}}{d\tau_i}\Big|_{dL_h^k=dL_h^j=dK_h=0} = \frac{L_h^k}{\sum_{i=1}^{m}[L_i^k(1-\tau_i)]}\left(w^{ks*} + \frac{\Psi}{(1-\beta^k\beta^j)}\right),$$

where

$$\Psi = -\overline{w}^k(1-\beta^k)L_h^k + \overline{w}^j(1-\beta^j)\beta^k L_h^j - (1-\beta^j)\beta^k[F_i(K_h,L_h^1,L_h^2) - \alpha r K_h].$$

In the case of firm level bargaining ($m = 1$), the direct effect can be expressed as

$$\frac{dw^{k*}}{d\tau_i}\Big|_{dL_h^k=dL_h^j=dK_h=0} = \frac{1}{(1-\tau_h)}\left(w^{ks*} + \frac{\Psi}{(1-\beta^k\beta^j)}\right),$$

which is unambiguously larger since

$$\frac{L_h^k}{\sum_{i=1}^{m}[L_i^k(1-\tau_i)]} < \frac{1}{(1-\tau_h)}.$$

Formula apportionment Finally, we discuss the implications of formula apportionment for our theoretical analysis. If firms have plants in more than one municipality, the local business tax in Germany uses formula apportionment to allocate the taxing rights to the different municipalities. The formula used in Germany is based on payroll as the only apportionment factor.[25] Given this, the impact of tax changes on wages may be different. Consider a company with plants in two jurisdictions i and j. After tax profits of the company are

$$P^{FA} = [F(K_i, K_{j,}L_i^1, L_i^2, L_j^1, L_j^2) - \sum_{k=1}^{2} w^k L_i^k - \sum_{k=1}^{2} w^k L_j^k](1-\tau_{ij}) - (1-\alpha\tau_{ij})r[K_i + K_j]$$

with obvious notation. We assume that wage bargaining takes place at the firm level, not at the plant level, and that wages paid to workers of a given skill group are the same in the two plants. The profit tax rate is now given by

$$\tau_{ij} = T + (1-T)\frac{t_i \sum_{k=1}^{2} w^k L_i^k + t_j \sum_{k=1}^{2} w^k L_j^k}{\sum_{k=1}^{2} w^k L_i^k + \sum_{k=1}^{2} w^k L_j^k}$$

The main difference to the case where firms just operate in one jurisdiction is that the profit tax rate itself now depends on wages and the distribution of employment at the two plants, i.e. $\tau_{ij} = \tau_{ij}(w^1, w^2, L_1^1...)$, with

$$\frac{\partial \tau_{ij}}{\partial w^l} = [t_i - t_j]\left[\frac{L_i^l}{L_i^m} - \frac{L_j^l}{L_j^m}\right] L_i^m L_j^m \frac{(1-T)}{\gamma} \quad l = 1, 2, l \neq m,$$

[25] In cases where this leads to an outcome which is obviously inappropriate, the tax rate can be divided differently. But in most cases the payroll based formula is applied.

where

$$\gamma = \left[1 + \frac{w^l L_i^l + w^m L_i^m}{w^l L_j^l + w^m L_j^m}\right]^2 [w^l L_j^l + w^m L_j^m]^2 > 0.$$

Assume, for instance, that municipality i has a higher tax rate than municipality j, and assume that the wage of the low-skilled increases. In this case the impact on the tax burden will depend on whether this increases the payroll share of the high or that of the low tax municipality. If the share of low-skilled is higher in jurisdiction i so that $\left[\frac{L_i^l}{L_i^m} - \frac{L_j^l}{L_j^m}\right] > 0$, the tax rate τ_{ij} will increase, and vice versa. Therefore the effect of a wage change on the tax rate is ambiguous for the general case.

The fact that the firm's profit tax rate is now a function of the wage rates also implies that the direct effect of a change in the local corporate tax rate t on wages as defined in the preceding sections is now ambiguous. The Nash maximand of the union-form bargaining problem is now given by

$$\Omega_i^{kFA} = \beta^k \ln Z_i^k + (1 - \beta^k) \ln P^{FA}$$

The equilibrium wage rates are

$$w^{kFA*} = \overline{w}^k + \frac{\beta^k}{(1 - \beta^k)} \frac{P^{FA}}{[(L_i^k + L_j^k)(1 - \tau_{ij}) - \Phi]} \quad k = 1, 2, \tag{3.7.1}$$

where

$$\Phi = \frac{\partial P^{FA}}{\partial \tau_{ij}} \frac{\partial \tau_{ij}}{\partial w^k}.$$

Equation (3.7.1) implicitly defines the two wage rates emerging from the bargaining process as reaction functions of the type $w^{lFA*} = w^{lFA*}(w^{mFA*}, t_i, t_j, T...)$. Differentiating equation (3.7.1) shows that the direct effect of a change in local corporate tax rates on the equilibrium wage rates is, in general, ambiguous.[26] This suggests that the incidence of the local corporate tax on wages in firms with plants in multiple jurisdictions could differ systematically from the incidence in firms which operate in one jurisdiction only, but the sign of the effects is ambiguous. In the empirical analysis, the role of formula apportionment is investigated by distinguishing between single and multi-plant firms.

[26] Unambiguous results only emerge if there is only one skill group. In this case, it is straightforward to show that the effects of a profit tax change in one jurisdiction on wages in that jurisdiction is smaller than it would be in a single plant firm.

3.8 Appendix B

Table 3.8.1: Robustness: effects on wages in levels

Model	(1)	(2)	(3)	(4)
log collection rate$_t$	-0.093**	-0.098**		
	(0.045)	(0.048)		
log collection rate$_{t-1}$		-0.019		
		(0.034)		
collection rate			-0.028**	-0.031**
			(0.013)	(0.014)
collection rate$_{t-1}$				-0.004
				(0.010)
log value added	0.004**	0.004**	0.004**	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)
log investment	-0.000	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
log employees	0.032***	0.033***	0.032***	0.033***
	(0.009)	(0.010)	(0.009)	(0.010)
log full-time hours	0.033	0.035	0.033	0.035
	(0.040)	(0.040)	(0.040)	(0.040)
work council	0.006**	0.007*	0.006**	0.007*
	(0.003)	(0.004)	(0.003)	(0.004)
share high-skilled employees	0.002	0.001	0.001	0.000
	(0.058)	(0.061)	(0.058)	(0.060)
share medium-skilled employees	-0.020	-0.023	-0.019	-0.023
	(0.048)	(0.052)	(0.048)	(0.051)
local unemp. rate	-0.026	0.043	-0.028	0.042
	(0.145)	(0.155)	(0.145)	(0.154)
community population	0.106*	0.137**	0.104*	0.134**
	(0.062)	(0.066)	(0.062)	(0.066)
log expenses	-0.013**	-0.013*	-0.013**	-0.013*
	(0.007)	(0.007)	(0.007)	(0.007)
log revenues	0.017**	0.019***	0.017**	0.019***
	(0.007)	(0.007)	(0.007)	(0.007)
local property tax rate	-0.079**	-0.087**	-0.076**	-0.083**
	(0.037)	(0.038)	(0.036)	(0.037)
Adjusted R^2	0.198	0.181	0.198	0.181
Observations	3512491	3204780	3512491	3204780
Groups	1085873	1014992	1085873	1014992
Clusters	395	395	395	395
Elasticity	-0.38	-0.46	-0.46	-0.54
Incidence	-0.53	-0.67	-0.64	-0.79

Note: All specifications include person, firm, municipal and yea r fixed effects as well as: skill-year, occupation-year, firm size-year, CBA type-year, state-year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Table 3.8.2: Robustness: Effects on log wages - different fixed effects

Model	(1)	(2)	(3)	(4)
Fixed Effects	Baseline	Only Worker	Only Firm	Only Municipal
log collection rate	-0.098**	-0.098**	-0.059	-0.009
	(0.046)	(0.046)	(0.049)	(0.068)
log value added	0.004**	0.004**	0.005***	0.012***
	(0.002)	(0.002)	(0.002)	(0.003)
log investment	-0.000	-0.000	-0.001	0.004**
	(0.001)	(0.001)	(0.001)	(0.002)
log employees	0.031***	0.031***	0.020**	-0.002
	(0.009)	(0.009)	(0.009)	(0.007)
log full-time hours	0.036	0.036	0.015	-0.264***
	(0.041)	(0.041)	(0.046)	(0.072)
work council	0.006**	0.006**	0.002	0.096***
	(0.003)	(0.003)	(0.004)	(0.013)
share high-skilled employees	-0.000	-0.000	0.030	0.099**
	(0.058)	(0.058)	(0.054)	(0.047)
share medium-skilled employees	-0.022	-0.022	-0.032	0.087**
	(0.049)	(0.049)	(0.041)	(0.039)
local unemp. rate	-0.023	-0.023	-0.059	-0.301
	(0.148)	(0.148)	(0.140)	(0.184)
community population	0.103*	0.103*	0.100	0.006
	(0.062)	(0.062)	(0.064)	(0.090)
log expenses	-0.011	-0.011	-0.010	-0.002
	(0.007)	(0.007)	(0.007)	(0.007)
log revenues	0.016**	0.016**	0.019***	0.017*
	(0.007)	(0.007)	(0.007)	(0.008)
local property tax rate	-0.081**	-0.081**	-0.049	-0.070
	(0.038)	(0.038)	(0.042)	(0.047)
Adjusted R^2	0.200	0.200	0.214	0.238
Observations	3305718	3305718	3305718	3305718
Groups	984019	984019	2919	1412
Clusters	395	395	395	395
Elasticity	-0.40	-0.40	-0.24	-0.04
Incidence	-0.56	-0.56	-0.33	-0.05

Note: Dependent variable: log monthly wage. All specifications include year fixed effects as well as year-industry fixed effects. Clustered standard errors in parentheses. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Chapter 4

Employment effects of corporate taxation*

4.1 Introduction

In public finance there is an ample theoretical and empirical literature on the effects of corporate taxes on investment showing that higher corporate tax rates increase the user cost of capital, which in turn reduces investment (see Chirinko, 1993; Hassett and Hubbard, 2002; Bond and Van Reenen, 2007, for surveys). In contrast, there is hardly any empirical evidence on the effect of corporate profit taxation on employment. The only exception seems to be the study by Harju and Kosonen (2012), who investigate corporate tax effects on many different firm outcomes, but do not find a significant effect on labor demand.

Given that labor and capital are the two standard production factors in economic theory, this imbalance in empirical attention is somewhat surprising. Yet, there might be a simple explanation for it: corporate tax effects are usually studied in an international context, where corporate tax rates are set at the national level. The conventional assumption in these studies is that labor is internationally immobile, which rules out any employment effect a priori. In contrast, this paper focuses on the effects of *local* corporate taxation in Germany, where labor has been found to be mobile (Burda and

* This chapter is based on a (so far unpublished) single-authored manuscript with the title “Employment Effects of Local Corporate Taxes” (see Siegloch, 2013).

Hunt, 2001; Elhorst, 2003; Niebuhr, Granato, Haas, and Hamann, 2012) and wages in the competitive sector are unaffected by local tax changes (see Chapter 3 and Fuest et al., 2013). Under these assumptions, I theoretically show that employment effects of local corporate taxes are negative if labor and capital are complements.

I empirically test my theoretical hypothesis, exploiting the institutional setting of the German local business tax, where on average 8% of the 11,441 German municipalities change their tax rate per year. Relying on administrative linked employer-employee panel data merged to data on the universe of the German municipalities from 1998 to 2008, I show that employment indeed reacts elastically to changes in the profit tax rate. A 1% increase in the effective statutory tax rate leads to a drop in employment of 1.2%. Put differently, a one euro increase in the company's tax bill yields a decrease of the total wage bill of 85 cents for given average wages.

I find that the negative employment effect of local corporate taxes is particularly pronounced for medium-sized firms with 50 to 500 employees, which have been found to face the highest relative corporate tax burdens (see Gebhardt and Siemers, 2011 for evidence on Germany and OECD, 2013 for findings for other industrialized countries). I find no effect for small firms with less than 50 employees and a negative but insignificant effect for firms with a staff of more than 500 workers. While many small German firms do not pay any corporate taxes, the non-significant effect for large firms could be either due to better tax planning opportunities, in particular for multinationals (see, e.g., Weichenrieder, 2009; Egger, Eggert, and Winner, 2010), or to reverse causality as locally more influential firms might impact municipal tax rates. Making the potential reverse causality mechanism explicit, I do not find evidence of firms influencing tax rates. Moreover, my results are very unlikely to be biased by omitted confounders since neither the inclusion of industry-year fixed effects nor of region-year fixed effects affects my point estimates. Furthermore, my results suggest that employment adjusts mainly at the extensive margin and not through changes in working hours. I find that high-skilled, full-time and female employment reacts most to tax changes. In addition, employment reacts more strongly if a firm is less profitable or has a work council.

Testing the central assumptions that underlie my theoretical model, I find that changes in municipal employment levels have a significantly negative effect on changes in employment in the other municipalities within the same labor market region, which indicates intra-regional labor mobility. Moreover, wages are not significantly affected by corporate tax changes, which is in line with the finding of a negligible indirect wage effect of corporate taxation (see Chapter 3 and Fuest et al., 2013). Last, I provide evidence suggesting that the negative employment effect is driven by lower investments and output, confirming the complementarity assumption on the production factors.

The remainder of the paper is structured as follows. Section 4.2 briefly describes the institutional design of German business taxation. In Section 4.3 I theoretically show that profit taxes have a negative employment effect if labor and capital are complements and labor is mobile. In Section 4.4 I present the datasets used. The empirical analysis is conducted in Section 4.5, while Section 4.6 concludes by discussing the policy implications of my findings.

4.2 Institutional setting

Corporate firms (*Kapitalgesellschaften*) face two profit taxes in Germany, the local business tax (LBT) and the corporate tax (CT). Both tax bases are identical and essentially consist of operating profits. Until 2007 LBT liabilities could be deducted from the tax base. The tax rate of the local business tax, τ_{LBT}, consists of two components: the basic federal rate (*Steuermesszahl*), τ_{fed}, which is set at the national level, and the collection rate (*Hebesatz*), cr. Thus $\tau_{LBT} = \tau_{fed} \cdot cr$. τ_{fed} was at 5.0% from 1998 to 2007 and decreased to 3.5% in 2008. The collection rate is autarkically set by each of the 11,441 German municipalities (*Gemeinden*) and usually varied between 250% and 450% in the period from 1998 to 2008 (5th and 95th percentiles). There is substantial cross-sectional and time variation in the municipal collection rates. See Section 3.4.1 in Chapter 3 for a detailed exposition. The corporate tax rate, τ_{CT}, has undergone several changes in recent years and was reduced from 45% in 1998 to 15% in 2008. In all years, a so-called solidary surcharge, *soli*, of 5.5% of the corporate tax rate is added.

In order to calculate the total effective tax rate for corporate firms, first, LBT and CT rates are added. Second, the deduction of the LBT liabilities from its own tax base has to be taken into account. The effective (statutory) marginal tax rate[1] for corporate firms, τ_{EMTR}^{corp}, from 1998 to 2007, is

[1] Note that this is an effective *statutory* marginal tax rate, as opposed to more conventional measures of the effective marginal tax rate which include tax base parameters.

$$\tau^{corp}_{EMTR} = \frac{\tau_{CT} \cdot (1 + soli) + \tau_{fed} \cdot cr}{1 + \tau_{fed} \cdot cr}. \tag{4.2.1}$$

Since 2008 the denominator of equation (4.2.1) is 1, as the LBT cannot be deducted from its own base anymore. For a more detailed description of corporate taxation in Germany see 3.2.1 of Chapter 3.

As regards the geographical and administrative setting, Germany has a total surface area of 357,000 km^2 (137,838 mi^2). Given 11,441 municipalities, the average municipality is small with an area of roughly 31 km^2 (12 mi^2). As in many other countries there are different administrative jurisdictions at different geographical aggregation levels. Municipalities are the smallest entities. The next larger type of jurisdiction is the county (*Kreis*). There are just above 400 counties in Germany, thus on average 28 municipalities make up a county. Larger municipalities – usually cities with more than 100,000 inhabitants – make up their own county (*kreisfreie Stadt*). The next higher level, though not being an official jurisdiction, is the labor market region. While there are several definitions, labor market regions delineate independent economic areas around an economic center where the appendant areas are defined on commuter flows. In this paper, I follow the rather narrow definition of the Federal Institute for Research on Building, Urban Affairs and Spatial Development, which differentiates between 258 labor market regions. Thus, there are on average 44 municipalities per labor market region. On the highest regional level there are 16 federal states (*Bundesländer*).

4.3 Theoretical framework

In this section I use a simple microeconomic model of labor demand to show under which circumstances corporate profit taxation affects a firm's employment decision. Let there be a representative firm that produces with a production function F and input factors capital K and labor L. I assume that F is a standard neoclassical production function with positive and decreasing returns to scale, $\frac{\partial F(\cdot)}{\partial i} = F_i > 0, \frac{\partial^2 F(\cdot)}{\partial i^2} = F_{ii} < 0, \frac{\partial \frac{\partial F(\cdot)}{\partial i}}{\partial j} = F_{ij} > 0$ with $i, j \in \{K, L\}, i \neq j$. Furthermore, I assume that $F(K, L)$ is strictly concave, implying that $F_{LL}F_{KK} - F^2_{KL} > 0$. This strict concavity assumption is needed to ensure strictly positive corporate profits, which is obviously necessary for this analysis.

The firm faces a corporate profit tax τ where a share $\alpha \in [0, 1]$ of the capital costs can be deducted from the tax base. An alternative interpretation of α is that it measures the share of capital that is financed by debt. Payments on debts, i.e. interests, can usually be deducted from the tax base, while payments on equity are normally paid out of the after-tax profits. Thus, usually $0 < \alpha < 1$. The company's after-tax profits Π are thus

$$\Pi = (1-\tau)[pF(K,L) - wL] - (1-\alpha\tau)rK,$$

where r is the interest rate, w the wage and p the output price. Firms choose capital K and employment L so that Π is maximized. Maximization yields the following first order conditions, determining factor demands

$$F_L = \frac{w}{p} \tag{4.3.1}$$

$$F_K = \frac{(1-\alpha\tau)}{(1-\tau)}\frac{r}{p}. \tag{4.3.2}$$

Labor demand is not directly affected by the profit tax as shown by equation (4.3.1). The same is true for the capital demand, if $\alpha = 1$, i.e. if all capital costs are deductible from the profit tax base. In that case τ would neither affect L nor K.

In the following I assume that $\alpha < 1$. Totally differentiating[2] equations (4.3.1) and (4.3.2) yields

$$F_{LL}dL + F_{LK}dK = \frac{1}{p}dw - \frac{1}{p^2}dp \tag{4.3.3}$$

$$F_{KK}dK + F_{LK}dL = \frac{1-\alpha\tau}{(1-\tau)p}[dr - \frac{1}{p}dp] + \frac{(1-\alpha)r}{(1-\tau)^2 p}d\tau. \tag{4.3.4}$$

I assume that firms are price takers in the product market and that the non-tax costs of capital are not affected by changes in the tax rate, i.e. $dp = dr = 0$.

As mentioned above, the average German municipality is small. Thus, I make the assumption that labor is perfectly mobile across jurisdictional borders.[3] As a consequence, a change in the local tax rate leaves wages in the competitive sector unchanged, i.e. $dw = 0$ (cf. also Section 3.3.3). Given this, equation (4.3.3) simplifies

[2] To keep it simple, I assume that $d\alpha = 0$.

[3] This assumption will be discussed and relaxed below.

to $dL = -\frac{F_{LK}}{F_{LL}}dK$. Plugging into equation (4.3.4) and rearranging shows that capital decreases as the profit tax rate increases

$$\frac{dK}{d\tau} = \frac{(1-\alpha)rF_{LL}}{(1-\tau)^2 p[F_{LL}F_{KK} - F_{KL}^2]} < 0, \tag{4.3.5}$$

since $F_{LL} < 0$ and, by the strict concavity assumption, $F_{LL}F_{KK} - F_{KL}^2 > 0$. The effect on employment is

$$\frac{dL}{d\tau} = \underbrace{-\frac{F_{LK}}{F_{LL}}}_{>0}\frac{dK}{d\tau} < 0. \tag{4.3.6}$$

Hence, I find a negative employment effect of corporate taxation which goes through capital. The rationale is straightforward: higher corporate taxes reduce capital. Due to the complementarity between capital and labor ($F_{KL} > 0$), the marginal product of labor has to decrease. According to condition (4.3.1) the marginal product of labor equals the real wage. If w and p are given, F_L cannot decrease. Thus, the marginal product of labor has to remain at its pre-tax-reform level. This can only be achieved by reducing L given $F_{LL} < 0$. Hence, the central result of my theoretical model is that employment decreases if corporate taxes rise. This hypothesis will be tested empirically in Section 4.5. In addition, I will also provide evidence on the underlying channel of this effect going through reduced investment (and output).

From the exposition so far, it is clear that the negative employment effect hinges on the assumption that wages are not affected by corporate tax changes. When looking at changes of national corporate taxes, this assumption seems quite unrealistic. Based on the seminal work by Harberger (1962), many general equilibrium studies have shown that labor bears a substantial share of the corporate tax burden through lower wages in small open economies (see, e.g., Kotlikoff and Summers, 1987 or Gravelle, 2013 for a survey of computable general equilibrium studies). Yet, these studies analyze the incidence of corporate taxes in an international context and under the assumption that labor is immobile. This result can also be sketched within the partial framework developed above by looking at the other limiting case of perfect labor immobility ($dL = 0$). It immediately follows from equations (4.3.3) and (4.3.4) that $\frac{dw}{d\tau} = \frac{1-\alpha}{(1-\tau)^2}\frac{F_{LK}}{F_{KK}} < 0$.

Thus, employment (and wage) effects crucially depend on the mobility assumptions of the factor inputs and the resulting corporate tax incidence on factor prices.

Of course, the two polar cases of perfect labor mobility and perfect labor immobility are rather of theoretical interest. Nevertheless, the analysis shows that if labor is somewhat mobile within the labor market region, local corporate taxes have a negative effect on employment. As discussed in Section 4.2, the local business tax rate is set by the municipality, which is a rather small jurisdiction. It seems reasonable that workers do not restrict their (on the) job search effort to the municipal area, but are willing to accept jobs within the county or within the labor market region. Note that labor market regions are explicitly delineated based on commuter flows. In fact, the regional mobility of labor has already been demonstrated in other studies (Burda and Hunt, 2001; Elhorst, 2003; Niebuhr et al., 2012). I provide further evidence of this mobility when testing the underlying assumption of the theoretical model in Section 4.5.3.

As mentioned above, most of the theoretical literature draws upon general equilibrium models to analyze corporate tax effects on production factors and their respective prices. Since the focus of this study is not the incidence of the corporate tax, I have opted for a partial analysis, which yields the same insights under the specific assumptions made and is more tractable (for inter-regional general equilibrium models see, e.g., McLure, 1969, 1970 or Jones, 1982). Yet, it is worthwhile to briefly discuss the corporate tax effects on the regional labor market equilibrium following the exposition of Bradford (1978). As argued above, it is a valid approximation that prices within the regional labor market, i.e. wages and capital costs, are not affected by an increasing tax rate in one small municipality. Instead production factors move out of the high-tax municipality: labor and capital will be spread equally over the other (lower-tax) municipalities in the region. This spread will slightly affect the marginal products of labor and capital in the other regions so that prices remain only approximately constant: in fact, given decreasing returns to scale, equilibrium wages (and capital costs) will decrease slightly. The aggregate welfare loss induced by these small price reductions is substantial and exceeds tax revenues and dead weight output losses in the high-tax municipality.

Following this argument, theory predicts small wage decreases following a tax increase, either because "factor prices may be constant but factor returns are not" (Bradford, 1978) or because labor is not perfectly mobile. Therefore, I expect that the wage effect of a local corporate tax increase is either small and negative or zero. This will also be tested below. In fact, Fuest et al. (2013) show in a related study that there is only a small (indirect) general equilibrium wage effect of local corporate taxes (see Chapter 3). In line with Arulampalam et al. (2012), they, however, find a substantial direct wage effect, which arises in a wage bargaining context where higher corporate taxes reduce the rents over which firms and unions bargain for *given* output, employment and investment.

4.4 Data

I combine two distinct data sources: first, administrative data on the universe of German municipalities, and, second, detailed administrative linked employer-employee data.

Municipal and regional data As far as municipal data are concerned, I make use of statistics provided by the Statistical Offices of the 16 German federal states (*Statistische Landesämter*). The states collect information on the fiscal and budgetary situation of the municipalities. After combining and harmonizing the annual state specific datasets, I obtain a panel on the universe of municipalities from 1998 to 2008 covering roughly 125,000 data points – i.e. municipality-years. Most importantly, the dataset contains information on the local collection rate, but also on the population size and municipal expenses and revenues. Moreover, I add data from the German federal employment agency on regional unemployment rates on the more aggregate county (*Kreis*) level to control for local labor market conditions. Finally, I merge the definition of labor market regions provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (*Bundesinstitut für Bau-, Stadt- und Raumforschung*) to my data (see Eckey et al., 2006).

Linked employer-employee data I merge the municipal data to the linked employer-employee dataset (LIAB) provided by the Institute of Employment Research (IAB) in Nuremberg, Germany (Alda et al., 2005). The employee data are a 2% sample of the administrative employment statistics of the German Federal Employment Agency (*Bundesagentur für Arbeit*), called the German employment register, which covers all employees paying social security contributions or receiving unemployment benefits (Bender et al., 2000). The employee information are recorded annually on June 30th and include information on wages, age, tenure, occupation, employment type (full-time, part-time or marginal employment) and qualification. I differentiate between three skill groups: high-skilled workers have obtained a college/university degree; medium-skilled have either completed a vocational training or obtain the highest high school diploma (*Abitur*); low-skilled have neither completed a vocational training nor obtained the *Abitur*. Civil servants are excluded as they are rarely observed in the social security data. I observe between 1.6 and 2.0 million workers per year.

The firm component of the LIAB is the IAB Establishment Panel (Kölling, 2000), which is a stratified random sample of the universe of all German establishments. The term "establishment" refers to the fact that the observational unit is the individual plant, not the firm; there can be several plants per firm.[4] Nevertheless, I will use the term firm and plant as synonyms when discussing the results unless this gives rise to confusion. The employer data covers establishments with at least one worker for whom social insurance contributions were paid. I extract the following establishment variables: value added, investment, number of employees, industry affiliation, total wage bill, legal type (corporate vs. non-corporate), collective agreement status, self-rated profitability (measured on a four-point scale ranging from good to poor), presence of a work council. Dropping all establishments with missing relevant variables, I observe about $5,000$ establishments per year.

Sample selection and descriptive statistics My observation period runs from 1998 to 2008. I select corporate firms that are liable to the local business tax. Table 4.4.1 shows some descriptive statistics of my baseline sample. The average monthly wage in the sample is 2,668 euros (all money variables are in 2008 euros). As expected, wages are increasing in skill and they are higher for men. The workforce consists of predominantly male, medium-skilled, full-time workers. The average firm has 291 employees and an annual value added of roughly 38 million euros. More than half of the firms are in the manufacturing sector. The average municipality is a small town with a population of 29,700 and a collection rate of 347%.

[4] In the context of the German business tax, the tax base of firms with multiple establishments is divided between municipalities according to formula apportionment based on the wage bill of the individual plants.

Table 4.4.1: Descriptive statistics, baseline sample, 1998-2008

	mean	sd	min	max	N
average wage	2668	805	500	5510	28052
wage high-skilled	3871	977	506	5510	17759
wage medium-skilled	2681	797	500	5510	27861
wage low-skilled	2331	721	506	5510	19405
wage male	2813	836	527	5510	27602
wage female	2390	784	483	5510	25898
wage blue-collar	2508	754	556	5510	26283
wage white-collar	3075	935	483	5510	26532
share high-skilled	0.08	0.13	0	1	28052
share medium-skilled	0.78	0.22	0	1	28052
share low-skilled	0.15	0.21	0	1	28052
share male	0.75	0.23	0	1	28052
share female	0.25	0.23	0	1	28052
share full-time	0.94	0.12	0	1	28052
share part-time	0.05	0.12	0	1	28052
share marg. employed	0.01	0.03	0	1	28052
share blue-collar	0.65	0.30	0	1	28052
share white-collar	0.35	0.30	0	1	28052
number of employees	291	1349	1	50524	28052
monthly wage bill (in 1000)	908	5366	0	224777	28052
fulltime hours	39	2	20	60	28052
annual value added (in 1000)	38407	265529	2	11688385	28052
annual investments (in 1000)	3589	25291	0	1755000	28052
share: sector union contract	0.50	0.50	0	1	28052
share: firm union contract	0.09	0.29	0	1	28052
share: no union contract	0.41	0.49	0	1	28052
share: stand alone plant	0.69	0.46	0	1	28052
share: part of multi-plant firm	0.30	0.46	0	1	28052
share: manufacturing	0.55	0.50	0	1	28052
share: construction	0.12	0.33	0	1	28052
share: traffic	0.06	0.23	0	1	28052
share: services	0.27	0.44	0	1	28052
collection rate (in %)	347	43	150	520	10722
population (in 1000)	29.70	114.22	0	3425	10722
local unemp. rate	0.12	0.06	0	0	10722
municipal expenses (in millions)	44.15	177.14	3	5971	10722
municipal revenues (in millions)	50.89	199.87	3	4416	10722

Source: LIAB and Statistical Offices of the Länder. *Notes:* All money variables in 2008 euros. Value added corresponds to total assets in the financial sector.

4.5 Empirical analysis

First, I empirically test my central theoretical hypothesis, namely the negative employment effect of corporate taxation (Section 4.5.1). Next, I use the rich LIAB data to estimate heterogeneous worker and firm effects. In a last step, I test the central assumptions underlying the theoretical model (Section 4.5.3), i.e. intra-regional labor mobility and the channel of the negative employment effect going through reduced capital.

4.5.1 Main results

In this section, I empirically investigate the employment effect of corporate taxation. Consider a firm i in municipality m, county c and year t. Following the theoretical model, I regress log employment $\ln(L_{imc,t})$ on the log of the collection rate in municipality m: $ln(cr_{mc,t})$. Note that the notation implies that corporate taxes immediately affect employment. Recall from Section 4.2 that there is, however, an implicit lag of 6 to 9 months between the two variables since the collection rate for year t is determined by the municipal council in the last quarter of year $t-1$ and employment of year t is measured as of June, 30th. The timing of the corporate tax effect will be analyzed below.

At the firm level, I condition on the log average wage and log full-time hours – summarized by vector $\mathbf{X}_{i,t}$. Note that controlling for the wage should not matter if regional labor mobility was perfect. I test this corollary in Section 4.5.3 together with assumption of regional labor mobility. Moreover, conditioning on full-time hours ensures that I focus on the *employment* effect of corporate taxation, thus the effect on the number of employees, shutting down potential adjustments in working hours. The margin of labor adjustments will also be discussed in greater detail below. Vector $\mathbf{Y}_{m,t}$ controls for municipal variables: local property taxes, population size, log annual revenues, log annual expenses. As stated above, these controls are important to rule out that the effects are simply driven by the labor market and/or fiscal situation of the municipality. For that reason I additionally include the county's unemployment rate $ur_{c,t}$. Moreover, I add three kinds of fixed effects: firm, municipal (which are collinear with county fixed effects) and year: μ_i, μ_m, μ_t. The regression model reads as follows:

$$\ln(L_{imc,t}) = \alpha ln(cr_{mc,t}) + \boldsymbol{\beta}\mathbf{X'}_{i,t} + \boldsymbol{\gamma}\mathbf{Y'}_{m,t} + \delta ur_{c,t} + \mu_i + \mu_m + \mu_t + \varepsilon_{imc,t}. \tag{4.5.1}$$

I assume that the error term $\varepsilon_{imc,t}$ is clustered at the county level. Year fixed effects μ_t account for any change in the tax base or in the federal rates of the local business tax and the corporate tax. By conditioning on μ_i and μ_m I wipe out any time-invariant confounding variable at the firm and municipal level (such as bargaining power or advantageous geographical location). Technically, I apply the spell fixed-effects estimator suggested by Andrews et al. (2006) by time-demeaning within each unique firm-municipality combination – time fixed effects are added manually.[5] The importance of including fixed effects for the identification of the corporate tax effect is discussed below. Of course, time-varying confounding variables and the simultaneity of corporate tax changes and employment fluctuations could bias my estimates. Both endogeneity concerns will also be addressed in the following.

Baseline results Table 4.5.1 shows the baseline results. In specification (1), I estimate equation (4.5.1) on the sample of liable corporate firms. I find a significant, negative effect of the log local collection rate on log employment of -0.293. To ease interpretation I transform the coefficient into an employment elasticity with respect to the effective (statutory) marginal tax rate (EMTR). This measure is reported at the bottom of Table 4.5.1. For model (1) I find that an increase of the EMTR by 1% leads to a decrease in employment of 1.19%. Additionally, I report an incidence measure which is defined as the reaction of the total wage bill – keeping wages constant – to a 1 euro increase in tax liabilities. For the first model the incidence is −85 cents.[6] Note that the other coefficients show sensible signs as full-time hours, wages and the unemployment rate have a significantly negative effect on employment.

In the following four specifications I split the sample by firm size. I find that firms with 50 to 500 employees show a large negative employment effect of corporate taxes. In contrast, small firms with less than 50 employees or very large companies with a workforce of more than 500 do not show significantly negative corporate tax effects. The U-shaped relationship between firm size and the employment effect could be explained by different relative tax burdens for firms of different size. In fact, recent evidence suggests that it is medium-sized firms which bear the highest relative tax burden both in Germany

[5] Note that only a few establishments change location, so in practice the firm dummies are collinear with the municipality dummies and the model is almost identical to a one-way fixed effects model.

[6] I obtain similar results when including the collection rate in levels instead of logs (see Table 4.7.1 in the appendix, Section 4.7).

Table 4.5.1: Baseline results

Model by # of employees	(1) all	(2) 1-50	(3) 50-250	(4) 250-500	(5) >500
log collection rate	-0.293**	0.068	-0.368**	-0.333**	-0.031
	(0.128)	(0.126)	(0.182)	(0.168)	(0.219)
log full-time hours	-0.252**	-0.096	-0.191*	0.37 5*	-0.302
	(0.101)	(0.127)	(0.112)	(0.193)	(0.203)
log average wage	-0.088***	-0.072***	-0.066***	0.009	-0.025
	(0.014)	(0.014)	(0.013)	(0.016)	(0.017)
local unemp. rate	-0.767***	-0.185	-0.727***	-0.846**	0.049
	(0.259)	(0.358)	(0.249)	(0.391)	(0.529)
municipal population	0.061	0.171	0.048	-0.443*	0.268
	(0.153)	(0.159)	(0.166)	(0.228)	(0.302)
log revenues	0.015	0.006	-0.012	-0.012	0.041*
	(0.015)	(0.025)	(0.016)	(0.021)	(0.024)
log expenses	-0.025*	-0.035	0.018	-0.009	0.056**
	(0.014)	(0.022)	(0.015)	(0.021)	(0.022)
local property tax rate	-0.148*	0.025	-0.112	0.055	-0.418***
	(0.078)	(0.108)	(0.079)	(0.072)	(0.118)
constant	6.104***	2.372	6.026***	9.992***	4.265
	(1.750)	(1.813)	(1.749)	(2.752)	(3.420)
Adjusted R^2	0.024	0.018	0.034	0.044	0.054
Observations	28052	12011	9990	2875	3176
Groups	8937	4453	3311	1020	929
Clusters	410	395	385	323	303
Employment elasticity	-1.19	0.28	-1.49	-1.35	-0.13
Euro incidence	-0.85	0.29	-1.70	-1.31	-0.10

Note: Dependent variable: log employees. All specifications include firm, municipal and year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

(Gebhardt and Siemers, 2011) and other industrialized countries (OECD, 2013). Very small firms are found to face disproportionally lower tax burdens. An explanation for lower relative tax burdens of large firm could be better tax planning opportunities, in particular for multinationals (see e.g., Weichenrieder, 2009; Egger et al., 2010).

Identification As usual, my estimates are prone to endogeneity due to reverse causality and/or omitted variables. Both endogeneity concerns have to be taken seriously in the context of this analysis. The first source of endogeneity, reverse causality, arises if firms (try to) influence municipal governments in setting the tax rate. To rule out this kind of simultaneity, I estimate a distributed lag model by iteratively adding the collection rates up to period $t-2$ to the baseline model.[7] Table 4.5.2 shows the results

[7] Recall that the implicit lag in the baseline model (collection rate in t on employment in t) is 6 to 9 months.

Table 4.5.2: Timing of the tax effect

Model	(1)	(2)	(3)
log collection rate$_t$	-0.293**	-0.104	-0.117
	(0.128)	(0.115)	(0.114)
log collection rate$_{t-1}$		-0.203**	-0.040
		(0.089)	(0.076)
log collection rate$_{t-2}$			-0.193**
			(0.088)
log full-time hours	-0.252**	-0.249**	-0.230**
	(0.101)	(0.103)	(0.096)
log average wage	-0.088***	-0.089***	-0.093***
	(0.014)	(0.015)	(0.014)
local unemp. rate	-0.767***	-0.719***	-0.696***
	(0.259)	(0.259)	(0.248)
community population	0.061	-0.070	-0.111
	(0.153)	(0.162)	(0.162)
log revenues	0.015	0.015	0.007
	(0.015)	(0.015)	(0.014)
log expenses	-0.025*	-0.025*	-0.020
	(0.014)	(0.014)	(0.014)
local property tax rate	-0.148*	-0.138*	-0.050
	(0.078)	(0.074)	(0.071)
constant	6.104***	7.491***	7.860***
	(1.750)	(1.829)	(1.818)
Adjusted R^2	0.024	0.025	0.026
Observations	28052	26281	24302
Groups	8937	8566	8111
Clusters	410	410	410
Long-term effect	-0.29	-0.31	-0.35

Note: Dependent variable: log total employees. All specifications include firm, municipal and year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

revealing three important insights. First, reverse causality seems to be no issue as it is always the corporate tax rate in the most distant period that is statistically significant. Second, the employment effect evolves over two years since the long-run effect, which is the sum of the lagged coefficients reported at the bottom of the table, increases as the lagged collection rates are added. Third, the baseline model without lagged collection rates is able to capture this long-run effect well and will thus be used in the remainder of this paper.

Next, I provide another test for reverse causality. A firm should only be able to influence the municipal tax rate if it is large and important. If an influential firm can prevent any tax change it disagrees with, the reverse causality is likely to bias my estimates towards zero. In that case there should be no association between local corporate taxes and employment for influential firms. Put the other way round, all tax changes that happen in a jurisdiction should neither affect investment nor employment decisions of influential firms since the changes happened with the (implicit) approval of the influential firm.

It is possible to make this reverse causality channel explicit. I generate an influence measure by defining the dummy variable "*influential firm*", which is equal to unity if the number of employees of a firm is larger than 1% of the population in the munici-

Table 4.5.3: Tests on reverse causality

Model by # of employees	(1) all	(2) 1-50	(3) 50-250	(4) 250-500	(5) >500
log collection rate	-0.301**	0.101	-0.179	-0.445***	-0.105
	(0.127)	(0.142)	(0.138)	(0.168)	(0.454)
log coll. rate x influential firm	0.155	-0.095	-0.255	0.150	0.200
	(0.165)	(0.164)	(0.189)	(0.177)	(0.516)
log full-time hours	-0.211**	-0.079	-0.148	0.410**	-0.319
	(0.100)	(0.126)	(0.109)	(0.191)	(0.205)
log average wage	-0.082***	-0.070***	-0.060***	0.010	-0.027
	(0.014)	(0.014)	(0.013)	(0.016)	(0.017)
influential firm	0.202	0.427**	0.573**	-0.024	0.129
	(0.199)	(0.208)	(0.236)	(0.234)	(0.699)
local unemp. rate	-0.729***	-0.134	-0.740***	-0.822**	-0.016
	(0.257)	(0.340)	(0.236)	(0.357)	(0.505)
municipal population	0.140	0.195	0.130	-0.326	0.333
	(0.148)	(0.153)	(0.157)	(0.204)	(0.295)
log revenues	0.017	0.009	-0.015	-0.007	0.039*
	(0.015)	(0.024)	(0.016)	(0.020)	(0.024)
log expenses	-0.026*	-0.040*	0.022	-0.004	0.050**
	(0.014)	(0.022)	(0.015)	(0.020)	(0.021)
local property tax rate	-0.116	0.024	-0.101	0.100	-0.393***
	(0.081)	(0.108)	(0.077)	(0.064)	(0.120)
constant	4.914***	2.017	4.592***	8.438***	3.582
	(1.706)	(1.741)	(1.749)	(2.375)	(3.607)
Adjusted R^2	0.072	0.034	0.089	0.099	0.126
Observations	28052	12011	9990	2875	3176
Groups	8937	4453	3311	1020	929
Clusters	410	395	385	323	303
Share of influential firms	0.24	0.06	0.28	0.49	0.60

Note: Dependent variable: log employees. All specifications include firm, municipal and year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

pality, which is an arbitrary but arguably a reasonable cut-off: in total roughly 25% of the firms are regarded as influential; taking an average mid-sized town of 30,000 inhabitants a firm must have at least 300 employees to be regarded as influential. I interact the "*influential firm*" dummy with the collection rate. As argued above, the reverse causality hypothesis is that there is a significantly smaller absolute corporate tax effect for influential firms. Yet, Table 4.5.3 shows that the interaction term "*log coll. rate x influential firm*" is never significant across specifications. This suggests that there is neither a reverse causality bias towards zero as hypothesized above nor in the other direction.

The second source of endogeneity could arise if there were unobserved factors influencing both the employment decision of a firm and the local tax rate. As discussed above, I account for any time-invariant confounder at the firm or municipal level by in-

Table 4.5.4: Tests for omitted variables

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log collection rate	-0.290**	-0.141	-0.241	-0.342***	-0.263**	-0.301**	-0.272
	(0.129)	(0.131)	(0.358)	(0.125)	(0.123)	(0.149)	(0.166)
log full-time hours	-0.250**	-0.877***	-8.682***	-0.257**	-0.199**	-0.271**	-0.254**
	(0.102)	(0.111)	(0.612)	(0.102)	(0.098)	(0.105)	(0.113)
log average wage	-0.088***	-0.046***	0.444***	-0.088***	-0.096***	-0.089***	-0.089***
	(0.014)	(0.013)	(0.057)	(0.014)	(0.014)	(0.013)	(0.014)
local unemp. rate	-0.752***	-1.572***	1.077	-0.732***	-0.557**	-0.437	-0.427
	(0.254)	(0.232)	(1.066)	(0.251)	(0.237)	(0.290)	(0.411)
municipal population	0.088	0.067***	0.197	-0.020	0.118	0.068	-0.001
	(0.148)	(0.024)	(0.422)	(0.032)	(0.137)	(0.159)	(0.181)
log revenues	0.017	0.016	-0.049	0.019	0.014	0.014	0.021
	(0.015)	(0.013)	(0.076)	(0.015)	(0.014)	(0.015)	(0.017)
log expenses	-0.022	-0.036**	-0.010	-0.021	-0.021	-0.000	0.000
	(0.015)	(0.015)	(0.081)	(0.014)	(0.014)	(0.015)	(0.016)
local property tax rate	-0.155**	-0.230***	0.399	-0.132	-0.185**	-0.084	-0.106
	(0.078)	(0.076)	(0.291)	(0.081)	(0.073)	(0.077)	(0.083)
constant	5.723***	7.958***	31.399***	6.879***	5.493***	5.608***	1.825
	(1.708)	(0.599)	(4.625)	(0.586)	(1.643)	(1.808)	(.)
firm FE	√			√	√	√	√
municipal FE	√		√		√	√	√
industry-year FE					√		
state-year FE						√	
labor market region-year FE							√
Adjusted R^2	0.025	0.065	0.122	0.025	0.060	0.035	0.079
Observations	27756	27756	27756	27756	27756	27756	27756
Groups	8834	8834	2586	8777	8834	8834	8834
Clusters	410	410	410	410	410	410	410
Employment elasticity	-1.18	-0.57	-0.98	-1.39	-1.07	-1.22	-1.10
Euro incidence	-0.84	-0.41	-0.70	-0.99	-0.76	-0.87	-0.78

Note: Dependent variable: log employees. All specifications include year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

cluding the respective fixed effects. Specifications (1) through (4) of Table 4.5.4 demonstrate the importance of controlling for such time-invariant confounders. Specification (1) shows the baseline results, controlling for firm and municipal fixed effects.[8] In specification (2) I do not account for any fixed effects (except for year dummies) and estimate a random effects model: the absolute corporate tax effect becomes smaller and insignificant. In specification (3) I only control for municipal but not for firm fixed effects. While the negative point estimate increases slightly compared to the prior specification, the corporate tax effect remains insignificant. When only controlling for firm fixed ef-

[8] Note that the number of observations is slightly smaller than in the baseline specification (1) of Table 4.5.1 as I dropped all firms that move counties, in order to have a consistent treatment of error terms (clustered at the county level) through all specifications of Table 4.5.4.

fects, results become significant, which is not surprising as only very few plants switch municipalities (within counties) during my observation period. Thus, by controlling for firm fixed effects I automatically control for municipal fixed effects in most cases. In sum, these results stress the importance of accounting for time-invariant confounders at the firm level.

Yet, it is much more difficult to deal with time-varying confounding variables. As many industries are regionally clustered (Sternberg and Litzenberger, 2004), industry specific shocks might drive both local tax rates and employment. Imagine a positive productivity (or demand) shock in a specific industry and regional industry clustering. It is likely that such a shock affects both the employment decisions of firms (due to increased production) and the fiscal situation of the respective municipalities. By adding industry-year fixed effects to my baseline specification, I can control for such kind of biases. The inclusion of industry-year fixed effects also circumvents a related identification problem of my empirical model. Given that the theoretical employment effect goes through investment and thus output, I can neither control for output nor for investment in the regression. Industry-year fixed effects can be used as a proxy for omitted and potentially confounding time-varying firm productivity. As model (5) of Table 4.5.4 shows, adding industry-year fixed effects hardly affects results.

On a similar note, any shock hitting the regional labor market might affect simultaneously the budget of municipalities and the employment behavior of firms. Hence, I start by adding state-year fixed effects to my baseline model in specification (6) to account for such potentially unobserved regional labor market shocks. Reassuringly, the coefficient on the collection rate in specification (6) hardly changes and remains highly statistically significant. Finally, I add labor market region-year fixed effects to control for labor market shocks at the very local level. Given 258 labor market regions and 11 years, 2,837 dummy variables are added to the model. Naturally, the multitude of dummy variables takes out much of the variation in collection rates. As a consequence standard errors increase and the tax coefficient becomes insignificant (specification (7)). Nevertheless, it is striking that the point estimate hardly changes. All this suggests that my results are quite robust to time-varying confounders.[9]

[9] There is more evidence that makes it unlikely that estimates are biased due to firm or local confounders. Table 4.7.2 in the appendix (Section 4.7) shows that omitting all firm and/or municipal controls does not hugely affect the coefficient on the collection rate, either.

Table 4.5.5: Placebo test

Model Sample	(1) Baseline	(2) Placebo
log collection rate	-0.293**	-0.023
	(0.128)	(0.255)
log full-time hours	-0.252**	-0.279
	(0.101)	(0.336)
log average wage	-0.088***	-0.270***
	(0.014)	(0.047)
local unemp. rate	-0.767***	0.077
	(0.259)	(0.530)
community population	0.061	0.691**
	(0.153)	(0.302)
log revenues	0.015	0.038
	(0.015)	(0.029)
log expenses	-0.025*	0.005
	(0.014)	(0.027)
local property tax rate	-0.148*	-0.013
	(0.078)	(0.167)
constant	6.104***	-0.986
	(1.750)	(3.443)
Adjusted R^2	0.024	0.064
Observations	28052	5026
Groups	8937	1615
Clusters	410	327
Employment elasticity	-1.19	-0.09
Euro incidence	-0.85	-0.01

Note: Dependent variable: log employees. All specifications include firm, municipal and year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Last, I provide a further identification test, exploiting the institutional feature that not all firms are liable to the local business tax in Germany. There are exemptions for certain firm types, e.g. for firms in the public and in the agricultural sector. I conduct a placebo test by running my baseline specification on corporate firms that are not liable to the LBT. Table 4.5.5 shows that the collection rate has no impact on employment of non-liable firms. However, it should be noted that employment reacts quite differently to the other control variables as well, which might indicate that non-liable firms are not a valid control group.

Margin of adjustment In general, the negative employment effect can materialize through layoffs and/or foregone hirings in case of a tax increase or through foregone layoffs and/or hirings in case of a tax reduction.[10] In order to shed further light on this

[10] Recall from Table 3.4.1 in Chapter 3 that about 90% of changes in local business tax rates are increases.

adjustment channel, I exploit a question in the IAB Establishment Panel on whether a firm intends to hire new staff in the remainder of the year (yes/no). I run a (firm) fixed effect logit regression of the intention to hire on dummy variables indicating either a tax increase or a tax decrease from period $t-1$ to t. Table 4.5.6 shows that the odds of hiring new staff are significantly higher in (the few) occasions where local collection rates decrease. The effect of tax increases is negative but statistically insignificantly so. This result suggests that parts of the negative employment effect of business taxation materialize through forgone hirings. Unfortunately, there is no question on intended layoffs in the firm survey.

Table 4.5.6: Odds ratios of intention to hire

Model	(1)	(2)	(3)
increase in coll. rate	0.972		0.985
	(0.088)		(0.089)
decrease in coll. rate		1.438**	1.435**
		(0.241)	(0.241)
log full-time hours	0.775	0.779	0.781
	(0.894)	(0.900)	(0.902)
log average wage	1.269**	1.265**	1.265**
	(0.137)	(0.136)	(0.136)
local unemp. rate	0.109	0.108	0.106
	(0.282)	(0.278)	(0.273)
community population	1.502	1.555	1.556
	(1.518)	(1.571)	(1.572)
log revenues	1.260	1.255	1.255
	(0.256)	(0.255)	(0.255)
log expenses	0.936	0.941	0.941
	(0.185)	(0.186)	(0.186)
local property tax rate	1.824	1.744	1.769
	(1.079)	(1.022)	(1.048)
Log likelihood	-4002.300	-4000.000	-4000.000
p-value LR test	0.00	0.00	0.00
Observations	11141	11141	11141

Note: Dependent variable: dummy variable on the intention to hire. All specifications include firm, municipal and year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Furthermore, firms can react to tax changes at the extensive margin (employment) and/or at the intensive margin (working hours). Recall that the baseline model focuses on the extensive margin conditional on full-time working hours.[11] Specification (2) of Table 4.5.7 shows that dropping working hours from the regression model does not af-

[11] Information on the usual full-time hours is available in almost every wave. If this information is not available values have been imputed by linear interpolation.

fect the results (specification (1) shows the baseline results from above). This suggests that profit taxes affect total hours mainly via the extensive margin. This interpretation is corroborated when looking at specifications (3) and (4) of Table 4.5.7, where the dependent variable is log total hours.[12] As soon as I condition on the number of employees, the effect of the collection rate on total hours becomes basically zero.

Table 4.5.7: Margin of adjustment

Model	(1)	(2)	(3)	(4)
Dependent variable	log employees		log total hours	
log collection rate	-0.293**	-0.291**	-0.298**	-0.007
	(0.128)	(0.127)	(0.131)	(0.010)
log full-time hours	-0.252**			
	(0.101)			
log employees				0.998***
				(0.001)
log average wage	-0.088***	-0.088***	-0.088***	0.000
	(0.014)	(0.014)	(0.014)	(0.001)
local unemp. rate	-0.767***	-0.777***	-0.739***	0.036*
	(0.259)	(0.259)	(0.259)	(0.022)
community population	0.061	0.051	0.089	0.038***
	(0.153)	(0.153)	(0.154)	(0.012)
log revenues	0.015	0.015	0.016	0.001
	(0.015)	(0.015)	(0.015)	(0.001)
log expenses	-0.025*	-0.025*	-0.026*	-0.001
	(0.014)	(0.014)	(0.015)	(0.001)
local property tax rate	-0.148*	-0.150*	-0.141*	0.009
	(0.078)	(0.077)	(0.081)	(0.008)
constant	6.104***	5.281***	8.543***	3.275***
	(1.750)	(1.700)	(1.715)	(0.127)
Adjusted R^2	0.024	0.024	0.023	0.991
Observations	28052	28052	28052	28052
Groups	8937	8937	8937	8937
Clusters	410	410	410	410

Note: Dependent variable: log employees and log total hours. All specifications include firm, municipal and year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

4.5.2 Heterogeneous effects

In the following subsection, I use the rich LIAB data to check whether there are heterogeneous effects both in terms of worker and firm characteristics. With regard to the former, I make use of the administrative employee part of LIAB to obtain employment

[12] I construct this measure using the (full/part-time) share of the firm, the number of employees and the average full-time hours. I assume that part-time (marginally employed) workers work half (a quarter) of the full-time hours.

shares of different labor inputs – differentiated by skill, work contract, gender and collar type. I calculate the number of workers for each labor input and re-estimate the baseline model for each group (conditional on the log of the group specific wage rather than the average wage within the firm).

Table 4.5.8 shows that absolute employment elasticities are increasing in skill: high-skilled employment reacts most strongly to changes in profit taxes with an elasticity of -1.42, followed by medium-skilled (-1.09), while the effect on the low-skilled staff (-0.88) is not statistically distinguishable from zero. This finding is consistent with the mobility assumption made above, since better qualified workers are likely to be more mobile and move to other firms in low-tax municipalities more quickly. Note that despite the higher elasticity for high-skilled, the incidence measure indicates that in money terms the reaction of medium-skilled employment matters most, which is due to the fact that the share of medium-skilled workers in the sample is about 75%. I also find that it is only full-time employment that significantly reacts to local tax changes. Part-time and non-standard employment (marginal employment) do not show a significantly negative corporate tax effect. Again, this could be explained by labor mobility: if there are fixed costs of changing jobs, it will be relatively more costly for a part-time worker to change employers. Interestingly, the effect on employment is mainly borne by women, while the effect on male employment is just above the threshold of statistical significance (though of almost identical magnitude). The last two columns of Table 4.5.8 show that the negative employment effect is stronger for blue collar employment.

Table 4.5.8: Heterogenous worker effects

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Employee type	high-skilled	medium-skilled	low-skilled	full-time	part-time	marg. employed	female	male	blue-collar	white-collar
log collection rate	-0.349**	-0.267**	-0.217	-0.329**	-0.058	0.083	-0.226**	-0.234	-0.415**	-0.218*
	(0.169)	(0.129)	(0.268)	(0.137)	(0.381)	(0.641)	(0.110)	(0.146)	(0.166)	(0.127)
log full-time hours	-0.290	-0.191*	-0.210	-0.168	-0.967***	-0.470	-0.330**	-0.181	-0.113	-0.309**
	(0.242)	(0.113)	(0.271)	(0.109)	(0.346)	(0.501)	(0.141)	(0.111)	(0.138)	(0.156)
respect. log wage	-0.253***	-0.105	-0.320***	-0.276***	0.025	0.069***	-0.348***	-0.155*	-0.281***	-0.115**
	(0.081)	(0.072)	(0.087)	(0.077)	(0.057)	(0.019)	(0.055)	(0.084)	(0.081)	(0.050)
local unemp. rate	0.462	-0.864***	-0.711	-0.833***	-0.199	0.334	-0.587*	-0.913***	-0.879***	-0.662**
	(0.414)	(0.289)	(0.542)	(0.264)	(0.631)	(1.647)	(0.316)	(0.282)	(0.309)	(0.315)
community population	0.994***	0.163	0.037	0.037	1.082***	-1.662*	-0.023	0.164	-0.018	0.229
	(0.270)	(0.160)	(0.321)	(0.156)	(0.417)	(0.887)	(0.163)	(0.167)	(0.187)	(0.162)
log revenues	0.025	0.018	-0.008	0.009	0.013	-0.031	0.023	0.019	0.014	0.018
	(0.029)	(0.016)	(0.025)	(0.016)	(0.043)	(0.107)	(0.019)	(0.016)	(0.018)	(0.019)
log expenses	-0.017	-0.026	-0.010	-0.019	-0.004	-0.035	-0.032*	-0.022	-0.021	-0.040**
	(0.029)	(0.016)	(0.027)	(0.015)	(0.042)	(0.114)	(0.017)	(0.017)	(0.018)	(0.018)
local property tax rate	-0.057	-0.123	-0.191	-0.096	-0.428**	-0.108	-0.188**	-0.152*	-0.150*	-0.138
	(0.105)	(0.084)	(0.121)	(0.079)	(0.166)	(0.479)	(0.074)	(0.082)	(0.090)	(0.089)
constant	-4.774	4.559**	6.350*	7.433***	-5.739	21.323**	7.708***	4.805**	7.675***	3.619*
	(3.334)	(2.091)	(3.742)	(2.012)	(4.660)	(9.954)	(2.026)	(2.145)	(2.147)	(1.964)
Adjusted R^2	0.023	0.011	0.039	0.023	0.018	0.017	0.029	0.014	0.022	0.009
Observations	17558	27855	19296	28051	15384	5447	25807	27587	26243	26480
Groups	5494	8861	6355	8937	5099	2747	8220	8753	8281	8429
Clusters	405	410	406	410	407	390	410	410	410	410
Employment elasticity	-1.42	-1.09	-0.88	-1.34	-0.24	0.34	-0.92	-0.95	-1.69	-0.88
Euro incidence	-0.19	-0.56	-0.09	-0.8 9	-0.01	0.00	-0.12	-0.56	-0.74	-0.26

Note: Dependent variable: log employees for specific worker group. All specifications include firm, municipal and year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Table 4.5.9: Heterogenous firm effects (medium-sized firms)

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log collection rate	-0.473***	-0.537***	-0.578***	-0.546***	-0.368**	-0.455**	-0.490**
	(0.176)	(0.168)	(0.184)	(0.170)	(0.146)	(0.213)	(0.204)
log collection rate ∗ services	-0.595						
	(0.496)						
log collection rate ∗ medium profitab.		-0.010**					
		(0.004)					
log collection rate ∗ sufficient profitab.		-0.026***					
		(0.005)					
log collection rate ∗ poor profitab.		-0.031***					
		(0.006)					
log collection rate ∗ stand alone			0.037				
			(0.077)				
log collection rate ∗ firm level				0.004			
				(0.009)			
log collection rate ∗ no agreement				-0.007			
				(0.010)			
log collection rate ∗ work council					-0.286**		
					(0.124)		
log collection rate ∗ local tax unimp.						-0.392	
						(0.506)	
log collection rate ∗ West Germany							-0.266
							(0.319)
log full-time hours	-0.212*	-0.203*	-0.209*	-0.199*	-0.231**	-0.275**	-0.200*
	(0.112)	(0.111)	(0.113)	(0.110)	(0.112)	(0.135)	(0.111)
log average wage	-0.062***	-0.064***	-0.062***	-0.063***	-0.061***	-0.062***	-0.063***
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.018)	(0.015)
local unemp. rate	-0.807***	-0.815***	-0.817***	-0.818***	-0.820***	-0.802**	-0.797***
	(0.286)	(0.291)	(0.288)	(0.289)	(0.291)	(0.313)	(0.288)
community population	-0.090	-0.078	-0.069	-0.075	-0.062	-0.165	-0.057
	(0.169)	(0.170)	(0.169)	(0.171)	(0.174)	(0.182)	(0.174)
log revenues	-0.014	-0.013	-0.013	-0.014	-0.011	-0.022	-0.014
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.017)	(0.016)
log expenses	0.005	0.004	0.005	0.005	0.000	0.012	0.006
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.016)	(0.015)
local property tax rate	-0.073	-0.080	-0.075	-0.075	-0.079	-0.068	-0.061
	(0.086)	(0.085)	(0.085)	(0.085)	(0.088)	(0.087)	(0.090)
constant	9.312***	8.087***	7.963***	8.024***	8.185***	9.220***	7.936***
	(2.207)	(1.886)	(1.874)	(1.894)	(1.929)	(2.052)	(1.905)
Adjusted R^2	0.039	0.041	0.038	0.036	0.037	0.038	0.036
Observations	12865	12865	12807	12865	12716	8973	12865
Groups	4090	4090	4078	4090	4073	2104	4090
Clusters	398	398	397	398	398	359	398

Note: Dependent variable: log employees. All specifications include firm, municipal and year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Next, I check whether there are heterogeneous firm effects using the large variety of information in LIAB. I interact the collection rate with dummy variables indicating firm characteristics, such as industry affiliation or profitability, and add the interaction term to the regression equation (4.5.1). Given that most of the firm characteristics are correlated with size, I restrict the sample to medium-sized firms with 50 to 500 employees.[13] Looking at broad industry classifications I find no significant differences in the effect between the manufacturing and the service sector (see specification (1) of Table 4.5.9). Yet, the point estimate for service sector firms is much more negative than for the manufacturing industry. Interestingly, the negative effect on employment seems to be decreasing in the profitability of the firm as shown by model (2). Specification (3) shows that the negative effect is stronger (though not statistically significantly so) in multi-establishment firms, which can shift employees to plants in other towns more easily. There is no significant difference between firms without collective bargaining agreements relative to firms with a sector or a firm level collective agreement in place (specification (4)).

Model (5) shows that the negative employment effect is significantly stronger if a work council exists. It is likely that work councils hamper firms from adjusting the workforce. In particular, laying off workers could be much more difficult. Hence, a potential explanation for the negative interaction effect in specification (5) could be that firms take tax increases as a (justified) excuse to adjust the workforce and additionally lay off even more workers to account for forgone past adjustments. In specification (6) I exploit a survey question in the LIAB on the importance of local taxes for the initial location decision of a firm. I split the firms into two groups, those which have regarded local taxation as unimportant for the location and those that have stated its relevance. Although the interaction term is not statistically significant, it is interesting to see that firms that regard local taxes as unimportant show a more negative employment effect. This finding can be interpreted along the lines of the reverse causality argument made above: if a local government knows that the municipal firms are sensitive to tax changes, policymakers might be less willing to increase taxes, which would bias estimates towards zero. Last, effects are not significantly different for Eastern and Western Germany (model (7)).

Finally, I also estimate the LBT effect for non-corporate firms, which are also liable

[13] Results for the full sample are available upon request.

Table 4.5.10: Corporate vs. non-corporate firms

Model	(1)	(2)
Legal form	corporate	non-corporate
log collection rate	-0.293**	0.033
	(0.128)	(0.094)
log full-time hours	-0.252**	-0.123
	(0.101)	(0.130)
log average wage	-0.088***	-0.067***
	(0.014)	(0.016)
local unemp. rate	-0.767***	0.459
	(0.259)	(0.405)
municipal population	0.061	-0.032
	(0.153)	(0.205)
log revenues	0.015	-0.008
	(0.015)	(0.023)
log expenses	-0.025*	0.000
	(0.014)	(0.022)
local property tax rate	-0.148*	-0.200**
	(0.078)	(0.101)
constant	6.104***	4.100*
	(1.750)	(2.130)
Adjusted R^2	0.024	0.016
Observations	28052	11012
Groups	8937	4340
Clusters	410	402

Note: Dependent variable: log employees. All specifications include year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

to the local business tax. Yet, the profits of non-corporate firms are taxed quite differently. In addition to the LBT non-corporate firms are, for instance, not subject to the corporate tax but to the personal income tax, where marginal tax rate depend on the taxable income. Moreover, there are certain LBT allowances for small non-corporate firms.[14] For these reasons, it is not possible to calculate the precise effective statutory marginal tax rate for non-corporate firms without making further assumptions. I, therefore, have excluded these firms from the baseline sample. As a test I estimate the baseline model on non-corporate firms, but find no effect of the local business tax on employment (see Table 4.5.10). This is probably due to the smaller size of non-corporate firms relative to corporate firms and to lower tax burdens for non-corporate firms in general.

[14] For a more detailed exposition of the taxation of non-corporate firms in Germany see Chapter 3.

4.5.3 Testing the model assumptions

In this subsection, I test the central underlying assumptions of the theoretical model presented in Section 4.3. In order to test intra-regional worker mobility, I estimate a model where the log average employment in region r and year t net of the employment in municipality m is my dependent variable, $\ln(L_{-m,r,t})$. Region r can either be the county or the labor market region, $r \in \{c, l\}$. The independent variable of interest is the log average employment in municipality m in region r at time t, $\ln(L_{m,r,t})$. I additionally control for variables at the regional level net of the contribution of municipality m, summarized in vector $\mathbf{R}'_{-m,r,t}$. More specifically, I condition (i) on the productivity of the region by including log average value added and log average investment; and (ii) on the fiscal situation of the region by controlling for the log average collection tax rate, the log average property tax rate as well as log average expenses and revenues. To deal with the endogeneity between employment in municipality m and employment in region r, I add region-year fixed effects to my model (denoted by $\boldsymbol{\mu_{r,t}}$), which account for any unobserved local shock affecting both the left-hand side variable and the regressor of interest. Note that these dummy variables also control for the regional unemployment rate and the population size so that the model identifies the within-region-mobility for a given population and for given unemployment. Adding municipality fixed effects μ_m, my model reads

$$\ln L_{-m,r,t} = \alpha \ln L_{m,r,t} + \boldsymbol{\beta}\mathbf{R}'_{-m,r,t} + \boldsymbol{\mu_{r,t}} + \mu_m + \varepsilon_{m,r,t}. \tag{4.5.2}$$

Results are presented in Table 4.5.11 and confirm the assumption of intra-regional labor mobility. Specification (1) shows that a decrease in municipality m's employment by 1% leads to a highly significant increase in the county's average regional employment (net of municipality m's contribution) of 0.07%. The respective elasticity for the regional labor market is 0.05%, as revealed by specification (4). The findings suggest, that workers are indeed quite mobile across municipalities within regions. Moreover, specifications (2) and (3) as well as (5) and (6) indicate that workers react rather quickly: the long-term mobility elasticity, which can be calculated by adding up the coefficients on lagged log municipal employees, e.g. -0.086 for model (3), is similar to elasticities shown in specification (1) and (4). This implies that most workers react to changed labor market conditions in their municipality within one year. While the estimates are

Table 4.5.11: Intra-regional mobility

Model	(1)	(2)	(3)	(4)	(5)	(6)
Region		county			labor market region	
log municipal employees$_t$	-0.070***	-0.081***	-0.091***	-0.046***	-0.052***	-0.058***
	(0.007)	(0.008)	(0.009)	(0.005)	(0.006)	(0.007)
log municipal employees$_{t-1}$		0.007**	0.005		0.001	0.000
		(0.003)	(0.004)		(0.002)	(0.003)
log municipal employees$_{t-2}$			-0.000			0.004*
			(0.003)			(0.002)
log regional investment	0.167***	0.157***	0.131***	0.194***	0.184***	0.166***
	(0.027)	(0.029)	(0.025)	(0.026)	(0.025)	(0.027)
log regional value added	0.194***	0.189***	0.155***	0.173***	0.176***	0.152***
	(0.033)	(0.035)	(0.030)	(0.029)	(0.031)	(0.029)
log regional property tax rate	0.616	0.810	0.826	0.913	1.179**	1.250
	(0.597)	(0.582)	(0.929)	(0.585)	(0.474)	(0.791)
log regional collection rate	-0.316	-0.243	-0.055	0.144	0.239	0.315
	(0.325)	(0.328)	(0.248)	(0.244)	(0.241)	(0.327)
log regional expenses	0.372	0.376	0.510**	0.568***	0.673***	0.397**
	(0.297)	(0.396)	(0.228)	(0.198)	(0.234)	(0.191)
log regional revenues	-0.068	-0.026	-0.253	-0.349	-0.477*	-0.208
	(0.325)	(0.435)	(0.186)	(0.232)	(0.273)	(0.204)
constant	-2.759	-3.921	-2.422	-3.359	-3.546	-3.456*
	(3.091)	(3.475)	(2.127)	(2.599)	(3.089)	(1.978)
Adjusted R^2	0.946	0.947	0.954	0.957	0.957	0.956
Observations	6278	5080	3900	7891	6350	4893
Groups	2293	1868	1512	2584	2097	1703
Clusters	200	200	190	204	204	196

Note: Dependent variables: log net regional employment (at county and labor market region level). All specifications include region-year fixed effects. Standard errors (in parentheses) clustered at respective regional level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

statistically significant, they also matter economically. The elasticity of specification (1) implies that of 100 workers leaving a municipality 15 are absorbed by the other municipalities in the county (the ratio for labor market regions as suggested by specification (4) is 100/11).

A corollary of the regional mobility is that wages are determined in the regional or even national labor market. This means that there should be only small general equilibrium wage effects of corporate taxes in municipality m, which has been shown theoretically and empirically in a related study (see Fuest et al., 2013 and Chapter 3).[15] As stated above, my baseline estimates are conditional on wages. In order to take a closer look at the wage effect, I re-estimate my baseline model using the log total wage bill as my dependent variable. Specification (1) of Table 4.5.12 verifies that – conditional on the average wage – this specification is equivalent to using log employees as the left-hand side variable. In model (2), I estimate the "unconditional" effect of profit taxes on the wage bill allowing wages to respond as well. As expected, estimates hardly change. In the last specification I look at the effect of the wage bill conditional on employment. I thus estimate the effect of profit taxes on the average wage. I find only a small and

[15] Note that there is, of course, a general equilibrium effect on wages in the municipalities absorbing mobile labor (Bradford, 1978).

Table 4.5.12: Wage effects

Model	(1)	(2)	(3)
log collection rate	-0.293**	-0.295*	-0.028
	(0.128)	(0.158)	(0.074)
log full-time hours	-0.252**	-0.230	0.002
	(0.101)	(0.140)	(0.092)
log average wage	0.912***		
	(0.014)		
log employees			0.914***
			(0.015)
local unemp. rate	-0.767***	-0.969***	-0.285*
	(0.259)	(0.288)	(0.156)
municipal population	0.061	-0.004	-0.065
	(0.153)	(0.169)	(0.073)
log revenues	0.015	0.013	-0.001
	(0.015)	(0.018)	(0.013)
log expenses	-0.025*	-0.017	0.007
	(0.014)	(0.018)	(0.013)
local property tax rate	-0.148*	-0.163	-0.030
	(0.078)	(0.101)	(0.054)
constant	6.104***	13.679***	8.768***
	(1.750)	(1.905)	(0.825)
Adjusted R^2	0.456	0.006	0.464
Observations	28052	28052	28052
Groups	8937	8937	8937
Clusters	410	410	410
Employment elasticity	-1.19	-1.20	-0.11
Euro incidence	-2.64	-2.66	-0.25

Note: Dependent variable: log total wage bill. All specifications include firm, municipal and year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

insignificant wage effect of corporate taxes of -0.02 (implying a wage elasticity of -0.1). This suggests that wages are indeed not very responsive to corporate tax changes, which should be the case if labor is mobile. Reassuringly, the wage elasticity of -0.1, though not being significant, is very close to the small indirect wage effect found by Fuest et al. (2013) (see Chapter 3).

The theoretical model demonstrates that the negative employment effect goes through reduced investment and eventually leads to a reduction in output. I test this channel by replacing the left-hand side variable of the regression equation (4.5.1), i.e. log employment, by log (net) investment and log value added. While I find no significant tax effect on log total investment (specification (1)), model (2) of Table 4.5.13 suggests that higher corporate taxes reduce net investment – yet, the p-value of 0.12 is just above the conventional significance threshold of 0.1. Nevertheless, I do find a negative and significant

Table 4.5.13: Investment and output effects

Model	(1)	(2)	(3)
Effect on	log total investments	log net investments	log value added
log collection rate	-0.369	-0.852	-0.423*
	(0.416)	(0.573)	(0.243)
log full-time hours	0.006	0.965	-0.116
	(0.381)	(0.705)	(0.189)
log average wage	0.073*	0.142**	0.048**
	(0.040)	(0.065)	(0.022)
local unemp. rate	-1.702*	-2.170	-1.700***
	(0.984)	(1.582)	(0.607)
municipal population	-0.182	0.381	-0.439
	(0.465)	(0.664)	(0.269)
log revenues	0.103	-0.079	0.017
	(0.068)	(0.102)	(0.036)
log expenses	-0.001	0.214**	-0.015
	(0.062)	(0.096)	(0.031)
local property tax rate	-0.157	0.198	-0.116
	(0.232)	(0.358)	(0.141)
constant	12.830**	-7.898	20.929***
	(5.203)	(7.825)	(3.142)
Adjusted R^2	0.053	0.008	0.156
Observations	28052	27048	28052
Groups	8937	8683	8937
Clusters	410	410	410

Note: Dependent variables: log (net) investment and log value added. All specifications include firm, municipal and year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

effect of the collection rate on value added in specification (3), which is in line with the theoretical model.

4.6 Conclusions

This paper analyzes how profit taxes affect employment – exploring a so far neglected effect of corporate taxation. The standard partial equilibrium model of labor demand shows a negative employment effect of corporate taxes, going through decreased investments under the assumptions that wages are not affected by municipal corporate tax changes and that labor is mobile across jurisdictions. I empirically test the negative employment effect by exploiting the institutional setting of the local business tax, whose rates are set autarkically by the 11,441 German municipalities each year.

Relying on rich administrative linked employer-employee panel data, I show that employment indeed declines if local business tax rates increase. An increase of the effective statutory tax rate by 1% leads to a drop in employment of 1.2%. Expressed in euro terms this means that a one euro increase in the company's tax bill yields a decrease of the wage bill of 85 cents. Moreover, I provide evidence that the central assumption for a negative employment effect – i.e. labor mobility and thus wage stickiness – is fulfilled in the institutional setting of the German local business tax.

My findings fill an important research gap. There has been hardly any evidence on the effects of corporate taxation on employment. The reason for this lack is that most corporate taxes are set at the national level. In an international context labor is arguably much less mobile. As a consequence one should not expect large employment reactions to corporate tax changes but rather a decrease in wages (see Felix and Hines Jr., 2009; Arulampalam et al., 2012; Fuest et al., 2013; Liu and Altshuler, 2013 and also Chapter 3). Hence, it is the special institutional setting of the German local business that gives rise to the negative employment effect. Of course, this limits the generalizability of my findings. Nevertheless, the results are still informative for policymakers since they underscore how crucial the mobility of production factors is when debating the labor market effects of corporate taxes. If a production factor is mobile and the jurisdiction is small (relative to the whole economy under study), local factor prices should not be affected but factor inputs will respond to the tax. This general lesson is true for both the national and the international context.

Yet, it should be noted that the negligible effect on local wages does not tell much about the overall (country-wide) incidence of the corporate tax. Labor (and capital) flee the high-tax jurisdiction, but are absorbed by the other (lower-tax) municipalities. This should yield small but in sum significant decreases in marginal products and thus in the prices for capital and labor. Bradford (1978)'s findings imply that these small price reductions induce welfare losses that even exceed tax revenues and the dead weight burden of the tax. Moreover, the effects of tax competition, which are quite complex in the setting of the German local business tax (Janeba and Osterloh, 2012), should be taken into account, when evaluating the total effect of the German local business tax.

4.7 Appendix

Table 4.7.1: Robustness: level vs. log model

Model	(1)	(2)
collection rate	-0.087**	
	(0.038)	
log collection rate		-0.293**
		(0.128)
log full-time hours	-0.252**	-0.252**
	(0.101)	(0.101)
log average wage	-0.088***	-0.088***
	(0.014)	(0.014)
local unemp. rate	-0.772***	-0.767***
	(0.259)	(0.259)
municipal population	0.060	0.061
	(0.154)	(0.153)
log revenues	0.015	0.015
	(0.015)	(0.015)
log expenses	-0.026*	-0.025*
	(0.014)	(0.014)
local property tax rate	-0.143*	-0.148*
	(0.080)	(0.078)
constant	6.044***	6.104***
	(1.753)	(1.750)
Adjusted R^2	0.024	0.024
Observations	28052	28052
Groups	8937	8937
Clusters	410	410
Employment elasticity	-1.36	-1.19
Euro incidence	-0.96	-0.85

Note: Dependent variable: log employees. All specifications include firm, municipal and year fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Table 4.7.2: Robustness: role of control variables

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log collection rate	-0.293**	-0.488***	-0.291**	-0.352***	-0.324**	-0.476***	-0.326***	-0.350***
	(0.128)	(0.112)	(0.125)	(0.119)	(0.127)	(0.112)	(0.124)	(0.117)
firm controls	Yes	No	No	Yes	Yes	Yes	No	No
municipal controls	Yes	No	Yes	No	Yes	No	Yes	No
year FE	Yes	No	Yes	Yes	No	No	No	Yes
Adjusted R^2	0.024	0.004	0.016	0.022	0.022	0.012	0.013	0.014
Observations	28052	28052	28052	28052	28052	28052	28052	28052
Groups	8937	8937	8937	8937	8937	8937	8937	8937
Clusters	410	410	410	410	410	410	410	410
Employment elasticity	-1.19	-1.98	-1.18	-1.43	-1.32	-1.93	-1.33	-1.42
Euro incidence	-0.85	-1.41	-0.84	-1.02	-0.94	-1.38	-0.94	-1.01

Note: Dependent variable: log employees. All specifications include firm and municipal fixed effects. Standard errors (in parentheses) clustered at county level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Chapter 5

Well-being effects of income taxation*

5.1 Introduction

Taxation is the main economic instrument in the hand of governments influencing individual budget constraints and therefore well-being. Given that the effect of income on subjective well-being (SWB) is presently one of the most important questions (see Clark et al., 2008, for a survey) in the SWB literature, it is surprising that there is no direct evidence for the effect of taxes on SWB. Accepting that income increases SWB, at least in cross-sectional analyses, implies that taxation should reduce it. Clearly, this effect is implicitly accounted for in the existing literature, as income *net of taxes* is systematically used in SWB regressions. However, so far, the direct effect of taxation on well-being has not yet received attention (an exception is Lubian and Zarri, 2011, who look at the specific relationship between tax morale and SWB). Analyzing the relationship between taxation and SWB – in comparison to net income – not only contributes to the literature on the role of income for SWB but especially provides a new perspective on a core question in the traditional literature in public and welfare economics: how do taxes affect individual well-being? This is important for both the political economy of tax policy (support for tax reforms) and the sustainability and efficiency of public

*This chapter circulates as “Happy Taxpayers? Income Taxation and Well-Being” (joint with Alpaslan Akay, Olivier Bargain, Mathias Dolls, Dirk Neumann and Andreas Peichl, see Akay et al., 2012).

finance (for instance through the level of tax compliance).

In this study, we use SWB data to proxy individual (experienced) utility (e.g., Kahneman and Sudgen, 2005; Layard, Mayraz, and Nickell, 2008) and regress it on taxes, net income and many socio-demographic characteristics, which are known determinants of SWB (Clark, 2002). Our empirical application relies on the German Socio-Economic Panel (SOEP) study, which has been used in important contributions to SWB research (e.g., Frijters, Haisken-DeNew, and Shields, 2004b). Identification of the specific tax effect, i.e. in isolation from the income effect, is based on tax reforms occurring over the 26 years of the panel.

We find a significant and positive effect of tax payments on well-being, conditional on net income (i.e. holding individual living standards constant). This finding is robust to different approaches including the way we introduce individual heterogeneity in the model, the flexibility of the SWB equation with respect to income and tax levels, as well as the estimator and sample used. In addition, we show that the effect conditional on *net* income is not driven by status or relative concerns (higher tax implying higher *gross* income in this setting). The positive conditional tax effect may be explained through different channels: higher taxation might imply better provision (or quality) of public goods (Luechinger, 2009; Luechinger and Raschky, 2009; Levinson, 2012) or more redistribution and insurance through the social security system (Alesina, Di Tella, and MacCulloch, 2004). In addition, positive well-being effects may arise from motives underlying tax morale (see Lubian and Zarri, 2011) or some 'citizenship' feeling of belonging (or contributing) to the society in the spirit of the procedural utility concept of (Frey and Stutzer, 2001). In order to provide evidence for these different channels which could be all consistent with some warm glow motive of paying taxes, we interact the conditional tax effect with a large number of characteristics. Among other things, we show that this effect is significantly larger for the low income group; for Eastern Germans, who have been brought up in a system where the government played a bigger role; for individuals who live in regions with local underprovision of public goods; and for individuals with a higher tax morale.

The rest of the paper is set up as follows. Section 5.2 reviews the existing SWB literature with respect to government activity and taxation. Section 5.3 describes our empirical approach. We present our results in Section 5.4 together with extensive sensitivity checks. In Section 5.5, we discuss the potential channels that might explain the positive conditional tax effect. Section 5.6 concludes.

5.2 Related literature

Our study is related to the literature on the link between public policy and well-being (Layard, 1980, 2006; Frey and Stutzer, 2012). In particular, the study by Layard (2006) takes stylized facts recovered by SWB research, such as adaptation and social comparison, and discusses their implications for optimal taxation. To the best of our knowledge, there are only two studies that empirically touch upon the (implicit) effects of income taxation on measures of SWB. Firstly, Oishi, Schimmack, and Diener (2012) use the Global Gallup Poll to show that the progressivity of the tax system increases a nation's SWB. Secondly, Lubian and Zarri (2011) find that self-reported tax morale (the moral obligation to pay taxes) has a positive effect on SWB using a 2004 cross-section of Italian household data. While Lubian and Zarri (2011) have direct information on tax morale and can also investigate different dimensions of it (which we cannot because of data limitations), we provide a different identification strategy (based on tax reforms over time) in addition to a broader perspective allowing for more channels through which taxation can influence SWB (public goods, redistributive preferences and tax morale).[1]

As (parts of) the tax revenues are used to finance public goods and the effect of paying taxes on SWB should capture this channel, our research is also related to the literature on the valuation and quality of public goods and their association with individuals' well-being. This link has been analyzed in a recent series of papers (Frey, Luechinger, and Stutzer, 2009; Luechinger, 2009; Luechinger and Raschky, 2009; Levinson, 2012). The main finding is that the underprovision of public goods (and as a consequence the prevalence of terrorism, pollution or flood disasters) has a negative effect on SWB. Another channel through which income taxation might affect well-being is redistribution. In fact, Oishi et al. (2012) interpret their results by stating that a fair redistribution of wealth increases a nation's well-being.[2] Similarly, Di Tella, MacCulloch, and Os-

[1] Besides income taxation, Kassenboehmer and Haisken-DeNew (2009) analyze the effect of social benefits on SWB, while Gruber and Mullainathan (2005) show that excise taxes on cigarettes increase the well-being of individuals with a higher probability to smoke.

[2] On the other hand, and somewhat puzzling, they state that the positive effect of progressivity comes through the citizens' satisfaction with public goods such as education and public transportation, while

wald (2003) show that higher unemployment benefits are associated with higher national well-being. In addition, Alesina et al. (2004) find that inequality has a negative effect on SWB, especially in Europe. Interestingly, Harbaugh, Mayr, and Burghart (2007) show that mandatory tax-like transfers activate parts of the brain that are linked to rewards processing. They interpret their finding in line with the "pure altruism" hypothesis stating that even mandatory transfers to finance public good (such as taxes) increase individuals' well-being. The authors argue that the reason for this positive effect lies in the fact that the mandatory transfers are used to ensure the provision of the public good and that its availability is eventually more important to individual well-being than the way it is financed.

As implied by the study of Lubian and Zarri (2011), the relationship between tax and SWB could also be influenced by the subjective rewards of acting according to (the spirit of) the law. In other words, cheating, that is tax evasion (avoidance), generates lower levels of well-being than fiscal honesty. Finally, the literature on group identity is related to our research since the act of paying taxes can be interpreted as paying the membership fee to become part of the society. In fact, there is some evidence that more intensive participation in a democracy through political institutions is associated with a higher SWB (Frey and Stutzer, 2001).

5.3 Empirical approach and identification strategy

5.3.1 Model and estimation

In order to empirically test the question of how taxes affect SWB, we regress SWB_{it} on (log) tax payments T_{it} conditional on (log) net income N_{it}. In addition we add a set of standard socio-demographic and economic characteristics of individuals $\mathbf{X}_{it}$ as well as

they also show that government size and SWB are negatively associated. There are several other studies on the size of the government and SWB which bring forward mixed results, ranging from a zero effect (Veenhoven, 2000) over a negative effect (Bjørnskov, Dreher, and Fischer, 2007) to an inverted U-shape relationship between government spending and SWB (Hessami, 2010).

person and time fixed effects μ_i, μ_t.[3] The empirical model reads as follows:

$$SWB_{it} = \alpha N_{it} + \beta T_{it} + \boldsymbol{\gamma} \mathbf{X}_{it} + \mu_i + \mu_t + \epsilon_{it}. \qquad (5.3.1)$$

As in Layard et al. (2008), we assume that the above specification is a proxy for the utility function of an individual. As the true functional form is unknown, we suggest alternative specifications in the sensitivity checks below that increase the flexibility of the relationship between well-being and tax/income, including polynomial forms of high degrees.

As common in the SWB literature, we assume that the net resources of a person matter for individual well-being, whether this person is aware of it or not. That is, we assume that individuals with a high living standard experience higher SWB levels. Hence, we expect the sign of α to be positive. Yet, we argue that previous models might have been under-specified as they ignore the specific role of taxation on well-being beyond the mere reduction of net income. In other words, the sign of β is unknown and the main object of our investigation.

In our baseline specification, we assume ϵ_{it} to be usual i.i.d. error terms and estimate the model linearly, taking SWB measured on a 11 point scale as a continuous variable. This gives us more flexibility to control for unobserved individual effects (fixed effects, quasi-fixed effects). In robustness checks, we also estimate ordinal (fixed effects) models, i.e. taking SWB_{it} as the latent utility. As in Ferrer-i Carbonell and Frijters (2004), we confirm that the two estimation methods lead to very similar results (see Section 5.4.2).

5.3.2 Identification

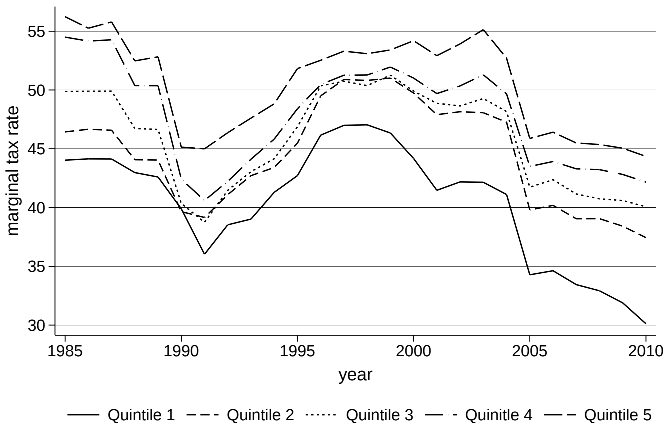
Tax $T_{it}(Y_{it}, Z_{it})$ is a function of market income Y_{it} and a subset Z_{it} of individual and household characteristics. Net income is calculated as $N_{it} = Y_{it} - T_{it}(Y_{it}, Z_{it})$. This means that tax payment and net income depend on the same gross income variable, implying a deterministic relationship. The tax function $T_{it}(Y_{it}, Z_{it})$ is highly non-linear in Germany. Hence, households with different characteristics Z_{it} (for instance having two versus three children, being married rather than cohabiting) will face a different tax

[3] $\mathbf{X}_{it}$ includes age, age-squared, skill, nationality, gender, marital status, household composition, health status, labor market status, working hours, region fixed effects (16 states (*Länder*)).

schedule.[4] This provides the possibility for cross-sectional (parametric) identification given the non-linearity of $T_{it}(Y_{it}, Z_{it})$.

However, this variation might not be enough for identification given the fact that characteristics Z_{it} also directly affect well-being, and given potential behavioral responses to taxation. Therefore, we rely on tax reforms, i.e. changes in the tax base and schedule (brackets, rates, deductions, etc.) over time, as an exogenous source of variation which is necessary to identify the tax effect. Figure 5.3.1 presents the development of effective marginal tax rates (EMTR) over time in Germany by income quintiles. It illustrates that there were indeed substantial changes in tax parameters all over the period. Moreover, tax reforms have not been uniform but have affected different income and demographic groups differently. This exogenous tax variation enables us to identify the conditional tax effect on SWB.

Figure 5.3.1: Effective marginal tax rates by quintile over time



Two important remarks have to be made at this stage. Firstly, our identification strat-

[4] German tax legislation is household-specific: Married couples file their taxes jointly and face tax reductions due to the income splitting system. The presence of children also changes tax liabilities due to allowances and credits. More variation is generated through individual characteristics like religion, occupation type, age or disability. For instance, individuals of Christian denomination pay church taxes, which accrue to between 8% and 9% of the income tax (depending on the region) and which are collected with the general income tax. Civil servants and self-employed are partially exempt from paying payroll taxes (which themselves are deductible from the income tax base), and there is regional variation in payroll tax rates. Certain professions face different levels of tax free earnings. Moreover, Germany does not employ a piece-wise linear tax schedule with flat rates for different brackets, as in most countries, but a unique formula with continuously increasing marginal tax rates. So even slight variations in gross income will yield different tax rates.

egy is related to the one applied in studies on the elasticity of taxable income (ETI, see Saez, Slemrod, and Giertz, 2012, for a recent overview). However, in this literature, changes in taxable income are the left hand side variable, therefore, only exogenous changes in the tax function (on the right hand side) are required for identification. In our case, we aim to identify two coefficients on the right hand side (income and tax), so that simultaneous variation in both gross incomes and the tax function is needed. Secondly, and as usual, our results could be affected by endogeneity issues such as reverse causality (happier individuals pay higher taxes). Our model specification mitigates endogeneity concerns since tax is a function of income and SWB can affect income (and hence tax) only through behavior (i.e. happier individuals may work harder, be more creative and enterprising and hence generate more income). However, recent research suggests that the causality runs from money to SWB implying endogeneity issues are limited (see, e.g., Luttmer, 2005, or Gardner and Oswald, 2007, as well as the evidence and references collected in Pischke, 2011). Nonetheless, we check if reverse causality goes through behavioral changes (income) by employing the same instrument (industry affiliation) as Pischke (2011) and by instrumenting taxes with the hypothetical tax payments in period t given the gross income in $t-1$ – again borrowing from the ETI literature (Saez et al., 2012). Results, presented in Section 5.4.1, are very similar to our baseline findings.

5.3.3 Data and selection

The German Socio-Economic Panel (SOEP) is a well-known survey of individuals in households living in Germany, which has been widely used for studying SWB (see, e.g., Frijters, Haisken-DeNew, and Shields, 2004a; Frijters et al., 2004b; Ferrer-i-Carbonell, 2005; Luechinger, Meier, and Stutzer, 2010). It is a representative survey of the entire German population with about 25,000 individuals living in more than 10,000 households per cross-section – East Germany was added in 1990 (Wagner et al., 2007). We select all waves, constructing a panel of about 270,000 individual-year observations for the years 1985 to 2010. The 26 waves of unbalanced panel data fulfill the above requirement of time variation in individual gross income and tax policies necessary for identification.

In each wave, the question "*How satisfied are you with your life, all things con-*

sidered?" is asked. The answer to this question is recoded on an 11-point scale, with 0 meaning totally unhappy and 10 meaning totally happy. The main explanatory variables are income and labor taxes which are taken from the data as well. Our measure of income N_{it} is net (after-tax) labor income of the month preceding the interview. The tax variable T_{it} comprises *both* income *and* payroll taxes (employee's social security contributions).

In the German context, the institutional setting that influences the perception of tax and income is as follows. Employees receive a monthly pay slip which informs them about their gross income as well as the income and payroll taxes (which are automatically withheld by the employer) to arrive at the net income which is directly transferred to their bank accounts.[5] Unlike the US, there are basically no additional deductions (such as retirement plans, insurances, garnishments, or charitable contributions) directly taken out of the gross income (there are some firm level pensions which receive a preferable tax treatment). Those payments are rather directly paid out of the net income in Germany.

Our baseline taxpayer sample is constructed as follows: We keep all individuals in households with strictly positive tax payments and the household head in working age (i.e. aged 16 to 65). The minimum tax payment usually corresponds to payroll taxes (social security contributions), which are phased-in as soon as a certain threshold (varying from 153 to 400 euros per individual per month over the observation period) is passed (*Mini-Job*). For a single household income taxes have to be paid when monthly taxable income exceeds 667 (180) euros in 2010 (1985). Our selection implies that non-working spouses in a taxpayer household (due to unemployment, voluntary non-employment or old-age) are also included in the sample.[6] We treat household incomes (tax payments) as a common good (bad) in the household, that is, we attribute the full household incomes and tax payments to both spouses. We implicitly equivalize household income by controlling for log household size and number of children in all regressions. The baseline sample covers almost 190,000 individual-year observations. Descriptive statistics of the

[5] Taxes on capital gains are also withheld – in that case by financial institutions. Unfortunately, we neither have information on capital income nor on capital gains taxes in the month preceding the interview. In most cases individuals are informed at the end of the year about the capital income taxes that have been withheld. This makes capital income taxes less salient at the beginning and in the middle of the year, which is precisely the time when the SOEP survey is conducted.

[6] Note that this selection does not affect the estimates. We obtain very similar results when excluding non-working spouses in taxpayer households from the sample.

dependent variable and the most important covariates are shown in Table 5.7.1 in the appendix (Section 5.7).

5.4 Empirical results

5.4.1 Baseline

Our main objective is to test the (conditional) effect of tax on SWB. Table 5.4.1 presents the main set of results applying the FE estimator and focusing only on the main regressors of equation (1), i.e. reporting the coefficients on net income and tax as well as marginal effects.[7] Without surprise, the first column confirms that the effect of net income on SWB is positive. Most importantly, the second row shows that the coefficient on tax payments is significant and positive. This implies that – conditional on net income and all other individual/household characteristics – individuals have higher SWB when paying taxes.[8]

Given that we use a log specification, we also report marginal effects in Table 5.4.1. The marginal effect of tax payments may seem small (0.00004) in absolute terms. Compared to the marginal effect of net income, it is however sizeable as indicated by the marginal rate of substitution (MRS) of 0.33.[9] Next we use alternative specifications to estimate the conditional tax effect.

A first issue may be related to the timing of tax payment compared to the date of

[7] The complete set of baseline results including all covariates is shown in Table 5.7.2 in the appendix (Section 5.7). In this and all of the following regressions, covariates show well-known patterns (Clark et al., 2008): SWB decreases with age and increases with the skill level; women are on average happier, while having children decreases SWB.

[8] When ignoring tax payments, we find a coefficient of net income of 0.345, which is in line with previous estimates based on SOEP data (Frijters et al., 2004a; Ferrer-i Carbonell and Frijters, 2004; Akay and Martinsson, 2009). It is slightly lower in our baseline results, 0.301, when adding tax payments. A likelihood ratio test shows that adding taxes to the model significantly increases the fit of the model with a χ^2 of 17.72 and a corresponding p-value of 0.0001.

[9] As explained before, the most natural specification includes net income and tax payments. In this case, variations in both gross income and tax functions allow identifying the two effects. Starting from a utility function of net income and tax, $U(N,T)$, our results imply: $\frac{dU}{dT}|_{dN=0} = 0.00004$. Alternatively, a model specified with gross income and tax should lead to the same results. Indeed we can write $U(N,T) = U(Y-T,T) = f(Y,T) = f(Y-T+T,T)$ so that $\frac{df}{dT}|_{d(Y-T)=0} = \frac{\partial f}{\partial Y} + \frac{\partial f}{\partial T}$. Empirically, we find with this alternative specification that $\frac{df}{dT}|_{d(Y-T)=0} = 0.00011 - 0.00005 = 0.00006$, which is statistically not significantly different from $\frac{dU}{dT}|_{dN=0} = 0.00004$.

Table 5.4.1: Well-being effects: baseline results

Model Specification	(1) Baseline	(2) Lagged tax	(3) Instrumented	(4) Income tax only
log net income	0.301***	0.320***	0.294***	0.327***
	(0.017)	(0.017)	(0.017)	(0.015)
log taxes	0.045***		0.024***	0.014***
	(0.009)		(0.004)	(0.003)
log taxes$_{t-1}$		0.009**		
		(0.004)		
adj. R^2	0.127	0.149	0.103	0.127
obs.	188412	150883	150316	188412
marg. eff. net inc.	0.00013	0.00014	0.00013	0.00014
marg. eff. taxes	0.00004	0.00001	0.00002	0.00003
MRS tax/net inc.	0.33	0.06	0.16	0.19

Note: Dependent variable: subjective well-being. Standard errors (in parentheses) clustered at person level. All regressions include standard controls variables (see Table 5.7.2 in the appendix (Section 5.7) for a complete set of coefficients) as well as person, state and year fixed effects. All money variables are in 2010 euros. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***). MRS stands for marginal rate of substitution between taxes and income.

interview (and hence measure of SWB). If individuals become aware of their tax liabilities only at the end of the year but are interviewed early in the year (SOEP interviews occurring between January and September), then the tax payments of the previous year may be the relevant information for our purpose. We, thus, use lagged instead of current taxes in our model. The second column of Table 5.4.1 shows that the tax effect remains positive and highly significant, but decreases relatively to using contemporary tax payments.

A second check concerns potential endogeneity of taxes. A first issue discussed in the SWB literature is that happier people might earn more so that there is potentially reverse causality between gross income and subjective well-being (Luttmer, 2005; Pischke, 2011). Although the empirical findings suggest that the causality runs from gross income to SWB, we follow Pischke (2011) and instrument gross income using industry wage differentials which can at least be party attributed to rents and not productivity. Secondly, our tax coefficient could be biased if individuals respond to changes in the tax code. Assume for instance that a tax cut is perceived as a future decrease in welfare payments or public goods. In that case some individuals may compensate by increasing labor supply (to save more) so that total tax liability does not vary much. We therefore borrow from the ETI literature (Saez et al., 2012) and use a tax-benefit calculator to construct a synthetic tax measure by applying the inflation-adjusted gross income of period $t-1$ to the tax schedule of the year t and simulate the tax payments a household would face in the absence of behavioral responses. The third column of Table 5.4.1 shows that neither the effect of income nor the effect taxes is hugely affected by instrumenting both variables (the same is true when instrumenting only one of the two variables and estimating the model with 2SLS). The MRS decreases slightly from 0.33 to 0.16.[10]

In specification (4), we finally look at the effects of income taxation only, i.e. we exclude payroll taxes from the tax variable. While income taxes are mostly used for redistribution and to finance classic public goods such as roads or defense, payroll taxes serve basically as insurance contributions in case of illness, unemployment and retirement. Hence, individuals could prefer paying one but not the other tax for various reasons. In addition, the fact that payroll taxes are proportional to income, do not vary across demographic characteristics and show less (real) variation over time makes identification of a payroll tax effect difficult. When focusing on income taxes only, we find a positive marginal effect similar to the baseline estimate.

5.4.2 Sensitivity checks

We conduct several additional sensitivity checks to make sure that our results are robust to assumptions and choices made.

Functional form. In the baseline model we include net income and taxes in logs, a standard non-linear specification. Since logs may not capture the actual relationship between SWB and net income/tax, we experiment with different specifications in levels or logs including quadratic and higher order polynomials (up to order 8) as well as income splines. As shown in Table 5.7.3 in the appendix (Section 5.7), the main result remains unchanged, with a significant, positive and fairly constant coefficient on tax; the MRS between net income and taxes is also very similar across specifications. This is true when both net income and tax enter with the same specification (e.g. quadratic income

[10] The first stage F-statistics are well above 10 in each estimation.

and quadratic tax) or in an asymmetrical way (e.g. quadratic net income and linear tax). The interaction term between net income and tax is significant and negative, indicating that the positive tax effect is smaller for richer individuals; we explore this point in more detail below. This result is reassuring and rules out concerns that taxes, being a non-linear function of income, would simply capture the non-linearity of the relationship between income and SWB. Results with Box Cox and Cobb Douglas specifications (not reported) also lead to this conclusion.

Estimator. Next, we check the robustness with respect to the estimator. Table 5.7.4 presents two linear models: the FE results (our baseline) and, following van Praag, Frijters, and Ferrer-i Carbonell (2003), a Mundlak-type (Quasi)-Fixed-Effects estimator (QFE). In the latter, we explicitly model the correlation between the time-invariant unobservables and all time-varying observables by including the within-person mean of those observables in the regression. Next, in column (3), we employ an Ordered Logit specification due to the ordinal scale of the SWB measure (results with Ordered Probit are very similar and not reported). Finally, we set up the "Blow-up and Cluster" Fixed Effects Ordered Logit Estimator suggested by Baetschmann, Staub, and Winkelmann (2011) to additionally account for individual fixed effects. Once accounting for individual fixed effects, using linear or ordered logit models does not make much difference, as indicated by Ferrer-i Carbonell and Frijters (2004). Our results are generally confirmed and the tax effect is significant with very similar MRS between income and tax of around 0.3. The exception is column (3) where we do not control for individual fixed effects. This indicates that the cross-sectional variation alone is not sufficient to identify the tax effect but changes in gross income and tax reforms over time are necessary.

Sample. In our baseline specification, we do not use population weights provided by the SOEP. As Table 5.7.5 in the appendix (Section 5.7) shows (column (1)), this choice does not affect the results. Moreover, we do not find big differences when estimating the model separately for singles and individuals in couples (regressions (2) and (3) in Table 5.7.5). Next, we extend the analysis to all individuals in the population, including non-workers and welfare recipients, and re-estimate our baseline model. Instead of net income, we use disposable income (i.e. net income plus government transfers) as some households do not have any taxable labor income. As Table 5.7.5 suggests, the coefficient on log taxes hardly changes (specification (4)). Results are neither affected when using a different, composite measure of taxes paid minus benefits received, which we call net taxation. The sign of net taxation decreases slightly, but remains positive and significant (specification (5)). Last, we check whether results are driven by the German reunification (not reported). Results do not change when restricting the sample to the post reunification period. Moreover, we find very similar results when looking at Western Germans only – both after 1990 or when focusing on the years around the reunification.

Status. As the SWB literature has extensively stressed the importance of relative concerns (e.g., Luttmer, 2005, among others), one potential explanation for the positive coefficient on tax is that higher taxes reflect higher gross income (when conditioning on net income). To check for possible status effects, we firstly control for relative income and relative taxes, defining the reference group according to region, gender, age and occupation. Our main result remains unaffected by the inclusion of relative income (relative income and taxes), i.e. the coefficient on tax becomes 0.045 (0.042) and is still significant at the 1%-level. Results do not change either when using a broader definition of the reference group or the median income instead of the mean. Secondly, we replicate our estimation using several measures of occupational prestige (we use the Standard Index of Occupational Prestige Scale (SIOPS) by Treiman, the International Socio-Economic Index of Occupational Status by Ganzeboom and the classification by Erikson-Goldthorpe-Portocarero). While we find that occupational prestige has a positive effect on SWB, it does not affect the coefficients on income and taxes. This makes it unlikely that status is driving our results – in particular, since controlling for the Ganzeboom index, which explicitly defines income as one source of prestige, does not affect the results. Moreover, our baseline coefficients do not change when including state-year and state-year-quintile fixed effects, which make other potential omitted variable biases unlikely as one would expect an omitted variable to be correlated with these fixed effects.

5.5 Discussion of results

Our empirical analysis shows that, conditional on net income, taxation has a positive, significant and robust effect on SWB. This result is in line with evidence from neuroscience: Harbaugh et al. (2007) show that mandatory transfers to charity, similar to taxes, activate those parts of the brain that are linked to rewards processing. This could give rise to a warm glow motive associated with paying taxes which could increase happiness (Owen and Videras, 2006).

But how can this positive tax effect be explained? In this section, we test three hypotheses which can theoretically explain the positive coefficient of taxes conditional on net income. Firstly, it might be explained by the fact that taxes are used to finance public goods. Hence, individuals who are consuming public goods more often or those living in regions with a relative underprovision of public goods might be happier to pay taxes. Secondly, the positive coefficient on taxes could be explained by redistributive preferences. There are several ways to test this hypothesis. Following Corneo and Grüner (2002), there are two relevant types of redistributive preferences in our setting. First, they could be driven by a high solidarity and/or a strong belief in the role of the state. Second, redistributive preferences could also be shaped by more self-centered behavior, such as risk aversion and the preference for a tight social safety net in case of a shock such as unemployment (a 'veil of ignorance' motive). Finally, the positive coefficient on taxes could also be due to the righteousness to pay taxes of some individuals in the population. Individuals with a high tax morale might feel morally obliged to pay taxes because it is the law. In that case, the positive coefficient on taxes would be explained by the negative utility of doing something unlawful. We test whether such kind of high tax morale could drive our results.

For each hypothesis we use a variety of (individual or household) characteristics which we interact with the tax and net income variables in order to obtain heterogeneity in the tax and income effects.[11] It is important to note that the three hypotheses are complementary rather than rivaling. For this reason each of the characteristics is allocated to at least one of the three hypotheses. Table 5.5.1 summarizes the predicted signs of

[11] For instance, let the dummy variable E be equal to unity if an individual is from Eastern Germany and 0 otherwise. Instead of using an omitted category, we can rewrite the standard model with interaction terms $SWB = \alpha_0 + \beta_Y Y + \beta_{YE} Y \cdot E$ as $SWB = \alpha_0 + \gamma_{YE} Y \cdot E + \gamma_{YW} Y \cdot (E - 1)$. The two models are equivalent if $\gamma_{YE} = \beta_Y + \beta_{YE}$ and $\gamma_{YW} = \beta_Y$.

Table 5.5.1: Hypotheses for the positive tax effect

Hypothesis	H1 Public goods	H2 Redistributive preferences	H3 Tax morale	Empirical findings Low inc.	Empirical findings high inc.
Relatively poor	+	+	+	++	
PG underprovision	+			++	+
Culturally active	+			o	++
Children in school	+			–	++
Small community	+	+		++	o
Return migrants	–	–		o	o
Born in the East	+	+		++	++
Leftist		+		+	++
Helpfulness		+		++	o
Risk averse		+		++	–
Frequent volunteer		+		o	o
High trust in others		+		–	o
Higher tax morale			+	+	+
Religiosity		+	+	++	–
Women			+	+	o
High-skilled			?	o	+
Self-employed			–	o	o

Note: + indicates a positive, – a negative and o no relationship. Double symbols indicate statistically significant differences at the 5%-level, single symbols show suggestive patterns that are not statistically significant at this level.

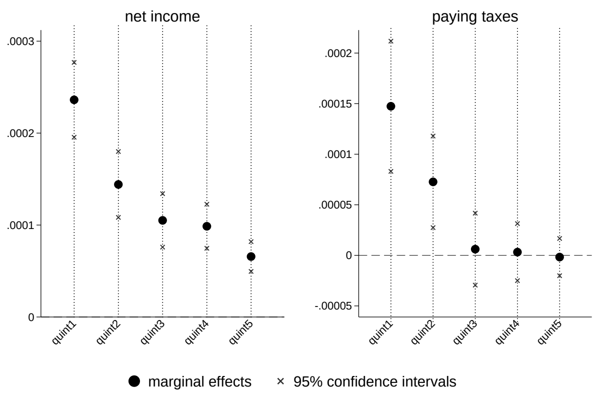
the coefficients for the interaction of each variable with the tax variable together with the empirical findings which will be discussed below (detailed regression results are reported in Table 5.7.6 in the appendix, Section 5.7).[12]

Income. Before going through each hypothesis, we start our analysis with income which is possibly related to all three hypotheses: Ceteris paribus, middle income individuals (who pay taxes but have a relatively low income) may have a higher willingness to pay for public goods (Epple and Romano, 1996), a higher preference for redistribution (Fehr and Schmidt, 1999) as well as a higher tax morale (Torgler, 2006) than high

[12] In addition to the interacted regressions, we re-estimate the baseline model including only the base dummy variables (without interactions) to make sure that the effects of income and taxes are not driven by compositional effects. Table 5.7.7 in the appendix (Section 5.7) shows that results do not change when including one or all dummy variables used for the subsequent interactions.

income individuals. We divide our taxpayer sample into income quintiles and calculate quintile specific marginal effects of net income and taxes. It is important to note that the bottom quintiles of the taxpayers distribution are actually part of the middle-class of the income distribution of the full population as only slightly more than 50% of the individuals pay income taxes. Figure 5.5.1 shows that marginal effects are declining in income (left panel). When looking at the marginal effect of paying taxes (right panel) only the bottom of the taxpayer distribution (the poorest 40 percent) have higher SWB when paying taxes. The marginal effects in quintiles 3 to 5 do not seem to be affected by taxes.

Figure 5.5.1: Marginal effects - by income quintile



Given the strong heterogeneous effects we find for different income quintiles, we additionally interact all subgroup dummies with a variable indicating whether the individual is in the lower (quintiles 1 and 2) or the upper part (3-5) of the income distribution to take out the income effect in the following analyses. We are thus particularly interested in whether individuals within the lower part of the income distribution have significantly different tax effects and whether there are certain subgroups within the upper part of the income distribution that derive a positive marginal effect from paying taxes.

Public goods. The first hypothesis we test is whether the positive coefficient on taxes conditional on net income is related to public goods. Unfortunately, we do not directly observe individual public good consumption and have to proxy it using various indicators. First, we exploit information on regional public good availability. We merge metropolitan area *(Raumordnungsregion)* data on public good expenditures per capita for the years 1997 to 2007 to the SOEP. The regional data on public good expenditures have been obtained from the Statistical Offices of the German federal states *(Statistische Landesämter)*. We check whether individuals living in regions with higher regional per capita expenditures and thus a higher average public good consumption have different marginal effects from paying taxes.[13] We group individuals into terciles of per capita public good expenditures. The top left panel of Figure 5.5.2 shows that individuals in the two lowest terciles, i.e. those living in regions where there is a (relative) underprovision of public goods, have a higher marginal effect from paying taxes in the lower part of the income distribution. In the upper part of the distribution – though not statistically significant at the 5% level –, the panel implies that individuals in regions with a low per capita public good expenditure derive a positive marginal effect from paying taxes, while the top tercile even has a negative marginal effect.

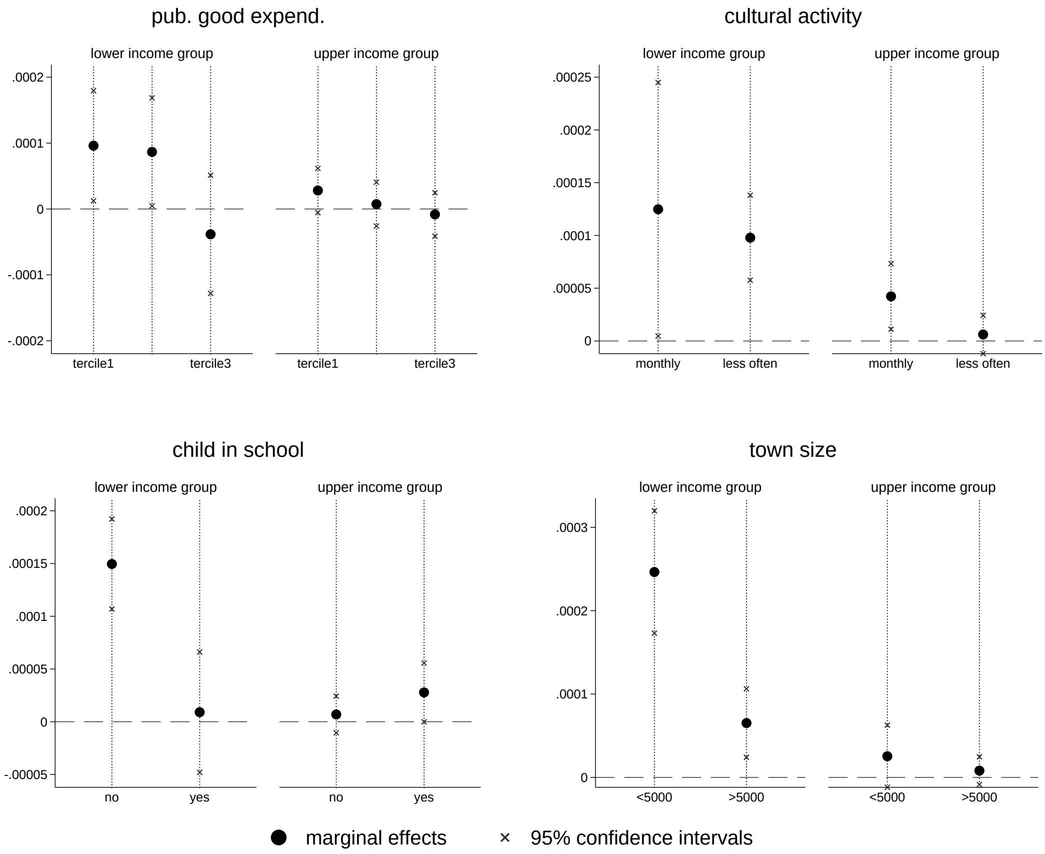
Next, we proxy public good consumption by using a SOEP question on cultural activity. This question asks how frequently individuals attend plays, concerts, and exhibitions which are at least partly publicly funded in Germany. As the top right panel of Figure 5.5.2 indicates, individuals in the upper part of the distribution who are culturally active are statistically significantly happy to pay taxes, whereas the marginal effect from paying taxes for inactive individuals is zero.

Third, we look at individuals in households with school-age children. Given that tax money is partly used to finance schools, the public goods hypothesis suggests that individuals with children in school derive a higher marginal effect from paying taxes. While our empirical findings support this rationale for the upper income group where individuals with children do even have significantly positive marginal effect from paying taxes, we find the opposite in the lower half of the income distribution (see bottom left panel of Figure 5.5.2).

A last test – on the border between public goods and preferences for redistribution – is to look at the size of the municipality the individuals live in. On the one hand, bigger cities provide more public goods and services, hence the willingness to pay should be higher in smaller cities due to the relative underprovision. On the other hand, social

[13] Note that we assess the effect of paying federal taxes although public good expenditure is rather local. Yet, communities are assigned a certain share of their collected federal taxes so that there is a direct link between the two. In Germany, there are no local income or sales taxes.

Figure 5.5.2: Marginal effects of taxes - public goods



cohesion is higher in smaller communities, which again would point to a higher willingness to pay taxes. In line with our prediction, we find in the bottom right panel of Figure 5.5.2 that individuals in the lower part of the income distribution who live in small communities (with less than 5,000 inhabitants) have a very high marginal effect from paying taxes, while the coefficient for individuals in larger communities is significantly smaller, though still positive.

Preferences for redistribution. An obvious attempt to explain differences in the effect of paying taxes on SWB is differentiating by the redistributive taste of individuals. Preferences for redistribution can be egoistic and driven by pecuniary motives or they can be shaped by societal values (Corneo and Grüner, 2000, refer to the first channel as "homo oeconomicus effect" to the second as "public values effect"). Alesina and Fuchs-Schündeln (2007) show that preferences for redistribution have been shaped by the political socialization in East and West Germany prior to the reunification. We can use the same hypothesis and look at whether there is an East-West divide in terms of preferences for taxation as well. We thus differentiate between individuals who lived in Eastern Germany and taxpayers who lived in Western Germany prior to the reunification in 1990. As it turns out from looking at the top left panel of Figure 5.5.3, Eastern Germans in the lower part of the income distribution have a significantly higher marginal effect of paying taxes than individuals who have lived in the West prior to 1990. The same is true for the upper part, where individuals from the East have a positive coefficient on the tax variable conditional on net income, whereas individuals from the West do not.

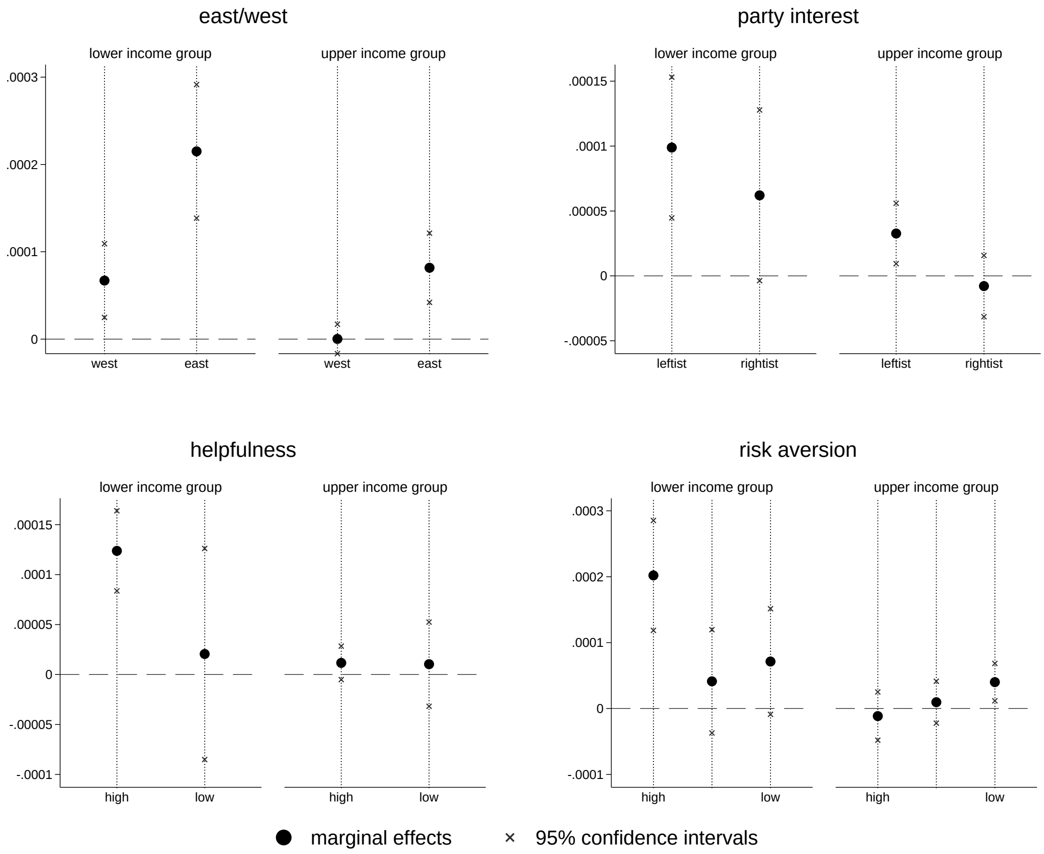
A second, related test is to check for partisan differences in the redistributive taste. Following Alesina and Angeletos (2005) we would expect individuals in favor of leftist parties (SPD, Die Grünen, PDS/Die Linke) to have a higher taste for redistribution and thus a higher marginal effect of paying taxes. Indeed, the top right panel of Figure 5.5.3 shows that leftists voters do have a more positive marginal tax effect. In fact, even in the upper part of the income distribution we find a positive and significant effect for individuals supporting leftist parties.

Theoretically, a high redistributive taste could be due to altruistic motives. We proxy altruism by a SOEP question on the "importance of being there for others" coded on a four point scale ranging from very important to unimportant. We dichotomize the variable which is included in the waves of 1990, 1992, 1995, 2004 and 2008. The bottom left panel of Figure 5.5.3 suggests that in both parts of the income distribution the individuals with a high preference towards altruism show a positive and significant marginal effect of paying taxes.

Another factor that could lead to a high redistributive taste is risk aversion. Risk averse individuals might like to pay taxes if they regard them as premiums to an insurance against income shocks. In order to test this hypothesis, we use a direct measure on individual risk aversion provided in the SOEP.[14] We group our population in terciles

[14] In the waves of 2004, 2006, 2008, 2009 and 2010, a question on self-rated risk aversion (ranging from 0 ('risk averse') to 10 ('fully prepared to take risks') is asked. We pool the answers to the questions of

Figure 5.5.3: Marginal effects of taxes - redistributive preferences



of high, medium and low risk aversion. The bottom right panel of Figure 5.5.3 reveals that individuals in the lower part of the income distribution only like to pay taxes if they have a high level of risk aversion (the pattern seems to be reversed for the high income group). For the other subgroups the marginal effect is not statistically significantly different from zero.

To sum up, the findings presented in Figure 5.5.1 (marginal effect decreasing with income) confirm the "homo oeconomicus effect", whereas the results presented in Figure 5.5.3 provide additional evidence in favor of the "public values effect".

all waves and assign an individual its mean risk aversion level.

Tax morale. According to Lubian and Zarri (2011) individuals with a higher tax morale have a higher level of SWB – suggesting another channel which could explain our positive coefficient of tax payments conditional on net income. As we do not have a question on tax morale in the SOEP, we run a regression of tax morale on a set of characteristics which have been identified to affect tax morale (such as age, skill, gender, religiosity, income and labor market status) using data from the World Value Survey.[15] Having determined the variables affecting tax morale, we make an out-of-sample prediction in the SOEP and determine the probability of having a low or a high tax morale. The upper left panel of Figure 5.5.4 shows that – though not statistically significant – the higher the tax morale the higher the marginal effect of paying taxes in both parts of the income distribution.

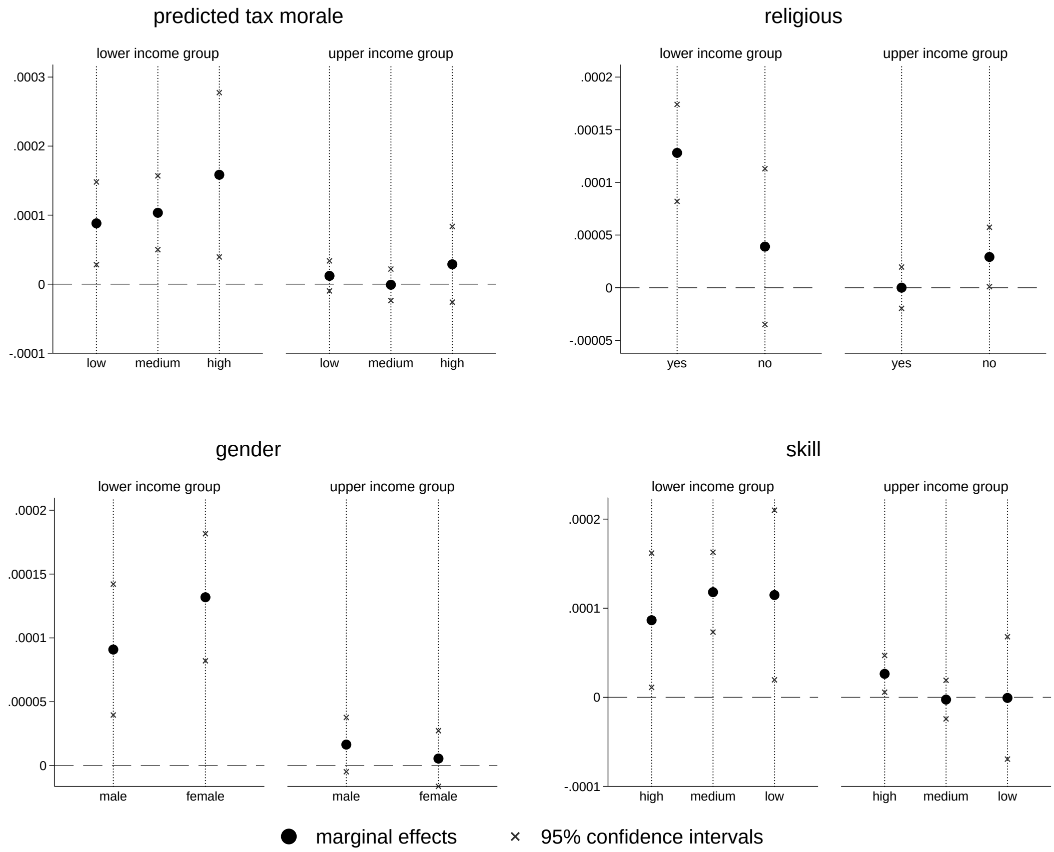
Secondly, we differentiate by religiosity. Religion does not only work as an internal moral enforcement device (Anderson, 1988), but also shows a strong and positive association with higher tax morale (Torgler, 2006). Looking at religion in Germany with its predominantly Christian population is especially interesting since members of the Christian churches (both Catholics and Protestants) have to pay church taxes. The church tax is directly linked to the income tax in two ways. First, the tax liability is a fixed share of the income tax (at the moment between 8% and 9% – depending on the state). Second, the church tax is collected with the income tax by the official tax authorities. While religiosity has been found to have a positive impact on SWB (Lelkes, 2006), in the context of our study the additional tax burden for members of the Christian church is of particular interest. In a way, Christians pay 'voluntarily' more taxes in exchange for certain services they receive from the church. The upper right panel of Figure 5.5.4 suggests that religiosity does not matter in the upper part of the distribution, but in the lower part only religious individuals have a significantly positive effect of paying taxes.

Third, it is a stylized fact in the tax morale literature that women have a higher tax morale (Alm and Torgler, 2006). While we do find that the marginal tax effect of women is slightly higher than for men in the lower income group, there does not seem to be a difference in the upper half of the distribution (see bottom left panel of Figure 5.5.4).

As far as qualification is concerned, the empirical findings in the tax morale literature

[15] Regression results are available on request. In line with the literature, tax morale increases (decreases) with age and education (income) and is higher (lower) for females and married (self-employed) individuals (see, e.g., Doerrenberg and Peichl, 2012).

Figure 5.5.4: Marginal effects of taxes - tax morale



are ambiguous, hinting at different signs in the relationship between skill level and tax morale in different parts of the income distribution (Doerrenberg and Peichl, 2012). As the bottom right panel of Figure 5.5.4 indicates, we find some suggestive evidence backing this hypothesis. In the lower part of the income distribution the marginal effect of taxes seems to be decreasing in skill, whereas in the upper half, better qualified individuals have a higher marginal effect of paying taxes.

Summary. In addition to the results discussed in detail above, we also investigated further variables where we did not find statistically unambiguous results. The last two columns of Table 5.5.1 summarize the empirical findings for all variables analyzed. For instance, we would have expected to find a negative coefficient for return migrants since they will not benefit from public goods in the future. In terms of redistributive taste, we would have expected individuals who volunteer regularly as well as individuals with a higher trust level to have positive marginal effects. Last, the literature on tax morale suggests that self-employed have a lower intrinsic motivation to pay taxes, results that we cannot confirm with our SWB regressions.[16]

Based on the results reported in Table 5.5.1, we now discuss the relative merit of our three hypothesis. Public goods are confirmed in about half of the checks both for the lower and the upper part. The relative low 'success rate' might be due to the quality of the proxies for public good consumption. The fact that there are no big differences between the lower and upper part could be due to the fact that public good consumption is rather equal across the income distribution. The redistributive taste hypothesis is confirmed more often for the lower than for the upper part of the distribution which might indicate self-interested redistributive tastes. Finally, for tax morale we confirm all checks for the lower part but none for the upper part. This is not surprising since tax morale is declining with income in our sample.

5.6 Conclusion

In this paper, we examine the effect of paying taxes on individual SWB. Using 26 waves of the German Socio-Economic Panel, we find that, conditional on net income, taxation has a positive, significant and robust effect on SWB. Several non-rivaling explanations for this finding are possible: public good consumption, redistributive tastes and an intrinsic motivation to pay taxes. Our analysis does not invalidate any of these hypotheses and all three are important to a certain degree for the whole population as different individuals can have different motives for paying taxes. Tests for heterogeneous effects, however, tend to support primarily the redistributive/insurance motive and, for the lower income group among tax payers, factors attributed to tax morale. All these channels could give rise to a warm glow motive associated with paying taxes (Owen and Videras, 2006).

[16] The main reason for the ambiguous findings for all these variables is probably the low statistical power of our regressions due to too small sample size, for e.g. return migrants, or due to questions which are not frequently asked in the SOEP (such as trust).

Admittedly, other channels could explain our results, which could not be tested in the present work due to data limitation. For instance, some 'citizenship' feeling of belonging to (or contributing to) the society might be important. Future research could investigate such channels or employ better data for the ones analyzed here. In addition, trying to isolate the channels of the positive tax effect and their relative importance (e.g. in controlled experiments) would be worthwhile. It would also be interesting to replicate our findings with data from other countries with a welfare state different from the German one (e.g. the US). In that way one could investigate if the conditional tax effect differs in different institutional and cultural settings.

5.7 Appendix

Table 5.7.1: Descriptive statistics, taxpayer sample

	mean	sd	min	max
subjetive well-being	7.09	1.7	0	10
gross income	3939.17	2514.2	667	116210
net income	2614.84	1572.7	350	114856
taxes	1324.32	1075.7	11	56412
age	41.06	11.0	17	99
gender	0.50	0.5	0	1
east	0.21	0.4	0	1
foreigner	0.13	0.3	0	1
high skilled	0.27	0.4	0	1
medium skilled	0.60	0.5	0	1
low skilled	0.12	0.3	0	1
household type	0.87	0.3	0	1
married	0.71	0.5	0	1
separated	0.02	0.1	0	1
divorced	0.07	0.3	0	1
widowed	0.02	0.1	0	1
household size	2.44	1.0	1	13
one child	0.11	0.3	0	1
two children	0.10	0.3	0	1
three children	0.02	0.2	0	1
more than three children	0.01	0.1	0	1
self-employed	0.07	0.2	0	1
civil servant	0.07	0.2	0	1
unemployed	0.03	0.2	0	1
pensioner	0.03	0.2	0	1
non-employed	0.11	0.3	0	1
very good health	0.21	0.4	0	1
good health	0.44	0.5	0	1
satisfactory health	0.22	0.4	0	1
poor health	0.09	0.3	0	1
bad health	0.03	0.2	0	1

Source: SOEP. *Note:* 188,412 observations. All money variables in 2010 euros.

Table 5.7.2: Well-being effects: baseline results, all covariates

Model Specification	(1) Baseline	(2) Lagged tax	(3) Instrumented	(4) Income tax only
log net income	0.301***	0.320***	0.294***	0.327***
	(0.017)	(0.017)	(0.017)	(0.015)
log taxes	0.045***		0.024***	0.014***
	(0.009)		(0.004)	(0.003)
log taxes$_{t-1}$		0.009**		
		(0.004)		
log working hours	0.054***	0.048***	0.052***	0.054***
	(0.012)	(0.014)	(0.014)	(0.012)
age squared	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
east	-0.180	-0.263**	-0.182	-0.180
	(0.117)	(0.132)	(0.137)	(0.117)
foreigner	-0.019	-0.006	-0.006	-0.018
	(0.056)	(0.062)	(0.062)	(0.056)
log hhsize	-0.042	-0.018	-0.042	-0.042
	(0.029)	(0.034)	(0.034)	(0.029)
household type	0.083***	0.065*	-0.031	0.084***
	(0.030)	(0.034)	(0.037)	(0.030)
high skilled	-0.137***	-0.167***	-0.157**	-0.136**
	(0.053)	(0.062)	(0.062)	(0.053)
medium skilled	-0.057	-0.077*	-0.075*	-0.056
	(0.040)	(0.046)	(0.045)	(0.040)
pensioner	0.264***	0.251***	0.256***	0.263***
	(0.054)	(0.061)	(0.059)	(0.054)
self-employed	-0.029	-0.057*	-0.075**	-0.041
	(0.029)	(0.033)	(0.032)	(0.029)
unemployed	-0.276***	-0.288***	-0.302***	-0.278***
	(0.049)	(0.055)	(0.054)	(0.049)
non-employed	0.195***	0.184***	0.191***	0.193***
	(0.041)	(0.047)	(0.046)	(0.041)
handicapped	-0.175***	-0.179***	-0.190***	-0.176***
	(0.040)	(0.044)	(0.044)	(0.040)
gender	0.036	0.002	-0.060	0.036
	(0.067)	(0.070)	(0.087)	(0.067)
married	0.093***	0.094***	0.132***	0.092***
	(0.022)	(0.025)	(0.026)	(0.022)
separated	-0.169***	-0.180***	-0.046	-0.169***
	(0.042)	(0.048)	(0.052)	(0.042)
divorced	0.150***	0.148***	0.153***	0.151***
	(0.036)	(0.041)	(0.042)	(0.036)
widowed	-0.066	-0.054	0.078	-0.067
	(0.092)	(0.103)	(0.111)	(0.092)
one child	0.033	0.020	0.021	0.032
	(0.020)	(0.023)	(0.024)	(0.020)
two children	0.018	-0.008	-0.005	0.017
	(0.027)	(0.031)	(0.031)	(0.027)
three children	0.070*	0.049	0.076*	0.069*
	(0.040)	(0.046)	(0.046)	(0.040)
more than three children	0.180**	0.102	0.132	0.178**
	(0.083)	(0.092)	(0.090)	(0.082)
good health	-0.377***	-0.372***	-0.369***	-0.377***
	(0.010)	(0.011)	(0.011)	(0.010)
satisfactory health	-0.852***	-0.838***	-0.835***	-0.852***
	(0.013)	(0.015)	(0.015)	(0.013)
poor health	-1.298***	-1.285***	-1.277***	-1.298***
	(0.019)	(0.020)	(0.020)	(0.019)
bad health	-1.954***	-1.944***	-1.929***	-1.954***
	(0.034)	(0.038)	(0.038)	(0.034)
adj. R^2	0.127	0.149	0.103	0.127
obs.	188412	150883	150316	188412

Note: Dependent variable: subjective well-being. Standard errors (in parentheses) clustered at person level. All regressions include person, state and year fixed. All money variables are in 2010 euros. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Table 5.7.3: Well-being effects: different functional forms

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
(net income)^1 [$\beta \cdot 1000$]	0.059***	0.107***	0.104***	0.107***	0.134***	0.665***	0.447***		0.000***		0.000***				
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)		(0.000)		(0.000)				
(net income)^2 [$\beta \cdot 1000$]		-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***				-0.000***				
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)				(0.000)				
(net income)^3 [$\beta \cdot 1000$]					0.000***	0.000***	0.000***								
					(0.000)	(0.000)	(0.000)								
(taxes)^1 [$\beta \cdot 1000$]	0.028***	0.004***	0.036***	0.034***	0.055***	0.016***	0.359***	0.000		0.000**					
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)		(0.000)					
(taxes)^2 [$\beta \cdot 1000$]			-0.000***	-0.000***	-0.000***		-0.000***			-0.000***					
			(0.000)	(0.000)	(0.000)		(0.000)			(0.000)					
(taxes)^3 [$\beta \cdot 1000$]					0.000***		0.000***								
					(0.000)		(0.000)								
net income $\cdot$ taxes [$\beta \cdot 1000$]				-0.000***											
				(0.000)											
(log net income)^1								0.342***		0.332***		0.301***	0.906***	0.794***	0.486**
								(0.017)		(0.017)		(0.017)	(0.203)	(0.213)	(0.232)
(log net income)^2													-0.040***	-0.032**	0.017
													(0.013)	(0.014)	(0.020)
(log taxes)^1									0.107***		0.085***	0.045***	0.045***	0.141**	0.435***
									(0.011)		(0.009)	(0.009)	(0.009)	(0.068)	(0.115)
(log taxes)^2														-0.007	0.009
														(0.005)	(0.007)
log net income $\cdot$ log taxes															-0.066***
															(0.019)
8th order poly. net inc.	No	No	No	No	No	Yes	Yes	No	No	No	No	No	No	No	No
8th order poly. taxes	No	No	No	No	No	No	Yes	No	No	No	No	No	No	No	No
adj. R^2	0.152	0.144	0.142	0.142	0.137	0.002	0.000	0.129	0.142	0.128	0.138	0.127	0.127	0.127	0.126
obs.	188412	188412	188412	188412	188412	188406	188406	188412	188412	188412	188412	188412	188412	188412	188412
marg. eff. net inc.	0.00006	0.00010	0.00010	0.00010	0.00011	0.00015	0.00015	0.00015	0.00005	0.00014	0.00008	0.00013	0.00012	0.00013	0.00012
marg. eff. taxes	0.00003	0.00000	0.00003	0.00003	0.00004	0.00002	0.00004	0.00000	0.00010	0.00002	0.00008	0.00004	0.00004	0.00003	0.00004
MRS tax/net inc.	0.48	0.04	0.29	0.26	0.34	0.11	0.26	0.02	2.09	0.11	1.03	0.33	0.34	0.28	0.37

Note: Dependent variable: subjective well-being. Standard errors (in parentheses) clustered at person level. All regressions include standard controls variables as well as person and year fixed effects. All money variables are in 2010 euros. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***). MRS stands for marginal rate of substitution between taxes and income.

Table 5.7.4: Well-being effects: by estimator

Estimator Model	Fixed Effects (1)	Quasi FE (2)	Ordered Logit (3)	FE O-Logit (4)
log net income	0.301***	0.316***	0.562***	0.499***
	(0.017)	(0.013)	(0.015)	(0.028)
log taxes	0.045***	0.041***	0.000	0.069***
	(0.009)	(0.008)	(0.009)	(0.015)
adj. R^2	0.127	0.299	0.086	0.084
obs.	188412	188412	188412	607600
marg. eff. net inc.	0.00013	0.00014	-0.00001	0.00021
marg. eff. taxes	0.00004	0.00004	-0.00000	0.00006
MRS tax/net inc.	0.33	0.29	0.00	0.30

Note: Dependent variable: subjective well-being. Standard errors (in parentheses) clustered at person level. All regressions include standard controls variables as well as person and year fixed effects. All money variables are in 2010 euros. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***). MRS stands for marginal rate of substitution between taxes and income.

Table 5.7.5: Well-being effects: different samples

Model	(1) Population weights	(2) Singles only	(3) Individuals in couples	(4) All individuals	(5)
log net income	0.299***	0.376***	0.308***		
	(0.017)	(0.064)	(0.018)		
log taxes	0.045***	0.060**	0.046***	0.031***	
	(0.009)	(0.027)	(0.010)	(0.007)	
log disp. income				0.236***	0.246***
				(0.014)	(0.013)
log net taxation					0.012***
					(0.002)
adj. R^2	0.125	0.044	0.142	0.261	0.260
obs.	186603	24037	164375	260480	260480
marg. eff. income	0.00013	0.00025	0.00012	0.00010	0.00010
marg. eff. taxes	0.00004	0.00008	0.00004	0.00004	0.00002
MRS tax/inc.	0.33	0.30	0.33	0.42	0.17

Note: Dependent variable: subjective well-being. Standard errors (in parentheses) clustered at person level. All regressions include standard controls variables as well as person and year fixed effects. All money variables are in 2010 euros. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***). MRS stands for marginal rate of substitution between taxes and income.

Table 5.7.6: Well-being effects: interaction models

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Log tax interacted with	income quintiles	pub. good expend.	cultural activity	child in school	town size	east/west	party interest	helpfuln.	risk aversion	predicted tax	religious	gender	skill
income quintile 1	0.068***												
income quintile 2	0.057***												
income quintile 3	0.006												
income quintile 4	0.005												
income quintile 5	-0.004												
lower income group, tercile 1		0.063**											
lower income group, tercile 2		0.056**											
lower income group, tercile 3		-0.026											
upper income group, tercile 1		0.046											
upper income group, tercile 2		0.012											
upper income group, tercile 3		-0.014											
lower income group, monthly			0.074**										
lower income group, less often			0.060***										
upper income group, monthly			0.071***										
upper income group, less often			0.009										
lower income group, no				0.087***							0.022		
lower income group, yes				0.007							0.083***		
upper income group, no				0.010							0.046**		
upper income group, yes				0.054*							0.000		
lower income group, smaller than 5000					0.141***								
lower income group, greater than 5000					0.041***								
upper income group, smaller than 5000					0.036								
upper income group, greater than 5000					0.012								
lower income group, west						0.043***							
lower income group, east						0.115***							
upper income group, west						0.000							
upper income group, east						0.115***							
lower income group, leftist							0.062***						
lower income group, rightist							0.039*						
upper income group, leftist							0.048***						
upper income group, rightist							-0.013						
lower income group, high								0.075***	0.128***	0.095***			0.054**
lower income group, low								0.012	0.044*	0.051***			0.070**
upper income group, high								0.018	-0.018	0.038			0.047**
upper income group, low								0.015	0.066***	0.019			-0.001
lower income group, medium									0.026	0.069***			0.072***
upper income group, medium									0.015	-0.001			-0.004
lower income group, male												0.057***	
lower income group, female												0.080***	
upper income group, male												0.025	
upper income group, female												0.008	
adj. R^2	0.126	0.090	0.141	0.127	0.128	0.056	0.131	0.110	0.123	0.124	0.101	0.126	0.126
obs.	188412	80801	164793	188412	188407	178121	134860	165241	97556	141772	141772	188412	188412

Note: Dependent variable: subjective well-being. Standard errors (in parentheses) clustered at person level. All regressions include standard controls variables , person and year fixed effects as well as base effects of the interactions. All money variables are in 2010 euros. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Table 5.7.7: Well-being effects: interaction groups

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log net income	0.223***	0.290***	0.295***	0.295***	0.278***	0.293***	0.211***
	(0.032)	(0.023)	(0.021)	(0.021)	(0.024)	(0.024)	(0.043)
log taxes	0.021	0.035***	0.040***	0.040***	0.043***	0.035***	0.040*
	(0.018)	(0.013)	(0.012)	(0.012)	(0.014)	(0.013)	(0.023)
relative log net income	-0.032	-0.010	-0.002	-0.002	0.023	-0.008	-0.023
	(0.042)	(0.030)	(0.028)	(0.028)	(0.032)	(0.031)	(0.055)
relative log taxes	-0.004	0.006	0.007	0.006	0.003	-0.001	0.015
	(0.028)	(0.020)	(0.019)	(0.019)	(0.021)	(0.021)	(0.037)
occ. prestige (Ganzeb.)	0.001	0.002***	0.002***	0.002***	0.002**	0.002***	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
pub. good expend.: tercile2	0.015						0.033
	(0.019)						(0.024)
pub. good expend.: tercile3	0.034						0.052
	(0.029)						(0.037)
cultural activity: less often		-0.063***					-0.069***
		(0.014)					(0.026)
child in school: yes			-0.043**				-0.062*
			(0.022)				(0.035)
town size: greater than 5000				-0.005			0.020
				(0.030)			(0.064)
party interest: rightist					-0.001		-0.143**
					(0.034)		(0.068)
predicted tax morale: medium						0.006	0.022
						(0.016)	(0.027)
predicted tax morale: high						-0.013	-0.023
						(0.030)	(0.058)
adj. R^2	0.104	0.130	0.120	0.118	0.116	0.119	0.125
obs.	65546	132534	151693	151690	110034	116099	36600
marg. eff. net inc.	0.00009	0.00012	0.00012	0.00012	0.00011	0.00012	0.00008
marg. eff. taxes	0.00002	0.00003	0.00003	0.00003	0.00004	0.00003	0.00003
MRS tax/net inc.	0.19	0.25	0.29	0.29	0.33	0.25	0.38

Note: Dependent variable: subjective well-being. Standard errors (in parentheses) clustered at person level. All regressions include standard controls variables as well as person and year fixed effects. All money variables are in 2010 euros. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***). MRS stands for marginal rate of substitution between taxes and income.

Chapter 6

Stabilizing effects of work sharing policies*

6.1 Introduction

The 2008-09 economic downturn has led to a broad discussion, both in the public and academic arena, on the likely distributional and fiscal consequences of the crisis and on which policy might be most effective at mitigating the adverse labor market and welfare consequences of the downturn. In fact, policy efforts to minimize welfare losses were seriously hampered by how little was known about the distribution of changes in employment and incomes and about the capacity of existing redistribution systems to soften the negative impacts of job and earnings losses. In this context the German experience is particularly interesting. While Germany has suffered a substantial drop in GDP (around 5 percent on average—an even larger slump than in the United States), employment levels and unemployment rates were unusually resilient as most of labor adjustments occurred at the intensive margin (working hours). This is in contrast to many other Western countries, which experienced far greater levels of layoffs. While many analysts and policy makers have focused on Germany's employment effects and its work-sharing policies (see, e.g., Hijzen and Venn, 2011, Cahuc and Carcillo, 2011), much less is known about precise distributional and fiscal consequences of alternative

* This chapter has been published as "Distributional Consequences of Labor-demand Shocks: The 2008-09 Recession in Germany" (joint with Olivier Bargain, Herwig Immervoll and Andreas Peichl, see Bargain et al., 2012).

labor market adjustments.

We investigate this question, focusing on the German situation for the years 2008-09. While it is possible to speculate about which groups are likely to be hardest-hit, detailed distributional studies are usually not available until the crisis is long over and decisions have already been made. For that reason we develop a straightforward approach to gauge the distributional and fiscal implications of large output changes at an early stage, i.e., without having the appropriate microdata. We first estimate labor demand on 12 years of high-quality, micro-level administrative employer-employee data (LIAB). The estimates are used to predict the labor-demand effects of the output shocks observed during the downturn at a disaggregated level (by industry and for labor inputs detailed by age, skill and contract type). Interestingly, we are able to transpose these labor market changes to household-level microdata commonly used for distributional analyses (the German Socio-Economic Panel, SOEP).[1]

Using this combined approach we analyze the first-round consequences of the recession for income changes at the household level. We suggest two contrasting scenarios when translating labor-demand reactions to earnings losses at the household level. The first polar case (*intensive scenario)* allows only for adjustments of employees' working hours rather than staff levels. Although being stylized, this scenario comes close to the observed German situation and also to that of other countries where much of the reductions in total working hours occurred at the intensive margin (e.g., Austria, Belgium, the Czech Republic, the Slovak Republic). The second polar case (*extensive scenario*) shows what happens if the same overall adjustment in total working hours occurs exclusively via layoffs and hires—a scenario more in line with the situation experienced in the United States, Greece, Ireland, Spain or the UK (OECD, 2010).[2]

[1] To the best of our knowledge, this is the first empirical study linking output changes to distributional and fiscal consequences using a detailed micro model of labor-demand responses. The approach is conceptually related to the literature on linking micro and macro models (see, e.g., Bourguignon et al., 2003 or Peichl, 2009 for a survey, and Bourguignon, Bussolo, and Pereira da Silva, 2008; Ferreira, Leite, Pereira da Silva, and Picchetti, 2008; Robilliard, Bourguignon, and Robinson, 2008; Hérault, 2010; Ahmed and O'Donoghue, 2010 for distributional and crisis-related analyses). In particular, our method is closer to the "top-down"approach which aims to approximate the effect of macro changes on income distribution. Further differences with related methods are discussed in the following sections.

[2] Our demand model, specified on total hours (rather than employment levels), captures the actual total labor demand adjustment (comprising both margins) reasonably well. In fact, we show that the German labor market performance was very much in line with past reaction to output changes as far as changes in total hours worked are concerned.

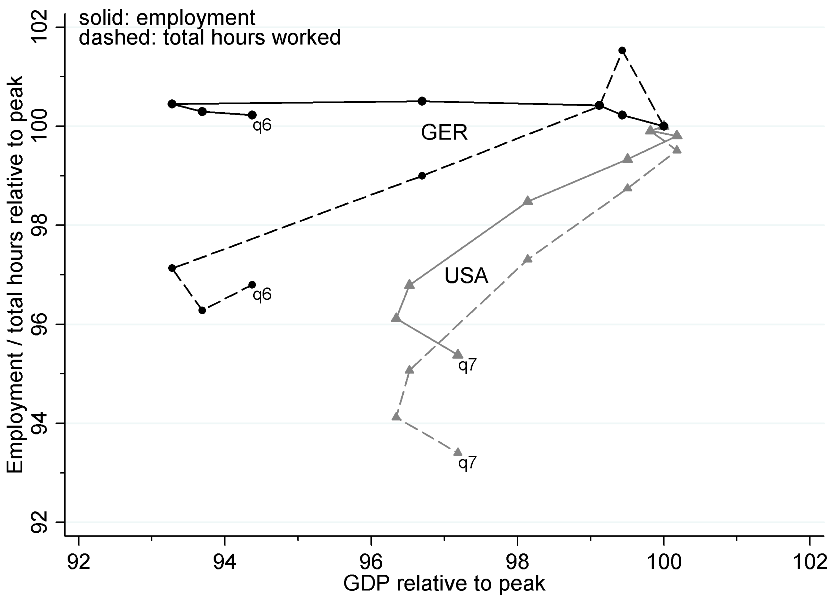
Our results show that low-skilled and non-standard workers face above-average risks of earnings losses. An examination of the resulting income losses reveals, however, that automatic stabilization by the tax-benefit system is effective in cushioning a significant share of the gross-income losses. Moreover, we find that the margin of adjustments does indeed matter. Given the likely pattern of job losses among different groups of workers, adjustments at the extensive margin result in a sizeable widening of the income distribution, increasing inequality and a rise in the number of poor people by more than 10 percent. In the intensive scenario poverty headcounts rise by under 4 percent, while most inequality measures are predicted to change little. Importantly, adjustments at the intensive margin are also preferable from a fiscal point of view—at least in the short-term. We also discuss the limits of our analysis, notably the fact that the hour-adjustment would be even more favorable in distributional terms in countries with less generous unemployment insurance. However, it is less effective if the economic structure encourages temporary work or does not provide incentives for firms to retain workers.

The remainder of the paper is structured as follows. Section 6.2 briefly summarizes the labor market changes in Germany during the crisis and contrasts them with the US experience. In Section 6.3 we lay out our empirical approach, present the data and the estimation of the labor-demand model. In Section, 6.4 we predict the first-round effect of output shocks on the demand for different labor inputs, compare them to observed labor market trends, and analyze the distributional consequences of labor market adjustments at the household level. Finally, we derive and discuss the fiscal consequences of working-hour reductions versus layoffs. Section 6.5 concludes.

6.2 The German labor market during the crisis

The German labor market performance has received considerable attention since the onset of the 2008-09 economic crisis. Figure 6.2.1 illustrates the unique adjustment patterns in Germany by contrasting output and employment changes against those observed in the United States.

Figure 6.2.1: Labor market adjustments: Germany vs. US



Source: OECD National Accounts database and Eurostat labor market statistics. *Notes:* Q0 is the quarter when GDP peaked (2007Q4 for US and 2008Q1 for Germany), and each data point refers to consecutive quarters since then.

During the recent economic crisis, Germany suffered particularly sizeable output losses of almost 7 percent since GDP peaked in 2008Q1. Yet employment levels, as shown by the black solid line, remained practically unchanged, suggesting an unusually low Okun's coefficient value. Nonetheless, Figure 6.2.1 shows that the crisis did have a significant effect on the German labor market. Up until 2009Q2, hours worked per employee (as well as total working hours in the economy) had declined by 4 percent (black dashed line). Hence, on aggregate, the adjustments materialized exclusively at the intensive margin (the difference between the solid and the dashed lines). In contrast to the German situation, US employment dropped by almost 5 percent despite a smaller drop in GDP (gray solid line). Most of the adjustment happened along the extensive margin, whereas working-hour reductions along the intensive margin accounted for only around one third of the drop in total hours worked (gray dashed line).

The specific adjustment witnessed in Figure 6.2.1 is partly the result of possibilities and constraints induced by labor market conditions and institutions (see, e.g., Möller, 2010; Eichhorst, Feil, and Marx, 2010; OECD, 2010). In the German context the government-supported short-time working scheme (*Kurzarbeit*) has tended to receive most of the attention. Yet while a substantial share (around 25 percent) of working-time reductions during the crisis to date can indeed be directly attributed to this program, other factors were more important on aggregate. The greatest reductions, accounting for more than one third of recorded changes in total hours worked, were due to opening clauses in collective agreements, which allowed temporary reductions in weekly working hours (and earnings), or to so-called "pacts for employment and competitiveness" between employers and employees (Bellmann, Gerlach, and Meyer, 2008). In addition, working-time accounts or "time banks", as well as substantially reduced overtime, accounted for around 20 percent each (Bach and Spitznagel, 2009).

In our analysis we set up a framework which is general enough to comprise both the intensive and extensive margin. This allows us to simulate two polar scenarios of adjustment which come close to the contrasted situations depicted in Figure 6.2.1. This will be described in the following section.

6.3 Empirical approach

To study the short-term effects of a large output shock on employment and income, we derive the likely patterns of demand-side adjustments using own labor-demand estimations. We assume a "right-to-manage" setting, with employment and hours chosen by the firm. Wages are fixed in the short term and labor inputs are the only margins of adjustment for firms (capital is constant). The labor-demand model is estimated on linked employer-employee data for Germany. In a second step the demand-side model is linked to household-level data, and tax-benefit simulations are conducted in order to derive the distributional consequences. In our approach the macro level output shocks are not derived from a stylized CGE-type of model but correspond directly to the observed changes per industry for the years 2008-09.[3] We ignore longer-term changes in prices and wages, which is justified in the German case, since wage adjustments were not a

[3]The method we suggest is rather general. It can also be applied as a tool for ex ante policy response analyses if one uses projections of output changes (instead of actual ones) in order to analyze forward-looking counterfactual scenarios.

primary channel for reducing labor costs during the downturn (Collective Agreement Archive, 2009; Bellmann and Gerner, 2011). Instead we focus on short-term labor-demand adjustments, which are the most immediate driver of household income losses during a labor market downturn. Before proceeding with the distributional analysis in Section 6.4, this section presents details on data sources and labor-demand estimations.

6.3.1 Data

The demand model relies on a high-quality linked employer-employee dataset (LIAB) from the Institute for Employment Research (IAB) in Nuremberg, Germany, (see Alda et al., 2005 for more information on the dataset and von Wachter and Bender, 2006 for a recent application). The firm component of the LIAB is the IAB Establishment Panel (Kölling, 2000). The term "establishment" refers to the fact that the observation unit is the individual plant, not the firm. The Establishment Panel is a representative, stratified, random sample including establishments with at least one worker for whom social contributions were paid. Information on employment levels and changes, staff qualifications, investment as well as industry affiliation and output are used.

The employee data stem from to the employment statistics of the German Federal Employment Agency (*Bundesagentur für Arbeit*) and are drawn from administrative records comprising all employees paying social security taxes or receiving unemployment benefits (see, e.g., Bender et al., 2000). The dataset covers about 80 percent of the people employed in Germany. Civil servants and family workers are excluded as they do not pay social security contributions. Information recorded in the data include employees' histories on daily wages, age, seniority, schooling, training, occupation, employment type (full-time, part-time or irregular employment), industry and region.

Data from the employee history are linked with the establishment sample year-by-year using a plant identifier. Since the unified sample for East and West Germany exists only since 1996, we focus on the period 1996-2007. We select establishments with at least 10 employees, in order to be able to identify substitution patterns between different types of workers. In total our resulting sample consists of $37,958$ establishment-year observations. The number of establishment-years is $19,520$ in manufacturing (51 percent of the total), $5,035$ in construction (13 percent), $1,847$ in transport and communications (5 percent), $10,956$ in services (29 percent) and 600 in financial services (2 percent).

For the distributional analysis we use the German Socio-Economic Panel (SOEP), a representative survey of the entire German population with around $25,000$ sample individuals living in more than $10,000$ households per cross-section (see Wagner et al., 2007). For the present paper we utilize information on labor market status, gross wage, job type, benefits, industry, working time, household composition, age, education levels and housing costs. We use the 2008 wave, which contains labor market information for the year 2007, in particular hours worked and wages. [4] In order to make the information consistent with the distributional analysis and the policy parameters as of January 1, 2009, we use a static ageing technique, which allows us to control for changes in global structural variables as well as income adjustments differentiated by income components (see Gupta and Kapur, 2000). We restrict the sample to the same industries as in the LIAB, but include the unemployed. This yields $5,532$ households and $9,218$ individuals.

To calculate net incomes and fiscal effects, we link the data to the tax and benefit simulation model of the Institute for the Study of Labor, IZAΨMOD (see Peichl et al., 2010). IZAΨMOD contains a tax-benefit calculator comprising all relevant features of the German tax and benefit system, such as income taxation and social insurance contribution rules, as well as unemployment, housing and child benefits.[5] Using the simulated tax and benefit payments, we can compute the disposable income for each household. We employ the population weights available in the SOEP. The results are therefore representative of the German population.

6.3.2 Labor demand model

We estimate a structural labor-demand model on the LIAB data. For our purposes it is essential to adopt a micro rather than a macro approach for mainly two reasons. Firstly, the explicit goal of our contribution is to assess the consequences of output changes on the demand for narrowly defined groups of workers. This implies that we have to ac-

[4] As explained in the introduction, it is precisely the lack of rapid microdata production that justifies our approach.

[5] Note that IZAΨMOD also has a behavioral module allowing for the simulation of labor supply reactions, which is not used in this application.

count for substitution patterns between different labor inputs at the firm level. Secondly, macro models of labor demand produce unbiased results only under quite restrictive assumptions with regard to employment adjustments (see Bresson, Kramarz, and Sevestre, 1992).

Following standard practice, we adopt the dual approach by assuming a constant output, specifying a cost function and using Shephard's lemma to derive the labor-demand functions (Hamermesh, 1986, 1993; Bond and Van Reenen, 2007). We opt for a Generalized Leontief specification as proposed by Diewert (1971), which is a linear second-order approximation to any arbitrary cost function. Importantly, it does not restrict the substitution elasticities of input factors. We follow the specification of Diewert and Wales (1987) and take a short-term perspective, assuming capital to be fixed (or perfectly separable from labor inputs). We also allow for non-constant returns to scale, which is important in the context of our study, since the output elasticities are not restricted to equal unity.

For a given firm there are $i = 1, ..., I$ labor inputs corresponding to the cells we define below. We ignore firm and time indices to clarify notations. We write C, the short-term labor costs of a firm, as follows:

$$C = \sum_i \alpha_i w_i + \sum_i \sum_j \alpha_{ij} w_j^{0.5} w_j^{0.5} Y + \sum_i (\beta_{YYi} w_i) Y^2, \tag{6.3.1}$$

with Y the firm-specific output and w_i the wage of labor group i. The symmetry condition $\alpha_{ij} = \alpha_{ji}, \forall i, j$, is the only restriction imposed on the coefficients. Differentiating C with respect to wages w_i yields the factor demands X_i, and dividing by Y gives the input-output ratio:

$$X_i / Y = \alpha_{ii} + \sum_{j \neq i} \alpha_{ij} \left(\frac{w_j}{w_i} \right)^{0.5} + \alpha_i \frac{1}{Y} + \beta_{YYi} Y, \tag{6.3.2}$$

which is the basis of our labor-demand estimation. Since we are analyzing the comparative-static effect of output shocks, our main measure of interest is the output elasticity of input (labor) demand, which is written as:

$$\epsilon_{iY} = \frac{\partial X_i}{\partial Y} \frac{Y}{X_i} = 1 - \frac{\alpha_i}{X_i} \tag{6.3.3}$$

6.3.3 Estimation

The detailed administrative data allow us to distinguish $I = 12$ labor inputs per industry. We differentiate between two skill/education levels, three age groups and two categories of employment contract. Skilled workers hold a university, polytechnical or college degree or have completed vocational training. Age groups are defined as 15-29 (young), 30-54 (middle-age) and 55-64 (old). We differentiate between full-time workers and a "non-standard" employment type category comprising both part-timers and irregular employment (short-term employment, temporary workers and those in marginal employment referred to as *Mini/Midijob* in Germany). We estimate input-output ratios separately for the five industries (manufacturing, construction, trade and communications, services and the financial sector), which gives $5 \times 12 = 60$ different cells for the distributional analysis.

There is clearly no complete congruence, and possibly a trade-off, between the definition of labor inputs used for the purpose of labor-demand estimation on the one hand and a disaggregated cell definition for precise distributional analyses on the other. We feel that the choice made here presents a reasonable balance. In particular, skill and age/experience groups constitute different types of productive factors for firms and also correspond to groups exposed to different risks of unemployment or working-time adjustments during a labor market downturn. One may wish more disaggregation for the distributional analysis (e.g., by gender or nationality) but this would be more difficult to justify in terms of labor-input differentiation. The output variable used for estimating the model is defined as business volume excluding intermediate inputs. For the financial sector we instead measure "output" as balance sheet total (banking) and total premiums paid (insurances).

We specify our labor-demand model with respect to total working hours—exploiting establishment level working-time information. This setup therefore captures changes in both employment (heads) and work intensity (hours) and implicitly assumes perfect substitutability between the two adjustment margins. To the best of our knowledge, an hours specification at the microlevel is unique. Most of the related studies estimating demand systems rely on the textbook head-count specification. In a few other papers the models are specified in terms of hours by appending working-hours measures to the data (see Hamermesh, 1993), but due to a lack of firm-level information, such working-hours measures normally rely on semi-aggregate averages (in most cases at the industry level) at a given point in time (see Freier and Steiner, 2010, for a recent example). Instead our micro approach is set up as follows: we first extract average full-time working hours at the establishment level directly from LIAB data. At this point we only have information on average full-time hours for a specific establishment in a certain year. We then extract mean working hours for each cell from the SOEP data. After which, we retrieve SOEP information on average full-time hours by industry and year, mirroring the available LIAB data. In a fourth step we calculate ratios of SOEP cell-specific working hours and SOEP industry-year full-time averages, which we finally apply to the LIAB data to construct a finely grained working hours distribution across our labor-demand cells in each establishment and year.

For the estimation we add two linear terms to the equations (6.3.2). We include time dummies to capture time trends as well as potential policy or business-cycle effects, and add disturbance terms ε_i for the $i = 1, .., 12$ inputs in each industry. The disturbance vector $\{\varepsilon_1, ..., \varepsilon_{12}\}$ is assumed to be multivariate and normally distributed, with mean vector zero and constant covariance matrix Ω. The system of 12 equations per industry is estimated using the Seemingly Unrelated Regressions (SUR) model proposed by Zellner (1962). SUR first employs equation-by-equation Ordinary Least Squares (OLS) estimation to obtain the covariance matrix of the error terms, Ω. A Feasible Generalized Least Squares estimation on the full system, conditional on Ω, is then conducted. Thus, SUR allows error terms to be contemporaneously correlated across regressions and is more efficient than separate OLS estimations.

It is useful to check the predictive power of the model. In Figure 6.3.1 we plot yearly relative changes in total hours worked as reported in the LIAB data against changes as predicted by the model for each industry over the period 1996-2007.[6] Predicted changes in working hours are derived by multiplying the industries' output elasticities with the industry-specific aggregate output change. With the exception of the financial sector, the graphs show the predictions to be rather accurate. This is reassuring with regards to the estimated model and provides confidence that using employment reactions to output changes over the entire period results in good approximations of employment changes in specific time periods.

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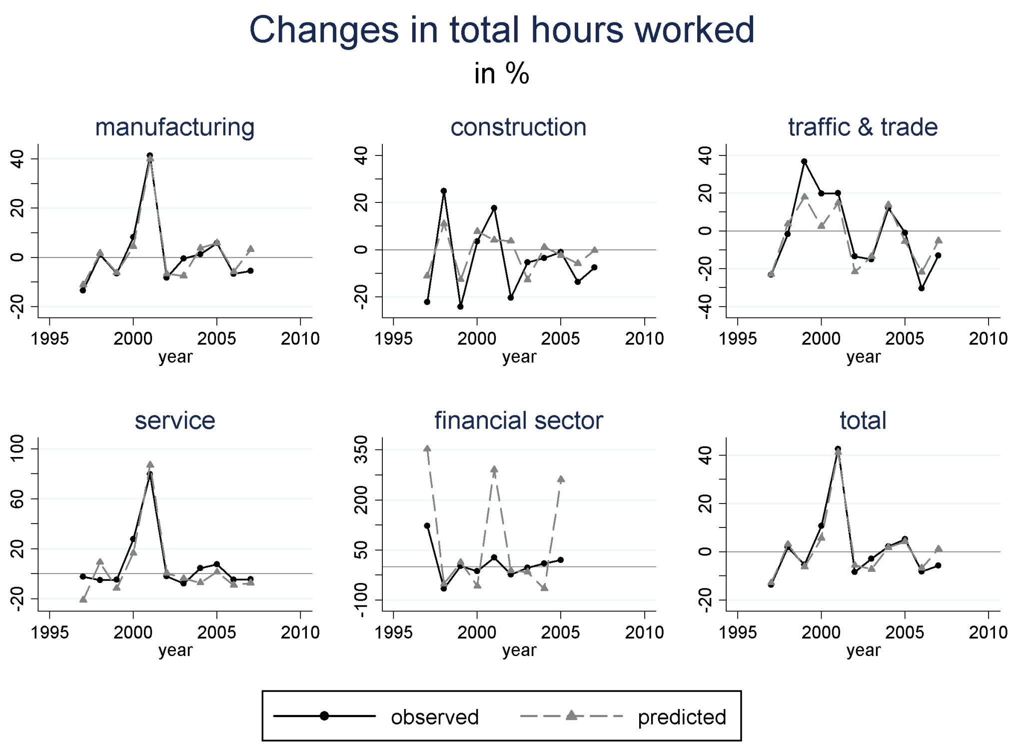
[6] Note that we could not use any observations for the financial sector for the years 2006 and 2007 because the LIAB output measure for these industries changed as of 2006.

Figure 6.3.1: Predictive power



Source: Observed hours form the LIAB, predicted hours calculated using LIAB output data and estimated elasticities.

Table 6.3.1 presents output elasticities of labor demand. For readability we present average elasticities for broader input groups in this table. Complete results for all 60 cells are reported in Table 6.6.1 in the appendix (Section 6.6). All group elasticities are positive—as predicted by theory. The average output elasticity across all cells is 0.64, which is well in line with other studies determining employment reactions to output shocks (normally output elasticities lie in $[0.5, 0.9]$, see e.g., Brechling and O'Brien, 1967; Fay and Medoff, 1985; Card, 1986). The results suggest that across all sectors unskilled employees are hired more quickly in a boom and fired faster during a recession. Output elasticities of young and, especially, older workers are also above average. As expected, those on nonstandard employment contracts are more likely to be affected by output changes than regular (full-time) employees.

Table 6.3.1: Output elasticities

Group	Man	Con	Tra	Ser	Fin	**Total**
Skilled	0.57	0.45	0.79	0.62	0.94	**0.59**
Unskilled	1.05	0.5	1.02	0.99	1.02	**0.96**
Young	0.74	0.55	0.02	0.72	0.87	**0.68**
Middle-age	0.62	0.41	0.92	0.61	0.96	**0.61**
Old	0.75	0.61	1.04	0.99	0.94	**0.82**
Full-time	0.65	0.43	0.80	0.63	0.95	**0.63**
Nonstandard	0.68	0.93	1.23	0.97	0.92	**0.83**
Total	**0.65**	**0.46**	**0.83**	**0.67**	**0.94**	**0.64**

Source: Own calculations using the LIAB. *Notes:* All numbers are averages weighted by the number of total hours in the respective cells. Man = Manufacturing, Con = Construction, Tra = Transport & Communications, Ser = Services, Fin = Financial Services.

6.4 Employment and distributional effects

We now model the impact of the crisis, first on employment using the labor-demand model, then on the household income distribution by feeding the predicted employment effects into the SOEP data. Our reference period for the output shock (and subsequent employment/distributional changes) is the period 2008-09, which corresponds to the recent downturn period in Germany.

6.4.1 Output shocks and predicted employment effects

Results are summarized in Table 6.4.1. The top panel reports changes in official output aggregates and employment by industry over the crisis period. Output, as measured by value added for each industry from German national accounts, dropped in all of the shown industries. Overall, the German economy shrunk by 5 percent over this period. In the selected sample of industries, value added declined by even more (8 percent). [7] In particular, the decline in manufacturing output, a slump of 18 percent, is noteworthy.

Employment changes are shown in headcounts (employment levels) as well as total hours worked, accounting for adjustments along both the extensive and intensive margin. It is evident that the output shock did result in sizeable overall labor-demand

[7] The difference is mostly due to the public sector, where value added actually increased during the crisis period.

effects. Yet there is a considerable difference between the margins of adjustment. While changes in employment levels are minimal, total hours worked dropped substantially over a relatively short period of time, with a very large drop of about 10 percent in the manufacturing sector.

Table 6.4.1: Output shocks and actual vs. predicted hours adjustments

	Man	Con	Tra	Ser	Fin	Total
	Official statistics					
	Output (value added, price adjusted)					
2008	496.4	78.8	130.5	949.6	76.1	1731.3
2009	406.2	77.7	119.2	917.6	74.9	1595.6
% change	-18	-1	-9	-3	-2	-8
	Employment levels (in 1000 workers)					
2008	7352	1741	2079	12420	1045	24637
2009	7163	1746	2067	12415	1042	24433
% change	-3	0	-1	0	0	-1
	Total hours worked (in millions)					
2008	10383	2680	3015	15827	1483	33387
2009	9352	2630	2915	15401	1457	31754
% change	-10	-2	-3	-3	-2	-5
	Predictions					
	Total hours worked (% change)					
Total	-12	-1	-7	-2	-2	-7
Skilled	-10	-1	-7	-2	-2	-6
Unskilled	-19	-1	-9	-3	-2	-11
Young	-14	-1	0	-2	-1	-8
Middle-age	-11	-1	-8	-2	-2	-7
Old	-14	-1	-9	-3	-2	-9
Full-time	-12	-1	-7	-2	-2	-7
Nonstandard	-12	-1	-11	-3	-1	-8

Sources: Value added from the German National Accounts (constant prices, chain-linked index, 2000 = 100). Official employment statistics from the Institute for Employment Research. Predictions are based on the LIAB. *Notes:* Man = Manufacturing, Con = Construction, Tra = Transport & Communications, Ser = Services, Fin = Financial Services.

The bottom panel of Table 6.4.1 shows changes in total hours worked across industries and for different groups of workers as predicted by the labor-demand model. For the prediction we multiplied reported industry output changes with the corresponding output elasticities of labor demand in each of the 60 cells. As we have chosen a "total hours" specification, our predictions are conceptually comparable to the official changes in total working hours shown in the top part of the table. Our predictions capture those overall changes well—both quantitatively and qualitatively. This match is reassuring in terms of the external validity of the estimated model and validates our implicit assumption that past elasticities provide a good approximation of present labor market responses. The correspondence between predicted and observed working-hours changes is also an important finding: it suggests that, despite its magnitude, the downturn in Germany has not resulted in a structural break of firm behavior. Only in the transport and communications sector do we overestimate the labor-demand reaction (possibly explained in part by stimulus spending benefiting this sector). Moreover, the table suggests that different types of workers are affected differently, with old, unskilled and non-standard workers suffering the most.

6.4.2 Cell identification and shock scenarios

We now feed the predicted employment shocks for each cell into the SOEP, a representative micro dataset often used for distributional analyses. The SOEP is informationally rich and allows us to differentiate by skill, age, employment group and industry, just as we did in the linked employer-employee data. Table 6.4.2 provides an overview of selected worker characteristics for both the LIAB and SOEP datasets. The table reveals that although general socio-demographic characteristics such as gender or nationality differ, the two datasets compare well as far as the dimensions of our cells are concerned. In particular, the age and employment-type distributions are almost identical.

The labor-demand model is specified in terms of total hours and hence accounts for adjustments at both the extensive and intensive margin. Yet the model cannot predict which margin is used by a particular firm or sector. Thus, we must suggest concrete scenarios of labor market adjustments to translate total hour changes into income changes at the cell level. Since actual labor-input adjustments during the 2008-09 crisis were mainly along the intensive margin in Germany, we first suggest a scenario where adjustments exclusively materialize as a change in worked hours (e.g., a switch from full-time to part-time employment). We simply change working hours proportionally in line with the total change in labor demand at the cell level, holding employment levels constant.

Table 6.4.2: Worker characteristics, wave 2007

General	LIAB	SOEP
Observations (persons)	1,828,126	9,218
Share of women	38.3	44.4
Share of foreigners	5.4	16.0
Share of working in East	20.6	16.2
Skill distribution		
Share of skilled	85.9	91.0
Share of unskilled	14.1	9.0
Age distribution		
Share of young	17.9	18.4
Share of medium-aged	67	68.2
Share of old	15.1	13.4
Mean age	41.8	41.6
Job distribution		
Share of full-timers	73.4	72.9
Share of nonstandard employees	26.6	27.1

Source: Own calculations using the LIAB and the SOEP.

In a second polar case we suggest a scenario where the same total hours adjustments only occur at the extensive margin through layoffs. That is, adjustments consist in changes in employment rates at the cell level. If the predicted change in labor demand for a given cell is $-X\%$, we randomly draw $X\%$ of workers within the SOEP cell and make them unemployed. This second scenario is closer to the adjustment pattern seen in countries where layoffs were more important than changes in average hours worked.

We feel that these two scenarios provide interesting counterfactuals for the distributional and fiscal impact of the labor market downturn, which highlight the role of the adjustment margin in shaping distributional outcomes and correspond reasonably well to the adjustment patterns observed in Germany and the United States. It is likely, however, that the differences between the distributional effects of our stylized scenarios provide upper bound estimates. First, adjustments will generally take place along both the intensive and the extensive margins. On a more technical level, we abstract from the facts that working-hours reductions will not be uniform within each cell and that unemployment risks within cells will not be evenly distributed. However, in the context of our distributional analysis, the random draw will have no noticeable impact as the cells are already disaggregated.[8]

6.4.3 Distributional and fiscal impacts

The distributional analysis is based on SOEP data before and after the two scenarios of labor-demand adjustment. We denote by "Base" the pre-crisis (baseline) situation, by "Intensive" the post-crisis scenario resulting from adjustments along the intensive margin only, and by "Extensive" the post-crisis scenario resulting from extensive-margin adjustments. Income distribution measures are based on household total income equivalized using the "modified OECD" scale. Capturing the household context (family size and composition) is of course a principal reason for performing the distributional analysis on SOEP-type data rather than using the worker-based LIAB directly.

Income and hours changes. We examine the distributions of both gross and net incomes in order to capture the cushioning effect of the tax-benefit system. We assume policy parameters as of January 1, 2009.[9] Table 6.4.3 shows large working-hours changes for workers in the manufacturing industry mirroring the predicted labor-input adjustments reported in Table 6.4.1.[10] Gross earnings follow changes in total working hours. They are not the same, however, since working hours are shown at the individual level, whereas incomes are measured on an "equivalized" household basis and, hence,

[8] For instance, in the case of the extensive scenario, any non-random modeling attempt would, in fact, run into difficulties, as it would have to utilize characteristics (such as age, education) that are similar to the ones used to distinguish cells. Also note that some intermediary scenarios based on more realistic combinations of the intensive and extensive margins could be suggested but would require additional assumptions. We keep this work for future research.

[9] It is important to note that net income calculations do not account for benefits (*Kurzarbeitergeld*) paid through the short-time working program (*Kurzarbeit*), as our data do not allow us to identify the likely recipients of these benefits. This is relevant when considering the distributional effects reported for the "intensive"scenario below. While this provides a lower-bound for the incomes of many of the workers affected by reduced working hours, recall that the large majority of working-hour reductions in 2009 (75 percent) were not on account of *Kurzarbeit*.

[10] Note that because the sampling frames for the SOEP and LIAB data are different and predictions from the demand model have been applied cell-by-cell to the SOEP, total working-hour changes by industry do not match exactly.

are also affected by the incomes of other family members. This is also why incomes can change for the non-employed and why relative changes in (household) earnings can exceed changes in (individual) working-hours reductions. Across industries it is, in particular, unskilled workers who are found to suffer the greatest earnings losses. The net incomes of young individuals also decline sharply. Average losses of the young are even larger than for the older age-group, despite the earlier finding in Table 6.4.1 that older workers are somewhat more likely to face job losses or working-time reductions than young workers. One reason is that older workers are more likely to be living with a partner whose income partly shields them from a drop in household incomes.

Table 6.4.3: Relative changes in earnings and hours – by socio-economic group

	Intensive			Extensive		
	Gross	Net	Hours	Gross	Net	Hours
Skilled	-3.6	-2.4	-3.3	-3.6	-2.2	-3.3
Unskilled	-6.6	-3.4	-6.3	-6.6	-2.7	-6.3
Young	-3.6	-2.7	-3.6	-3.6	-2.4	-3.6
Middle-age	-3.8	-2.2	-3.0	-3.9	-2.0	-3.1
Old	-3.5	-1.8	-3.3	-3.6	-1.7	-3.4
Full-time	-3.6	-2.6	-3.4	-3.6	-2.3	-3.4
Nonstandard	-4.7	-2.7	-4.6	-4.7	-2.4	-4.6
Nonemployed	-4.3	-1.7	.	-4.2	-1.2	.
Manufacturing	-9.4	-7.0	-11.2	-9.2	-6.2	-11.2
Construction	-1.3	-0.9	-0.7	-1.3	-0.8	-0.8
Transport-Comm	-6.3	-4.2	-7.0	-6.4	-3.8	-7.1
Services	-2.8	-1.8	-2.2	-3.0	-1.8	-2.3
Financial	-2.4	-1.8	-1.5	-2.4	-1.5	-1.5
Total	**-3.7**	**-2.5**	**-3.5**	**-3.7**	**-2.2**	**-3.5**

Source: Own calculations using the SOEP and IZAΨMOD. *Notes:* Incomes are equivalized (modified OECD scale), working hours are shown on an individual basis. All changes in %. Sample: working-age individuals and household members.

It is striking that the net income effects are more sizeable in the intensive scenario. This is because hours in the intensive scenario are equally reduced for everybody who is working in a specific cell. Hence, every worker in this cell suffers an equal, but relatively small, income loss. Tax burdens also decline for these workers, which is why income losses are smaller on a net basis than before taxes. In the extensive scenario certain workers are laid off—resulting in a sharp drop of their gross income. On top of reduced tax burdens, a considerable part of the earnings loss tends to be offset by an entitlement to unemployment benefits. Consequently, the income cushioning effect of the tax-benefit system is larger than under the intensive scenario, and the difference between net and gross income changes is more sizeable as a result. Note that these effects also operate for non-employed individuals, who can be sharing a household with job losers entitled to unemployment benefits.

Comparing changes in gross and net income gives some indication of the effectiveness of social safety nets at absorbing some of the income losses. The income of low-skilled workers is likely to be relatively close to the level of minimum-income benefits. Safety-net benefits, therefore, absorb a large part of their earnings losses on average resulting in large differences between gross and net earnings changes. Reflecting the 400/800 euro ceiling on monthly earnings in the German *Mini/Midijob* program, the wages of many workers in the "nonstandard" category are also especially low. However, these jobs are particularly attractive for secondary earners. Because of their higher-earning partners, they are then less likely to receive means-tested benefits when losing all or part of their own earnings.

Income distribution. Table 6.4.4 presents changes of incomes and working hours by decile groups. Interestingly, relative net income losses in the "intensive" scenario are very similar from deciles 4 to 10. Perhaps even more strikingly, the lowest two decile groups experience the smallest net income changes—showing the effectiveness of the benefit system. A somewhat similar picture emerges if labor-demand adjustments take place entirely through layoffs. Once again, net income losses tend to be less severe than in the intensive scenario. This is not the case, however, for the first two decile groups. The reason is that those at the bottom of the income distribution tend to be entitled to means-tested benefits, which ensure that their net incomes change very little in both the intensive and extensive scenarios. As a result, whether or not those affected by earnings losses are entitled to unemployment benefits makes little difference, and net income changes for the two scenarios are more similar for the bottom two deciles than for middle-class households.

Table 6.4.4: Relative changes in earnings and hours – by income decile

	Intensive			Extensive		
	Gross	Net	Hours	Gross	Net	Hours
1	-3.7	-0.3	-3.2	-4.1	-0.6	-3.1
2	-3.8	-0.6	-3.8	-3.8	-0.7	-3.7
3	-3.9	-2.0	-3.8	-3.7	-1.2	-3.6
4	-3.8	-2.7	-3.5	-3.7	-1.7	-3.5
5	-3.8	-2.9	-3.5	-4.0	-2.2	-3.7
6	-4.3	-3.0	-3.9	-4.2	-2.5	-3.8
7	-3.6	-2.6	-3.5	-3.7	-2.5	-3.7
8	-3.7	-2.8	-3.4	-3.6	-2.6	-3.4
9	-3.4	-2.5	-3.1	-3.2	-2.3	-3.1
10	-3.8	-2.5	-3.3	-3.9	-2.5	-3.4
Total	**-3.7**	**-2.5**	**-3.5**	**-3.7**	**-2.2**	**-3.5**

Source: Own calculations using the SOEP and IZAΨMOD. *Notes:* Incomes are equivalized (modified OECD scale), working hours are shown on an individual basis. All changes in %. Decile groups are for the selected sample only (working-age individuals and household members) and are based on the "pre-crisis" baseline.

Distributional measures. Table 6.4.5 reports a range of global distribution measures (Gini, General Entropy, inter-decile ratio), as well as absolute and relative poverty headcount (Foster-Greer-Thorbecke: FGT0) and poverty intensity (FGT1, FGT2) indicators. As customary the poverty line is defined as 60 percent of median income. Consistent with the results by income deciles, overall inequality is reduced in the "intensive" scenario. The income distribution is compressed, as parts of the working population suffer income losses, while the net incomes of the non-employed change less. In the "extensive" scenario, however, inequality rises, as some workers are laid off while others are not affected by the crisis at all. Because the incidence of job losses is particularly high for groups which tend to have low incomes even prior to unemployment (e.g., young and low-skilled workers), this additional unemployment yields a further dispersion of the income distribution. The difference between the inequality measures in the two scenarios illustrates that facilitating working-hours adjustments can play an important role in limiting the growth of income disparities during a downturn.

This can also be seen when looking at poverty measures. In the intensive scenario the share of the poor as indicated by the headcount ratio using a constant poverty line

(FGT0) increases only slightly, while we see a substantial rise of more than 10 percent in the extensive case. Other poverty indicators arrive at quantitatively similar results. But interestingly, with a variable poverty line (FGT0v), the number of poor in the intensive scenario actually goes down, since median income (and hence the poverty threshold) drops more strongly than incomes at the very bottom of the distribution. These results underline the importance of evaluating relative poverty measures alongside absolute changes in income levels—especially when assessing the distributional consequences of rapid economic changes.

Table 6.4.5: Inequality and poverty measures

	Base	Intensive		Extensive	
	Net	Net	Δ (in%)	Net	Δ (in%)
Gini	0.324	0.323	-0.385	0.330	1.637
GE0	0.176	0.174	-1.193	0.181	2.972
GE1	0.197	0.197	-0.161	0.203	3.079
P9010	4.251	4.175	-1.807	4.307	1.304
FGT0	0.205	0.213	3.588	0.229	11.653
FGT1	0.048	0.050	2.142	0.054	12.388
FGT2	0.019	0.020	4.289	0.023	19.085
FGT0v	0.205	0.195	-5.067	0.214	4.516

Source: Own calculations using the SOEP and IZAΨMOD. *Notes:* Measures are based on equivalized disposable incomes (modified OECD scale) and refer to the selected sample only (working-age individuals and household members). The poverty line is set at 60 percent of median income (of the total population) and is either constant, using the baseline median (FGT0, FGT1, FGT2), or varies, using the median of each scenario (FGT0v).

Fiscal effects. Finally, we shed some light on the role of the margin of adjustment for government budgets. Table 6.4.6 shows the fiscal effects of the two scenarios relative to the baseline case, i.e., the German tax-benefit system as of January 1, 2009, without any crisis-related employment changes. As one would expect, both scenarios result in a highly negative effect on the government budget. Tax revenue and social insurance contributions (SIC) decrease, as labor earnings drop for those employees affected by the crisis. It is interesting to note the differences between the two scenarios in terms of taxes and SIC. In the intensive case the proportional hours reduction in combination with the progressive income taxation and regressive SIC yields higher relative tax revenue reductions. In the extensive scenario employment reductions are highest in the middle part of the income distribution (cf. Table 6.4.4), where SIC payments are higher than tax liabilities. As the highly progressive German income tax is concentrated at the top (with the top 10 percent paying more than 55 percent of the income tax revenue), the reduction in tax revenue is relatively lower than the decrease in SIC. Due to higher benefit expenditures, the fiscal consequences of the extensive scenario are, however, substantially more severe (benefit payments increase by 6 percent). In total, the government's budget decreases by 7 percent in this case. This yields an eventual shortfall which is approximately 3 billion euros higher than in the intensive scenario, given our considered population sample.[11]

Table 6.4.6: Fiscal effects

	Intensive		Extensive	
Changes	in billion euros	in %	in billion euros	in %
Tax revenue	-5.6	-4.2	-3.0	-2.3
Social insurance contributions	-5.4	-3.2	-6.3	-3.8
Benefit payments	-1.0	1.1	-5.3	5.9
Total budget effect	-11.9	-5.7	-14.6	-7.0

Source: Own calculations using the SOEP and IZAΨMOD. *Notes:* Percentage changes refer to each category (ex: tax revenue goes down by 4.2 percent in intensive scenario)

Discussion. A principal result of the analysis is that sharing earnings losses in a downturn among larger groups of workers can produce less inequality—and lower immediate fiscal costs—than widespread layoffs. In general, the distributional advantages of achieving capacity adjustments through working-hours reductions, rather than layoffs, are greater in countries with lower automatic stabilizers, i.e., with less generous unemployment benefits and lower average tax burdens than Germany. In this respect, our results confirm the wide held view that distributional effects of the US labor mar-

[11] In a back-of-the-envelope calculation one could argue that the German short-term working scheme was an efficient investment for the initial phase of the crisis—costing a similar amount (3 billion euros per year)—encouraging reductions in total working hours and thus keeping many employees in the workforce.

ket adjustments – massive layoffs occurring under a much less redistributive tax-benefit system – have been particularly severe.

Yet the question remains whether countries should adopt a strategy of working-time reductions to minimize layoffs. The answer depends largely on the nature of the labor market downturn and on the specific initial conditions (such as the structure of the economy or labor market institutions) in each country. In Germany conditions for working-hours reductions were, in many respects, ideal. First, the greatest output losses were suffered in the export-oriented manufacturing industry. Firms in this sector had both the motivation and the financial resources to retain valuable skilled workers during a temporary period of severely reduced output demand. Second, the output shock was indeed temporary: external demand for German manufacturing goods recovered; and the jobs of workers with reduced hours therefore remained viable after the downturn. Third, and as discussed in Section 6.2, policy developments prior to the downturn (especially working-time accounts and specific provisions in collective agreements), as well as policy responses to the crisis (e.g., the short-time working scheme, *Kurzarbeit*), had strongly facilitated working-hours adjustments.

On the other hand, these types of job protecting measures tend to reinforce employer incentives to hoard highly educated or experienced workers, while less attractive jobs may be cut more quickly (Hijzen and Venn, 2011; Cahuc and Carcillo, 2011). In other words, working-hours adjustments may in fact worsen the relative position of poorly protected low-skilled and non-standard workers, who were shown to be particularly likely to suffer earnings losses in a downturn. This is likely to be a concern in highly segmented labor markets, e.g., in Spain (but also in Germany, where non-standard forms of employment have become more common).

The specificity of the output shock also has to be borne in mind, when assessing the merits of exporting certain national policies to other countries. If the sectoral incidence of output shock is different, labor hoarding might be much less beneficial for firms, and hence less widespread as a result. If, for instance, firms in the affected sector are severely credit-constrained, they may have little choice but to lay off workers. More importantly, lower output demand may not be temporary as recessions are frequently accompanied by structural changes. Policies that actively encourage firms to delay layoffs in these cases can be an obstacle to a necessary restructuring process and, hence, hold back economic recovery.

6.5 Conclusion

In this paper we analyze the distributional and fiscal impact of the 2008-09 crisis in Germany. We base our analysis on a disaggregated labor-demand model, which is justified by the fact that labor-demand changes are the principal driving factor of household income losses in the early phase of a labor market downturn. The predicted adjustments are then combined with detailed household microdata to translate changes in individual earnings into income changes at the household level. Thus, the method can be used before detailed income data become available and can therefore aid timely policy responses to output shocks.

The choice of Germany is interesting, since, on the one hand, it suffered from a severe output drop, which translated into a substantial labor market downturn—like many other Western countries. However, on the other hand, the adjustments occurred almost exclusively at the intensive margin, with employment levels and unemployment rates remaining unusually stable. This reflects in part Germany's policy measures before and during the crisis—facilitating labor-cost adjustments via working-hours reductions rather than layoffs.

Our labor-demand model is flexible enough to capture the real-world demand reactions following the German recession well. At the same time, the approach enables us to assess the distributional and fiscal consequences—in particular with respect to the margin of adjustment. More precisely, we propose two polar cases to assess the importance of the different margins. The first scenario, close to the German experience, assumes that all employment adjustments take place via such working-hour reductions. The second one better reflects the situation in countries such as the United States, Greece and Spain, where adjustments of employment levels were far greater.

Our results show that low-skilled and non-standard workers faced above-average risks of earnings losses, in particular if they worked in the manufacturing sector where output reductions were very large. When examining the resulting income losses, it transpires, however, that automatic stabilization by the tax-benefit system is effective in cushioning a significant share of the gross-income losses—especially among low-income groups (cf. also Dolls et al., 2012). As far as the margin of adjustment is concerned, we show that while promoting working-hour adjustments through work-sharing and other measures cannot prevent significant income losses, it can be highly effective in avoiding very large increases in income poverty and fiscal costs. In those two dimensions the German policy responses to the crisis were successful.

Nevertheless, the conditions for working-hours reductions in Germany were ideal, as the output drop mostly occurred in the export-oriented sectors, where motivation to hoard skilled labor was high and firms had the necessary financial resources to do so. We, therefore, argue that whether the German policy can be successful in other countries crucially depends on initial conditions (especially the structure of the economy and labor market institutions) as well as the specificities of the output shocks.

From a methodological point of view, we use recent historical data to make inference about the effects of the current labor market downturn. The demand model provides an interesting "average" approximation of short-term effects of output shocks. Yet institutional changes over recent years may have affected the demand for different groups of workers in complicated ways, and the policies put in place during the crisis had their own specific effects. Hence, an important, but challenging, improvement would consist in explicitly modeling policy institutions (such as *Kurzarbeit*) in the labor-demand estimation. Another obvious limitation is that the adopted short-term horizon goes along with the assumption of constant wage levels. Although it would be worthwhile to model wage variations by interacting labor demand and supply iteratively in order to attain equilibrium (see Chapter 2 and Peichl and Siegloch, 2012), we have argued that this assumption is not too restrictive in the context of our study, as wage reductions were not a primary response to the labor market downturn in Germany.

6.6 Appendix

Table 6.6.1: Output elasticities per cell

Cell values	Man	Con	Tra	Ser	Fin
Sk/You/FT	0.67	0.42	-0.09	0.63	0.88
Sk/You/NS	0.96	-0.29	0.78	0.94	0.76
Sk/Mid/FT	0.53	0.45	0.85	0.52	0.96
Sk/Mid/NS	0.50	2.10	1.21	0.97	0.95
Sk/Old/FT	0.77	0.40	0.99	0.98	0.93
Sk/Old/NS	0.62	0.29	2.22	1.00	0.97
USk/You/FT	0.95	1.17	-0.20	0.99	1.10
USk/You/NS	0.99	0.89	0.95	0.96	0.95
USk/Mid/FT	1.15	-0.35	1.30	0.99	1.04
USk/Mid/NS	0.41	-0.32	1.26	0.99	1.00
USk/Old/FT	0.89	3.09	0.74	1.04	1.00
USk/Old/NS	0.25	0.36	-0.33	0.99	0.96

Source: Own calculations using the LIAB. *Notes:* (U)Sk = (Un)skilled, You=Young, Mid= Middle-age, FT = full-time, NS = Nonstandard. Man = Manufacturing, Con = Construction, Tra = Transport & Communications, Ser = Services, Fin = Financial Services.

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Sebastian Siegloch - Curriculum Vitae

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Personal Details

Born	December, 31 1982 in Bonn (Germany)
Citizenship	German
Languages	German: native; English: fluent; French: fluent

Professional career

Employment

Since 01/2013	**Research Associate** Institute for the Study of Labor (IZA), Bonn, Germany
10/2009 – 12/2012	**Resident Research Affiliate** Institute for the Study of Labor (IZA), Bonn, Germany
03/2009 – 09/2009	**Student Research Assistant** Institute for the Study of Labor (IZA), Bonn, Germany
04/2008 – 08/2008	**Student Research Assistant** Center for Public Economics, University of Cologne (Chair Prof. Dr. C. Fuest) and FiFo Institute for Public Economics), Germany
04/2007 – 02/2008	**Student Teaching Assistant** Department of Political Economy, University of Cologne (Chair Prof. Dr. P. Funk), Germany

Research visits

06/2012 – 07/2012	Oxford University, Centre for Business Taxation, UK

Education

Since 10/2009	**PhD Candidate** University of Cologne (Germany), Supervisor: Prof. Dr. Clemens Fuest
04/2006 – 09/2009	**Diplom-Volkswirt** (equiv. M.A. Economics), Grade: 1.3 (best degree) University of Cologne (Germany)
09/2008 – 03/2009	**Graduate Studies in International Politics** Institute d'Etudes Politiques (Sciences Po), Paris (France)
10/2004 – 03/2006	**Vordiplom** (equiv. B.A. Economics), Grade: 1.5 University of Cologne (Germany)
10/2003 – 09/2007	**Fachjournalist** (equiv. B.A. Journalism) Cologne School of Political and Economic Journalism, Germany

Publications

A. Refereed International Journals

201x

Comparing Inequality Aversion across Countries When Labor Supply Responses Differ, *International Tax and Public Finance*, forthcoming, DOI: 10.1007/s10797-013-9277-9 (with O. Bargain, M. Dolls, D. Neumann and A. Peichl)

Fiscal Union in Europe? Redistributive and Stabilising Effects of a European Tax-Benefit System and Fiscal Equalisation Mechanism, *Economic Policy*, forthcoming, (with O. Bargain, M. Dolls, C. Fuest, D. Neumann, A. Peichl, and N. Pestel)

	Welfare, Labor Supply and Heterogeneous Preferences: Evidence for Europe and the US, *Social Choice and Welfare*, forthcoming (with O. Bargain, A. Decoster, M. Dolls, D. Neumann, and A. Peichl)
	The Politician's Wage Gap - Insights from German Members of Parliament, *Public Choice*, forthcoming DOI: 10.1007/s11127-012-9921-4 (with A. Peichl and N. Pestel)
2012	Distributional Consequences of Labor-demand Shocks: The 2008-09 Recession in Germany, *International Tax and Public Finance*, Vol. 19(1), 118-138 (with O. Bargain, H. Immervoll and A. Peichl)
	Accounting for Labor Demand Effects in Structural Labor Supply Models, *Labour Economics*, Vol. 19(1), 129-138 (with A. Peichl)

B. Refereed German Journals

2013	Ist Deutschland wirklich so progressiv? Einkommensumverteilung im europäischen Vergleich, *Vierteljahrshefte zur Wirtschaftsforschung*, Vol. 82(1), 111-127 (with A. Peichl and N. Pestel)
2012	Effizient, einfach und gerecht: Ein integriertes System zur Reform von Einkommensteuer und Sozialabgaben, *Perspektiven der Wirtschaftspolitik*, Vol. 13(3), 196-213 (with M. Löffler, A. Peichl, N. Pestel and H. Schneider).
2011	Einfach ist nicht immer gerecht: Eine Mikrosimulationsstudie der Kirchhof-Reform für die Einkommensteuer, *Vierteljahrshefte zur Wirtschaftsforschung*, Vol. 80(4), 147-160 (with M. Löffler, A. Peichl, N. Pestel and H. Schneider)
	Reform der Hartz-IV-Hinzuverdienstregelungen: Ein verfehlter Ansatz, *Perspektiven der Wirtschaftspolitik*, Vol. 12(1), 12-26 (with A. Peichl, N. Pestel and H. Schneider)

C. Policy Contributions and Non-Refereed Journals

2011	Bemessungsgrundlage kontra Fünf-Stufen-Tarif: Eine Simulationsanalyse des Reformvorschlags nach Rose, *Wirtschaftsdienst*, Vol. 91(5), 328-332 (with A. Peichl, N. Pestel and H. Schneider)
	Alter Wein in neuen Schläuchen: Der Fünf-Stufen-Steuertarif der FDP auf dem Prüfstein, IZA Standpunkte No. 27 (with A. Peichl, N. Pestel and H. Schneider).
	Reform der Hinzuverdienstregeln in der Grundsicherung: Kein Entkommen aus der Transferfalle, IZA Standpunkte No. 26 (with A. Peichl and H. Schneider)
2009	Die Steuerreformpläne der neuen Bundesregierung und das Bürgergeld: Eine Simulation von Risiken und Nebenwirkungen, *Wirtschaftsdienst*, Vol. 89(12), 805-812 (with D. Neumann, A. Peichl and H. Schneider)

D. Papers in the Editorial Process

Happy Taxpayers? Income Taxation and Well-Being (with A. Akay, O. Bargain, M. Dolls, D. Neumann and A. Peichl), IZA Discussion Paper No. 6999

Partisan Tax Policy and Income Inequality in the U.S., 1979-2007 (with O. Bargain, M. Dolls, H. Immervoll, D. Neumann, A. Peichl and N. Pestel), IZA Discussion Paper No. 7190

Do Higher Corporate Taxes Reduce Wages? Micro Evidence from Germany (with C. Fuest and A. Peichl), IZA Discussion Paper No. 7390

E. Work in Progress

Employment Effects of Local Corporate Taxes

Is soccer good for you? The motivational impact of big sport events on the unemployed (with P. Dörrenberg)

Wage Bargaining and Profit Shifting in Multinational Firms (with M. Simmler)

Local Tax Competition and Firm Location (with F. Weinhardt)

Productivity vs. Signaling: the labor market returns from schooling (with A. Lichter, A.Peichl and G. Pfann)

Exporting and Heterogeneous Labor Demand: Micro-level evidence from Germany (with A. Lichter and A. Peichl)

Labor productivity, labor supply and air quality (with A. Peichl und N. Pestel)

The implications of firms' reactions to demand shocks for workers (with A. Hijzen, A. Peichl and Z. Wolf)

F. Simulation Models

Since 2009	Development of IZAΨMOD: Behavioral microsimulation, labor supply and labor demand model. See "Documentation IZAΨMOD: The IZA Policy SImulation MODel", IZA Discussion Paper No. 4865 (with A. Peichl and H. Schneider)
	User of EUROMOD. (See http://www.iser.essex.ac.uk/research/euromod)

Selected Research Projects and Grants

2012	"Geringfügige Beschäftigung: Situation und Gestaltungsoptionen" (with W. Eichhorst, T. Hinz, P. Marx, A. Peichl, N. Pestel, E. Thode and V. Tobsch on behalf of the Bertelsmann Foundation), IZA Research Report No. 47
	"OECD Employment Outlook 2012, Chapter 2.3: What Makes Labour Markets Resilient? Microeconomic analysis of the role of structural policies and institutions for labour market resilience" (with A. Peichl on behalf of the OECD)
2011	"Finanzielle und Beschäftigungswirkungen unterschiedlicher Tarifverläufe alternativer Hinzuverdienstregelungen gegenüber dem Status Quo" (with M. Löffler, A. Peichl, N. Pestel and H. Schneider on behalf of the German Federal Ministry of Economics and Technology), IZA Research Report No. 46
	"Kurzexpertise zur Aktivierung von Fachkräftepotenzialen - Frauen und Mütter" (with W. Eichhorst, M. Kendzia, A. Peichl, N. Pestel and V. Tobsch on behalf of the German Federal Ministry for Labour and Social Affairs), IZA Research Report No. 39
2010	"Changes in the redistributive capacity of tax-benefit systems: Simulation-based indicators" (IZA on behalf of OECD)
	"Gutachten zur Berechnung von Vorschlägen zur Neuregelung der Erwerbstätigenfreibeträge" (with A. Peichl, N. Pestel and H. Schneider on behalf of the German Federal Ministry of Labour and Social Affairs), IZA Research Report No. 32

Presentations

Conferences and Workshops

2013	7th Annual Symposium of the Centre for Business Taxation at Oxford University, Oxford, Germany.
2012	68th Congress of the International Institute of Public Finance (IIPF), Dresden, Germany; European Meeting of the International Microsimulation Association, Dublin, Ireland; 17th Annual Meeting of the Society of Labor Economists, Chicago, USA.
2011	67th Congress of the International Institute of Public Finance (IIPF), Ann Arbor, USA; IZA Workshop: Unemployment Insurance in the Recession, Bonn, Germany; 3rd General Conference of the International Microsimulation Association, Stockholm, Sweden; 16th Annual Meeting of the Society of Labor Economists, Vancouver, Canada.
2010	ECB/CEPR Labour Market Workshop "Unemployment Developments After the Crisis", Frankfurt, Germany; ZEW Workshop "Recent Developments in Behavioural Microsimulation", Mannheim, Germany; 1st Essex Microsimulation Workshop, Colchester, UK; 66th Congress of the International Institute of Public Finance (IIPF), Uppsala, Sweden; OFCE Workshop "Public Finances After the Crisis", Paris, France; 3rd Doctoral Meeting of Montpellier, Montpellier, France; IZA/OECD Workshop "Economic Crisis, Rising Unemployment and Policy Responses: What Does It Mean for the Income Distribution?", Paris, France.

Invited Seminars

2011	University of Dortmund, Germany.

Awards and Scholarships

06/2013	**Young Scholar's Prize for the best paper in business taxation**, Annual Symposium of the Centre of Business Taxation at the University of Oxford
10/2009 – 09/2012	**IZA Scholarship for Doctoral Students** Institute for the Study of Labor (IZA), Bonn, Germany
09/2008 – 02/2009	**Socrates/Erasmus Scholarship for Studying Abroad** European Commission, Brussels, Belgium
06/2006 – 09/2009	**Scholarship of the German National Academic Foundation** German National Academic Foundation (Studienstiftung des deutschen Volkes), Bonn, Germany
09/2009	**Best Diploma in Economics** University of Cologne, Germany
03/2006	**Best Undergraduate Degree in Economics** University of Cologne, Germany

Other Research Activities

Refereeing	International Tax and Public Finance, Labour
Memberships	International Institute of Public Finance (IIPF), The Society of Labor Economists (SOLE), European Economic Association (EEA)

Courses and Summer Schools

04/2012	"15th IZA European Summer School in Labor Economics" by Andrew Oswald and Rainer Winkelmann, Buch/Ammersee, Germany
01/2012	"7th Winter School on Inequality and Social Welfare Theory", University of Verona, Canazei, Italy
05/2011	"Topics in Econometrics and Statistics: Microeconometric Analysis" by Frank Vella, Bonn Graduate School of Economics, University Bonn, Germany
02/2011	"Advanced Microeconomics" by Oliver Gürtler, Cologne Graduate School of Economics, University of Cologne, Germany
01/2011	"6th Winter School on Inequality and Social Welfare Theory", University of Verona, Canazei, Italy
12/2010	"Program Evaluation" by Marco Caliendo, Cologne Graduate School of Economics, University of Cologne, Germany
07/2010	"Panel Data Linear Analysis" by Badi Baltagi, Barcelona Graduate School of Economics, Microeconometrics Summer School, Spain
	"Dynamic and Non-Linear Panel Data Models" by Sergi Jiménez, Barcelona Graduate School of Economics, Microeconometrics Summer School, Spain
	"Policy Evaluation" by Pedro Carneiro, Barcelona Graduate School of Economics, Microeconometrics Summer School, Spain
06/2010	"Econometric Methods for Demand Systems in Economics" by Melvyn Weeks, Queen Mary College, London, UK
02/2010	"Advanced Microeconometrics" by David A. Jaeger, Cologne Graduate School of Economics, University of Cologne, Germany
	"Time Series Analysis" by Karl Mosler, Cologne Graduate School of Economics, University of Cologne, Germany
	"Multivariate Methods" by Oliver Grothe, Cologne Graduate School of Economics, University of Cologne, Germany
01/2010	"Training Course: Microsimulation" by Alan Duncan, Institute for Fiscal Studies (IFS) / University College of London, London, UK
09/2009	"Statistical Analysis with Stata/Mata" by David M. Drukker, IZA, Bonn, Germany
07/2009	"Treatment Effect Estimation and Selection Models" by Jeffrey F. Wooldridge, Bonn Graduate School of Economics and IZA, Bonn, Germany
07/2008	"Game Theory II" by Achim Wambach, Cologne Graduate School of Economics, University of Cologne, Germany

Teaching

2013	Research Methods in Public Economics, PhD / master course, University of Cologne (with A. Peichl)
2008	Seminar "Macroeconomics", Cologne School of Political and Economic Journalism
	Tutorial "Introduction to Macroeconomics", Department of Economics and Social Sciences, University of Cologne (Prof. Dr. P. Funk)
2007	Tutorial "Introduction to Macroeconomics", Department of Economics and Social Sciences, University of Cologne (Prof. Dr. P. Funk)

Supervision

Since 2010	Supervision of several master theses in Public and Labor Economics, University of Cologne.

Media Appearance

05/13/2013	"Gewerbesteuer trifft Mitarbeiter der Unternehmen", *Frankfurter Allgemeine Zeitung*
05/10/2013	"Vom Glück höherer Steuern", *Frankfurter Allgemeine Zeitung*
12/19/2012	"Research Suggests People Who Pay More Taxes Are Happier", *Wall Street Journal Blog*
04/04/2011	"Sind Deutschlands Politiker überbezahlt?", *Handelsblatt*
02/20/2011	"Politiker verdienen viel mehr als Manager", *Welt am Sonntag*
10/04/2010	"Deutschland hui, Amerika pfui", *Frankfurter Rundschau*
	"Vorbild in der Krise", *Berliner Zeitung*
12/11/2009	"Ein Geschenk für Reiche", *Süddeutsche Zeitung*

Non-scientific Professional Experiences

02/2007 – 09/2009	**Free lance contributor** Welt am Sonntag, in charge of economic book reviews, Berlin, Germany
10/2003 – 09/2007	**Several journalistic internships** among others at Focus in Berlin, Die Welt in Frankfurt, Federal Press Office in Berlin, Spiegel Online in Hamburg
06/2003 – 01/2007	**Project assistant** abc consultants gmbh, Bonn, Germany

Bonn, June 28, 2013