UNIVERSITÄT
HEIDELBERG
ZUKUNFT
SEIT 1386

Essays on Labor Economics

Inauguraldissertation zur Erlangung des Doktorgrades
des Fachbereichs Wirtschaftswissenschaften der
Ruprecht-Karls-Universität Heidelberg

vorgelegt von
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Heidelberg
März 2019

Acknowledgements

First and foremost I would like to thank my advisor Christina Gathmann for her guidance and great support throughout the last 5 years. I am very thankful for her many ideas and inspirations, from which my work has benefited a lot. I greatly appreciate her focus on clarity and a careful combination of theory and empirical work, which was a brilliant role model for my own work. Furthermore, I also thank Melanie Arntz for being the second referee on the dissertation committee.

I further thank the Department of Economics at Heidelberg University and especially my colleagues at the chair of labor economics. I thank them for their good company and for the pleasant and friendly working environment. Special thanks are due to Sabine Morawa, whose authority in non-academic matters is beyond question and who taught me many valuable life lessons. Besides, I am very grateful for my fellow doctoral students at the chair of labor economics. My work has benefited greatly from the many stimulating discussions with them and their insightful comments. I also thank them for the great experiences I made with them. Both Anna Hautmann and Anna Busse were exceptionally great office mates. Behind a wall of screens, I have always had great colleagues, who shaped the friendly atmosphere in my office. I also thank the guys in the neighboring office for listening to my new thoughts every time I tried a new research idea. They also diligently endured my regular outbursts of joy or sadness, depending on how good the respective idea was. Ole started his doctoral studies together with me and became a trusted friend in the last 5 years. Matthias has withstood a tough 3-judge trial to become part of this wonderful team and has been a fantastic addition ever since. Furthermore I thank Franziska, who helped me a lot to get along at the HdBA.

Finally, I would like to thank my family, my friends and above all Susanne for their unconditional support, encouragement and love, without which I would not have come this far.

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Chapter 1

Introduction

Work is the primary activity in most people's adult lives. Not surprisingly, work-related issues are discussed intensively in the public debate. This is particularly evident in labor market policies, which have a far-reaching impact on the lives of a large majority of people.

How labor markets react to technological change, demographic shifts or institutional changes has important consequences for the distribution of prosperity and opportunities throughout society. Knowledge of the labor market's response to changes in demand, supply or its institutional framework is essential to design effective labor market policies.

The aim of this dissertation is to contribute to a better understanding of labor market responses both to single policies and larger trends. It comprises three essays covering a broad spectrum of related labor market issues. The effects of labor market institutions on employment and wages are a common theme of all three essays.

The second chapter examines the labor market responses to a specific institutional change. In this chapter, I analyze, how fixed-term employment contracts affect employment, wages and the careers of labor market entrants. I exploit variation from a 2001 German reform, that made using temporary employment contracts more difficult for small establishments. Using data from the Mikrozensus and the German social security records I find, that the reform led to a decrease in the use of fixed-term contracts but had only a negligible impact on employment. Notably, I provide novel

evidence for a lower bargaining power of workers in fixed-term contracts. Moreover, I also find that the reform increased the job stability of labor market entrants.

The third chapter has a wider perspective, as it examines the evolution of the East German wage structure in response to changes in the workforce, labor demand and institutions. In this chapter, which is joint work with Christina Gathmann we analyze the East German Wage structure over the last two decades and compare it to the West. Our results suggest that wage inequality in the East exceeds that in the West, especially at the top of the wage distribution. We also show that wage inequality is no longer rising in Germany and even declining in East Germany after 2009. We analyze the role of changes in workforce composition through selective entry and exit into employment on wage inequality and show that accounting for these changes can only partly explain the observed pattern. Moreover, we examine the role of two large institutional changes, de-unionization and sector-specific minimum wages. We analyze the decline in collective bargaining in the late 1990s and find that it affected different segments in the labor market in the East compared to the West, as unionization was much less prevalent at the bottom of the wage distribution in East Germany than in the West. Sector-specific minimum wages, in turn, have been an important factor in the decline in wage inequality since 2009. Lastly, we also analyze, how labor demand has affected the wage distribution. We find that a slowdown in the supply of highly educated workers, likely due to outmigration, plus an upward trend in high-skilled demand led to a strong increase in their skill premium, which contributed considerably to upper tail inequality. Moreover, we also analyze the role of occupational polarization and trade-related industrial shifts for the East German wage structure and find that they had only a weak impact on wage inequality.

The fourth and last chapter of this dissertation has a slightly more distant connection to labor market institutions. In this chapter, which is also joint work with Christina Gathmann, we propose a new way to estimate labor supply elasticities from hypothetical choice questions. Importantly, we devise a new approach that allows us to estimate a friction-less preference for leisure, while most quasi-experimental

approaches yield a compound measure combining both preferences and frictions. Such frictions could be caused both by optimization errors and restrictions in the production schedule of firms to specific hours/wages bundles. Measuring, a friction-less labor supply elasticity allows us to get a better measure of a true behavioral elasticity that matters for long-term welfare, as well as an initial assessment of the potential magnitude of frictions. This is related to the broad themes of labor market institutions of this dissertation in two ways. First, labor supply elasticities (especially with regard to non-labor income) are an important measure for the design of equalizing institutions in the labor market, as they yield a measure for the size of the disincentive effects of transfers and benefits. Second, having a measure for a frictions-less labor supply elasticity could enable economists to identify which frictions are due to institutional barriers, like legal limits on working times or the availability of child-care arrangements.

Even though the chapters of the thesis cover a wide range of topics, they share important aspects. First, all chapters have an empirical focus and rely on data-sets that are representative for the German population. Second, all chapters examine how wages or employment respond to changes in labor market policies or overall market conditions. Lastly, each chapter also provides information that can contribute to the regulatory process, as all of them address some institutional features of the labor market. In this respect, the direct analysis of fixed-term regulation in the second chapter, the assessment of the role of minimum wages or trade unions for wage inequality in the third chapter, and the estimate of hypothetical labor supply responses in the fourth chapter all offer policy-relevant information.

Hereafter, I provide a brief introduction to the various chapters of my thesis.

1.1 Employment Protection and Fixed Term Contracts Evidence from a German Reform

In the second chapter, I examine the impact of temporary employment contracts on wages, employment and the career trajectories of labor market entrants. Governments across Europe have liberalized temporary labor contracts to stimulate

employment. However, due to worries about the long-term outcomes of these reforms, there are several recent policy proposals advocating renewed restrictions of fixed-term employment.(see The Economist, 2018; Reuters, 2016; Zeit, 2018)

In this chapter, I use variation from a 2001 German reform, that made it more difficult for small establishments to use fixed-term contracts. Generally, employers in Germany have to provide an objective reason why a new job is fixed-term. Before the reform, the courts only examined stated reasons based on whether the employer could possibly circumvent employment protection. Since small establishments were exempt from employment protection they were also not limited in their use of fixed-term contracts. The reform introduced a specific list of objective reasons why a contract could be fixed-term that now applied to all plants regardless of their size. The list of reasons was largely identical to the ones courts examined for larger establishments before the reform. Consequently, small plants had a new restriction on the use of temporary employment, while nothing changed for larger plants.

Intuitively, small plants should use relatively fewer fixed-term contracts after the reform, as using them becomes more complicated. However, it is not clear from the outset how employment and wages should react. Therefore, I use a theoretical search and matching model based on Cahuc et al. (2016) to derive some empirical predictions on job-creation and destruction and the conversion of temporary into permanent contracts. The model predicts that both job-creation and job-destruction should decline after the reform.

Using a difference-in-differences approach and data from the Mikrozensus and the German social security records, I then compare small and larger establishments before and after the reform. I find that the reform led to a decline in the use of fixed-term contracts by small establishment. Moreover, I find the negative effects on job-creation and destruction that my theoretical framework predicts. However, the overall effects of the reform on employment were negligibly small. Remarkably, the reform had positive wage effects. Interestingly my theoretical framework allows for positive wage effects of the reform if the bargaining power of workers is lower in fixed-term contracts. Then the lower probability that a new job will be temporary

after the reform also reduces the probability that an employees next job-match has a low bargaining power. This in turn increases her outside option and thereby raises her wage. Thus, the positive wage effect I find indicates that bargaining power is lower in fixed-term contracts. Since a large part of the policy debates on fixed-term work centers on the effects of fixed-term contracts on young workers, i also examine how the reform affected the careers of labor market entrants. For affected labor market entrants I find and increase in cummulated wages and a decrease in the time out of work in the first 5 years after entry. Combined, these results indicate that the reform has had little impact on employment, but positive wage effects and increased the long-term job security of labor market entrants.

1.2 Evolution of the East German Wage Structure

The third chapter of this dissertation, which is co-authored with Christina Gathmann, examines the development of wage inequality in East Germany between 1995 and 2014. The sharp increase in wage disparities in several countries over the recent decades has led to renewed public interest and a lively debate on the causes of wage inequality. The role of labor market institutions like unions and minimum wages in compressing the wage structure is central to this debate (see e.g. Lee (1999); Autor et al. (2016b) for minimum wages; and Card (1992); DiNardo et al. (1996) for unions).

While much of the previous literature has focused on the United States, the considerable differences in wage inequality trends across countries raise a number of questions. The key issue is to what extent different institutional structures across countries can explain the different inequality trends.

The German labor market offers a unique setting to explore this topic. Europe's largest economy underwent a astounding change from sluggish economic growth and rising unemployment in the 1990s to stable employment growth since 2004. Both the Great Recession and the Euro Crisis had only little impact on growth and employment. This remarkable transformation from the 'sick man of Europe' to an 'economic superstar' drew a lot of attention to Germany's unique labor market institutions (Dustmann et al., 2014).

Yet, what is probably most special about the German economy, is the unprecedented merger of two very different states into a common institutional structure. A large economic literature studied the transition of the East German labor market (see e.g. Hunt, 2002; Orlowski and Riphahn, 2009) and its consequences for inequality (e.g. Biewen, 2001; Fuchs-Schüdeln et al., 2010). However, most recent studies on the German wage structure have focused on West Germany arguing that the East German labor market is structurally too distinct to analyze the two together (Dustmann et al., 2009; Card et al., 2013). It is not clear how East German wage inequality has evolved in the last decades, that these studies analyze.

Using detailed administrative data on workers and their employers, we compare the evolution of wage inequality in East and West Germany over the past decades. The trends in the two regions are very similar: wage inequality, irrespective of how we measure it, has increased for most of the period. While there is almost no wage growth in the middle of the East or West German wage distribution, there are sizable wage gains at the top and real wage losses at the bottom. Since 2010, however, wage inequality leveled of and even reversed, mostly because of wage gains at the bottom of the wage distribution.

Subsequently, we explore which factors explain the development of the wage structures in the East and to which extent these forces differ from those in West Germany. We have four main findings. First, we show that the composition of the labour force and selective entry and exit do not have much influence on the wage structure in East and West Germany. Second, the reduction in union coverage does not play a role in the rising wage inequality in East Germany, despite a stronger decline than in the West. The main reason for this surprising result is that union coverage rates at the bottom of the East German wage distribution are significantly lower than in West Germany. Third, the introduction of industry-specific minimum wages can explain all of the reversal of wage inequality in East Germany after 2009. Since wages are about 25% lower in East Germany than in the West and minimum wages are mostly set at the same level for all of Germany, their impact is considerably higher in the East. Finally, we turn to the demand side to explain the substantial

increase in top-end wage inequality before 2010. In East Germany two-thirds of this increase can be accounted for by wage differentials between education and age groups and inter-industry differentials, while these factors explain only one third of the increase in West Germany. A stagnant supply of high-skilled workers plus an increasing trend in high-skilled demand, partly due to high outmigration rates of young and skilled workers, is the main factor for the growth in the East German skill premium and its large impact on inequality at the top of the wage distribution.

1.3 A Novel Approach to Estimate Labor Supply Elasticities: Combining Data from Actual and Hypothetical Choice

In the last chapter of my dissertation, which is a joint work with Christina Gathmann, we propose a new approach to estimate labor supply elasticities. To identify preferences for leisure, we pose a sequence of hypothetical labor supply choice scenarios to a representative sample of respondents in Germany.

Since the preference for leisure determines how individuals trade off leisure time and income, it is a key parameter in labor economics. Insights into the income / leisure trade-off are particularly important for the design of successful economic policies, as this trade-off determines the response of labour supply to social transfers or taxes.

Accordingly, an extensive economic literature has estimated labor supply elasticities using different econometric approaches. However, a number of recent contributions (e.g. Keane, 2011; Meghir and Phillips, 2010; Pencavel, 2016) argue that the common empirical approaches might not identify the preference for leisure after all. Rather, many estimates of the labor supply elasticity are a compound measure that captures both preferences and frictions. Such frictions could be caused both by optimization errors and restrictions in the production schedule of firms to specific hours/wages bundles.

We estimate a friction-less preference for leisure from hypothetical choice scenarios. For our approach, we ask the participants of a representative survey of

the German population about their choices in a variety of hypothetical labor supply scenarios. Each scenario consists of two options that define a combination of hours worked, wages and other income. Survey participants then choose which alternative they would prefer.

Our approach could provide important new insights to supplement existing econometric knowledge with additional information that is not readily available with observed actual choices. Since the hypothetical choice scenarios are not limited by what nature offers, we can elicit choices over a wide range of wage and hours and get estimates even for individuals, who not be affected in typical quasi-experimental settings.

Our results show that preferences for leisure from hypothetical choices are larger than those from observed choices pointing to the importance of frictions. We also document that preferences for leisure differ substantially along observable and unobservable dimensions. These results suggest that estimates from local variation might not be a good proxy for labor supply responses in the broader population.

Although this points to behavioural long-term elasticities greater than previous estimates, the measures are still relatively close to other literature. As a result, the disincentive effects of progression taxation lie in a similar order of magnitude, even taking into account possible frictions.

Chapter 2

Employment Protection and Fixed Term Contracts: Evidence from a German Reform

2.1 Introduction

Over the last three decades, the use of fixed-term contracts has increased considerably in large parts of Europe. This trend has been driven by several reforms that have removed restrictions on fixed-term work. The aim of these reforms was to increase the flexibility of firms to respond to economic changes.[1] As the political costs for a more wide-ranging revision of employment protection were high, the strict dismissal protection rules for permanent employees remained largely unchanged, while fixed-term jobs were liberalized. However, concerns about the long-term effects of these reforms have recently led to increased political efforts in several European countries to reverse some of these liberalizations.[2]

An extensive economic literature studies the effects of firms' access to temporary work when strict employment protection provisions apply to open-ended contracts.

[1] Such flexibility measures were often motivated by fears that strict employment protection has rendered European labor markets rigid and was thereby detrimental to employment. These fears were linked to an economic debate that has examined the institutional differences between the US and several European countries (Bertola, 1999; Nickell, 1997). In this debate, increases in European unemployment in the 1980s were often attributed to more rigid labor markets in Europe compared with the US.

[2] Political proposals to again restrict fixed-term contracts were discussed in Spain, Italy and Germany (see The Economist, 2018; Reuters, 2016; Zeit, 2018). These political debates focused both on worries about the circumvention of employment protection through fixed-term contracts and the impact on the stability of young workers' careers .

Remarkably, the assessment of the benefits of fixed-term employment is mixed in the related economic theory. Although temporary contracts could offer companies the opportunity to hire less skilled workers without risking high firing costs in case of non-performance (Bentolila and Saint-Paul, 1992), there could be some adverse effects. For instance, employers might exhibit a different hiring behavior, when both types of contract are easily available. As firms face high firing costs for open-ended contracts, while fixed-term contracts without firing costs are available, employers might substitute from permanent to fixed-term contracts. (Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002; Cahuc et al., 2016). Consequently, a combination of strict legal requirements for the termination of permanent jobs with weak restrictions on the creation of temporary contracts could contribute to a segmentation of the labor market with protected insiders in permanent employment and outsiders who remain in repeated temporary contracts in lower paid entry level jobs.

In this paper I analyze a 2001 German reform that made it more difficult for small establishments to use fixed-term contracts. Before the reform plants below the employment protection threshold of 5 employees did not have to provide a legal justification for the use of temporary contracts. Prior to the reform, jurisdiction had only considered the possible circumvention of employment protection as the sole criterion for the admissibility of a fixed-term contract. Thus, only plants affected by employment protection had to provide a justification, why a job was fixed-term. The reform introduced a list of objective grounds, why a contract could be fixed-term. These objective grounds were largely identical to those considered by the courts for larger plants before the reform, but were evaluated by the courts regardless of plant-size after the reform. Thus, the legal barriers to hire employees on fixed-term contracts for plants below the employment protection threshold rose compared with those above the threshold.

To derive empirical predictions for this reform, I use a theoretical search and matching model by Cahuc et al. (2016) that explicitly describes the contract-type choice of firms. In the model firms hire workers to take advantage of production opportunities with different expected length. I first present the equilibrium conditions

for job creation, job destruction and the share of jobs that are started with a permanent contract. Moreover, I outline, how these conditions are related to employment and wages. Then I present the results of a comparative static analysis to show, how these variables change as the use of fixed-term contracts becomes more difficult.

I then use a basic differences-in-differences research design to answer the following questions: (1) How did the reform influence the take-up of fixed-term contracts? (2) What were the effects on flows to and out of employment? (3) How did the reform affect wages? (4) What impact did the reform have on the careers of labor market entrants?

As the first step, I show that the share of fixed-term workers decreased in treated plants, both for new contracts and overall. For new contracts the fixed-term share decreased by approximately 3 percentage points whereas the overall post-reform fixed-term share decreased by 0.7 percentage points. The decrease of 3 percentage points amounts to 10 % of the average use of fixed-term contracts in Germany. Thus, these effects have considerable economic significance.

In a next step, I analyze how the reform has affected employment flows. Contrary to the large effects on the share of fixed-term contracts, I only find very minor employment effects associated with the reform. Both the decline in job creation and the increase in the conversion from temporary to permanent contracts are quite small. This suggests that the strong effect on the share of fixed-term contracts is partly due to more jobs starting directly with a permanent contract.

For earnings, the theoretical model predicts that a temporary contract restriction can have a positive wage effect, if the bargaining power of workers is worse in fixed-term contracts and the likelihood that a prospective new contract is temporary decreases. This effect should particularly emerge if the employment response to a reform is small and the reduction in the fixed-term share is large. Consistent with this model prediction and my other empirical results, I find a 2.3 % increase in the wages of new contracts.

Lastly, I also examine some long-term effects for labor market entrants, who joined affected firms after the reform. The core result of this analysis is a sizable increase in cumulated wages over the first years in the labor market and a decrease in the time out of work and the number of jobs. Moreover, I provide suggestive evidence that the likelihood to remain fixed-term decreased for this group of labor market entrants.

Together these results imply that the reform had only little effect on employment but a positive effect on new contract wages and increased longer term job security for labor market entrants.

This article contributes to several strands of economic literature. First, it adds further evidence to the literature that studies, how employment protection rules relate to the use of fixed-term contracts and how reforms of temporary contracts affect their share in total employment (Centeno and Novo, 2012; Bassanini and Garnero, 2013; Hijzen et al., 2017). While much of this work is based on aggregate cross country data (Lazear, 1990; Kahn, 2010; Garibaldi and Violante, 2005; Bassanini and Garnero, 2013), several more recent studies have examined the effects of changes in fixed-term legislation using with-in country variation and micro data. However, many of these studies (Autor et al., 2007; Aguirregabiria and Alonso-Borrego, 2014; Cappellari et al., 2012) are at the firm level, whereas certain interesting outcomes of reforms such as long-term wage effects require employee data. Consequently, this article adds further evidence to a smaller set of empirical work that combines employee data with reform variation (e.g. García-Pérez et al., 2018; Hijzen et al., 2017; Saggio et al., 2018).

Second, in contrast to most other literature, I analyze a scenario in which fixed-term rules became more stringent in Germany. Most other studies are concerned with fixed-term contract liberalizations in Southern European countries like Spain and Italy. This is particularly interesting, since Southern European countries traditionally have a different approach to employment protection than Germany (see. Boeri et al., 2011). For example, temporary contracts are used much more

intensively in some countries such as Spain.[3] Moreover, the rigor of employment protection laws also differs between Southern Europe and Germany (OECD, 2013). Thus, the evidence in this paper is obtained under different labor market conditions and allows or a better understanding of the effects of fixed-term work restrictions in countries where temporary employment is used less.

Third, my work documents novel positive wage effects for a restriction of fixed-term contracts. This is evidence for lower negotiating power for workers in temporary employment relationships.

Lastly, the article also contributes to a growing literature that analyzes whether fixed-term contracts offer long-term opportunities or if they bind employees into low-paid entry-level positions (Booth et al., 2002; Ichino et al., 2008; Autor and Houseman, 2010; Saggio et al., 2018). I find only minor effects on employment but relatively strong increases in labor market entrants' cumulated wages and job security. This assessment of the impact of the reform on later outcomes for labor market entrants also relates to a literature on the long-term effects of labor market conditions at entry (Altonji et al., 2016; Oreopoulos et al., 2012).

The article proceeds as follows. The next section discusses the institutional background of the reform and describes how employment protection and fixed-term work are regulated in Germany. Section 3 provides a discussion of the theoretical mechanisms that determine how a limitation of fixed-term contracts might affect the use of different contract types, employment and wages. Section 4 describes the datasets, I use for the analyses. Section 5 presents the empirical strategy Section 6 reports the results. Section 7 provides a range of robustness checks for my findings. Finally, section 8 concludes.

[3]For example, about 30 % of all employment contracts in Spain are fixed-term, compared to only 12 % in Germany (see figure 2.A1 in the appendix).

2.2 Institutional Background

2.2.1 Fixed-term contract law in Germany

Most employment contracts in Germany are commonly unlimited. If a firm dismisses a permanent employee, significant firing costs are incurred in the form of notice periods, severance payments or administrative effort. In most cases, half a month's salary per year of employment is paid as severance payment. The firm must also observe notice periods ranging from 2 weeks to 6 months, depending upon the seniority of the employee.

In addition, larger establishments have to comply with further firing restrictions from the Dismissal Protection Act (Kündigungsschutzgestz). Specifically, employers have to provide evidence that one of the particular dismissal reasons named in the law is satisfied. The act only allows dismissals related to the personal situation of the person to be dismissed (e.g. long-term sickness), breach of contractual duties (e.g. fraud or theft), or operational reasons related of the business of the employer. For a dismissal due to operational reasons, which is the most common type of dismissal, the employer must show, that the job position permanently ceases to exist and no other appropriate vacant job exists in the entire firm. Since the burden of proof for a dismissal is relatively high, employees and employers often agree on severance payments to avoid lengthy legal disputes. Whether these stricter firing rules apply is entirely determined by the number of full-time employees.[4] In 2001 the size threshold for the employment protection law was at 5 employees.[5]

Alternatively firms can hire employees under fixed term contracts. Once a fixed term contract reaches its termination date, it can be dissolved without any dismissal costs. However, it is more difficult to justify the termination of a temporary contract before it expires. In general, fixed-term contracts are only permitted if employers

[4]More precisely, newer versions of this law are based on the number of full-time equivalents, where workers up to 20 hours per week are counted with a factor of 0.50, and workers up to 30 hours with a factor of 0.75

[5]Before 1997 the threshold for dismissal protection was at 5 employees. For the period from 1997 to 1998 it was increased to 10 employees. Between 1999 and 2004 it was reduced back to 5 employees and since then it has again been set at 10 employees. The 1996 and 1999 employment protection reforms are analyzed by Bauer et al. (2007), while Bauernschuster (2013) discusses the 2004 reform.

state an objective reason, why a job could not be permanent (e.g. project work or replacement during sick leave).[6] However, before the 2001 reform, which I analyze in this paper, smaller firms were exempt from providing a justification for using fixed-term contracts.

2.2.2 The 2001 Part-Time and Fixed-Term Contracts Act

Table 2.1: Reform variation

	Before 2001	**After 2001**
Subject to EPL (≥5 Employees)	Objective reasons (EPL circumvention)	Objective reasons listed in law
Not subject to EPL (<5 Employees)	No restriction	Objective reasons listed in law

NOTE.- This table summarizes the relevant variation from 2001 Part-Time and Fixed-Term Contracts Act.

In January 2001, the Part-Time and Fixed-Term Employment Act was signed into to law to implement the EU Directive 1999/70/C. This new law changed the rules concerning the justification of fixed-term contracts.[7] Prior to the reform, the judiciary assessed the admissibility of grounds for the use of a temporary contract by examining whether the contract could possibly be used to circumvent dismissal protection. By definition, establishments below the employment protection threshold could not circumvent dismissal protection. As a result, these establishments were not restricted in their use of fixed-term contracts. This changed as the new law

[6]There is an exception to this rule for contracts shorter than two years. Since 1985 firms are allowed to use these shorter temporary contracts without naming an objective reason. After 2 years a firm can not legally offer workers a further fixed-term contract without naming an objective reason. This exception from the default is based on a temporary exemption to boost employment that was introduced in 1985. This rule for short fixed-term contracts was renewed two times and lastly made permanent. However, these rules remained unaffected by the reform. See Hunt (2000) for an analysis of the 1985 law that introduced short-term fixed-term contracts without objective reason.

[7]The same law also introduced new rules, which allowed longer fixed term contracts without objective reasons for workers above the age of 58. However, these rules should act in the same direction as the rest of the law, as firms above the employment protection threshold have a higher incentive to make use of temporary contracts.

specified a list of legal justifications for a contract limitation regardless of the employment protection status.[8]

The objective reasons listed in the law are largely similar to those courts evaluated to determine whether the use of fixed-term contracts did constitute a employment protection circumvention before the reform. However, these new reasons are now evaluated by the courts independently of their potential for the circumvention of employment protection laws and plants below the employment protection threshold also have to providence evidence that these reasons are satisfied. Thus, after the reform hiring workers in fixed-term contracts based on an objective reason became comparatively harder for firms below the employment protection threshold (see Table 2.1 for a short overview of the variation introduced by the law).

This introduces variation in the potential to use fixed term contracts between the firms that are subject to employment protection rules and the firms that are not. Thus, I can compare workers in firms above and below the employment protection threshold before and after the reform.

2.3 Theory

Intuitively, plants below the employment protection threshold should use relatively fewer fixed-term contracts after the reform, as the legal requirements for their admissibility increase. However, the effects on other outcomes such as overall employment or wages are not clear from the outset. To derive further predictions on the impact of the reform, I introduce a search and matching model based on Cahuc et al. (2016), which explicitly examines firms' choices between permanent and fixed-term jobs.[9] I use this model to qualitatively study, how a increase in the costs of fixed term contracts should affect the employment behavior, the type of contracts chosen and wages in establishments below the employment protection threshold.

[8]The reasons include temporary need of the job on the part of the employer, trial periods or fixed-term periods following training or studies, employment to substitute another employee, reasons related to the person of employee or the nature of the job and the limited availability of public funds.

[9]Additionally I also incorporate some extensions of the model from Saggio et al. (2018) into my theoretical framework

In the model, firms hire workers to take advantage of production opportunities with different anticipated durations. Specifically, jobs differ in the arrival rate of shocks that renders them unproductive. Firms and employees jointly maximize a match surplus that depends on this rate and share it through Nash bargaining. Fixed-term and open-ended contracts differ in their termination rules.

Firms have to pay a firing cost to dismiss an employee on an open-ended contract if she becomes unproductive. However employees on fixed-term contracts can be dismissed free of charge after the contract term, yet not before. If a fixed-term employee becomes unproductive before the contract has ended, the firm has to keep paying the employees salary until the contract term expires. Jobs that start fixed-term can be converted to permanent when the contract expires. Alternatively fixed-term contracts can be terminated free of charge after the term date. Renewing the job with another fixed-term contract is not possible. These rules lead to different surpluses for permanent and temporary contracts for a given expected productive duration of a job.

In a first step, this allows me to distinguish different thresholds on the shock arrival rate that determine whether jobs are started permanent, started fixed-term and then converted to open-ended contracts or started on temporary contracts but terminated after the term or not created at all. In a second step, I can derive how a change in the costs of writing a fixed-term contract affects these thresholds. This also yields predictions about job creation, job destruction and the share of new contracts that are started permanent. Lastly, I can analyze how this increase in contract writing costs affects wages.

For an increase in the cost of establishing fixed-term contracts, the model predicts two countervailing influences on overall employment. For one, fewer fixed-term jobs are created. At the same time, it increases the incentive to retain fixed-term employees in permanent contracts, which reduces job destruction.[10]

[10]This basic result has been obtained by a large part of the theoretical literature on fixed-term contracts (e.g. Alonso-Borrego et al., 2005; Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002). However, much of the theoretical literature on fixed-term contracts models them as screening device to learn about productivity (e.g. Faccini, 2014; Blanchard and Landier, 2002), which is at odds with two empirical observations. First, there exist both contracts that are either

Beyond these predictions on employment, I also study, how the reform should affect wages of permanent and fixed-term employees. In the baseline that abstracts from differences in bargaining power between contract types, there is only a negative wage effect on wages through a decrease in labor market tightness. However, once I assume that employees have less bargaining power in temporary jobs, a positive effect on wages is possible. This effect results from an increase in the value of the outside option of the employee, as the probability that a prospective new job is permanent and she therefore is able to exercise greater negotiating power in this job increases.

I will now outline the basic premise of the model and describe the equilibrium conditions for job creation, job destruction and wages.

2.3.1 Model Setup

The model economy consists of identical, infinitely-lived, risk neutral workers and firms, who face the same discount rate r. Since workers are identical, their total mass is normalized to 1. Labor is the only input used by perfectly competitive firms. All jobs produce the same quantity of output $y > 0$ per unit of time, but production opportunities differ in their expected duration. This difference between the expected durations is modeled as shocks, which reduce the output produced per time unit to $y = 0$ and arrive at the Poisson rate λ.[11] Job seekers and vacancies meet according to a standard constant returns to scale matching technology and the job-type $\lambda \in [\underline{\lambda}; \bar{\lambda}]$ is randomly drawn from a distribution with $\lambda \sim G(\lambda)$ on match.

Firms and workers maximize a job-type dependent match surplus of $S(\lambda)$ and share it using Nash bargaining. Depending on the size of this match surplus, they choose between permanent and temporary contracts. Permanent contracts are open ended but are terminated if the job becomes unproductive. At termination the

shorter than the legal probation period or longer than the typically estimated time needed for screening. Second, a learning perspective ignores the higher prevalence of fixed-term contracts in industries with short production opportunities (e.g. Bassanini and Marianna, 2009)

[11] There is empirical evidence that fixed-term contract use depends strongly on the length of production opportunities. For example, Dräger and Marx (2017) find that workload fluctuations increase the likelihood of hiring fixed-term workers in countries with less flexible labor markets.

employer pays a firing cost f to dissolve an unproductive permanent contract.[12] Temporary contracts have an endogenous duration $D(\lambda)$ till they expire and can not be ended before. If a job becomes unproductive before the end of its entire term, the company must continue to pay the employee's wage until the contract expires.[13] If the contract stays productive for the whole duration $D(\lambda)$, workers and firms decide whether to dissolve the employment relationship free of any firing cost or whether to establish a permanent contract with a new wage. Agreeing upon another fixed-term contract after the term is not possible.[14] Firms pay contract writing costs that differ between fixed term (c_{FT}) and permanent contracts (c_P).[15] The reform is later modeled as an increase in the contract writing costs for fixed-term contracts c_{FT}.

The difference between the surplus of a temporary contract with optimal duration $S_{FT}(\lambda, D^*(\lambda))$ and the surplus of a permanent contract $S_p(\lambda)$ determines the contract type choice in equilibrium. I provide a detailed definition of the surplus by contract type in appendix 2.B.1.

2.3.2 Equilibrium Conditions

Cahuc et al. (2016) show that, given that both types of contract exist in an equilibrium, there are three unique endogenously determined levels of λ that determine job creation, job destruction and the initial type of contract.

First, the level λ_P with $S_P(\lambda_P) = 0$ determines whether fixed-term jobs are continued after the termination date. Second, the value λ_{FT} with $S_{FT}(\lambda_{FT}) = 0$ specifies a bound for temporary job creation. Lastly, λ_E with $S_P(\lambda_E) = S_{FT}(\lambda_E)$

[12] f is assumed to be a red-tape cost and not a transfer from the firm to the worker (such as a severance pay) as such transfers can be neutralized by appropriately designed contracts.

[13] This represents the standard case in German fixed-term contract law, as a jointly determined dismissal provision between the employee and the firm is required for the premature termination of fixed-term contracts. Deviations from this basic rule are only possible in special cases like fraud or theft. Moreover, not all jointly determined termination provisions are legally justified. Similar rules also apply in other European countries like France, Belgium and Italy.

[14] Although it is theoretically possible to establish consecutive fixed term contracts with a valid objective reason in Germany, the law regards this as the default case. Renewing a fixed-term contracts requires a special new objective reason for an extension. Moreover the courts evaluate the number of past contracts and the total employment duration to establish whether a new fixed-term contract was valid.

[15] Both terms represent legal costs of writing contracts. c_{FT} is higher than c_P as firms need to provide an admissible objective reason to establish fixed-term contracts.

defines a level of λ at which firms are indifferent whether they should start a job with a fixed-term or permanent contract. The necessary condition for the existence of such a type of equilibrium with $\lambda_E > \lambda_P > \lambda_{FT}$ is that $S_{FT}(\lambda_P) > 0$.[16]

Figure 2.1: Choice Between Fixed-Term and Permanent Contracts

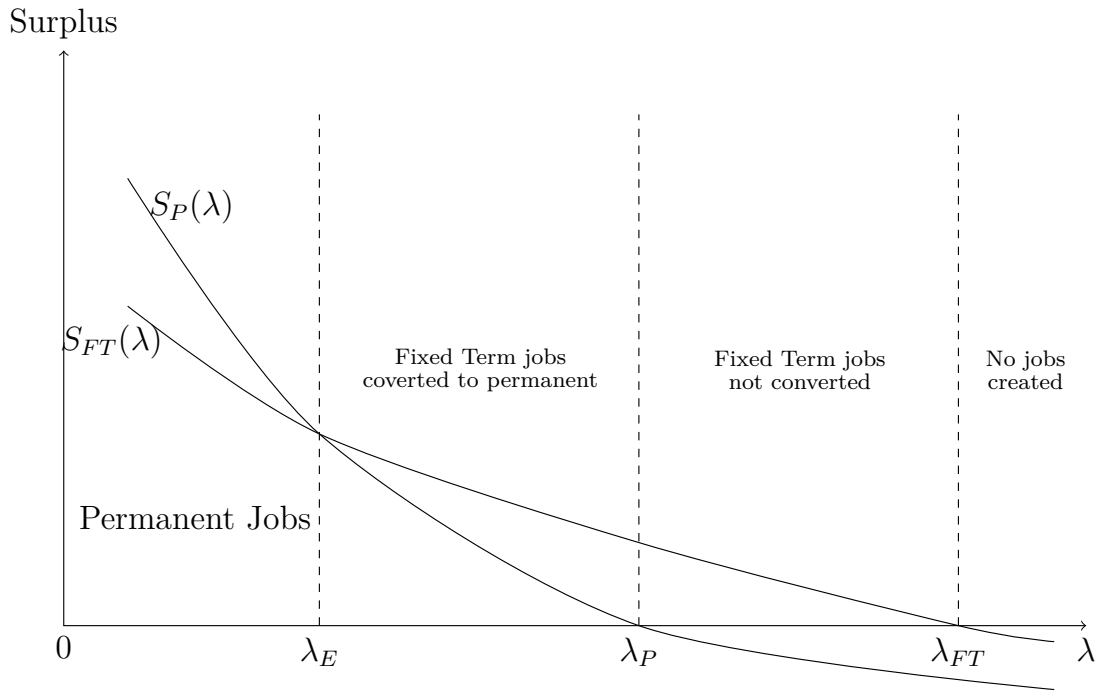


Figure 2.1 illustrates the contract choice for different levels of λ.[17] For values of λ below λ_E, the expected duration of a production opportunity is sufficiently large to create jobs with permanent contracts. For values of $\lambda \in [\lambda_E, \lambda_P]$ jobs are created with a fixed-term contract but converted to permanent contracts if they stay productive until $D^*(\lambda)$. As the surplus of continuing the job in a permanent contract is below zero for $\lambda \in [\lambda_P, \lambda_{FT}]$, jobs in this range are started fixed-term and destroyed once the contract expires. Jobs with very high shock arrival rates above λ_{FT} generate negative surplus even for fixed-term contracts and are not created.

The equilibrium in the labor market is defined by the conditions that determine the parameters λ_{FT},λ_P and λ_E and a condition on the matching between workers and vacancies. Unemployed workers u find vacancies v according to a standard

[16] A more detailed overview of the equilibrium conditions can be found in the appendices 2.B.3 and 2.B.3.

[17] The figure is based on figure 6 in Cahuc et al. (2016)

constant returns to scale aggregate matching function $m(v, u)$ (Pissarides, 1979). Therefore, the vacancy fill rate $q(\theta)$ and the employment finding rate $\theta q(\theta)$ solely depends on the ratio of the number of vacancies v over the number of unemployed workers u, which is the labor market tightness $\theta = \frac{v}{u}$. Not filling a vacancy implies a cost of $\kappa > 0$. If there is a match both parties learn the true value of λ and use the contract type rules for λ to decide whether they enter an employment relation. If the worker and the firm sign a contract, they negotiate a wage using Nash bargaining. The share of the surplus retained by workers is $\gamma_c \in [0, 1)$ with $c = \{FT, P\}$. Similarly to Saggio et al. (2018) I will later discuss the reform effects both for an equal bargaining power ($\gamma_{FT} = \gamma_P = \gamma$) for fixed-term and permanent workers and for a case were fixed-term workers have a lower bargaining power ($\gamma_{FT} < \gamma_P$) .

If all profitable opportunities for job creation are exploited, the expected profit for vacant jobs is equal to the cost κ. This yields the free entry condition in equation 2.1, which specifies the labor market tightness in the equilibrium.

$$\kappa = q(\theta) \left[(1 - \gamma_P) \int_{\underline{\lambda}}^{\lambda_E} S_p(\lambda) dG(\lambda) + (1 - \gamma_T) \int_{\lambda_E}^{\lambda_{FT}} S_T(\lambda) dG(\lambda) \right] \tag{2.1}$$

Moreover, this condition can be used to pin down the value of the outside option in the equations that define λ_E, λ_P and λ_{FT}. The value of this outside option is simply given by the sum of a flow utility of unemployment z and the expected surplus share of a job evaluated at the job finding rate $\theta q(\theta)$.

$$rU = z + \theta q(\theta) \gamma_P \int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda) dG(\lambda) + \theta q(\theta) \gamma_{FT} \int_{\lambda_E}^{\lambda_{FT}} S_{FT}(\lambda) dG(\lambda) \tag{2.2}$$

Rearranging the free entry condition to get an expression for $\int_{\lambda_E}^{\lambda_{FT}} S_{FT}(\lambda) dG(\lambda)$ and substituting this into equation 2.2 yields

$$rU = z + \theta q(\theta) \frac{\gamma_{FT} \kappa}{1 - \gamma_{FT}} + \theta q(\theta) \frac{\gamma_P - \gamma_{FT}}{1 - \gamma_{FT}} \int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda) dG(\lambda). \tag{2.3}$$

Note that the last term in equation 2.3 becomes zero, if the rent-sharing parameter does not differ between contract types. This term is the valuation of the additional rent that workers can extract under a permanent contract. Thus, for $\gamma_{FT} = \gamma_P$ the

value of the outside option solely depends on z, γ, κ and the labor market tightness θ, while it additionally depends on λ_E for differential rent-sharing.

Substituting the value of the outside option from equation 2.3 into the equations defining γ_{FT}, γ_P and γ_E provides a system of equations that specifies the equilibrium $(\theta^*, \lambda_E^*, \lambda_P^*, \lambda_{FT}^*)$.

2.3.3 Wages

Continuing wage payments under a permanent contract satisfy the following Nash bargaining first order condition:

$$\gamma_P \left[\Pi_P(\lambda) + f\right] = (1 - \gamma_P) \left[V_p(\lambda) - U\right] \tag{2.4}$$

Consequently this implies that the wage for a permanent contract is given by

$$w_p = \gamma_p(y + rf) + (1 - \gamma_p)rU, \tag{2.5}$$

which would reduce to the outside option rU if the workers bargaining power γ_P was zero. The continuing wage for fixed-term contract can be obtained in similar way and is given by the following wage equation:

$$w_{FT} = \gamma_{FT} \left(\frac{ry}{r + \lambda} \frac{1 - e^{-(r+\lambda)D^*(\lambda)}}{1 - e^{-rD^*(\lambda)}} \right) + (1 - \gamma_{FT})rU \tag{2.6}$$

The term $\frac{1-e^{-(r+\lambda)D^*(\lambda)}}{1-e^{-rD^*(\lambda)}}$ evaluates the odds of a shock not occurring before the duration $D^*(\lambda)$ has elapsed and is decreasing in D^* for a fixed λ.

This is because, at a given expected productive duration of $1/\lambda$, the probability that a job will become unproductive during the contract term increases with the length of D^*. Thus, as D^* decreases in U for a fixed λ, an increase in the value of U leads to an increase in both the first and the second term of w_{FT}.

2.3.4 Comparative statics

I now summarize how an increase in the cost of establishing a fixed-term contract c_{FT} affects the equilibrium values $(\theta^*, \lambda_E^*, \lambda_P^*, \lambda_{FT}^*)$ and the equilibrium wages

for permanent and fixed-term contracts, if rent-sharing is identical for both contract types. A more detailed overview of the comparative statics is in the appendices 2.B.2 and 2.B.3.

For a fixed level of labor market tightness, creating jobs with high shock arrival rates becomes less desirable as λ_{FT} declines. However, there is also a feedback channel to this effect. As less jobs are created after the cost increases, market tightness decreases, which in turn leads to a decrease in the value of the outside option. Lastly a decline in the outside option shifts λ_{FT} upwards. However, it can be shown that the direct effect dominates, and overall λ_{FT} declines for an increase in c_{FT}.

Furthermore, more fixed-term contracts are converted to permanent contracts as λ_P increases with an increase in c_{FT}. This comes from both a direct effect of costs on λ_P and an indirect effect through decreasing labor market tightness.

There are two countervailing effects on the parameter λ_E that determines whether jobs are started with fixed-term or permanent contracts. First, there is a positive direct effect on λ_E, since for a fixed θ it is more costly for firms to establish fixed-term contracts. Second, a declining labor market tightness could possibly offset this effect by a decrease of $U(\theta)$, which negatively affects λ_E. The overall effect on λ_E is not clear from the onset and depends on different model parameters.

Generally the only possible wage effect on permanent wages in the setup with identical bargaining across contract types is negative, as decreasing labor market tightness leads to a decrease of the outside option and hence also to a decrease of the permanent wage $w_p(\lambda)$. A decline in $U(\theta)$ can also lead to a decrease in fixed-term wages. However, there is a second effect in the opposite direction for fixed-term wages as the reform leads to an increase in the optimal duration of fixed-term contracts, which in turn leads to an increase in the the first wage term in equation 2.6.

For the case of differential rent sharing, the value of the outside option does not only depend on labor market tightness θ but also on the parameter λ_E that determines whether a job is created on a fixed-term or a permanent contract. Intuitively, a higher λ_E raises the value of unemployment, as the chance that a

new employment contract is permanent and lets employees keep a larger share of the match surplus increases.

This influence of the parameter λ_E on the value of unemployment adds additional indirect effects to the reform effects on the parameter λ_{FT} and λ_P. Interestingly, these additional influences can act in different directions, as the reform effect on λ_E is ex-ante ambiguous. For example, if the reform increases λ_E, this leads to a decrease of $U(\lambda_E, \theta)$, which has a negative impact on λ_{FT}, whereas in the case where the reform decreases the λ_E, this raises $U(\lambda_E, \theta)$ and thus also λ_{FT}.

Overall the direct effect of the reform on λ_T still dominates provided that λ_E increases, which leads to lower job-creation in affected firms after the reform. However, the effect on job-destruction is now indeterminate as λ_P can be affected in either direction depending on the basic parameters of the model.

Table 2.2: Model predictions

1. Equal bargaining power $\gamma_{FT} = \gamma_P$		**2. Less bargaining power in temporary jobs** $\gamma_{FT} < \gamma_P$	
$\lambda_{FT} \downarrow$	Less job creation as jobs with very short expected productive durations are no longer created	$\lambda_{FT} \downarrow$	Less job creation as jobs with very short expected productive durations are no longer created
$\lambda_P \uparrow$	More temporary jobs are converted to permanent contracts once their term expires	$\lambda_P \updownarrow$	Effect on contract conversion remains a priori indeterminate
$\lambda_E \updownarrow$	A priori indeterminate effect on contract-type at start	$\lambda_E \updownarrow$	A priori indeterminate effect on contract-type at start
$w_{FT} \downarrow, w_P \downarrow$	Negative wage effect as labor market tightness decreases	$w_{FT} \updownarrow, w_P \updownarrow$	Indeterminate wage effect as an increasing λ_E can lead to a higher outside option.

NOTE.- This table contains a short overview of the main model predictions.

Interestingly, differential rent sharing between permanent and fixed-term contracts now also allows for positive wage effects. This happens if the overall effect of an increase in c_{FT} on $U(\lambda_E, \theta)$ is positive. As in the case with equal rent sharing, this effect is again clear for permanent wages, but can be counteracted by a reaction through the optimal duration for fixed-term wages.

Table 2.2 provides a short summary of the main model predictions.

2.4 Data Sources

2.4.1 Mikrozensus

To analyze the 2001 reform in fixed-term employment rules, I use the Mikrozensus, a repeated cross-sectional survey of 1% of the German population. My analysis is based on data between 1996 and 2010 as information on the contract type is only available for survey waves after 1996. Multiple characteristics make the data particularly suitable for examining fixed-term employment.

First, the data contain information on whether a contract is temporary and also includes its official duration for time periods before and after the reform. Contrary to that, German Social Security data only includes the fixed term status of employees for years after 2011. Second, the data includes questions on the plant size, which can be used to determine whether a workers plant is subject to employment protection laws. Finally, the Mikrozensus is a large representative sample of the German population, including about 14000 new employment relationships per year, allowing me to analyze the effect of fixed term employment legislation on hiring behavior.

I distinguish three skill groups based on the highest educational qualification. An individual is medium-skilled if she has completed an apprenticeship or graduated from high school (*Abitur*). A person is high-skilled if she graduated from college or university. However, the proportion of high skilled workers in treated plants is very small.[18] Therefore, I exclude high-skilled workers from my sample.

The Mikrozensus also provides information on net personal income that combines wage income with earnings from self-employment, rental properties, pensions as well as other public transfers (like welfare or child benefits). I convert net personal income to 2014 prices using the national consumer price index and use this information to analyze the wage effects of the reform.

For the analysis, I restrict the sample to West German individuals between the ages of 20 and 62. I further exclude people, that are either self-employed, in

[18]For new contracts, there are less than 41 observations in 2001, and ca. 50 observations per year on average

civilian or military service or in vocational training, since the legal rules regarding employment protection and fixed term work do not apply to these groups.

2.4.2 Social Security Data and Establishment History Panel

While the Mikrozensus contains detailed information on contract types and wages, it is not suitable for analyzing employment effects. Since it is a repeated cross-sectional data set, each person is observed at only one point in time.

However, administrative social security data allows for a more detailed analysis of the theoretical predictions on job-creation and destruction, as it is possible to follow individuals over time. Although contract types are not directly observable in the social security data for the reform period, this allows for both analyses on the transition between unemployment and employment and an analysis on the long-term reform effects for labor market entrants. Consequently, I use a 2% random sample of the population of workers and plants covered by the social security system in Germany, to study employment effects and long-term reform outcomes in more detail.

I apply the same sample restrictions as for the Mikrozensus data to make the results more comparable across data sets. Since the education variable in the social security data is missing for about 20% of the observations and exhibits some inconsistencies over time, I use the panel structure of the data to impute education in the current year from past and future spells following Fitzenberger et al. (2006).

The data provide information on each individual's employment status in the social security system as of June 30th each year. Moreover, the wage variable reports the average daily wage for the employment spell that contains this reference date. As with virtually all social security data, the wage variable is right-censored at the social security threshold. I impute censored wages under the assumption that the error term in the wage regression is normally distributed allowing for separate variances by year and gender (Gartner, 2005). However, since the data-set is restricted to low- and medium-skilled individuals less than 4% of wage observations are affected by the imputation procedure. Furthermore, I also convert wages to 2014 prices using the national consumer price index.

2.4.3 Descriptive statistics

Table 2.3: Proportions of fixed-term employees by gender, age and education

Women								
	20 to 26	27 to 32	33 to 38	39 to 43	44 to 48	49 to 54	55 to 59	60 to 62
Low Education	14.7%	7.5%	6.5%	5.9%	4.6%	3.4%	2.7%	2.7%
Middle Education	20.9%	7.9%	5.4%	5.3%	4.6%	3.3%	2.6%	3.2%
Men								
	20 to 26	27 to 32	33 to 38	39 to 43	44 to 48	49 to 54	55 to 59	60 to 62
Low Education	18.3%	7.9%	4.8%	3.8%	3.2%	2.7%	2.6%	2.9%
Middle Education	25.9%	10.7%	4.8%	3.3%	3.1%	2.8%	3.1%	3.8%

NOTE.- This table displays the proportion of fixed-term employees with-in gender, age and education groups
Source: Mikrozensus sample for West German employees aged 20 to 65

In the following, I display some summary statistics from the Mikrozensus sample. Table 2.3 shows the proportions of fixed-term employees for 8 different age and education groups separately for women and for men over the entire observation period. The proportion of fixed term contracts in the two youngest age groups is much higher than in all other age groups. Additionally in these two age groups the fixed-term proportion increases with education, while the difference between different education types is much smaller for older age groups. Gender differences are relatively small.

The fixed-term shares for younger workers is relatively high reaching almost 26 % for male middle-educated workers under 26. For workers above the age of 26 the fixed-term shares are drastically smaller. However, 9.9 % of all contracts observed are fixed-term, which is close to the OECD average of 11.2 %. (OECD, 2017).

Figure 2.A2 in the appendix plots the time trend of the proportion of fixed term contracts both for the overall working population and for new contracts. Both measures have increased markedly over the sample period. Between 1996 and 2010 the proportion of fixed term contracts for new employment relationships increased by 15 percentage points.

Lastly, I report some summary statistics by employment protection status and time period in table 2.4.

Matching the stronger incentive to use temporary contracts, the proportion of fixed-term employees is higher in firms that are affected by employment protection.

Table 2.4: Summary Statistics for the Mikrozensus data

	Workers in firms that are not affected by employment protection			
	Pre reform period (1996-2000)		Post reform period (2001-2010)	
	Mean	St. Dev.	Mean	St. Dev.
Fixed Term	0.044	0.205	0.060	0.238
Real net monthly income	1931.946	1082.619	2025.035	1421.328
Hours worked per week	30.477	13.382	28.213	13.617
Female	0.653	0.476	0.658	0.474
Age	40.093	11.429	41.200	11.334
Service Sector	0.680	0.466	0.717	0.450
Middle Education	0.769	0.422	0.773	0.419
Observations	30,423		149,592	
	Workers in firms that are subject to employment protection			
	Pre reform period (1996-2000)		Post reform period (2001-2010)	
	Mean	St. Dev.	Mean	St. Dev.
Fixed Term	0.053	0.225	0.075	0.264
Real net monthly income	2019.406	967.815	2101.062	1238.247
Hours worked per week	35.077	9.340	34.477	10.082
Female	0.436	0.496	0.456	0.498
Age	40.624	10.967	41.428	10.923
Service Sector	0.539	0.498	0.615	0.486
Middle Education	0.832	0.374	0.828	0.378
Observations	247,605		752,799	

NOTE.- This table provides means and standard deviations for fixed-term status, real income, hours worked and age by employment protection status both for the pre-reform and post-reform periods. Moreover it also reports the proportion of female workers, medium educated individuals and employees in the service industry.
Source: Mikrozensus sample for West German employees aged 20 to 65

Average wages are only slightly higher for firms above the employment protection threshold. Average working hours are however larger. Moreover, the proportion of female workers is higher for firms below the employment protection bound.

While the average age is nearly identical for the two groups, firms that are not affected by employment protection tend to employ more low skilled individuals and are more likely to be active in the services sector of the economy.

Altogether, employees in firms above and below the employment protection threshold are generally quite similar.

2.5 Empirical Strategy

The Part-Time and Fixed-Term Contracts Act of 2001 has raised the regulatory requirements for the use of temporary contracts for plants below the employment protection threshold. This induces variation in the relative costs of writing fixed term contracts between firms above and below the employment protection threshold. Thus, I can use a simple difference-in-differences research design, which compares employees in treated plants (i.e. not subject to employment protection) to those in control plants (i.e. subject to employment protection) before and after the reform.

For outcome variables such as the likelihood of a new contract to be fixed-term, the transition from unemployment to employment and net wages the estimation equation is given by

$$\textsc{Outcome}_{ipt} = \alpha \textsc{No EPL}_{ip} \times \textsc{Post 2001}_t + \beta \textsc{Plant-size}_p + \gamma \textsc{year}_t + \lambda \mathrm{X}_{ipt} + \varepsilon_{ipt}, \tag{2.7}$$

where i indexes individuals, p indexes plant-size categories, and t indexes years. I include year and firm size dummies, as well as a set of control variables X_{ipt} for the age, education and the gender of the individual and the industry of the firm at the 2-digit level .

The variable $\textsc{No EPL}_{if} \times \textsc{Post 2001}_t$ is an interaction effect between the employment protection status of a workers firm and an indicator variable for years after 2001. I use the respectively applicable plant-size limit for each year to determine, whether a firm is subject to employment protection or not. Since I control for plant-size and year fixed effects, the effect of the reform α is identified by the change in the respective outcome variable in firms above the employment protection threshold, relative to the other firms, in 2001 or later relative to 2000 or earlier.

My identification strategy requires that plants do not deliberately change their size in response to the reform and move from the control into the treatment group. However, I can use information on plant size, to analyze whether plants changed their size around the threshold after the reform. Moreover, I can later also further

alleviate these concerns by comparing plants that are farther away from the actual employment protection threshold.

Furthermore, other legal changes that are happening at the same time and affecting firms along the employment protection threshold differentially can not be distinguished from the reform effect. However, there were no major changes to employment legislation at the same time and the other rules introduced in the same law were unrelated to the employment protection status of firms and fixed-term employment.

Lastly, parallel trends between the treatment and control group is the central assumption for the validity of my identification strategy. I address this issue in several ways. I graphically plot the development for all available years before and after the reform. Moreover, I also extend the above regression to account for timing of the effects to see, whether there are any significant pre-reform differences once I account for fixed group characteristics and controls.

2.6 Results

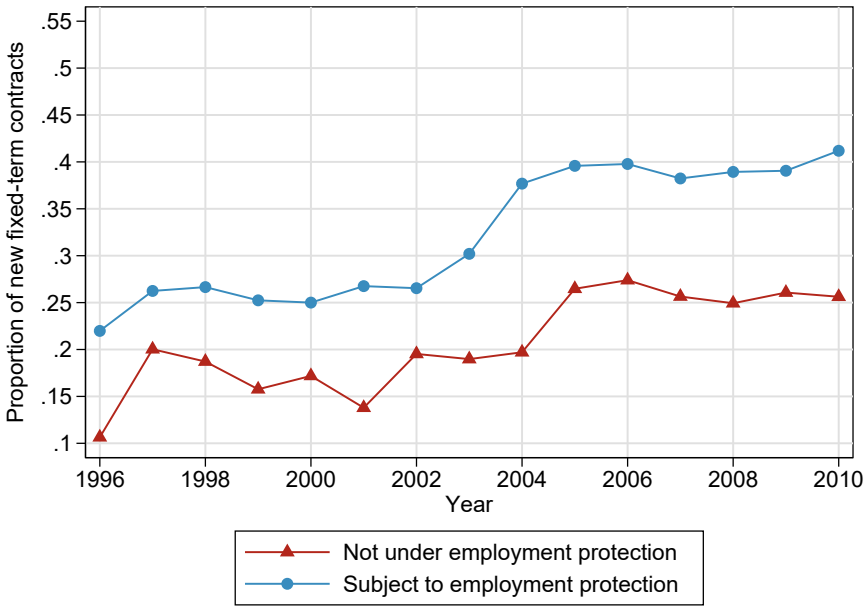
The baseline of the theoretical model predicts both a decline in temporary job creation and an increase in the conversion of fixed-term contracts into permanent jobs. Thus, if the reform had any effect, I should observe a response in the share of fixed-term contracts in the treatment group.

2.6.1 Use of fixed-term contracts

Consequently, I begin by assessing the impact of the reform on the uptake of fixed-term contracts by plants that are not subject to employment protection rules. As a first step, I plot the proportion of new contracts that are fixed term.

Figure 2.2 shows that up to 2001 the trends in hiring fixed-term employees moved roughly in parallel for both types of plants and diverged afterwards. After 2001, there is an increase in the use of fixed-term contracts for new hires in both the treatment and the control group. However, the increase is considerably larger for plants that are subject to employment protection.

Figure 2.2: Trends for the proportion of fixed-term contracts by employment protection status



NOTE.- The figure plots the proportion of new fixed term contracts by employment protection status.
Source: Mikrozensus sample for West German employees aged 20 to 65

Besides, this first overview is in line with the expected incentives for the use of fixed-term contracts. Plants larger than the employment protection threshold have a stronger incentive to make use of temporary employment and consequently have a larger share of fixed-term contracts pre-reform. As the 2001 reform has made it more difficult for plants not covered by employment protection to use fixed-term contracts, the trends between the two groups diverge.

Next, I explore the effect of the reform more formally. Table 2.5 contains the results of difference-in-difference regressions for the fixed-term share in both new contracts and all employment relationships. Columns (1) to (3) limit the sample to new contracts, while columns (4) to (6) are calculated for the whole sample of all employment relationships. Each column presents a regression of the fixed term status of an employment contract on plant-size category and year fixed effects. The plant-size categories for all regressions are plants of 1 to 5 employees, 6 to 9 employees, 10 to 19 employees and 20-49 employees. The plant-size category

Table 2.5: Difference-in-Difference Equations for likelihood of being fixed-term (New contracts)

	(1)	(2)	(3)	(4)	(5)	(6)
	New contracts			Overall		
No EPL×Post 2001	-0.0404***	-0.0302***	-0.0316***	-0.00895***	-0.00510**	-0.00695**
	(0.00566)	(0.00533)	(0.00535)	(0.00157)	(0.00175)	(0.00237)
Female	-0.00534	-0.0510***	-0.0500***	0.00363	-0.0153***	-0.0155***
	(0.00648)	(0.00366)	(0.00376)	(0.00265)	(0.00203)	(0.00201)
Medium Education	-0.0656***	-0.0702***	-0.0704***	-0.0392***	-0.0394***	-0.0395***
	(0.00730)	(0.00711)	(0.00738)	(0.00203)	(0.00208)	(0.00208)
Age	-0.0151***	-0.0144***	-0.0142***	-0.0170***	-0.0167***	-0.0167***
	(0.00210)	(0.00178)	(0.00186)	(0.00151)	(0.00148)	(0.00147)
Age^2	0.000159***	0.000152***	0.000149***	0.000167***	0.000163***	0.000163***
	(2.63e-05)	(2.17e-05)	(2.28e-05)	(1.53e-05)	(1.48e-05)	(1.47e-05)
Constant	0.493***	0.595***	65.21**	0.451***	0.476***	12.04**
	(0.0413)	(0.0381)	(23.26)	(0.0318)	(0.0331)	(4.271)
Wild Bootstrap CI	[-0.054;-0.022]	[-0.041;-0.012]	[-0.043;-0.017]	[-0.012;-0.005]	[-0.011;-0.001]	[-0.013;-0.001]
Plant-size fixed effects	YES	YES	YES	YES	YES	YES
Year Fixed Effcts	YES	YES	YES	YES	YES	YES
Industry Fixed Effects	NO	YES	YES	NO	YES	YES
Industry Trends	NO	NO	YES	NO	NO	YES
Observations	45,571	45,103	45,103	519,207	513,112	513,112
R^2	0.042	0.069	0.072	0.036	0.046	0.047

Note.- This table contains the results of regressions of the main difference-in-differences estimation equation for the likelihood that a contract is fixed-term. Columns (1) to (3) restrict the sample to new contracts, while columns (4) to (6) are computed for the sample of all employment relationships.
Cluster-robust standard errors for firm size clusters in parentheses. $***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

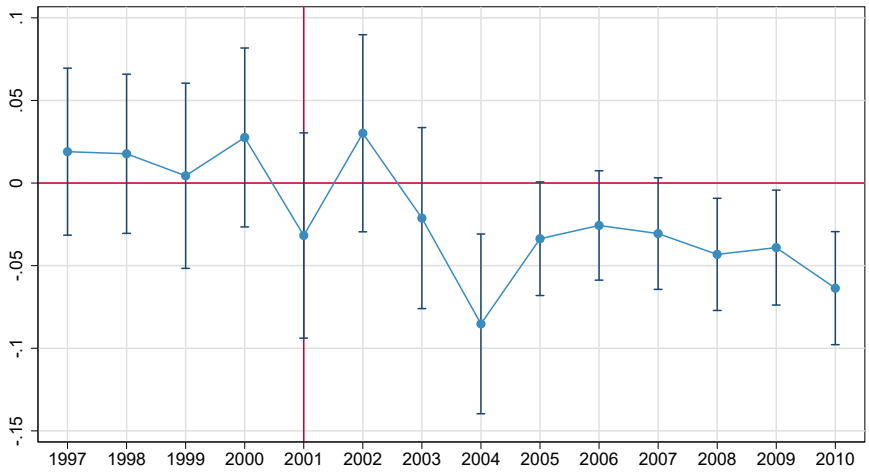
increases due to data limitations for firms above 10 Employees.[19]

To account for within plant-size dependence of hiring behavior, I cluster standard errors at the level of plant-size categories. Since the number of plant-size clusters is relatively small I also report wild bootstrap confidence intervals (Cameron et al., 2008) where appropriate.

The coefficient of -0.0404 in column (1) indicates a 4.04 p.p. decrease in the fixed-term share of new contracts in firms not subject to employment protection. Adding additional industry fixed effects reduces the estimated effect to 3 percentage points. Standard errors also remain stable and the estimated effect is highly statistically significant in all regressions. Furthermore, the effect remains stable at 3 percentage points, even if I allow for differential industry-specific trends over time in column (3). Since the average fixed-term share for new contracts before the reform was 35%, the effect represents an economically substantial 10 % decrease.

[19]I analyze plant-size restrictions and different levels for fixed-effects in the robustness section.

Figure 2.3: Coefficient plot for the dynamics of the reform



NOTE.- The figure plots the coefficient of interactions between years and the employment protection threshold indicator in a regression of the fixed-term status of a new contract on firm-size fixed effects, year-fixed effects, the respective interactions and a all control variables included in table 2.5.
Source: Mikrozensus sample for West German employees aged 20 to 65

The overall fixed-term share of contracts in treated firms is also declining accordingly. The coefficient of -0.00695 in Column (6) indicates a 0.696 p.p. decrease in firms affected by the reform. This effect is also sizable, as the average fixed-term share of all contracts in the sample is 9.9 %.

Next, I examine whether the effect of the reform is persistent or reverses after some time. For this I re-estimate the same regression as in Column (1) of Table 2.5, but replace the single difference-in-difference indicator with interactions between each calendar year and an indicator for plants that are not subject to employment protection. The coefficients for these interactions are plotted in figure 2.3.

For pre-reform years, the coefficients of the interactions of employment protection status and the calendar year are close to zero and statistically insignificant. Consequently the trends in both types of firms before the reform are largely parallel.

Interestingly, the reaction to the reform is slightly delayed, as the main decrease is from 2002 to 2003. After 2003 there is a clear difference in the development of the uptake of fixed-term contracts by employment protection status. The difference slightly recedes after 2005 but stays stable a about 3 percentage

points. Thus the reform is largely persistent at effect sizes similar to the simple difference-in-differences specification.

2.6.2 Employment

The main predictions of the theoretical model discussed in section 2.3 concern the destruction and creation of jobs. The basic model with equal bargaining power across both contract types, predicts both a decrease in the creation of fixed-term jobs and an increase in the conversion of jobs from fixed-term into open-ended contracts (see Table 2.2).

A decline in temporary job creation implies a decrease in the likelihood that a jobseeker will move out of unemployment. At the same time, the increase in the conversion of fixed-term contracts into permanent contracts should reduce the likelihood that employed individuals will become unemployed. Hence, I next examine the impact of the reform on flows into and out of unemployment.

As the Mikrozensus lacks the panel structure that would be necessary to analyze detailed employment flows, I use social security data, to estimate the reform effects on employment flows. I report these results in table 2.6. While column (1) reports results on the likelihood to switch from non-employment to employment, column (2) contains the results for the probability to switch from an employment into an non-employment spell. Thus, the first column is indicative about overall job creation under the new rules, while the second represents the impact on job destruction. The columns (3) and (4) are based on the same specification, but here the official unemployment status is used as the base category instead of non-employment. Therefore only job-seekers, who are officially registered as unemployed, are considered.

The $-0,0091$ coefficient in the (1) column indicates a 0.9 percentage point decrease in the likelihood that non-employed individuals moves into employment in plants that are not affected by employment protection legislation. Given that on average 5.9 % of the non-employed switch into employment per year, the effect of the reform on the transition from non-employment to employment is relatively small.

Table 2.6: Employment effects of the reform - IEB Results

	(1)	(2)	(3)	(4)
	flows from/to non-employment		flows from/to unemployment	
	job creation	job destruction	job creation	job destruction
No EPL×Post2001	-0.0091***	-0.0079***	-0.0037**	-0.0074***
	(0.00302)	(0.0023)	(0.00155)	(0.00162)
Female	-0.00436*	-0.00771***	-0.01102***	-0.00911***
	(0.00236)	(0.0015)	(0.00102)	(0.00096)
Medium Education	0.02646***	0.01971***	-0.01004***	0.01067***
	(0.00102)	(0.0013)	(0.00049)	(0.00049)
Age	-0.00891***	-0.0006**	-0.00393***	0.00141***
	(0.00023)	(0.00027)	(0.00015)	(0.00006)
Age^2	0.0000695***	0.0000059	0.0000539***	-0.00002***
	(0.0000028)	(0.0000035)	(0.0000016)	(0.0000007)
Constant	0.3104***	0.09301***	0.14889***	0.01401***
	(0.00494)	(0.00474)	(0.0032)	(0.00184)
Plant-size fixed effects	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES
Observations	3228829	3228829	2401018	3228829
R^2	0.02729	0.00459	0.00714	0.00683

Note.- This table contains the results of regressions of the main difference-in-differences specification on flows from non-employment to employment, employment to non-employment, unemployment to employment and employment to unemployment .
Cluster-robust standard errors for firm size clusters in parentheses.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

In addition column (2) also reports a 0.79 p.p. decrease in the probability to move from a job in a plant below the employment protection threshold into non-employment after the reform. Together with the results from column (1), this implies a negligible overall effect on the transition from non-employment to employment, since the effects in both columns are within a standard error range.

The results are very similar when I restrict the analysis to flows from and to unemployment. Here however, the flow from unemployment to employment exhibits a smaller decrease of only 0.37 percentage points, while the decrease in the transition from employment to unemployment is similarly sized at 0.74 percentage points. Although this suggests an overall positive effect on employment, the effect is again not economically significant.

In sum, the reform has only a minimal impact on employment flows. This suggests that the overall decrease in the temporary contract share is not only due to a decrease in job creation and an increase in contract conversion, but also that the

likelihood of starting a new job with a permanent contract has likely increased.[20]

2.6.3 Wages

I can use an identical approach to compute the wage effects of the reform. It is not clear from the outset, what wage effect should be expected for the reform. While theory predicts negative wage effects in a baseline model with equal rent sharing in both contract types, positive wage effects of the reform are also possible if workers have less bargaining power in fixed-term contracts. In this case, a lower probability that a new employment contract is fixed-term after the reform implies an increase in the value of the workers' outside option and thus also their wages. Given that the observed employment effects of the reform are small, while the decrease of the temporary contract share is large, the basic prerequisites for this case appear to be fulfilled. A post-reform increase in wages in treated plants would therefore suggest lower bargaining power in fixed-term contracts.

Table 2.7: Wage effects of the reform

	(1)	(2)	(3)	(4)	(5)	(6)
		New contracts			All contracts	
	Overall	Permanent	Fixed Term	Overall	Permanent	Fixed Term
No EPL×Post 2001	0.0233***	0.0146	0.0340**	0.00896***	0.00605*	0.00888
	(0.00701)	(0.00931)	(0.0116)	(0.00234)	(0.00301)	(0.00855)
Female	0.0931***	0.116***	0.0652**	0.00376	0.0409	0.0312
	(0.0248)	(0.0326)	(0.0227)	(0.0201)	(0.0317)	(0.0184)
Medium Education	0.225***	0.215***	0.205***	0.197***	0.199***	0.145***
	(0.00491)	(0.00528)	(0.0111)	(0.00427)	(0.00483)	(0.00700)
Age	0.0341***	0.0448***	0.00193	0.0450***	0.0505***	-0.0193***
	(0.00396)	(0.00163)	(0.00520)	(0.00259)	(0.00163)	(0.00260)
Age^2	-0.000394***	-0.000516***	-2.74e-05	-0.000458***	-0.000538***	0.000260***
	(4.95e-05)	(2.12e-05)	(6.70e-05)	(3.06e-05)	(2.04e-05)	(3.43e-05)
Constant	6.394***	6.162***	6.882***	6.301***	6.160***	7.394***
	(0.0842)	(0.0409)	(0.102)	(0.0660)	(0.0513)	(0.0575)
Wild Bootstrap CI	[0.008;0.043]	[-0.007;0.041]	[0.007;0.056]	[0.004;0.014]	[-0.002;0.014]	[-0.015;0.036]
Plant-size fixed effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	51,647	30,623	15,328	622,565	460,470	51,237
R^2	0.044	0.058	0.029	0.054	0.058	0.015

Note.- This table contains the results of regressions of the main difference-in-differences estimation equation for log net personal income. Cluster-robust standard errors for firm size clusters in parentheses.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

[20] In the theoretical framework this represents an increase in the parameter λ_E

Consequently, I analyze the impact of the reform on the wages for fixed-term and permanent employees, both for new contracts and for all employees. The results of the corresponding log wage regressions are shown in table 2.7. The first three columns contain regressions for workers in new contracts, with regression results for the overall wage effect of the reform in column (1). Furthermore, the regression in column (2) is restricted to new permanent contracts, while column (3) displays results for new temporary employees. Column (4) contains the results of a wage regression for all employees, while the sample for column (5) is confined to all permanent employees. Lastly, column (6) is computed for the sample of all fixed-term employees.

Overall, the results in column (1) indicate a highly statistically significant 2.33 % increase in wages for new contracts in the treatment group after the reform. Since the effect for the entire work-force reported in columns (4) is significantly smaller, the wage effects of the reform seam to be driven by new contracts.

The point estimates in columns (2) and (3) suggest that this wage increase is larger for fixed-term employees at 3.4 % than for permanent employees at 1.46 %. However, the coefficient for new permanent contracts is not measured precisely and it cannot even be excluded that the effect for open-ended contracts is zero. Moreover, the standard error is so large that the coefficient for open-ended contracts is within a distance of two standard errors from the coefficient for fixed-term contracts. Consequently, comparisons of the effect size between the contract types are not feasible.

2.6.4 Long-term outcomes for labor market entrants

So far I have analyzed the reform effects on the fixed-term share, employment and wages. Next, I abstract from the predictions of the theoretical framework and examine the long-term impact of the reform on labor market entrants. Questions about the long-term impact of starting a career in fixed-term work are often at the center of the policy debate on temporary work. In particular, the debate focuses on whether fixed-term contracts are a stepping stone to a permanent job or a dead-end for labor market entrants. Thus, I extend my analysis to examine how

entry into the labor market under the new fixed-term employment policy affects the long-term outcomes for post-reform entrants.

Table 2.8: Longterm effects of the reform

	(1) weeks non-employed	(2) weeks unemployed	(3) Log cumulative earnings	(4) Likelihood Same Employeer	(5) Number of Jobs
No EPL Entry × Post 2001 Entry	-3.0777***	-0.3138***	0.1404***	-0.0099***	-0.2288***
	(1.0957)	(0.0592)	(0.0091)	(0.0038)	(0.0207)
Female	-10.4925	-0.636	-0.2898	0.0328	-0.5964
	(0.5215)	(0.0292)	(0.0039)	(0.0017)	(0.01)
Medium Education	-31.2032	-0.5778	0.524	0.0495	0.3866
	(0.8681)	(0.041)	(0.005)	(0.002)	(0.0114)
Age	-3.858	-0.0178	0.0337	0.0131	0.2128
	(0.2313)	(0.0095)	(0.0012)	(0.0005)	(0.0027)
Age^2	0.0618	0.0004	-0.0005	-0.0002	-0.0029
	(0.00332)	(0.00014)	(0.00002)	(0.00001)	(0.00004)
Constant	123.205	3.7773	10.3459	-0.0979	-0.8147
	(4.0647)	(0.1762)	(0.022)	(0.0091)	(0.0527)
Plant-size at Entry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Entry Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	214833	214833	214804	214833	214833
R^2	0.0305	0.0147	0.0899	0.0230	0.0549

Note.- This table contains the results of regressions of the difference-in-differences specification for long-term effects of the reform on post-reform entrants. All columns refer to outcomes in the first five years after labor market entry.
Cluster-robust standard errors for firm size clusters in parentheses.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

Once again, I use social security data since it allows me to track individuals who have entered the labor market around the reform over time. In a similar Differences-in-Differences setup as before, I compare the difference in the outcomes of post and pre-reform entrants, who entered the labor market in plants below and above the 5 employee threshold. This allows me to analyze outcomes such as the likelihood to still be employed at the same employer as in the entry year, the cumulative earnings and times out of employment for entrants in the first five years after entry and the number of jobs in the first 5 years after entry.

I report the results of the respective regressions in Table 2.8. All regressions in this table are based on difference-in-differences specifications that compare outcomes of entry cohorts in the first 5 years after entry across the plant-size threshold in the entry year.

The outcome variable for the regression in column (1) is the number of weeks without employment in the first 5 years in the labor market, while column (2) reports

the effect on the official duration of unemployment over the same period. Column (3) contains results on log cumulative earnings. Moreover, the regression results for the probability to remain at the same employer over the entire 5 year period are in column (4). Lastly, column (5) reports the reform effect on the number of jobs.

Column (1) and (2) suggest that both the time out of employment and weeks of unemployment declined considerably for post-reform entrants. Moreover column (3) indicates a sizable increase in the earnings in the first five years of employment. Although the likelihood to remain at the same employer marginally declines, column (5) shows that the average number of jobs also declines. Together, the results suggest that the stricter fixed-term hiring rules for small plants led to an increase in job security and long-term wages for labor market entrants.

Table 2.9: Likelihood of long-term fixed-term employment for labor market entrants

	(1) Overall	(2) 2001 to 2005	(3) 2005 to 2010
No EPL × Entry after 2001	-0.0312***	-0.0405***	-0.0321***
	(0.00481)	(0.00889)	(0.00380)
Female	0.0218***	0.0165***	0.0222***
	(0.00229)	(0.00428)	(0.00185)
Age	-0.00748***	-0.00634*	-0.00789***
	(0.00213)	(0.00321)	(0.00192)
Age^2	9.19e-05***	7.44e-05*	9.61e-05***
	(2.52e-05)	(3.90e-05)	(2.32e-05)
Medium Education	-0.0375***	-0.0209	-0.127***
	(0.0115)	(0.0173)	(0.0318)
Constant	0.227***	0.199***	0.338***
	(0.0489)	(0.0613)	(0.0406)
Wild Bootstrap CI	[-0.041;-0.019]	[-0.065;-0.023]	[-0.040;-0.023]
Plant-size fixed effects	YES	YES	YES
Year fixed effects	YES	YES	YES
Entry Year fixed effects	YES	YES	YES
Observations	95,423	22,930	78,520
R^2	0.033	0.040	0.037

Note.- This table contains the results for the likelihood to stay fixed-term in later years than the reform year.
Cluster-robust standard errors for firm size clusters in parentheses.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

Since the social security data do not contain information on the type of contract for the relevant time-frame, I have to resort to information from the Mikrozensus to assess how the reform has affected the long-term likelihood of remaining fixed-term. Similarly to the other long-term outcomes, I use information on the first

year in the labor market to compare entry cohorts across firm sizes for later years. However, the Mikrozensus does not contain information on the firm-size at entry and I can not track individuals across time. Therefore, I compare individuals across their contemporaneous employment protection status and not the employment protection status at labor market entry.

Although this has the disadvantage that plants in later years could differ from the establishment at entry with regard to the employment protection status, this still gives me suggestive evidence for the long-term likelihood of remaining in temporary employment. The estimates for the long-term probability of staying fixed-term are in table 2.9.

The first column suggests that the probability to be fixed-term has decreased by 3.12 percentage points for workers who entered the labor market after 2001 and work in plants that are not subject to employment protection. Columns (2) and (3) contain the same estimation for two distinct time-periods. For column (1) this time-period is 2001 to 2005, whereas it is 2005 to 2010 for column (2). Splitting the sample over time shows, that this decrease in the likelihood to be fixed-term is roughly persistent.

2.7 Robustness

Next, I conduct some additional specification checks to assess the robustness of my findings. First, I provide a more direct analysis for the parallel trend assumption between plants that are affected by employment protection and those that are not in section 2.7.1. Second, I examine, whether deliberate changes in the plant-size in response to the reform pose a threat to my identification strategy in section 2.7.2. Finally, I discuss in section 2.7.3, how changed sample restrictions affect my results.

2.7.1 Parallel pre-trends

In addition to estimating leads and lags of the treatment indicator (see figure 2.3), I calculate placebo reform regressions on the likelihood to be fixed-term, where I shift the introduction year of the reform to years prior to the reform.

Table 2.10: Placebo tests for the main specification

	(1) 1997	(2) 1998	(3) 1999	(4) 2000
No EPL × Post Placebo Year	0.0156	0.00880	0.00827	0.0176
	(0.0118)	(0.0114)	(0.0113)	(0.0203)
No EPL × Post 2001	-0.0493***	-0.0465***	-0.0473***	-0.0564***
	(0.0101)	(0.0118)	(0.0117)	(0.0180)
Female	-0.00535	-0.00536	-0.00535	-0.00535
	(0.00648)	(0.00649)	(0.00649)	(0.00648)
Age	-0.0151***	-0.0151***	-0.0151***	-0.0151***
	(0.00210)	(0.00210)	(0.00210)	(0.00209)
Age^2	0.000159***	0.000159***	0.000159***	0.000159***
	(2.62e-05)	(2.63e-05)	(2.63e-05)	(2.62e-05)
Medium Education	-0.0655***	-0.0656***	-0.0656***	-0.0656***
	(0.00731)	(0.00730)	(0.00730)	(0.00730)
Constant	0.488***	0.491***	0.492***	0.492***
	(0.0415)	(0.0413)	(0.0422)	(0.0412)
Wild Bootstrap CI	[-0.012;0.045]	[-0.033;0.033]	[-0.014;0.074]	[-0.023;0.076]
Plant-size fixed effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
Observations	45,571	45,571	45,571	45,571
R^2	0.042	0.042	0.042	0.042

Note.- This table contains difference-in-differences regressions for placebo reforms for the years 1997 to 2000.
Cluster-robust standard errors for firm size clusters in parentheses. $***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

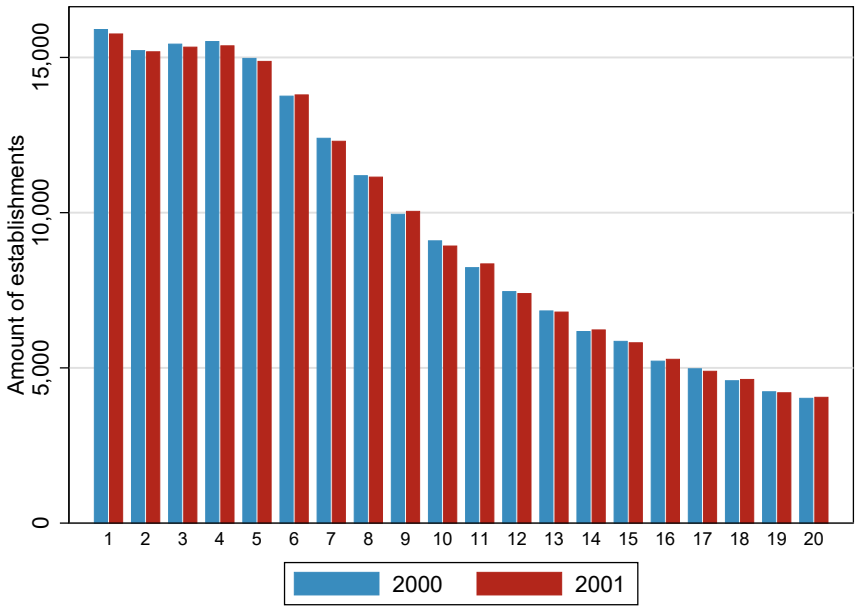
Table 2.10 shows the results for these regressions. All placebo reform indicators regardless of the placebo year are statistically insignificant and very close to zero. Taken together, this is further evidence for the validity of the parallel trend assumption.

2.7.2 Plant-size response of the reform

If plants react to the reform by changing their size around the employment protection threshold, there would be contagion of the treatment group into the control group. To assess this concern, I use data from the establishment history panel to analyze, how the plant-size distribution changed around the reform.

I start by plotting the plant-size distribution for the pre-reform year 2000 and the year 2001 in figure 2.4. For the sake of clarity, only firms up to a size of 20 employees are taken into account in this figure. That is sensible, as the relevant threshold at which manipulation was possible is 5 employees. In total, 60 % of all establishments in the data set are smaller than 20 employees.

Figure 2.4: Plant-size distribution before and after reform



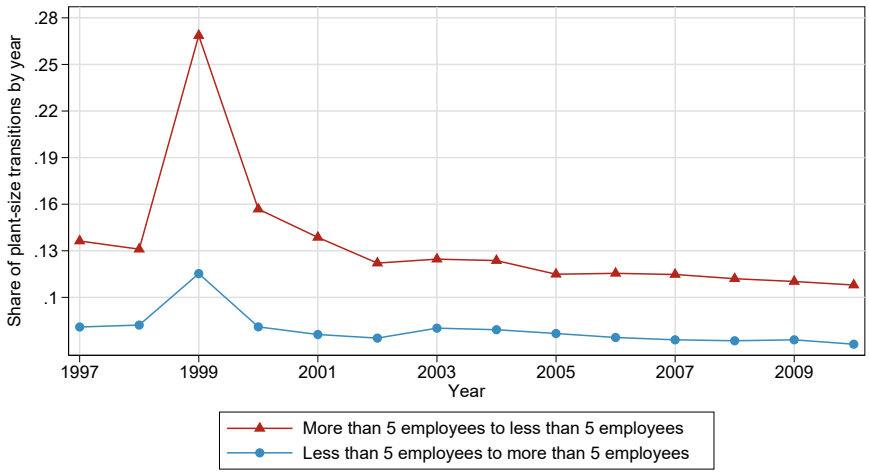
NOTE.- The figures plots the establishment-size distribution for the years 2000 and 2001 for establishments with less than 20 employees.
Source: Establishment history panel

The distribution is almost identical for the two years, hinting at only small changes in plant-size for existing establishments. However, since changes are still possible that would not be evident in the overall plant-size distribution, I next use the panel structure of the data, to examine whether the rates of transition around the plant-size threshold of 5 Employees was different in the reform year.

I plot two transition rates for the 2001 employment protection threshold of 5 employees in figure 2.5. The first is the share of all firms that had less than 5 employees in the previous year and more than 5 employees in the current year. The second one is the share of plants, which had more than 5 employees in the previous year but are smaller in the current year.

While there were substantial adjustments during the reform of the employment protection threshold in 1999, the transition rates in 2001, the year of the fixed-term contract reform, are roughly at their average level. Therefore, changes in plant-size around the fixed-term reform are not a cause for concern.

Figure 2.5: Plant-size transitions



NOTE.- The figures plots the transitions rates of firms with less than 5 employees in the previous year to more than 5 employees in the current year and vice versa.
Source: Establishment history panel

Nevertheless, I also compute some of my main results for subsamples that exclude plant-size-categories right at the employment protection threshold to further alleviate concerns about potential plant-size manipulation. For this I estimate the main regressions for the likelihood of being fixed-term with two restrictions. The first restriction is that i exclude plant-sizes of 4 or 9 employees. These are the plant-sizes right below the employment protection thresholds for different years in the sample. For the second restriction, I only consider the time period around the reform, during which the employment protection threshold was at 5 workers and exclude plants right at the threshold. The estimates for these regressions are in table 2.11.

Both restrictions have little effect on the outcome of the likelihood that a new contract is fixed-term. The difference-in-differences coefficient changes very little and is close to the original estimate. Moreover, the statistical significance of the results also remains largely unchanged.

This mostly also applies to results for the wage effect on new contracts. However, for the second restriction the wage effect becomes statistically insignificant. This is likely due to large loss of observations in this sub sample.

Table 2.11: Robustness: Excluding firms right at the threshold

	(1)	(2)	(3)	(4)	(5)	(6)
		Fixed Term			Log net income	
	All	Restriction 1	Restriction 2	All	Restriction 1	Restriction 2
No EPL × Post 2001	-0.0404***	-0.0405***	-0.0516***	0.0227***	0.0242***	0.00503
	(0.00566)	(0.00610)	(0.0143)	(0.00579)	(0.00642)	(0.0216)
Age	-0.0151***	-0.0155***	-0.0172***	0.0367***	0.0368***	0.0404***
	(0.00210)	(0.00233)	(0.00269)	(0.00300)	(0.00332)	(0.00463)
Age^2	0.000159***	0.000165***	0.000187***	-0.000428***	-0.000430***	-0.000449***
	(2.63e-05)	(2.91e-05)	(3.04e-05)	(3.70e-05)	(4.09e-05)	(5.83e-05)
Female	-0.00534	-0.00558	0.00273	0.111***	0.105***	0.0648**
	(0.00648)	(0.00695)	(0.00706)	(0.0302)	(0.0310)	(0.0279)
Medium Education	-0.0656***	-0.0658***	-0.0572***	0.228***	0.226***	0.176***
	(0.00730)	(0.00780)	(0.0137)	(0.00432)	(0.00481)	(0.0178)
Constant	0.493***	0.494***	0.565***	6.306***	6.301***	6.261***
	(0.0413)	(0.0467)	(0.0602)	(0.0757)	(0.0811)	(0.0990)
Wild Bootstrap CI	[-0.055;-0.023]	[-0.056;-0.025]	[-0.116;-0.019]	[0.010;0.036]	[0.012;0.042]	[-0.062;0.074]
Plant-size fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Observations	45,571	41,670	11,903	43,398	39,675	11,494
R^2	0.042	0.043	0.030	0.053	0.051	0.050

Note.- This table contains difference-in-differences regressions with different sample restrictions for firms right at the employment protection threshold. For Restriction 1 firms with 4 and 9 employees are excluded. For Restriction 2 the sample is limited to time-periods, when the employment protection bound was at 5 Employees and firms with 4 Employees are excluded. Cluster-robust standard errors for firm size clusters in parentheses.
$***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

In summary, deliberate manipulation of the plant-size around the reform does not seem to impair the validity of my estimates.

2.7.3 Additional Specification Checks

Finally, I also examine how the restriction of the sample to certain maximum plant-sizes and the specification of the plant fixed effects affect my findings. For this purpose, I explore two modifications to the main specification. For one, I limit the maximum plant-size in the sample to 20 or 10 employees.[21] And secondly, I calculate all estimates with more detailed fixed effects for plant-size categories.

In the baseline estimation the fixed-effects on plant-size categories are defined in size steps of 5 employees, whereas the detailed fixed-effects directly represent plant-sizes in the smallest available increment.

[21] When I restrict the maximum plant-size to 10 employees, I also restrict the observation period to years where the employment protection threshold was at 5 employees, as this restriction excludes all plants above the employment protection threshold for years where the threshold was at 10 employees.

Table 2.12: Robustness: Plant-size restrictions (Fixed-term share)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Standard Sample			Under 20 Employees			Under 10 Employees		
No EPL × Post 2001	-0.0404***	-0.0397***	-0.0300***	-0.0363***	-0.0357***	-0.0255**	-0.0350*	-0.0345*	-0.0406**
	(0.00566)	(0.00545)	(0.00536)	(0.00879)	(0.00863)	(0.00821)	(0.0168)	(0.0169)	(0.0162)
Female	-0.00534	-0.00455	-0.0509***	-0.0114	-0.0104	-0.0542***	-0.0128	-0.0129	-0.0413***
	(0.00648)	(0.00613)	(0.00370)	(0.00678)	(0.00632)	(0.00319)	(0.00952)	(0.00963)	(0.00564)
Age	-0.0151***	-0.0152***	-0.0144***	-0.0138***	-0.0138***	-0.0131***	-0.0203***	-0.0202***	-0.0203***
	(0.00210)	(0.00210)	(0.00178)	(0.00206)	(0.00205)	(0.00158)	(0.00364)	(0.00363)	(0.00338)
Age^2	0.000159***	0.000160***	0.000152***	0.000142***	0.000143***	0.000137***	0.000221***	0.000221***	0.000226***
	(2.63e-05)	(2.63e-05)	(2.18e-05)	(2.56e-05)	(2.56e-05)	(1.96e-05)	(4.17e-05)	(4.16e-05)	(3.81e-05)
Medium Education	-0.0656***	-0.0663***	-0.0707***	-0.0593***	-0.0602***	-0.0640***	-0.0398**	-0.0405**	-0.0406**
	(0.00730)	(0.00700)	(0.00696)	(0.00784)	(0.00746)	(0.00716)	(0.0139)	(0.0139)	(0.0135)
Constant	0.493***	0.457***	0.566***	0.464***	0.428***	0.538***	0.632***	0.627***	0.747***
	(0.0413)	(0.0416)	(0.0412)	(0.0377)	(0.0393)	(0.0383)	(0.0852)	(0.0849)	(0.116)
Wild Bootstrap CI	[-0.055;-0.020]	[-0.054;-0.021]	[-0.044;-0.013]	[-0.060;-0.005]	[-0.054;-0.007]	[-0.046;0.004]	[-0.076;0.005]	[-0.072;0.004]	[-0.075;-0.001]
Plant-size fixed effects	Standard	Detailed	Detailed	Standard	Detailed	Detailed	Standard	Detailed	Detailed
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry fixed effects	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	45,571	45,571	45,103	33,593	33,593	33,255	6,132	6,132	5,996
R^2	0.042	0.043	0.070	0.032	0.033	0.061	0.019	0.020	0.044

Note.- This table contains difference-in-differences regressions for the likelihood of a new contract being fixed term for different sub-samples of the data and different fixed effects specifications. For columns (4) to (7) the sample is restricted to firms with less than 20 Employees. The regressions in the last three columns include only firms with less than 10 Employees and time-periods, when the employment protection bound was at 5 Employees. Cluster-robust standard errors for firm size clusters in parentheses. $***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

The results for these specification checks for the likelihood that a new contract is fixed-term are in table 2.12. None of the specification changes has a large impact on the coefficient of interest. The estimated effect size remains roughly at 3 to 4 percentage points for any of the specifications. However the standard errors are larger in the sample, where I reduce the maximum plant size to 10 employees and the observed time-frame accordingly.

Moreover, the wage effect for new contracts also differs only minimally across the various specification checks. I present the results for these effects in table 2.A1 in the appendix. Again there are only marginal changes in the effect size across specifications.

In general, though, the specification checks suggest that my results are robust against changes in plant-size constraints and the use of more detailed fixed-effects.

2.8 Conclusion

This chapter examines how a 2001 reform that raised the legal requirements for the use of temporary contracts for small firms affected employment, wages and the take-up of fixed-term contracts.

I find a sizable decrease in the utilization of fixed-term contracts for affected plants, yet only negligibly small effects on employment. In line with the theoretical

predictions for a scenario in which workers have less bargaining power in fixed-term contracts and the proportion of new fixed-term contracts decreases, I also find positive reform effects on wages.

Furthermore, I also find some evidence that the reform has contributed to a reduction in labor market segmentation for new entrants. For labor market entrants, who joined affected firms after the reform year, I find an increase in cumulated wages in the first 5 years, as well as a reduction in the time out of work. In addition, I find suggestive evidence that the likelihood of remaining in temporary employment for longer periods of time is diminishing.

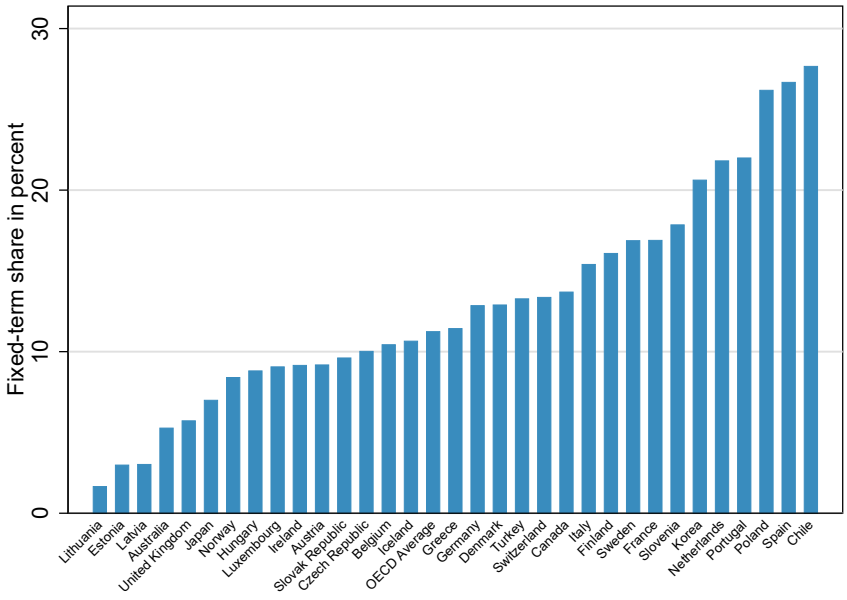
Together, my findings contribute to an economic literature that is critical of fixed-term employment as an exception to otherwise strict employment protection in permanent jobs. My findings suggest that restrictions on fixed-term contracts could be beneficial to workers, at least in the short term. However, this abstracts from the longer-term adaptations of firms to changed legislation. For example, firms could respond to increased labor costs by replacing labor with capital. Thus, longer-term adjustments are an interesting point for future research.

Lastly, my findings for a German restriction of fixed-term contracts from 2001 offer further insights for the current plans of the German government to limit fixed-term employment. Recently, the ruling coalition parties have agreed to reduce the maximum permissible duration of fixed-term contracts, to limit the number of renewals and to set a new quota for the maximum proportion of fixed-term employees for larger firms (Coalition Agreement, 2018). The debate that preceded these plans often focused on possible negative employment effects of a significant restriction on firms' use of temporary jobs.

Although the current German plans to limit the use of fixed-term contracts are more extensive than the previous reform, the results of this article at least suggest that fears about detrimental employment effects seem less warranted. However, whether the more broad new reforms will have similar outcomes will ultimately be a question for further research.

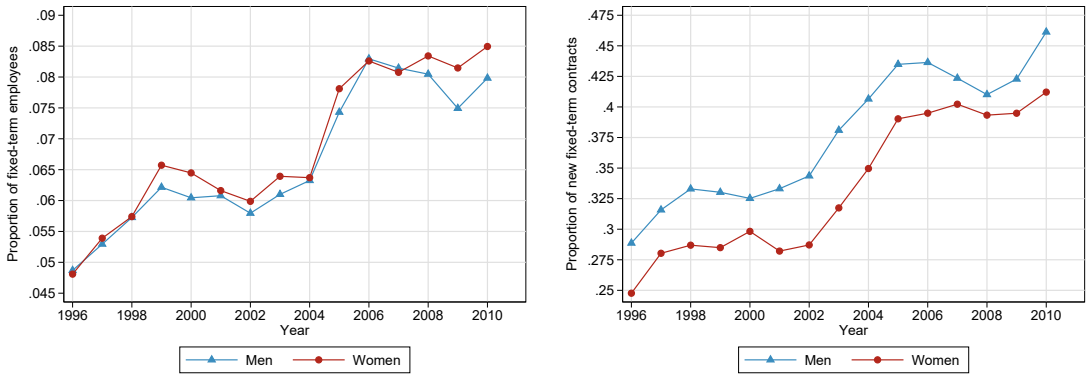
2.A Additional tables and figures

Figure 2.A1: Share of fixed-term employment in OECD countries 2017



NOTE.- The figures plots the share of employment contracts that are fixed-term for OECD countries
Source: OECD (2018), Temporary employment (indicator)

Figure 2.A2: Proportion of fixed-term contracts over time



NOTE.- The figures plots the proportion of fixed term contracts for all employees (left panel) and for all new contracts seperately for men and women
Source: Mikrozensus sample for West German employees aged 20 to 65

Table 2.A1: Robustness: Plant-size restrictions (Wages)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Standard Sample			Under 20 Employees			Under 10 Employees		
No EPL × Post 2001	0.0227***	0.0227***	0.0150**	0.0257***	0.0256***	0.0173**	0.0302**	0.0292**	0.0223*
	(0.00579)	(0.00581)	(0.00582)	(0.00716)	(0.00715)	(0.00739)	(0.0102)	(0.00999)	(0.0110)
Female	0.111***	0.111***	0.160***	0.140***	0.141***	0.189***	0.130***	0.131***	0.151***
	(0.0302)	(0.0305)	(0.0292)	(0.0324)	(0.0328)	(0.0307)	(0.0239)	(0.0242)	(0.0285)
Age	0.0367***	0.0367***	0.0342***	0.0398***	0.0397***	0.0374***	0.0477***	0.0473***	0.0459***
	(0.00300)	(0.00297)	(0.00313)	(0.00259)	(0.00255)	(0.00281)	(0.00441)	(0.00429)	(0.00412)
Age^2	-0.000428***	-0.000427***	-0.000400***	-0.000465***	-0.000463***	-0.000438***	-0.000538***	-0.000532***	-0.000521***
	(3.70e-05)	(3.63e-05)	(3.84e-05)	(3.28e-05)	(3.18e-05)	(3.55e-05)	(5.52e-05)	(5.33e-05)	(5.17e-05)
Medium Education	0.228***	0.227***	0.203***	0.229***	0.228***	0.196***	0.160***	0.159***	0.120***
	(0.00432)	(0.00447)	(0.00727)	(0.00527)	(0.00565)	(0.00988)	(0.0253)	(0.0257)	(0.0248)
Constant	6.306***	6.278***	6.212***	6.226***	6.193***	6.116***	6.116***	6.091***	6.033***
	(0.0757)	(0.0791)	(0.0972)	(0.0640)	(0.0660)	(0.0771)	(0.0860)	(0.0854)	(0.0878)
Wild Bootstrap CI	[0.011;0.038]	[0.010;0.038]	[0.003;0.030]	[0.011;0.052]	[0.013;0.053]	[0.002;0.045]	[0.007;0.057]	[0.005;0.057]	[-0.004;0.050]
Plant-size fixed effects	Standard	Detailed	Detailed	Standard	Detailed	Detailed	Standard	Detailed	Detailed
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry fixed effects	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	43,398	43,398	42,937	31,996	31,996	31,661	5,953	5,953	5,818
R^2	0.053	0.053	0.078	0.060	0.060	0.089	0.059	0.060	0.096

Note.- This table contains difference-in-differences regressions for the log-personal income for new contracts for different sub-samples of the data and different fixed effects specifications. For columns (4) to (7) the sample is restricted to firms with less than 20 Employees. The regressions in the last three columns include only firms with less than 10 Employees and time-periods, when the employment protection bound was at 5 Employees. Cluster-robust standard errors for firm size clusters in parentheses. $***$ $p < 0.01$, $**$ $p < 0.05$, $*$ $p < 0.1$

2.B Model derivations

In this appendix, I will provide further details of the theoretical framework set out in section 3. In particular, I provide detailed definitions of the surplus of creating jobs by contract type (2.B.1). Moreover, I discuss the comparative statics of an increase in contract writing costs for temporary contracts for both the case of equal bargaining power (2.B.2) across contract types and lower bargaining power (2.B.3) in fixed-term contracts.

2.B.1 Expected surplus for job creation

The expected surplus for creating a job of type λ is given by the sum of the associated expected profit of the firm and the workers valuation of the match minus the workers outside option. For a permanent contract the expected profit of the firm is given by

$$\Pi_P(\lambda) = \int_0^\infty \left[\int_0^\tau [y - w(\lambda)] e^{-rt} dt - f e^{-r\tau} \right] \lambda e^{-\lambda\tau} d\tau - c_P = \frac{y - w(\lambda) - \lambda f}{r + \lambda} - c_P, \tag{2.8}$$

where the inner integral represents the discounted sum of expected profits until a random termination date τ, while the term $fe^{-r\tau}$ is the discounted value of the firing costs at this date. The whole expression in the brackets is then integrated over the poisson process density $\lambda e^{-\lambda\tau}$ that determines at which date τ a job of type λ becomes unproductive.

Similarly a workers valuation of a job is given by

$$V_P(\lambda) = \int_0^\infty \left[\int_0^\tau w(\lambda) e^{-rt} dt + U e^{-r\tau} \right] \lambda e^{-\lambda\tau} d\tau = \frac{w(\lambda) + \lambda U}{r + \lambda}, \tag{2.9}$$

where U is the workers valuation of the outside option of the match. The surplus for a permanent contract is then given by

$$S_P(\lambda) = \frac{y - rU - \lambda f}{r + \lambda} \tag{2.10}$$

The expected profit of a firm for a fixed term contract is the sum of the discounted profit flow up to an endogenous date $D(\lambda)$ and the discounted value of continuing

in a permanent contract at time D less the cost of writing the contract c_{FT}.

$$\Pi_{FT}(\lambda, D) = \int_0^D [ye^{-\lambda\tau} - w(\lambda, D)]e^{-r\tau}d\tau + \max(\Pi_P(\lambda), 0) \cdot e^{-\lambda D} \cdot e^{-rD} - c_{FT} \quad (2.11)$$

Note that in the discounted flow of profits up to D only the productivity y is evaluated at its survival probability, while wages are only discounted with r. This reflects that employers have to keep paying the wage until D if the productivity shock arrives before the expiration date of the contract. Moreover, the continuation value is simply the discounted maximum of either the permanent contract profit for the same job-type λ or 0.

Similarly a workers valuation of of a new fixed-term contract is given by

$$V_T(\lambda, D) = \int_0^D w(\lambda, D)e^{-r\tau}d\tau + \max(V_P(\lambda), U) \cdot e^{-\lambda D} \cdot e^{-rD} + U(1 - e^{-\lambda D})e^{-rD} \quad (2.12)$$

Lastly, just as for an open-ended contract, the expected surplus for a fixed-term contract is defined as the sum of the firms expected profit and the workers' valuation less her external option.

$$S_{FT}(\lambda, D) = \int_0^D \left[ye^{-\lambda\tau} - rU\right] e^{-r\tau}d\tau + \max(S_P(\lambda), 0) \cdot e^{-(r+\lambda)D} - c_{FT} \quad (2.13)$$

In the equilibrium workers and firms will choose the optimal duration $D^*(\lambda)$, since it maximizes the expected surplus for a contract given λ. Hence $D^*(\lambda)$ is obtained from the first order condition of equation 2.13 with regard to duration D:

$$\frac{\partial S_T(\lambda, D)}{\partial D} = ye^{-\lambda D} - rU - (r + \lambda)e^{-\lambda D}\max(S_P(\lambda), 0) = 0 \quad (2.14)$$

2.B.2 Case 1: Equal bargaining power across contract types

In the equilibrium four conditions are satisfied.

First, jobs are only created if the surplus of a temporary employment contract is greater than zero. The parameter λ_{FT} is the maximum shock arrival rate at which jobs can be created and is determined by the point at which the surplus of a

temporary contract with optimal contract duration is zero. To obtain this expression, I substitute the first-order condition of the fixed-term contract surplus with regard to the duration D from equation 2.14 into $S_{FT}(\lambda_{FT}) = 0$. As a result, the equilibrium condition on the fixed-term job creation $h^{FTJCR}(\lambda_{FT}, \theta, c_{FT})$ is given by

$$h^{FTJCR}(\lambda_{FT}, \theta, c_{FT}) = \frac{y - rU(\theta)}{r + \lambda_{FT}} + \lambda_{FT} \frac{U(\theta)\left(e^{-rD^*(\lambda_{FT})} - 1\right)}{r + \lambda_{FT}} - c_{FT} = 0. \tag{FTJCR}$$

Second, as jobs with a shock arrival rate above λ_P are not continued after the end of a temporary contract, the fixed-term job destruction is obtained from the point at which the surplus of a permanent contract is zero $S_P(\lambda_P) = 0$.

$$h^{FTJDR}(\lambda_P, \theta) = \lambda_P - \frac{y - rU(\theta) - rc_P}{c_P + f} = 0 \tag{FTJDR}$$

Third, the parameter λ_E, which specifies whether jobs start with a fixed-term or permanent contract, is obtained by equating the surplus of a fixed-term contract at optimal duration with the surplus of an unlimited contract.

$$h^{PvsFT}(\lambda_E, \theta, c_{FT}) = \lambda_E \frac{U(\theta)\left(e^{-rD^*(\lambda_E)} - 1\right)}{r + \lambda_E} + \frac{\lambda_E f}{r + \lambda_E} + (c_P - c_{FT}) = 0 \tag{PvsFT}$$

Lastly, the fourth condition is a free entry condition for firms and equalizes the expected surplus of a job with the costs of its creation κ.

$$h^{EC}(\theta, c_{FT}) = \kappa - q(\theta)(1 - \gamma)\left[\int_{\underline{\lambda}}^{\lambda_E} S_p(\lambda) dG(\lambda) + \int_{\lambda_E}^{\lambda_T} S_T(\lambda) dG(\lambda)\right] = 0 \tag{EC}$$

The valuation of unemployment in the first three equations $U(\theta)$ is an increasing function in the labor market tightness theta and is given by equation 2.2. By substituting this in the three conditions, I obtain an equilibrium with the parameters $(\theta^*, \lambda_E^*, \lambda_P^*, \lambda_{FT}^*)$.

Next I obtain the effect on an increase in c_{FT} on these parameters by using total differentials of the equilibrium conditions

1. *Impact on job creation:* The effect on λ_{FT} is calculated from total differentials of the free-entry condition and the fixed-term job creation rule.

$$\frac{\partial \lambda_{FT}}{\partial c_{FT}} = \underbrace{-\frac{\frac{\partial h^{FTJCR}}{\partial c_{FT}}}{\frac{\partial h^{FTJCR}}{\partial \lambda_{FT}}}}_{\text{Direct Effect}} + \underbrace{\frac{\frac{\partial h^{FTJCR}}{\partial \theta}}{\frac{\partial h^{FTJCR}}{\partial \lambda_{FT}}} \times \frac{\frac{\partial h^{EC}}{\partial c_{FT}}}{\frac{\partial h^{EC}}{\partial \theta}}}_{\text{Equilibrium Feedback Effect}} \tag{2.15}$$

First, note that the derivatives for the fixed-term job creation rule with regard to c_{FT}, θ and λ_{FT} are:

$$\begin{aligned}
\frac{\partial h^{FTJCR}(\lambda_{FT},\theta,c_{FT})}{\partial c_{FT}} &= -1 < 0 \\
\frac{\partial h^{FTJCR}(\lambda_{FT},\theta,c_{FT})}{\partial \theta} &= \frac{\partial U}{\partial \theta}\left[\frac{-r}{r+\lambda_{FT}} + \frac{\lambda_{FT}(e^{-rD^*}-1)}{r+\lambda_{FT}}\right] \\
&= -\frac{\partial U}{\partial \theta}\left[\frac{r}{r+\lambda_{FT}} + \frac{\lambda_{FT}(1-e^{-rD})}{r+\lambda_{FT}}\right] < 0 \\
\frac{\partial h^{FTJCR}(\theta,\lambda_{FT},c_{FT})}{\partial \lambda_{FT}} &= \frac{ye^{-(r+\lambda_{FT})D^*(\lambda_{FT})}[(r+\lambda_{FT})D^*(\lambda)+1]-y}{(r+\lambda_{FT})^2} < 0
\end{aligned}$$

The expression for $\frac{\partial h^{FTJCR}(\theta,\lambda_{FT},c_{FT})}{\partial \lambda_{FT}}$ is calculated using a derivative of $S_{FT}=0$ with regard to $\lambda_F T$ and $\lambda_P > \lambda_{FT}$. The negative sign of the expression stems from the fact that $e^{-x} < \frac{1}{x+1}$ for $x>0$:

$$\frac{ye^{-(r+\lambda_{FT})D^*(\lambda_{FT})}[(r+\lambda_{FT})D^*(\lambda)+1]-y}{(r+\lambda_{FT})^2} < \frac{y\,\frac{1}{(r+\lambda_{FT})D^*(\lambda)+1}\,[(r+\lambda_{FT})D^*(\lambda)+1]-y}{(r+\lambda_{FT})^2} = 0$$

Second, the derivatives of the free-entry condition are:

$$\begin{aligned}
\frac{\partial h^{EC}(\theta,c_{FT})}{\partial c_{FT}} &= -q(\theta)(1-\gamma)\left[\int_{\underline{\lambda}}^{\lambda_E}\frac{\partial S_P(\lambda)}{\partial c_{FT}}dG(\lambda) + \int_{\underline{\lambda}}^{\lambda_E}\frac{\partial S_{FT}(\lambda)}{\partial c_{FT}}dG(\lambda)\right] \\
&= -q(\theta)(1-\gamma)\left[\int_{\lambda_E}^{\lambda_{FT}} -1\; dG(\lambda)\right] \\
&= q(\theta)(1-\gamma)\Big[G(\lambda_{FT}) - G(\lambda_E)\Big] > 0 \\
\frac{\partial h^{EC}(\theta,c_{FT})}{\partial \theta} &= -(1-\gamma)q'(\theta)\Big[\int_{\underline{\lambda}}^{\lambda_E} S_p(\lambda)dG(\lambda) + \int_{\lambda_E}^{\lambda_{FT}} S_{FT}(\lambda)dG(\lambda)\Big] \\
&\quad -(1-\gamma)q(\theta)\Big[\int_{\underline{\lambda}}^{\lambda_E}\frac{\partial S_P(\lambda)}{\partial \theta}dG(\lambda) + \int_{\lambda_E}^{\lambda_{FT}}\frac{\partial S_{FT}(\lambda)}{\partial \theta}dG(\lambda)\Big] > 0
\end{aligned}$$

The positive sign in the derivative with regard to θ follows from the definition of $q(\theta)$ as decreasing function of θ and from the negative signs of both $\frac{\partial S_P(\lambda)}{\partial \theta}$ and $\frac{\partial S_{FT}(\lambda)}{\partial \theta}$.[22]

[22]Since $S_P(\lambda) = \frac{y-rU-\lambda f}{r+\lambda}$ it follows that $\frac{\partial S_p(\lambda)}{\partial U} < 0$. Moreover the value of U is increasing in labor market tightness. Therefore it holds that: $\frac{\partial S_p(\lambda)}{\partial \theta} = \frac{\partial S_p(\lambda)}{\partial U(\theta)} \cdot \frac{\partial U(\theta)}{\partial \theta} < 0$. Similarly, it also applies that $\frac{\partial S_{FT}(\lambda)}{\partial U(\theta)} < 0$, which implies $\frac{\partial S_{FT}(\lambda)}{\partial \theta} < 0$

Together, this implies a negative direct effect of an increase of c_{FT} and a positive feedback effect. Saggio et al. (2018) show that the direct effect dominates. Thus, the overall effect of an increase of the contract writing costs on job creation is negative.

2. *Impact on job destruction:* The total differential of the fixed-term job destruction rule is

$$\frac{\partial h^{FTJDR}}{\partial \lambda_P} d\lambda_P + \frac{\partial h^{FTJDR}}{\partial \theta} d\theta = 0 \tag{2.16}$$

Since $\frac{\partial h^{FTJDR}}{\partial \lambda_P} = 1$ and $\frac{\partial h^{FTJDR}}{\partial \theta} = \frac{r}{c_P + f} \frac{\partial U(\theta)}{\partial \theta}$ this implies that the effect of the reform on the conversion of fixed-term contracts to permanent is given by:

$$\frac{\partial \lambda_P}{\partial c_{FT}} = \frac{r}{c_P + f} \frac{\partial U(\theta)}{\partial \theta} \frac{\partial \theta}{\partial c_{FT}} > 0 \tag{2.17}$$

As fewer temporary jobs are converted into open-ended contracts and are terminated instead when λ_P rises, job destruction also increases.

3. *Impact on the type of contract at job start:* From the total differential of $h^{PvsFT}(\lambda_E, \theta, c_{FT})$ it follows that:

$$\frac{\partial \lambda_E}{\partial c_{FT}} = - \underbrace{\frac{\frac{\partial h^{PvsFT}}{\partial c_{FT}}}{\frac{\partial h^{PvsFT}}{\partial \lambda_E}}}_{\text{Direct Effect}} - \underbrace{\frac{\frac{\partial h^{PvsFT}}{\partial \theta}}{\frac{\partial h^{PvsFT}}{\partial \lambda_E}} \times \frac{\partial \theta}{\partial c_{FT}}}_{\text{Feedback Effect}} \tag{2.18}$$

The derivatives used in equation 2.18 are

$$\begin{aligned}
\frac{\partial h^{PvsFT}(\lambda_E, \theta, c_{FT})}{\partial c_{FT}} &= -1 < 0 \\
\frac{\partial h^{PvsFT}(\lambda_E, \theta, c_{FT})}{\partial \lambda_E} &= \frac{-\lambda_E f r \frac{\partial D^*}{\partial \lambda}(\lambda_E) e^{-rD^*(\lambda_E)}}{(1 - e^{-rD^*(\lambda_E)})(r + \lambda_E)} \geq 0 \\
\frac{\partial h^{PvsFT}(\lambda_E, \theta, c_{FT})}{\partial \theta} &= -\lambda_E \frac{\partial U(\theta)}{\partial \theta} \frac{1 - e^{-rD^*(\lambda_E)}}{r + \lambda_E} < 0
\end{aligned}$$

There are again a direct effect and feedback effect on λ_E, albeit the overall effect remains undetermined this time. However, Saggio et al. (2018) show that the direct effect is larger then the feedback effect if

$$\lambda_T < \frac{y - r c_{FT}}{f + c_{FT}} \tag{2.19}$$

4. *Effect on wages:* Wages are only affected through the valuation of the outside option. The derivatives of wages with regard to the cost of writing fixed-term contracts are:

$$\frac{\partial w_P}{\partial c_{FT}} = (1-\gamma)r\frac{\partial U(\theta)}{\partial \theta}\frac{\partial \theta}{\partial c_{FT}} < 0$$
$$\frac{\partial w_{FT}}{\partial c_{FT}} = \frac{ry}{r+\lambda}\left[-\frac{re^{rD^*}(1-e^{-(r+\lambda)D^*})}{(1-e^{rD^*})^2} + \frac{(r+\lambda)e^{-(r+\lambda)D^*}}{(1-e^{rD^*})}\right]\frac{\partial D^*}{\partial U}\frac{\partial U(\theta)}{\partial \theta}\frac{\partial \theta}{\partial c_{FT}}$$
$$+ (1-\gamma)r\frac{\partial U(\theta)}{\partial \theta}\frac{\partial \theta}{\partial c_{FT}} < 0$$

2.B.3 Case 2: Differential bargaining power across contract types

I now consider the case in which workers in temporary contracts can only extract a lower rent from the match-surplus compared to permanent contracts. In this case, the equilibrium conditions remain largely similar to the case of equal rent sharing. However, as discussed in the main text, the valuation of the outside option in equation 2.3 now also depends on λ_E. Thus the changed equilibrium conditions for this case are

$$h^{FTJCR2}(\lambda_{FT}, \lambda_E, \theta, c_{FT}) = \frac{y - rU(\theta, \lambda_E)}{r+\lambda_{FT}} + \lambda_T \frac{U(\theta, \lambda_E)\left(e^{-rD^*(\lambda_{FT})} - 1\right)}{r+\lambda_{FT}} - c_{FT} = 0 \quad \text{(FTJCR2)}$$

$$h^{FTJDR2}(\lambda_P, \lambda_E, \theta) = \lambda_P - \frac{y - rU(\theta, \lambda_E) - rc_P}{c_P + f} = 0 \quad \text{(FTJDR2)}$$

$$h^{PvsFT2}(\lambda_E, \theta, c_{FT}) = \lambda_E \frac{U(\theta, \lambda_E)\left(e^{-rD^*(\lambda_E)} - 1\right)}{r+\lambda_E} + \frac{\lambda_E f}{r+\lambda_E} + (c_P - c_{FT}) = 0 \quad \text{(PvsFT2)}$$

$$h^{EC2}(\theta, \lambda_E, c_{FT}) = \kappa - q(\theta)(1-\gamma_P)\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda)dG(\lambda)$$
$$- q(\theta)(1-\gamma_{FT})\int_{\lambda_E}^{\lambda_{FT}} S_{FT}(\lambda)dG(\lambda) = 0 \quad \text{(EC2)}$$

Next, I again use total differentials of these conditions to derive the effects of the reform on the parameters of interest. In a first step, I separately evaluate the reform effect on λ_E and θ if the respective other parameter remains constant. I use a total differential of $h^{EC2}(\theta, \lambda_E, c_{FT})$ to evaluate the reform effect on labor market

tightness. For a given level of λ_E the effect of an increase in c_{FT} on θ is negative as

$$\frac{\partial \theta}{\partial c_{FT}} \mid_{\lambda_E \text{ is constant}} = -\frac{\frac{\partial h^{EC2}}{\partial c_{FT}}}{\frac{\partial h^{EC2}}{\partial \theta}} < 0 \tag{2.20}$$

This results from the positive sign of both the numerator and the denominator

$$\begin{aligned}
\frac{\partial h^{EC2}(\theta, \lambda_E, c_{FT})}{\partial c_{FT}} &= q(\theta)(1-\gamma)\Big[G(\lambda_{FT}) - G(\lambda_E)\Big] > 0 \\
\frac{\partial h^{EC2}(\theta, \lambda_E, c_{FT})}{\partial \theta} &= -q'(\theta)\left[(1-\gamma_P)\int_{\underline{\lambda}}^{\lambda_E} S_P(\lambda) dG(\lambda) + (1-\gamma_{FT})\int_{\lambda_E}^{\lambda_{FT}} S_{FT}(\lambda) dG(\lambda)\right] \\
&\quad + q(\theta)\Big[(1-\gamma_P)\int_{\underline{\lambda}}^{\lambda_E} \frac{\partial S_P(\lambda)}{\partial \theta} dG(\lambda) + (1-\gamma_{FT})\int_{\lambda_E}^{\lambda_{FT}} \frac{\partial S_{FT}(\lambda)}{\partial \theta} dG(\lambda)\Big] > 0
\end{aligned}$$

Similarly, a total differential of $h^{PvsFT2}(\lambda_E, \theta, c_{FT})$ can be used to evaluate the change in λ_E for an unchanged level of labor market tightness.

$$\frac{\partial \lambda_E}{\partial c_{FT}} \mid_{\theta \text{ is constant}} = -\frac{\frac{\partial h^{PvsFT2}}{\partial c_{FT}}}{\frac{\partial h^{PvsFT2}}{\partial \lambda_E}} > 0 \tag{2.21}$$

This results stems from the different signs of the two derivatives used

$$\begin{aligned}
\frac{\partial h^{PvsFT2}(\lambda_E, \theta, c_{FT})}{\partial c_{FT}} &= -1 < 0 \\
\frac{\partial h^{PvsFT2}(\lambda_E, \theta, c_{FT})}{\partial \lambda_E} &= \frac{-\lambda_E f r \frac{\partial D^*}{\partial \lambda}(\lambda_E) e^{-rD^*(\lambda_E)}}{(1-e^{-rD^*(\lambda_E)})(r+\lambda_E)} > 0
\end{aligned}$$

Thus, an increase in the contract writing costs for fixed-term contracts leads to a higher proportion of jobs that start in permanent contracts, if the effect of labor market tightness is not taken into account. Together with the above result on the effect of changes on c_{FT} on labor market tightness this shows that that reactions to the reform on λ_E and θ counteract each other. However, labor market tightness still overall decreases, while the effects on the valuation of unemployment and λ_E are not clear from the outset.[23]

Next, I use these results to analyze job creation, job destruction, the likelihood that a job starts on a permanent contract and wages.

[23] See Saggio et al. (2018) for a detailed discussion and a graphical representation of the interaction of λ_E and θ.

1. *Impact on job creation:* Using the total differential of $h^{FTJCR2}(\lambda_{FT}, \lambda_E, \theta, c_{FT})$ reveals a direct effect of c_{FT} on λ_{FT} and two feedback effects through λ_E and θ.

$$\frac{\partial \lambda_{FT}}{\partial c_{FT}} = -\frac{\frac{\partial h^{FTJCR2}}{\partial c_{FT}}}{\frac{\partial h^{FTJCR2}}{\partial \lambda_{FT}}} - \frac{\frac{\partial h^{FTJCR2}}{\partial \theta}}{\frac{\partial h^{FTJCR2}}{\partial \lambda_{FT}}} \frac{\partial \theta}{\partial c_{FT}} - \frac{\frac{\partial h^{FTJCR2}}{\partial \lambda_E}}{\frac{\partial h^{FTJCR2}}{\partial \lambda_{FT}}} \frac{\partial \lambda_E}{\partial c_{FT}} \tag{2.22}$$

with

$$\frac{\partial h^{FTJDR2}(\lambda_{FT}, \lambda_E, \theta, c_{FT})}{\partial \lambda_{FT}} = \frac{y e^{-(r+\lambda_{FT})D^*(\lambda_{FT})}[(r+\lambda_{FT})D^*(\lambda)+1] - y}{(r+\lambda_{FT})^2} < 0$$

$$\frac{\partial h^{FTJDR2}(\lambda_{FT}, \lambda_E, \theta, c_{FT})}{\partial \lambda_E} = -\frac{\partial U}{\partial \lambda_E}\left[\frac{r}{r+\lambda_{FT}} + \frac{\lambda_{FT}(1-e^{-rD})}{r+\lambda_{FT}}\right] < 0$$

$$\frac{\partial h^{FTJDR2}(\lambda_{FT}, \lambda_E, \theta, c_{FT})}{\partial \theta} = -\frac{\partial U}{\partial \theta}\left[\frac{r}{r+\lambda_{FT}} + \frac{\lambda_{FT}(1-e^{-rD})}{r+\lambda_{FT}}\right] < 0$$

$$\frac{\partial h^{FTJDR2}(\lambda_{FT}, \lambda_E, \theta, c_{FT})}{\partial c_{FT}} = -1 < 0$$

Thus, the first two terms still represent a direct effect that leads to a decrease in λ_{FT} for an increase in c_{FT} and a feedback effect that increases λ_{FT} through a decline in the outside option as the labor market becomes less tight. However the third term is a feedback effect that acts through changes in the likelihood that new jobs are started with permanent contracts. If the reform leads to an increase in λ_E this second term acts in the same direction as the direct effect, as the increase in the share of new permanent contracts increases the outside option which in turn leads to a decline in λ_{FT}. Moreover, Saggio et al. (2018) show that in this case the expected overall effect is the same both for differential and equal rent-sharing. However, if λ_E decreases the overall effect on λ_{FT} is not clear.

2. *Impact on job destruction:* Using a total differential of the fixed-term job destruction rule yields an indeterminate effect of the reform on λ_P

$$\frac{\partial \lambda_P}{\partial c_{FT}} = \frac{r}{c_P + f}\left[\frac{\partial U(\theta, \lambda_E)}{\partial \theta}\frac{\partial \theta}{\partial c_{FT}} + \frac{\partial U(\theta, \lambda_E)}{\partial \lambda_E}\frac{\partial \theta}{\partial \lambda_E}\right] \lesseqgtr 0 \tag{2.23}$$

3. *Impact on the type of contract at job start:* Similarly to the other case, a combination of the free-entry condition (EC2) and the contract type rule (PvsFT2) allow for a wide range of decreases and increases in λ_E.

4. *Effect on wages:* Wages are only affected through the valuation of the outside option. The derivatives of wages with regard to the cost of writing fixed-term contracts are:

$$\frac{\partial w_P}{\partial c_{FT}} = (1-\gamma)r\left[\frac{\partial U(\theta,\lambda_E)}{\partial \theta}\frac{\partial \theta}{\partial c_{FT}} + \frac{\partial U(\theta,\lambda_E)}{\partial \lambda_E}\frac{\partial \theta}{\partial \lambda_E}\right] \lesseqgtr 0$$

$$\frac{\partial w_{FT}}{\partial c_{FT}} = \left(\frac{ry}{r+\lambda}\left[-\frac{re^{rD^*}(1-e^{-(r+\lambda)D^*})}{(1-e^{rD^*})^2} + \frac{(r+\lambda)e^{-(r+\lambda)D^*}}{(1-e^{rD^*})}\right]\frac{\partial D^*}{\partial U} + (1-\gamma)r\right)$$
$$\times\left[\frac{\partial U(\theta,\lambda_E)}{\partial \theta}\frac{\partial \theta}{\partial c_{FT}} + \frac{\partial U(\theta,\lambda_E)}{\partial \lambda_E}\frac{\partial \theta}{\partial \lambda_E}\right] \lesseqgtr 0$$

Contrary to the other case, if the overall reform effect on the outside option is positive, positive wage effects are also possible.

Chapter 3

Evolution of the East German Wage Structure[1]

3.1 Introduction

Wage inequality has been on the rise in many countries over the past decades, drawing a lot of attention among scholars and the public alike. Most of the attention has focused on the United States (see the overviews in Katz and Murphy, 1992; Bound and Johnson, 1992; Card and DiNardo, 2002; Lemieux, 2006; Autor et al., 2008; Acemoglu and Autor, 2011). The dispersion of wages in the United States had increased both at the bottom and top of the wage distribution during the 1980s. Since the 1990s, log wage gaps between the 10th and 50th percentiles has been largely flat; log wage gaps between the 90th and 50th percentiles, in contrast, increased by 10 log points between 1995 and 2012 (Autor et al., 2016a).

There is still a heated discussion about the forces underlying these developments in the wage structure – and whether these are specific to the U.S. labor market, like the growth of high-skilled supply, or apply more broadly, like technological change, to all advanced economies. A key question is about the role of labor market institutions like unions and minimum wages in compressing the wage structure (see e.g. Lee (1999); Autor et al. (2016b) for minimum wages; and Card (1992); DiNardo et al. (1996) for unions) or, in accounting for the sizable cross-country differences in the evolution of wage inequality.

[1]This chapter is co-authored with Christina Gathmann

Contrary to the common perception that wage inequality remained unchanged in Continental Europe, the prominent study by Dustmann et al. (2009) documents that wage dispersion has increased in West Germany between the 1980s and 2004 as well. While in the 1980s wage dispersion rose mostly at the top of the wage distribution, the 1990s also saw rising wage inequality at the bottom. The authors provide evidence that most of the movements at the top of the wage distribution are accounted for by technology driven demand shifts while movements at the bottom are better explained by supply shifts and the decline of union coverage in West Germany since the mid-1990s.

Interestingly, most recent studies on German wage inequality focus on West Germany arguing that the East German labor market is structurally too distinct to analyze the two together (Dustmann et al., 2009; Card et al., 2013). Alternatively, other works study the German labor market as a whole and hence abstract from any structural differences between East and West Germany altogether (e.g. Dauth et al., 2014, 2017b; Dustmann et al., 2014). Dustmann et al. (2014), for instance, document how Germany evolved from being the "sick man of Europe" with high unemployment to Europe's economic superstar with strong employment growth even during the financial crisis. A third and entirely separate literature investigates the economic transition of the East German labor market (see e.g. Hunt, 2002; Orlowski and Riphahn, 2009) and its consequences for inequality (e.g. Biewen, 2001; Fuchs-Schüdeln et al., 2010).

Even more than twenty-five years after unification GDP per capita, wages and labor productivity in East Germany still lag behind, while unemployment rates exceed those in West Germany (Burda, 2008; Burda and Hunt, 2011; Burda and Severgnini, 2018).[2] The lag persists despite sizable outmigration, in particular by

[2] A particular focus of macroeconomic studies has been on the question to what extent the East German economy has converged to its perceived West German benchmark. Under ideal conditions, in particular similar institutions, free trade and mobility of capital and labor, we expect full convergence as production factors move to equalize factor returns across regions. Yet, the speed of adjustment crucially depends on the savings rate and mobility of production factors. The latter depends, among others, on the generosity of the social welfare system and hence, whether individuals move out of an economically depressed region, for example Sinn and Westermann (2001).

young workers, from East Germany (Fuchs-Schündeln and Schündeln, 2009; Hunt, 2006; Uhlig, 2008) and despite large capital investments, which have made the East German economy on average more capital intensive than the West German economy (Keller (2000); Burda and Hunt (2011) or Burda and Severgnini (2018)). The lack of convergence has lead many to label East Germany a second "Mezzogiorno" mirroring the sharp division in economic fortunes between Northern and Southern Italy (Sinn and Westermann, 2001; Uhlig, 2008).

It is unclear, however, whether East Germany experienced similar developments in its wage structure than West Germany. Has wage inequality also increased in East Germany and is it above or below West German levels? Furthermore, is the East German wage structure shaped by the same combination of supply and demand side forces than West Germany? Or, are there still peculiarities related to the transition process that shape the East German wage structure? Most importantly, while labor market institutions are uniform across the country, does their impact vary with the underlying structure of the labor market? In this chapter, we provide answers to these important questions.

Using detailed administrative data on workers and their employers, we compare the evolution of wage inequality in East and West Germany over the past decades. We begin our analysis in 1995 to abstract from the turmoils of the immediate post-unification years when unemployment skyrocketed following the shut down of many former East German firms; and wages were pushed higher than productivity gains in East Germany to reduce the large wage gaps to West Germany (Akerlof et al., 1991). This study asks what has happened in the East German labor market in the twenty years after the initial turmoil.

The descriptive analysis reveals that the observed changes in the wage structure in East and West Germany over the past decades have a lot in common: wage inequality, irrespective of how we measure it, has risen for most of the period. While there is almost no wage growth in the middle of the East or West German wage distribution, there are sizable wage gains at the top and real wage losses at the bottom. Since 2010, however, wage inequality leveled of and even reversed,

mostly because of wage gains at the bottom of the wage distribution. Despite the similarities in the overall development, there are also two noteworthy differences between East and West: First, the wage distribution in East Germany is more dispersed than in West Germany by the end of our sample period. The higher wage dispersion is primarily accounted for by sizable wage gains at the top of the East German wage distribution that exceed the wage gains at the top in West Germany. Wage earners at the 85th percentiles gained 20% between 1995 and 2014 but only 13% in West Germany over the same period. The second difference is that the decline in wage inequality after 2009 is much more pronounced in East Germany than in West Germany.

We then investigate what factors explain the evolution of the wage structures in the East; and to what extent these forces differ from those in West Germany. We have five main findings. First, we show that the composition of the workforce and selective entry and exit have little effect on the wage structure in both East and West Germany. Compositional changes through educational upgrading and demographic ageing have little influence on the structure of wages. Adjusting for workforce composition matters somewhat more for overall wage inequality at the top in both East and West Germany. East and West Germany have been subject to different population and employment dynamics: East Germany lost about 10% of its population between 1995 and 2010 though population levels have stabilized since then. Further, East German employment declined by 10% as well between 1995 and 2004, while employment has increased again since. We first show that the average labor market leaver is somewhat below the median earner, but above the 15th percentile in terms of wages and observable characteristics. Similarly, the average entrant after 2004 is also located somewhere between the 15th and 50th percentile of the wage distribution. Imputing wages for leavers until 2004 and entrants since 2004 suggest few effect for top end wage inequality (the 85-50 wage gap) and slightly reduces wage inequality at the bottom (the 50-15 wage gap).

Our second finding is that the decline of union coverage plays no role for the rising wage dispersion in East Germany – though union coverage declined even

more dramatically in East Germany than in West Germany between 1995 and 2004. The main explanation for this surprising result is that union coverage rates are at a much lower level at the bottom of the East German wage distribution, where most work is in the service sector, than in West Germany. Hence, the plummeting coverage rates do little harm to East German wages at the bottom in East Germany, but are responsible for some of the wage losses at the bottom in West Germany. As coverage rates increase at higher wage percentiles, wages at the top of the East German wage distribution would have been even higher in the absence of declining coverage rates. We find no such effect for top end wages in West Germany.

The adoption of minimum wages in selected industries, in turn, is the main driver for the turnaround in wage inequality after 2009. East German wages are about 25% lower than West German wages, while minimum wages are either uniform across the country or only slightly lower in East Germany – making the effective minimum wage considerably higher in East Germany than in West Germany. We then show that the sector-specific minimum wages can explain all of the turnaround in wage inequality at the bottom of the East German distribution since 2010. While minimum wages also raise West German wages at the bottom, the effect is much smaller in absolute terms.

We then turn to the demand side for explaining the stark increase in top end wage inequality prior to 2010. In East Germany, a large fraction (about 35%) are accounted for by wage differentials between education and age groups; and another 30% are accounted for inter-industry differentials. Hence, these two factors can explain two-thirds of the rise in wage inequality at the top in East Germany; they explain less than one-third of rising inequality at the top in West Germany. The importance of between-group wage differentials is also reflected in the skill premium between the high- and medium-skilled, which has been rising much faster in East Germany than in West Germany until 2010. A slowdown in the supply of highly educated workers plus an upward trend in high-skilled demand in East Germany, in part explained by disproportionately high outmigration rates of young and skilled workers, is the main factor responsible for the strong growth in East Germany's

skill premium. Finally, we show that two prominent demand side forces fail to explain the rise of East German inequality at the top: routine-biased technological change and trade exposure. While routine-biased technological change generates employment polarization, it has little effect on the East or West German wage structure. Similarly, the sizable expansion of trade, a key motor for Germany's strong economic performance and employment growth, has only small wage effects. Hence, wage dispersion in East Germany, where the economy's export share has traditionally been low, is unaffected by trade exposure. The contribution of trade to top end wage inequality is somewhat more important in West Germany with its many export-oriented industries, but remains modest overall.

The chapter proceeds as follows. The next section introduces the data sources we use to analyze the evolution of the wage structure. Section 3 provides several stylized facts about wage growth and wage inequality in East Germany over the past decades and compares it to West Germany. Section 4 analyzes whether shifts on the labor supply side may account for the observed changes in the East German wage structure. Section 5 assesses the influence of labor market institutions as possible drivers of employment changes, while section 6 turns to the labor demand side and the evolution of wages. Finally, section 7 concludes.

3.2 Data Sources

To analyze the development of the wage structure in East Germany and to compare it with West Germany, we merge administrative data based on individual social security records with additional aggregate data on trade, technology and labor market institutions. We discuss the main data source here; information on the aggregate data can be found in appendix 3.A.

3.2.1 Individual Social Security Records (SIAB), 1995-2014

Our main data are a 2% random sample of the population of workers and plants covered by the social security system in Germany.[3] We observe for each individual

[3]The social security data cover around 80% of the German labor force excluding civil servants, military personnel and the self-employed.

whether she is employed within the social security system or whether she collects unemployment benefits as of June 30th each year. The panel structure allows us to follow each employee even if a worker changes jobs or moves to another plant or region. As we are interested in the wage structure of the East German economy, we focus on individuals whose workplace is based in one of the new states (including Berlin) or who draw unemployment benefits in East Germany. We thus include employees originating from West Germany or abroad in our East German sample if they are employed or registered as unemployed in East Germany. We include employees originating from East Germany migrating to West Germany in our comparison sample of West German employees. Below, we investigate outmigration and commuting from East to West Germany as one channel of labor supply adjustment.

The wage variable records the average daily wage for the employment spell that contains the reference date (June 30th).[4] Like most social security data, our wage variable is right-censored at the social security limit. As wages are lower in East Germany, the share of censored wages in our data is lower in East Germany (6.6%) than in West Germany (13.6%). We impute censored wages under the assumption that the error term in the wage regression is normally distributed allowing for separate variances by year and gender separately for East and West Germany. We convert wages to 2014 prices using the national consumer price index.

We also know the detailed occupational and industry classification (at the 3-digit level) of each employment spell. We distinguish three skill groups based on the highest educational qualification. A person is low-skilled if she has neither finished a high school or vocational degree. An individual is medium-skilled if she has completed an apprenticeship or graduated from high school (*Abitur*). A person is high-skilled if she graduated from college or university. In the raw data, the education variable is missing for about 20% of the observations and contains some inconsistencies over time. We use the panel structure of the data to impute missing educational information and remove inconsistencies using past and future spells

[4]Because employers are required to update records only at the end of each year, this variable may capture wage changes that occurred from January to December of the same year.

following (Fitzenberger et al., 2006). We keep the small number of observations with missing education even after imputation.

We restrict our sample to individuals between the ages of 20 and 62. We exclude irregular, marginal and seasonal employment, apprenticeship and partial retirement spells. Further, we focus in our analysis on full-time workers, which we define as working at least 30 hours per week.[5] Finally, we restrict our analysis to men for two reasons. The main reason is a change of reporting full-time work in the social security records in 2011, which makes it impossible to define a sample of full-time working women consistently over time.[6] The second reason we restrict the analysis to men is that our main focus is on the comparison between the wage structures in East and West Germany over the past decades. Our analysis thus deals with two dimensions explaining the changes in the wage structure over time and regions. The differences between men and women would add yet another layer of complexity, which we leave for future research.

Table 3.B1 in the appendix shows summary statistics on aggregate economic indicators, industry structure and wages for the East and West German sample in 1995, 2004 and 2014. That the transition from a socialist to a successful market economy would have to a long and ropy process can be gauged from the aggregate economic indicators. At the beginning of our sample period in 1995, East Germany's GDP per capita was almost one-third lower than West Germany's GDP per capita. By 2014, East German GDP is still one-quarter below West Germany's GDP. Over the same period, employment rates have increased and unemployment rates decreased – though they are still above West German levels in 2014. The lower level of economic activity is also evident in wages. In 1995, median wages in East Germany are 30 percent lower than in West Germany and there is little convergence

[5]While information on actual working hours is not available over the full time period we study, the data contain a indicator for full-time and part-time work. A job is classified as full-time if working hours per week correspond or exceed the standard working time for full-time workers defined in the collective bargaining agreement of the firm or respective industry.

[6]Prior to 2011, transitions between full- and part-time work within the same establishment were often not reported by employers. The change in reporting requirements in 2011 thus generated a huge spike in the share working part-time in 2011 among women, but not men (see Ludsteck and Thomsen, 2016, for a detailed discussion).

in wage levels over the next two decades. In fact, East German wages in 2014 are still below the wage levels West Germany had in 1995.[7] We do, however, see some convergence in the industry structure between East and West Germany. East Germany still has a smaller manufacturing sector than West Germany, but the employment gap between East and West Germany has narrowed substantially – from 18% in 1995 to 8% in 2014. The service sector has been larger in East Germany, but the employment gap narrowed to around 5% in 2014. The East German construction sector, which employed almost 18% of all employees in 1995, has shrank to a more reasonable 9% by 2014.

3.2.2 Linked Employer-Employee Data (LIAB), 1996-2014

Our main data do not contain information on union coverage, which has been shown to influence the wage structure in West Germany during the 1990s and 2000s (Dustmann et al., 2009; Card et al., 2013). To analyze how changes in union density have affected the wage structure in East Germany, we rely on the LIAB, a linked employer-employee data set. The LIAB combines the IAB Establishment Panel, a large-scale survey of plants, with social security records of all workers who were employed in the surveyed firms as of June 30 each year (see Klosterhuber et al., 2016, for a detailed description).

The LIAB has surveyed plants in West Germany since 1993, but covers East German plants only since 1996. As the IAB establishment panel oversamples large establishments and small states, we employ cross-sectional weights for adjustment. An establishment can recognize a trade union either by joining an employers' association or through direct negotiations between the firm and the union. Therefore, the union variable in the LIAB distinguishes between industry-level agreements, which are negotiated at a regional and industry level, firm-level agreements through direct negotiations between the plant and a union, or no agreement at all.

[7] There is some modest convergence between East and West at the bottom and top of the distribution: wages at the 15th percentile are 32% lower in 1995 and 27% lower in 2014, while wages at the 85th percentile are 29% lower in 1995 and 25% in 2014.

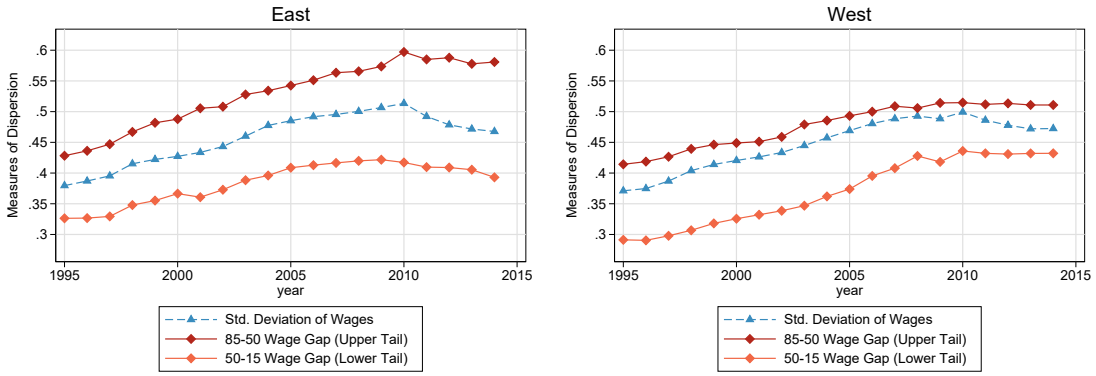
3.3 Stylized Facts about the Wage Structure in East and West Germany

We start out with several stylized facts about the evolution of the East German wage structure over the past two decades and compare them to the developments in West Germany. Figure 3.1 plots commonly used measures of wage inequality: the standard deviation of log wages to assess overall inequality. We further show the log wage gap between the 85th and 50th percentiles (the 80-50 Wage Gap) and the log wage gap between the 50th and 15th percentile (the 50-15 Wage Gap) to trace wage inequality in the top and bottom half of the wage distribution.[8] The left panel of figure 3.1 refers to East Germany, the right panel to West Germany.

Overall wage inequality rises steadily in both East and West Germany between 1995 and 2009. The fanning out of the wage distribution comes to a stop and even reverses after 2009. While the standard deviation of wages rose in East Germany from 0.37 to 0.51 in a 15-year period (1995-2009), it declined to 0.46 over the next five years (2010-2014). Comparing the evolution in the upper and lower part of the wage distribution over time suggests that the reversal is explained by a reduction in inequality at the bottom (the 50-15 wage gap) and a leveling off at the top (the 85-50 wage gap) after 2009.

[8] All measures of wage inequality are based on imputed wages rather than censored wages. The percentile wage gaps are not affected by the imputation as less than 0.1 % of the observations below the 85th percentile are censored. The standard deviation of wages is by definition somewhat lower for censored wages, but its evolution over time is very similar to that for imputed wages in both regions. Results for censored wages are available upon request.

Figure 3.1: Measures of Wage Inequality



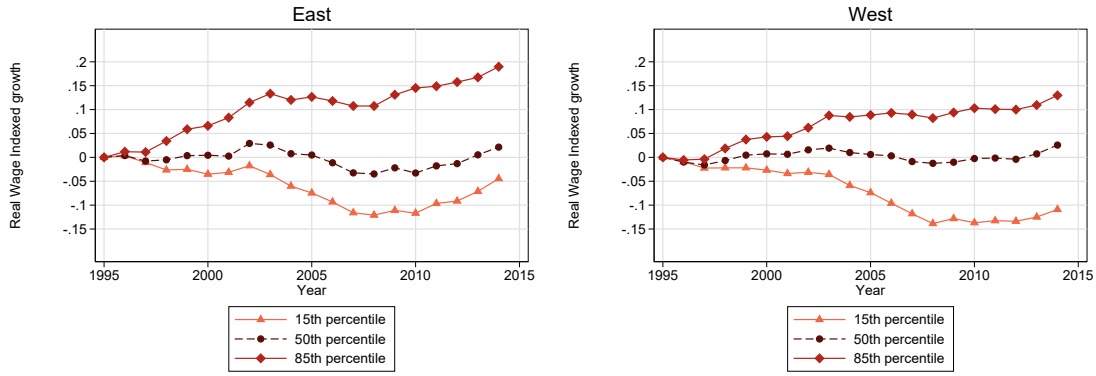
NOTE.- The figures plot three measures of wage inequality: The standard deviation of log wages, the log wage gap between the 85th and 50th percentiles and the log wage gap between the 50th and 15th percentiles.
Source: 2% SIAB Sample for male full-time workers between the ages of 20 and 62.

The most striking feature of figure 3.2 is that wage inequality at the top has been rising faster in East Germany (left panel) than in West Germany (right panel). While the 80-50 log wage gap was around 0.42 in East and West Germany in 1995, it rises to 0.6 in East Germany until 2009, but to only 0.5 in West Germany. The higher dispersion at the top is all the more noteworthy as East German wages are on average about 30 percent lower than wages in West Germany (see table 3.B1).[9] In contrast, the 50-15 wage gap shows a similar development in East and West, though the rise is slightly more pronounced in West Germany and shows no reversal after 2009.

To track real wage gains and losses across the wage distribution, figure 3.2 plots the evolution of real daily wages at the 15th, 50th and 85th percentile indexed to 1995. Consistent with the rise in overall inequality, the wage distribution has been fanning out in both West and East Germany over the past two decades. There is little real wage growth (of only 2%) for the median wage earner in East and West over the 20-year period. Yet, the top of the distribution has experienced sizable wage gains in both parts of the country, while the bottom of the distribution suffered real wage losses. A closer look reveals striking differences in the observed wage gains and

[9]The fact that wage inequality in East Germany exceeds that of West Germany at the top of the distribution is not an artefact of censoring. The wage gaps shown in figure 3.1 are based on imputed and hence, uncensored wages. Furthermore, we find a very similar pattern if we plot the 75-50 log wage gap, for which the share of censored wages is very small.

Figure 3.2: Real Wage Growth at the 15th, 50th and 85th Percentiles



NOTE.- The figures show real wage growth at the 15th, 50th, and 85th percentiles of the wage distribution relative to 1995.
Source: 2% Social Security Records (SIAB) for male full-time workers between the ages of 20 and 62.

losses between East and West. Wage gains at the top of the distribution are much larger in East Germany than in West Germany: Real wages at the 85th percentile rose by 19% in East Germany between 1995 and 2014; in West Germany, real wages at the top grew by only 13% – or a third less than in East Germany. At the bottom of the wage distribution, wages evolved similarly in East and West until about 2009: Real wages at the 15th percentile declined by 12-14% in both parts of the country between 1995 and 2009. After 2009 however, wages at the bottom recovered more in East Germany than in West Germany. Wages at the 15th percentile grew by 7% in the East, but only by 3% in the West between 2010 and 2014. As a result, over the whole period, real wages at the 15th percentile declined by -11% in West Germany, but only by -4% in East Germany. The pattern of relative wage gains and losses shown in figure 3.2 also explain why the 50-15 wage gap has been rising until 2009 (see figure 3.1): the rise has less to do with wage gains by the median earner, but more so with wage losses at the bottom of the German wage distribution.

Our results speak against the widespread view that little has changed in East Germany after the immediate post-unification period. On the contrary, wage inequality in East Germany has increased even more than in West Germany between 1995 and 2014, especially at the top of the distribution. While the wage distribution

fanned out continuously in both parts of the country prior to 2010, the development has been stopped and even reversed in East Germany since 2010 – mostly because of relative wage gains at the bottom. The reversal of wage inequality stand in stark contrast to developments in the United States where wage inequality, esp. at the top of the wage distribution, continues to increase.

3.4 Supply-Side Changes

The observed shifts in the wage structure may be influenced by changes in workforce composition or by selective entry and exit through un- or non-employment, retirement and migration. We analyze each of these factors in turn.

3.4.1 Workforce Composition

Table 3.1 traces the composition of the workforce and the wage structure in East Germany across education and age groups. We report the employment share, the 85-50 and 50-15 log-wage gaps by education and age in 1995, 2004 and 2014. All reported values are calculated using imputed and hence, uncensored wages. To the extent that our imputation method does not fully capture the long right tail in wages, the 85-50 wage gap might understate true wage inequality at the top among the group of high-skilled. The corresponding table for West Germany is contained in table 3.B2 in the appendix. The employment shares suggest sizable educational upgrading of the East German workforce over time. The share of low-skilled workers (without a high school or vocational degree) declines from 4.2% to 1.8% and the share of medium-skilled workers decreases from 80.4% to 78.5% between 1995 and 2014. The share of high-skilled workers with a college or university degree, in contrast, increases from 15.4% to 19.7% of the same period. A comparison with West Germany, reveals that East Germany actually has a more educated workforce than West Germany – a legacy of its socialist past: the share of low-skilled, i.e. individuals with no high school or vocational degree, is 3 times higher in West Germany (5.8% in 2014) than in East Germany (only 1.8% in 2014). Yet, the share of college-educated increased much more in West Germany – in fact,

almost doubled from 10.5% in 1995 to 20% in 2014 – than in East Germany. The employment shares further indicate that the East German workforce is aging. The share of young employees (20- to 36-year-olds) in all education groups declines from 42.5% in 1995 to 31.6% in 2014. The share of older workers (47- to 62-year-olds) in turn expands from 26.6% to 41.5% over the same period. A similar pattern is observed in West Germany (see table 3.B2). These sizable shifts in the educational and age composition of the East German workforce may account for the rising wage inequality if the dispersion in unobserved skills, for instance, and hence, the variability in wages is higher among older and more educated workers (Lemieux, 2006). Yet, table 3.1 shows that overall wage dispersion does *not* increase

Table 3.1: East German Wage Inequality by Education and Age

	Low Education								
	1995		2004		2014		1995	2004	2014
	50-15 gap	85-50 gap	50-15 gap	85-50 gap	50-15 gap	85-50 gap	Employment Share		
20-36	0.43	0.33	0.61	0.46	0.29	0.44	1.7%	1.2%	1.0%
36-47	0.34	0.34	0.73	0.38	0.42	0.46	1.2%	1.0%	0.3%
47-62	0.34	0.34	0.50	0.39	0.57	0.35	1.3%	0.8%	0.5%
All	0.39	0.33	0.67	0.48	0.41	0.50	4.2%	3.0%	1.8%
	Medium Education								
	1995		2004		2014		1995	2004	2014
	50-15 gap	85-50 gap	50-15 gap	85-50 gap	50-15 gap	85-50 gap	Employment Share		
20-36	0.28	0.30	0.33	0.39	0.31	0.39	36.9%	26.7%	25.2%
36-47	0.29	0.34	0.34	0.42	0.34	0.48	24.4%	30.0%	21.0%
47-62	0.32	0.40	0.37	0.42	0.34	0.46	19.2%	23.0%	32.3%
All	0.30	0.33	0.34	0.42	0.33	0.44	80.4%	79.7%	78.5%
	High Education								
	1995		2004		2014		1995	2004	2014
	50-15 gap	85-50 gap*	50-15 gap	85-50 gap*	50-15 gap	85-50 gap*	Employment Share		
20-36	0.38	0.32	0.45	0.36	0.41	0.28	3.9%	3.6%	5.4%
36-47	0.35	0.32	0.43	0.34	0.48	0.29	5.5%	6.2%	5.6%
47-62	0.35	0.32	0.55	0.36	0.57	0.35	6.1%	7.4%	8.7%
All	0.37	0.32	0.49	0.34	0.49	0.34	15.4%	17.3%	19.7%

NOTE.- The table shows 85-50 and 50-15 log wage gaps in East Germany both across and within age and education groups as well their employment shares in 1995, 2004 and 2014. The results are based on imputed and hence, uncensored wages. The star denotes that the the 85th wage percentile for the high-skilled is above the censoring bound.
Source: 2% SIAB Sample for East German male full-time workers between 20 and 62 years of age

monotonically with education: instead, wage dispersion is highest for low-skilled workers throughout the period and lowest for the medium-skilled. Medium-skilled workers, however, experience a monotonic increase in wage dispersion across age groups. The pattern of rising wage dispersion across age groups becomes more

pronounced even for the low- and high-skilled (except the 85-50 wage gap for the low-skilled) over time. Furthermore, wage dispersion within education and age groups rises substantially between 1995 and 2004, but levels off or even declines for most age and education groups between 2004 and 2014. Exceptions are the 47- to 62-year-olds where inequality increases for all education groups over time; and the 85-50 wage gap of the low- and medium-skilled 36- to 47-year-olds.

Hence, while compositional changes are likely to play some role, the evolution of within-group inequality suggest that workforce composition cannot explain most of the observed changes in the East German wage structure. The wage patterns for East Germany in table 3.1 differ from those observed in West Germany (see table 3.B2 in the appendix) where overall and within-group inequality have been rising over the 1995-2014 period. They also appear to differ from other advanced economies like the United States which has experienced a long-term rise in both overall and within-group inequality (Autor et al., 2008).

We next assess the quantitative importance of compositional changes for the wage structure using the inverse probability weighting approach by DiNardo et al. (1996). The basic idea is to decompose the wage distribution in period t $f_c(w|t)$ into a price effect and a composition effect as follows:

$$f_c(w|t) = \int f(w|x, T = t)\psi_x dF(x, T = t) \quad \text{with} \quad \psi_x = \frac{dF(x|T = 1995)}{dF(x|T = t)}$$

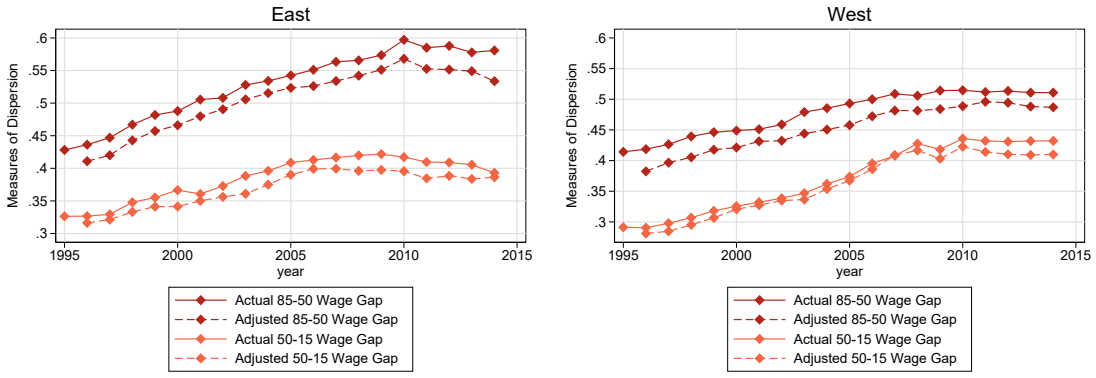
The weight ψ_x reflects the over- or underrepresentation of the characteristics x in the current period t relative to the reference period (here, 1995). Technically, ψ_x is an an inverse probability weight computed from the propensity score of being observed in the reference period conditional on observed characteristics. We estimate the propensity score using predetermined variables only: eight age groups and three education groups as well as a full set of interactions.

Holding the workforce composition constant at 1995 levels but allowing prices of different skill groups to change, we then construct conterfactual wage distributions for each year t between 1996 and 2014. Reweighting the 2014 wage distribution to the 1995 demographics, for instance, generates the counterfactual wage distribution if the

demographic characteristics had remained at their 1995 level, but employees would be paid according to 2014 skill prices. Note that the decomposition method abstracts from general equilibrium effects by assuming that skill prices do not respond to compositional changes. Hence, the reweighting approach does not account for changes in skill prices resulting from relative supply changes, for instance. We will investigate the link between skill prices and relative supply changes in more detail below. Figure 3.3 plots the actual and counterfactual 85-50 and 50-15 wage gaps for East Germany (left panel) and West Germany (right panel). Wage dispersion would have evolved very similarly in both parts of the country over time if we held the education and age composition fixed at their 1995 levels (the dashed lines). Compositional changes matter more at the upper tail (85-50 wage gap) of the wage distribution – both in East and in West Germany. At the bottom of the wage distribution, in contrast, compositional adjustments play little role in East Germany and almost no role in West Germany. These conclusions do not depend on the choice of reference period: we find similar counterfactual wage gaps if we re-weigh to the workforce composition in 2004 or 2014 instead (see table 3.B4 in the appendix).[10]

[10]Looking at individual quantiles, wage growth from 1995 to 2004 would have been slightly weaker across the entire distribution when the demographic composition is held constant. Wage losses at the bottom of the distribution would have been more pronounced (7.5% rather than 6%) and wage gains at the 85th percentile would have been 10% rather than the actual 12%. For the 2004-2014 period, workforce composition has little effect on observed wage patterns in East Germany.

Figure 3.3: Wage Gaps Adjusted to 1995 Demographics

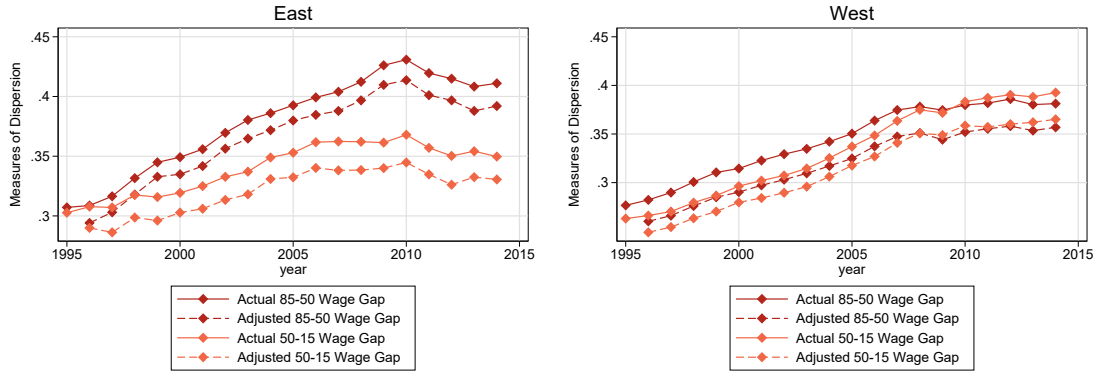


NOTE.- The figures plot the (log) wage gap between the 85th and 50th percentiles and the (log) wage gap between the 50th and 15th percentiles. The solid lines show actual wage gaps and the dashed lines counterfactual wage gaps after adjusting workforce composition to 1995 demographics. The left panel refers to East Germany, the right one to West Germany.
Source: 2% SIAB Sample for male full-time workers between the ages of 20 and 62.

Figure 3.3 does not distinguish between- and within-group (or residual) inequality, which is useful to tie down the underlying sources of rising inequality. We thus perform the same re-weighting approach to 1995 demographics on residual wages, which we obtain from a Mincer-type regression of log wages on three education groups, eight age groups and a full set of interactions, estimated separately for each year. Figure 3.4 reveals that compositional changes are somewhat more important for explaining residual inequality at the bottom of the East and West German wage distribution. Its explanatory power for residual wage inequality is with at most 10% still modest, however. In fact, residual wage inequality at the top and bottom still follows the same time pattern in both East and West whether we account for workforce composition or not. As such, compositional changes cannot account for the rise in residual wage inequality. As before, these results also hold for alternative choices of the reference period (see appendix table 3.B4). Finally, a comparison of figures 3.3 and 3.4 shows that residual inequality in East Germany increases more slowly than overall inequality between 1995 and 2010: while the overall 85-50 wage gap rises by 0.17 log points, the residual 85-50 wage gap rises by only 0.12. Hence, between-group inequality plays an important role in East

Germany at the top of the distribution. In West Germany, all of the rise in the 85-50 wage gap by 0.10 log points occurs within education and age groups. At the bottom of the wage distribution, we observe the opposite pattern: all of the increase in East Germany's 50-15 wage gap is accounted for by within-group wage differences. In West Germany, the overall 50-15 wage gap increases by 0.14 log points, while the residual 50-15 wage gap rises by 0.11 log points – hence, changes in wage differentials between skill groups account for about 20% of the rise at the bottom of the West German distribution.

Figure 3.4: Residual Wage Gaps Adjusted to 1995 Demographics



NOTE.- The figures plot the residual wage gap (in logs) between the 85th and 50th percentiles and the log wage gap between the 50th and 15th percentiles. The solid lines show actual residual wage gaps, the dashed lines residual wage gap after adjusting workforce composition to 1995 demographics. The left panel refers to East Germany, the right one West Germany.
Source: 2% SIAB Sample for male full-time workers between the ages of 20 and 62.

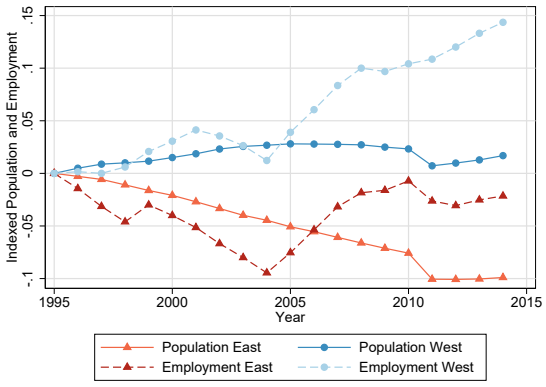
Overall then, compositional changes in the workforce can neither explain the striking growth in East German wage inequality, esp. at the top, until 2010 nor its turnaround since then. We therefore turn next to the question whether selective changes along the employment margin had an influence on the East German wage structure.

3.4.2 Changes along the Employment Margin

East Germany, like many post-communist countries in Central and Eastern Europe, experienced sizable declines in population and employment during the transition

period. Figure 3.5, which plots population and employment of East and West Germany relative to 1995 reflects this pattern. Note that the numbers in figure 3.5 refer to all ages (for population) and all employees (for employment). During the 1995-2014 period, the population in East Germany shrank by 10%. Between 1995 and 2009, East Germany lost about 100,000 inhabitants each year; since 2010, the trend has been reversed and population numbers have stabilized. More than half of the decline in East Germany's population is accounted for by the substantial net outmigration to West Germany as shown in the left panel of figure 3.B1 in the appendix.[11] Outmigration to West Germany came to a halt by 2014, however. The modest population gains since 2010 are accounted for by inflows of international migrants to East Germany (mostly Berlin) as shown in the right panel of figure 3.B1 in the appendix. The population in West Germany, in contrast, has actually increased slightly between 1995 and 2014 as West Germany absorbed the positive net immigration from abroad (except during the 2008/2009 financial crisis when net migration rates were negative) and the net outmigration from East to West Germany.

Figure 3.5: Employment and Population



NOTE.- The figure shows population and employment in East and West Germany. Employment is measured relative to the working-age population. The jump in population between 2010 and 2011 arises from projection adjustments after the Population Census of 2011.
Source: German Federal Statistical Office.

[11] In addition, fertility rates plummeted after unification (see e.g. Chevalier and Marie, 2017), but then recovered and reached West German levels by 2008. Since 2008, fertility rates in East Germany exceed West German fertility rates (Arntz et al., 2014).

Figure 3.5 further indicates substantial employment losses of around 10% in East Germany between 1995 and 2004.[12] Since 2004, employment has risen steadily and, by 2014, East Germany has about reached the employment level it had in 1995. West Germany in contrast, saw few employment changes prior to 2004, but a substantial increase since then. By 2014, employment in West Germany is 15% higher than in 1995. In sum, the evidence suggests that Germany experienced large employment gains since 2004 – but West Germany has benefited from it even more than East Germany. The cause for this employment miracle is still debated: A prominent explanation attributes the employment growth to declining labor costs and wage moderation fueled by the decline of unions' bargaining power (Dustmann et al., 2014). Another argument points to the export industries as motor for net employment gains (Dauth et al., 2016). Finally, several authors have linked the growth in employment to the comprehensive labor market reforms (Hartz I-IV) that were implemented in Germany between 2003 and 2005 (see Krause and Uhlig, 2012; Launov and Wälde, 2013; Krebs and Scheffel, 2013; Burda and Seele, 2016; Hartung et al., 2018).[13] For the purpose of identifying the sources of wage inequality, it does not matter which of these channels drives employment growth. What is important here is whether higher inflows influence the observed wage distribution indirectly through changing workforce composition and earnings potential. We analyze the direct influence of unions and trade on the wage structure in more detail in the next sections.

The evidence in figure 3.5 clearly demonstrates that we cannot ignore changes along the employment margin when studying the East German wage structure. The decline in East German employment before 2004 might compress wage inequality and overstate wage growth in East Germany, for instance, if those leaving the labor

[12] These employment losses were preceded by sizable employment losses following the shutdown of many companies shortly after unification (Insitut für Wirtschaftsforschung Halle, 2014).

[13] Several authors argued that declining replacement rates in the unemployment benefit system (Hartz IV reform) increased job finding rates through higher search efforts (Krebs and Scheffel, 2013) or through changes in firms' vacancy posting (Krause and Uhlig, 2012). Launov and Wälde (2013), in turn, attribute the higher job finding rates to improved placement by local employment offices (the so-called Hartz III reform). Hartung et al. (2018), in turn, provide evidence that most of the increase in employment is accounted for by a decline in separation rates after the labor market reforms.

market earn lower wages than the average East German employee. The decline in population, in turn, might have the opposite effect as existing evidence suggests that outmigration was disproportionately higher among young people and the high-skilled (Hunt, 2006; Uhlig, 2008; Fuchs-Schündeln and Schündeln, 2009). The stark employment growth in both East and West Germany after 2005, in contrast, might reduce observed wage growth and overstate the rise in wage inequality if those entering the workforce earn below-average wages.

To better understand the nature of selection, we now compare the characteristics of individuals leaving the East German labor market between 1995 and 2004 and those entering the East German labor market between 2004 and 2014 to the average East German worker. The top panel of table 3.B5 in the appendix compares employed stayers (with a job in East Germany) between the 15th and 50th wage percentile and at the median wage to all leavers (who were employed in East Germany in year t, but not in $t+1$), and to the subgroup of leavers with a new job in West Germany. The latter category combines migrants from East to West and commuters who work in West Germany, but continue to live in East Germany. Leavers are older and more likely to be high-skilled than stayers, while outmigrants are younger and more skilled than stayers. In terms of wages, leavers including outmigrants are close to the median earner in 1995; by 2004, leavers perform worse than the median East German worker. Instead, leavers are located somewhere between the 15th and 50th percentile of the East German wage distribution by 2004. Hence, leavers become more negatively selected between 1995 and 2004, but never come from the very bottom (below the 15th percentile) of the East German wage distribution.

The bottom panel of table 3.B5 compares those entering the labor market in the 2004-2014 period during the employment miracle to stayers. Here, we distinguish between all entrants (who do not have a record in t, but a social security record in $t+1$) and labor market entrants (who appear in the social security records for the first time). Entrants are of similar age and much more likely to be high-skilled than the typical employee (stayer) at the median or between the 15th and 50th percentile range. Not surprisingly, labor market entrants are much younger and

less likely to be medium-skilled than stayers. On average, entrants have lower earnings than the median worker, but are located in the range between the 15th and 50th wage percentiles both in 2004 and 2014.[14] Not surprisingly, first-time entrants have the lowest earnings but improve their relative position over time. Overall, the consequences of the net employment losses between 1995 and 2004 on the East German wage structure are not clear as migrants are positively selected but other leavers negatively selected compared to stayers. In contrast, the individuals who found new jobs during the 2004-2014 period are negatively selected compared to stayers. As such, we would expect that selection into work overstates the rise in wage inequality after 2004.

To assess the impact of selective entry and exit on the wage structure more formally, we impute missing wages using alternative assumptions on the earnings of leavers and entrants following the literature on the Black-White or gender wage gaps (see, e.g., Johnson et al., 2000, Chandra, 2003, and Neal, 2004, who study the Black-White wage gap; or Hunt, 2001, and Olivetti and Petrongolo, 2008, who analyze gender wage gaps).[15] We thus define a new wage variable, which is equal to the observed wage w_{it} for employed workers and equal to an imputed wage $\tilde{w}_{it}$ for observations with missing wages. Our first approach exploits the panel structure of our data to impute wages using an individual's past (for leavers) or future (for entrants) wage information. That imputation method assigns an imputed wage that is close to an individual's true earnings potential; yet, it only captures individuals who remain attached to the labor market. The second approach imputes wages based on observable characteristics (education, age and year). In effect, we assign the mean wage within each cell to individuals with missing wages belonging to the same education and age group in each year. This method will impute wages for

[14] The evidence that entrants after 2004 have lower earnings capacity than the median earner, but do not come from the very bottom of the wage distribution is in line with Price (2018). He shows that individuals who found a new job after the Hartz IV reforms earned 4-8% less than the average employee but found few wage effects overall because of shorter non-employment spells.

[15] Alternatively, one could use a control function approach to correct wages for selection along the employment margin. Unfortunately, a quasi-experimental setting being absent, it is difficult to identify an exclusion restriction that would affect job finding or employment probabilities but has no effect on the individual's earnings capacity.

many more individuals than the first method but will not capture differences in unobservables. Our third method uses the information from table 3.B5 that leavers and entrants are, in terms of their earnings capacity and observable skills, located somewhere between the 15th and 50th percentiles of the wage distribution. We thus assign wages based on the assumption that leavers or entrants are close to the median wage earner; or somewhat negatively selected by assigning a wage equal to workers at the 15th percentile. We view these two alternative assumptions as an lower and upper bound of how selection affects the wage structure. The resulting 85-50 and

Table 3.2: Log Wage Gaps after Imputation

	Imputation of leavers' wages			
	1996		2004	
	50-15 Gap	85-50 Gap	50-15 Gap	85-50 Gap
No Imputation	0.33	0.44	0.40	0.53
Carried-forward wages	0.33	0.44	0.39	0.55
Imputation on observables	0.33	0.41	0.40	0.51
Leaver wages set to 50-pct.	0.30	0.39	0.37	0.48
Leaver wages set to 15-pct.	0.26	0.46	0.31	0.57
	Imputation of entrants' wages			
	2004		2014	
	50-15 Gap	85-50 Gap	50-15 Gap	85-50 Gap
No Imputation	0.40	0.53	0.41	0.58
Carried-backward wages	0.41	0.54	0.40	0.59
Imputation on observables	0.39	0.51	0.40	0.55
Entrant wages set to 50-pct.	0.37	0.49	0.38	0.54
Entrant wages set to 15-pct.	0.33	0.56	0.34	0.61

NOTE.- The table shows 50-15 and 85-50 log wage gaps including individuals who have no full-time job spell next year (top panel); and the same wage gaps when the wages of entrants in the East German labor market are included. Carried-forward wages use the panel structure to impute wages; imputation on observables predicts missing wages based on education, age and year. The last two imputation methods set missing wages to the 50th percentile or the 15th percentile of employees in the particular year.
Source: 2% SIAB Sample for East German male full-time workers between 20 and 62 years of age

50-15 wage gaps using the different imputation methods are shown in table 3.2.[16] For leavers, we start in 1996 so we can impute wages from past employment spells. The top panel of table 3.2 shows that the 85-50 wage gap remains largely unaffected

[16]For West Germany, we present the same imputation procedures for leavers and entrants in table 3.B3 in the Appendix.

by accounting for leavers independent of the imputation method used. This result is not surprising as leavers come from the lower part of the wage distribution on average. It is somewhat more surprising that there is little effect on the 50-15 wage gap as well. The only exception is when we assign all leavers the wage of the 15th percentile, which reduces the 50-15 wage gap by 0.07-0.09 log points (or about 20%).

For entrants, the bottom panel of table 3.2, we find very similar effects. The imputed 85-50 wage gaps are a bit lower or higher than the observed 85-50 wage gap but the difference is very small. For the 50-15 wage gap, we find deviations of 0.07 log points (or about 35%) when we assign entrants the 15th percentile wage of stayers for non-employment spells prior to actual entry. In sum, selection along the employment margin, if anything, overstates the growth in wage inequality at the bottom of the distribution. Accounting for selective exit and entry along the employment margin cannot explain the fast growth in East German wage inequality at the top of the distribution until 2009.

Overall, accounting for changes through job leavers and the composition of the workforce more broadly cannot explain the changes in the wage structure documented in the previous section. We next turn to the question how labor market institutions influenced wage inequality in East Germany.

3.5 The Role of Labor Market Institutions

How might labor market institutions help to explain the faster growth in wage inequality in East Germany prior to 2010 and its reversal since 2010? Legally, labor market institutions do not differ between East and West Germany. However, because the institutions operate in labor markets that differ in their underlying structure, they might have different consequences for wage inequality. During our sample period, several industries have introduced sector-specific minimum wages. Furthermore, Germany, like other countries, has experienced a significant decline in union coverage over time. We explore the contribution of each of these factors on the wage structure in turn.

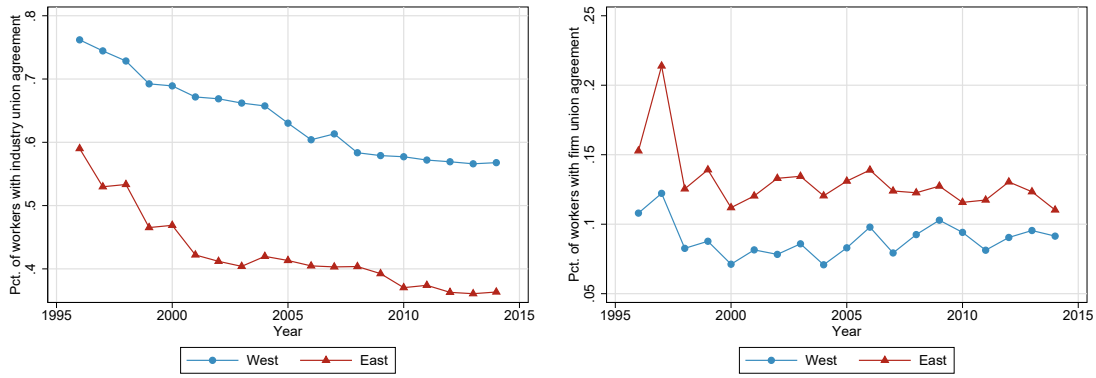
3.5.1 Decline in Unionion Coverage

We first assess whether unions and their decline play a role for the rising wage inequality in East Germany until 2010. Several studies have pointed to the contribution of de-unionization to the widening wage distribution in the United States (e.g. DiNardo et al., 1996; Card et al., 2004; Farber et al., 2018) and in West Germany (Dustmann et al., 2009; Antonczyk et al., 2010; Fitzenberger et al., 2013).

In Germany, trade unions can negotiate collective bargaining agreements either with employer associations at the industry and regional level or with individual firms. If a trade union has reached an agreement with an employers' association, the negotiated result applies to all employees of establishments that are members of the employers' association, irrespective of whether an employee is member of the union or not. While collective bargaining agreements between unions and employers' associations cover whole industries, bilateral agreements between unions and firm apply only to employees of the particular firm.

To assess the role of unions, we use the matched employer-employee data (LIAB) combining plant-level information on the type of bargaining agreement with social security records of all workers in that plant. Figure 3.6 shows a sharp decline in trade union coverage until 2005 and a much more modest decline since then. In West Germany, the proportion of employees covered by an industry-wide collective agreement fell from 76% in 1996 to 63% in 2005 and to 57% in 2014. Collective bargaining declined even more dramatically in East Germany: the share of workers covered by an industry-level trade union agreement decreased from 59% in 1996 to 40% in 2005 and to 36% in 2014. The decline cannot be explained by inter-industry shifts in employment; rather, union coverage declines in all industries – those with traditionally high coverage rates like manufacturing and those without like services (Antonczyk et al., 2010).

Figure 3.6: Collective Bargaining Coverage

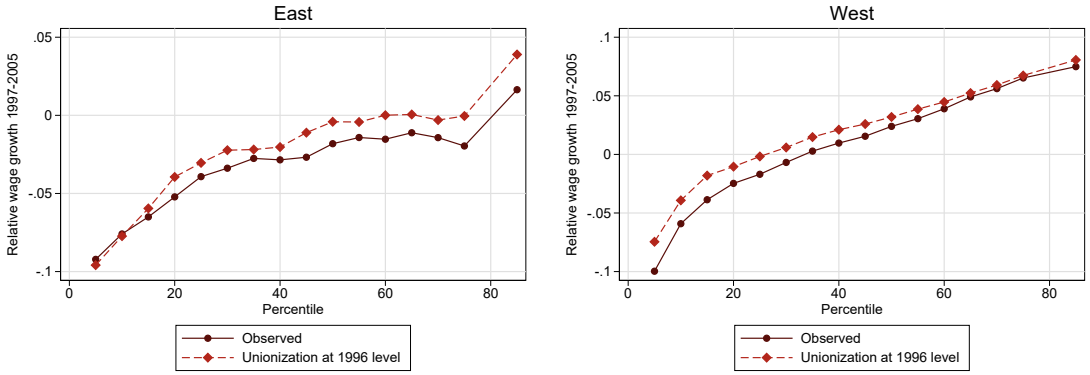


NOTE.- The figures display the share of workers covered by an industry-level (left panel) or firm-level (right panel) collective bargaining agreement.
Source: LIAB Data

The right panel of figure 3.6 shows that firm-level agreements are generally rare, but more common in East Germany than in West Germany. The share of workers covered by firm-level agreements declines from 10-15% to about 8-12% since 2000 and remained roughly stable since. The patterns in figure 3.6 suggest that the decline in union coverage might have its strongest impact on wage inequality prior to 2005 – and with a potentially stronger effect in East Germany. To quantify how declining union coverage rates influence the East German wage structure, we again use inverse probability weighting (DiNardo et al., 1996). We hereby focus on the period from 1996, the first year in the LIAB data, and 2005, as union coverage rates leveled off in both East and West Germany thereafter. To construct the counterfactual, we reweigh the 2005 wage distribution to reflect the 1996 level of union coverage. We use a flexible specification including all possible interactions between the three states of collective bargaining (sector, firm or none), eight age and three education groups to estimate the propensity score. Based on the propensity score, we then compute the weights. Note that the chosen specification accounts for changes in workforce composition as well as differential changes in union coverage rates across education and age groups. It is also important to stress that the reweighting approach is flexible enough to allow for differential union wage premiums at the bottom and top of the wage distribution. As before, the approach abstracts from general equilibrium

effects assuming that these union wage premiums are unaffected by declining union coverage rates. As existing evidence suggests that the causal effect of union coverage on wages is close to zero, this assumption seems innocuous.[17] Figure 3.7 shows the observed real wage growth between 1996 and 2005 (the blue line) and the reweighted counterfactual using the 1996 union coverage rates (the dashed red line).

Figure 3.7: Unionization and Wage Inequality



NOTE.- The figures compare the actual change in log real wages between 1996 and 2005 with a scenario where unionization are adjusted to their 1996 level for both East Germany (left panel) and West Germany (right panel).
Source: LIAB Data

The panel for East Germany on the left-hand side reveals a surprising pattern: higher union coverage rates in 2005 would not have raised wages at the bottom of the wage distribution. Quite on the contrary: The union coverage effects are positive above the 20th percentile and strongest in the middle part of the East German wage distribution. In sharp contrast, the pattern is the exact opposite in West Germany: Higher union coverage in West Germany would have raised wages at the bottom of the wage distribution. The effect on wage inequality declines above the 20th percentile and becomes zero for wages above the 60th percentile. Why would low-wage workers in East Germany not benefit from the higher union coverage rates that prevailed in 1996? One potential explanation is that coverage rates are lower at the bottom of the wage distribution than in the middle or top of the wage distribution.

[17] OLS regressions indicate a union wage premium of about 4-6% in Germany, while the evidence using instrumental variable regressions suggest no return (Antonczyk et al., 2011).

Table 3.3: Union Coverage by Wage Percentiles

	Share of workers without union coverage							
	East				West			
	Below 15	15 to 50	50 to 85	Above 85	Below 15	15 to 50	50 to 85	Above 85
1996	46.81%	23.89%	13.80%	14.49%	18.03%	9.41%	12.56%	13.23%
2005	65.74%	43.10%	29.37%	29.89%	38.27%	21.45%	24.54%	27.10%

NOTE.- The table shows the share of the share of workers without any bargaining agreement for workers earning below the 15th percentile ('Below 15'), workers earning between the 15th and 50th percentiles ('15 to 50'), workers earning between the 50th and 85th percentiles ('50 to 85') and workers earning above the 85th percentile ('Above 85').
Source: LIAB data for male full-time workers between the ages of 20 and 62.

Table 3.3 shows union coverage rates in 1996 and 2005 for workers at different percentiles of the wage distribution. The most striking feature emerging from table 3.3 is that union coverage rates are much lower for East German workers earning below the median wage than West German workers. Almost half of East German workers with wages below the 15th percentile are not covered by any collective bargaining agreement already in 1996. By 2005, the share without coverage has increased to two-thirds of all employees with wages below the 15th percentile.

To the extent that the weaker coverage translates into a worse bargaining position during wage negotiations, the decline of union representation had little effect on the bottom of the wage distribution in East Germany. Instead, the decline of unions muted wages in the middle and upper part of the distribution (see also DiNardo et al., 1996, for similar evidence). If coverage rates had remained at their 1996 level, wage inequality at the top would have increased even more. Overall then, unions play only a limited role in explaining the evolution of wage inequality in the East – in sharp contrast to West Germany where the decline of unions contributed to rising wage inequality (see also Dustmann et al., 2009).

3.5.2 Sectoral Minimum Wages

Sector-specific minimum wages were first introduced in Germany in 1997. Since 1996, the federal government could declare collective bargaining agreements as binding for all workers in an industry irrespective of union coverage. The main intention of the law was to protect domestic employees and employers from foreign competition by defining legal standards, among them a minimum wage. While

the first minimum wages covered relatively small industries, several large sectors, like temporary agency work, building cleaners or care and nursing, introduced minimum wages between 2007 and 2013.[18]

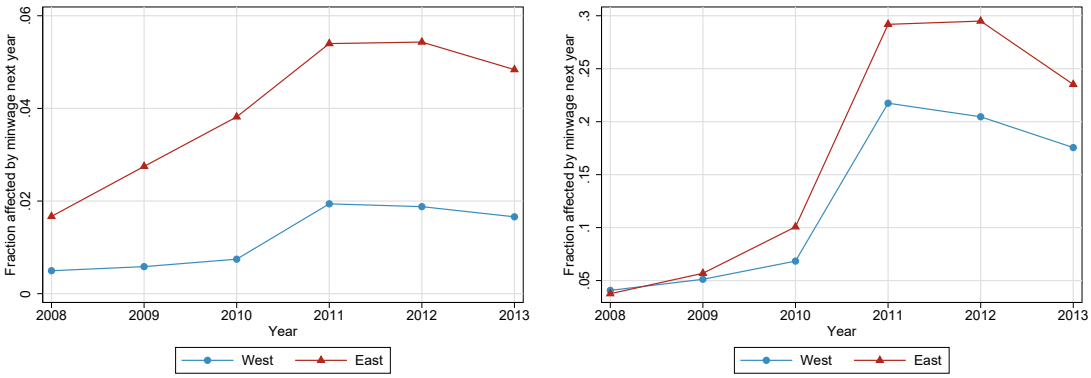
We obtain data on minimum hourly wages listed in collective bargaining agreements from the Federal Statistical Office (German Federal Statistical Office, 2017). In 2014, the hourly minimum wage ranges from 7.86 Euros per hour in temporary work to 11.92 Euros per hour in mining. The minimum wages apply to the whole country in about half of the industries, while they are lower in East Germany in the remaining 50%. The maximum differential in minimum wages for the same industry between East and West is 15% for building cleaners. As wage levels are almost 30% lower in East Germany (see table 3.2), minimum wages have been more binding in the East German labor market. Sector-specific minimum wages are thus a promising candidate for explaining the reversal in wage inequality, especially in East Germany, since 2009. We restrict our analysis to the period from 2008 to 2014 as most sector-specific minimum wages were introduced over this period.[19] We convert hourly values into monthly wages using 30 working hours per week for our sample of full-time workers.[20]

[18] See appendix 3.A for a list of the industries. A national minimum wage covering all sectors was introduced only after the end of our sample period on January 1,2015.

[19] As minimum wage industries are identified at the 5-digit level only, the restriction to the 2008-2014 period also help us avoid inconsistencies due to structural breaks in the industry classification in 1993, 2003 and 2008.

[20] Assuming a 30-hour workweek provides a conservative estimate of the share of affected workers if the typical workweek is above 30 hours.

Figure 3.8: Sectoral Minimum Wages: Share of Affected Workers

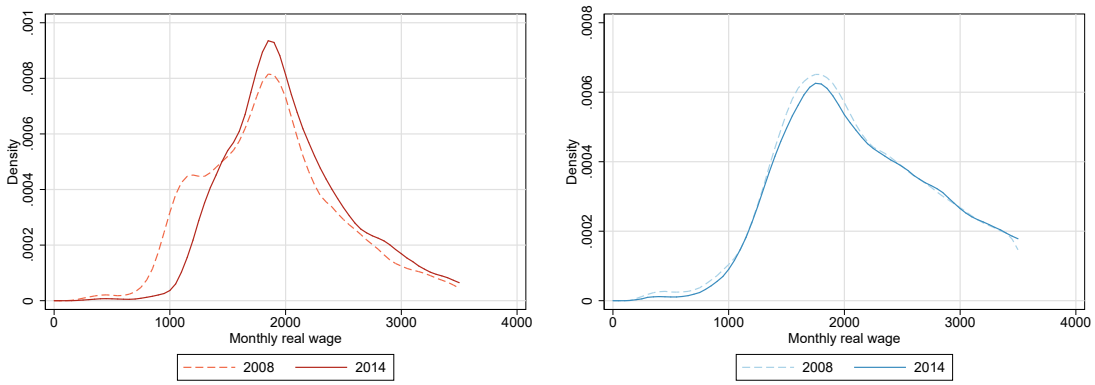


NOTE.- The figures show the share of workers potentially affected by a minimum wage, i.e. with wages below the minimum wage in their industry in the following year. The left panel refers to all full-time workers, the right panel includes wage earners up to the 15th percentile only.
Source: 2% SIAB Sample for male full-time workers between 20 and 62 years of age.

Figure 3.8 plots the fraction of workers whose wage is below next year's minimum wage in their respective industry relative to all workers (in the left panel) and workers with wages up to the 15th percentile (in the right panel). The share of workers potentially affected by sector-specific minimum wages is much higher in East Germany (6% of all workers by 2012) than in West Germany (only 2% of all workers) reflecting the lower wage levels in East Germany. Compared to low-wage earners the share of East German workers affected by minimum wages even triples from around 10% to 30% between 2008 and 2012.

To explore the distributional effect of sector-specific minimum wages, figure 3.9 plots the wage distributions in East Germany (in 2008 and 2014) separately for all industries with a sector-specific minimum wage (left panel) and for all other industries (right panel). The most striking feature of figure 3.9 is that there is a noticeable shift to the right at the bottom of the wage distribution in industries with a minimum wage. In contrast, the wage distribution is unchanged for all other industries.

Figure 3.9: Wage Distributions in Industries with and without Minimum Wages



NOTE.- The figures plot the wage distributions in East Germany for industries with a sector-specific minimum wage (left panel) and all other industries (right panel) in 2008 and 2014. Monthly wages above 3500 Euros are omitted for better visibility.
Source: 2% SIAB Sample for male full-time workers between 20 and 62 years of age

Figure 3.9 indicates that sector-specific minimum wages are a good candidate to explain the decline in inequality at the lower tail in East Germany. To substantiate this claim, we again use the reweighting method to calculate counterfactual wage distributions for 2014 if sector-specific minimum wages had not been introduced. For industries without a sector-specific minimum wage, we use the actual wage distribution of that industry in 2014. For industries with a sector-specific minimum wage, we use the actual wage distribution above the minimum wage and the 2008 distribution for wages below the minimum wage. Our decomposition imposes four assumptions: first, the method abstracts from spillover effects on other industries, i.e. the adoption of minimum wages in one industry does not affect wages in other industries. If wages in other industries were pushed up, however, we would underestimate the contribution of minimum wages to wage growth. Under the assumption of no cross-industry spillovers, we can use the actual wage distribution in 2014 for industries without a minimum wage. The counterfactual wage density for 2014 is then simply the sum of the counterfactual densities for each minimum wage industry and the actual density of all industries without a minimum wage evaluated at 2014 industry shares ϕ_i:

$$f(w|x, t = 14, m_{08}) = \sum_{i=1}^{I} \phi_i f_i(x, t = 14, m_{08})$$

We further assume that there are no spillovers to wages above the respective minimum wage in a covered industry.[21] Then, the counterfactual 2014 industry distribution is identical to the respective actual distribution for wages above the respective minimum wage:

$$\mathbb{1}(w > m_{14}) f_i(x, t = 14, m_{08}) = \mathbb{1}(w > m_{14}) f_i(x, t = 14, m_{14})$$

The available evidence suggests some positive spillover on wage earners above the minimum wage in selected industries (see Aretz et al., 2013, for evidence on the roofing industry). In that case, we underestimate the contribution of minimum wages to overall wage growth. A third assumption is that the counterfactual wage densities in 2014, which would have emerged without minimum wages (or minimum wages set at their 2008 level), are proportional to the conditional wage densities in 2008. Hence,

$$\mathbb{1}(w \leq m_{14}) f_i(x, t = 14, m_{08}) = \psi(x, m_{14}) \mathbb{1}(w \leq m_{14}) f_i(x, t = 08, m_{14}),$$

where $\psi(x, m_{14})$ is the inverse probability weight. The weight is computed from the propensity score that an observation is from the year 2014 (rather than 2008) using three education, eight age groups and all interactions as predictors of the propensity score.

Finally, we abstract from negative employment effects of minimum wages. If individuals displaced in a minimum wage industry earn lower wages in a different industry, we would observe a shift to the left, which would reduce the contribution of minimum wages to the decline in wage inequality after 2009. Given that existing evaluations of the sector-specific minimum wages have found few disemployment effects, this assumptions seems again of minor concern (see Fitzenberger and Doerr, 2016, for an overview). Based on these four assumptions, we compute the counterfactual wage density for 2014 from separate kernel density estimates for each industry in East Germany for 2008 and 2014. We then integrate the estimated counterfactual density to a CDF and use its inverse to obtain estimates for the counterfactual wages at the median and the 10th or 15th percentiles. We present the

[21] While Autor et al. (2016b) find evidence for some spillover effects in the United States, they cannot rule out that these are due to misreporting.

Table 3.4: Minimum Wage Counterfactual for 2014

	2008	2014		Change from 2008 to 2014	
	Actual	Actual	Counterfactual	Actual	Counterfactual
10-percentile	1325	1475	1400	11.3%	5.7%
15-percentile	1468	1594	1550	8.6%	5.6%
50-percentile	2222	2371	2350	6.7%	5.7%
50-15 Log Wage Gap	0.415	0.397	0.416	-0.018	0.001

actual percentiles in 2008 and 2014 together with the estimated counterfactual wages in table 3.4. In the counterfactual scenario, East German wage growth at the 10th percentile would have been 50%, wage growth at the 15th percentile would still have been 35% lower between 2008 and 2014. Even at the median, wage growth would have been lower without sector-specific minimum wages.[22] Sector-specific minimum wages therefore account for a sizable share of wage growth at the bottom of the wage distribution since 2009. Moreover, the last row of table 3.4 shows that the adoption of sector-specific minimum wages can explain all of the decline in East German wage inequality and the leveling of West German wage inequality since 2009.

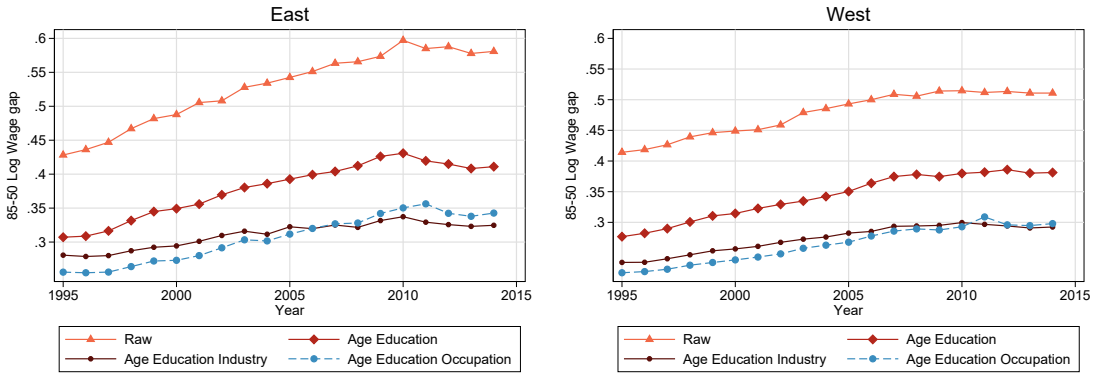
3.6 The Labor Demand Side

While sector-specific minimum wages can account for the turnaround in wage inequality in recent years, the fast rise in East German wage inequality, especially at the top, prior to 2009 remains unexplained. Our next step is thus to identify the type of demand-side changes that account for the growth in the 85-50 wage gap. To provide some guidance about the potential forces, figure 3.10 plots several estimates of the 85-50 wage gap: the raw wage gap, the wage gap after taking out observable skills (education, age and their full interaction), the wage gap after adding 3-digit industry fixed effects in addition to the skill variables; and finally, the wage gap after accounting for 3-digit occupation fixed effects in addition to the skill variables.

[22] In West Germany, wage growth between 2008 and 2014 is overall much lower: only 4% at the 10th percentile and between 2.7-3.6% at the 15th and 50th percentiles, respectively. Using the same decomposition as for East Germany, we find that sector-specific minimum wages contribute a similar share to wage growth in the lower tail: sector-specific minimum wages account for the majority of the modest wage gains at the 10th and 15th percentiles but play no role for median wages.

In East Germany (shown in the left panel), 30% (0.05/0.17) of the increase in the 85-50 wage gap between 1995 and 2009 is explained by wage differentials between skills. In contrast, observable skills cannot explain any of the rise in top wage inequality in West Germany (shown in the right panel). Industries play an important role in explaining the rise of residual wage inequality at the top. In East Germany, inter-industry wage differentials (within skill groups) account for about 35% (0.06/0.17) of the rise in top wage inequality; in West Germany, the contribution is with 30% (0.03/0.10) only slightly lower. Taken together, wage differentials between observable skills and industries account for 65% of the increase in East German wage inequality at the top. In West Germany, in contrast, 70% of the rise in top wage inequality occurs within skill and industries.

Figure 3.10: Raw and Residual 85-50 Wage Gaps



NOTE.- The figures show the raw 85-50 wage gap, the residual wage gap (three education, eight age groups and all interactions estimated separately in each year); the residual wage gap after accounting for 3-digit industries; and the residual wage gap after controlling for 3-digit occupations.
Source: 2% SIAB Sample for male full-time workers between 20 and 62 years of age.

3.6.1 Wage Differentials between Education Groups

Wage differentials between educational groups are important to explain the evolution of inequality at the top in East Germany. We first turn to the role of supply and demand forces in shaping the evolution of skill premiums over time. As we seek to explain the rise in inequality at the top, we focus on the skill premium between the high-skilled (those with a college or university degree) and medium-skilled

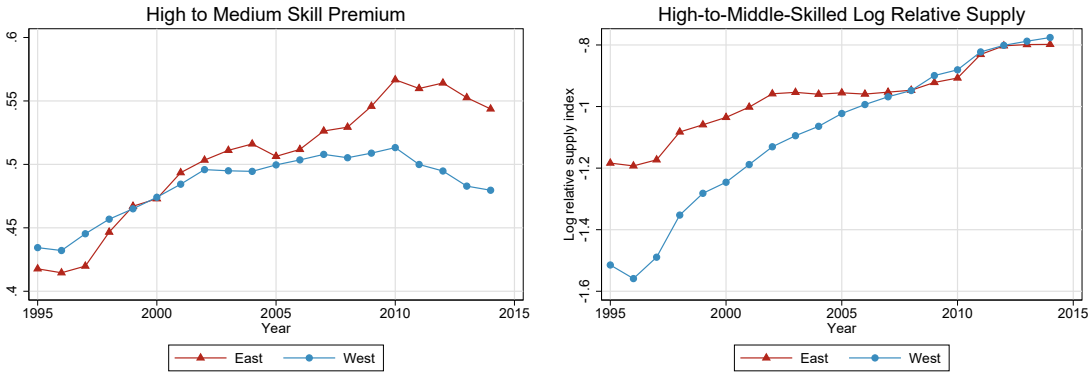
(those with a vocational or high school degree). We obtain the skill premiums from separate regressions of imputed log wages in East or West Germany on education and age categories as well as a full set of interactions for each year from 1995 to 2014. We then compute the wage differentials between the high- and medium-skilled using the average education and age composition of the workforce over the whole period (calculated separately for East and West Germany). Hence, changes in the demographic composition of the workforce through demographic aging, for instance, cannot explain the observed changes in the skill premiums. The left panel of figure 3.11 shows that the skill premium for high-skilled workers has been rising in both East and West Germany until 2010 and then declined sharply thereafter. Even more importantly, the skill premium rises much faster in East Germany than in the West: while the skill premiums are similar until 2000, the East German skill premium for the high-skilled exceeds the West German one by around 5 percentage points in 2010 and 2014. The higher growth in East Germany suggests one of two things: a stronger growth in the demand for high-skilled workers or a slower growth in the supply of high-skilled workers in East Germany compared to West Germany.

The evidence in table 3.1 suggests that the high-skilled share has expanded more slowly in the East than in the West. To capture the influence of growth in relative supply on the skill premium, we calculate relative skill supplies. We first compute average wages in 1995 for each of 24 education-age cells (three education and eight age groups) normalized by the average wage of the reference group (37-40 years-old, medium-skilled individuals).[23] We choose the start of our sample period in 1995 as reference year in order to abstract from potential demand side shifts in later years. To calculate the labor supply of an age-education cell, we then take the total number of employees in each 24 education-age cell in year t times the normalized 1995 wage. The relative supply of high-skilled to medium-skilled labor in a given year measured in efficiency unit is then calculated as the sum

[23]The results do not depend on the particular choice of the reference group. This skill supply measure is similar to an efficiency unit representation of the relative supply of college and non-college labor as in e.g. Autor et al. (2008).

of high-skilled employees over all age groups in that year divided by the sum of medium-skilled employees across all age groups in the same year.

Figure 3.11: Skill Premiums and Relative Skill Supplies



NOTE.- The left panel plot the age-adjusted skill premium between high- and medium-skilled men for East and West Germany. The right panel plots the relative skill supply of high-skilled relative to medium-skilled workers measured in efficiency units.
Source: 2% SIAB Sample for male full-time workers between 20 and 62 years of age.

The right panel of figure 3.11 shows that the relative skill supply of high-skilled workers has been rising in East and West Germany throughout the 1995-2014 period. As the skill premium increased in both regions until 2010 as well (see left panel of figure 3.11), the relative labor demand for high-skilled workers must have increased even faster than the supply of high-skilled workers. Yet, the growth in the relative skill supply of high-skilled workers has been much slower in East Germany because of high outmigration rates of high-skilled employees and those planning to obtain a university education prior to 2010. After 2010, the relative supply continues to increase in East and West but the skill premium actually declines pointing to a slowdown in the demand for high-skilled workers.

The corresponding skill premium and relative supply of medium- to low-skilled (those without a highschool or vocational degree) workers is contained in figure 3.B2 in the appendix. The medium-low skill premium is much lower in East Germany than in West Germany, converges toward the West German one until around 2009 and then declines again thereafter. The relative supply of medium-skilled workers, which is computed in a corresponding fashion to the relative supply of

high-skilled workers, is higher in East Germany, but grows at a similar rate in East and West Germany between 1995 and 2014. The East German pattern is consistent with a positive relative demand shift for medium-skilled workers until 2008. The development after 2008 is consistent with the evidence above that sector-specific minimum wages, by pushing up wages at the bottom of the distribution, reduced the skill premium between medium- to low-skilled workers in East Germany.

To assess the role of relative supply changes on wages of different skill groups more formally, we use an aggregate CES production function with high-skilled (with a college degree) and all other labor in East Germany as inputs (see e.g. Goldin and Katz, 2007, for a similar approach).

$$Y_t = A_t \left[\lambda_t C_t^{\rho} + (1-\lambda_t) N_t^{\rho}\right]^{\frac{1}{\rho}} \tag{3.1}$$

Moreover, labor without a university or college degree N is measured through a CES-production function combining medium- and low-skilled workers as inputs:

$$N_t = \left[\theta_t L_t^{\eta} + (1-\theta_t) M_t^{\eta}\right]^{\frac{1}{\eta}} \tag{3.2}$$

The shares of different types of labor are represented by the technology parameters λ_t and θ. These parameters reflect, among other things, any changes in production through technology or labor market institutions other than the aggregate labor inputs modeled in the production function. The elasticity of substitution between the medium- and low-skilled workers is represented by $\sigma_{ML} = \frac{1}{1-\eta}$. Assuming that labor is paid its marginal product, we obtain

$$log\left(\frac{w_{C_t}}{w_{N_t}}\right) = log\left(\frac{\lambda_t}{1-\lambda_t}\right) - \frac{1}{\sigma_{CN}} log\left(\frac{C_t}{N_t}\right), \quad \text{and} \tag{3.3}$$

$$log\left(\frac{w_{M_t}}{w_{L_t}}\right) = log\left(\frac{\theta_t}{1-\theta_t}\right) - \frac{1}{\sigma_{ML}} log\left(\frac{M_t}{L_t}\right). \tag{3.4}$$

Equations (3.3) and (3.4) are estimated in two steps. We first estimate equation (3.4) to obtain σ_{ML}; we then use this estimate to compute the quantity of labor without a college degree, which we use as input to estimate equation (3.3). In the estimation, the first terms in both equations, which represent changes in technology or the institutional framework over time, are substituted with linear time trends.

To account for generated regressor bias, we bootstrap standard errors in the second step. The estimates in columns (1) and (3) are negative, but only statistically

Table 3.5: Two-level CES Production Function Estimation

	(1)	(2)	(3)	(4)
	East		West	
	Medium-to-Low	College-to-Noncollege	Medium-to-Low	College-to-Noncollege
Relative Supply	-0.106	0.273***	-0.0646*	0.269***
	(0.103)	(0.0807)	(0.0353)	(0.0507)
Time Trend	0.0114**	0.00284	0.00665***	-0.00747***
	(0.00493)	(0.00203)	(0.00150)	(0.00201)
Constant	0.378	1.012***	0.324***	1.046***
	(0.314)	(0.172)	(0.0688)	(0.104)
Observations	20	20	20	20
R^2	0.616	0.932	0.970	0.871

Notes: The table shows estimates of the determinants of skill premiums using a CES production framework. Columns (1) and (2) present estimates for East Germany, whereas columns (3) and (4) contain results for West Germany. We estimate two-level CES production functions for labor with a college degree and those without a college degree (shown in columns (2) and (4)) where the latter combines low- and medium-skilled workers estimated in the first step (and shown in columns (1) and (3)). The standard errors in even columns are bootstrapped to adjust for generated regressor bias in the second step.
Source: 2% SIAB Sample for male full-time workers between 20 and 62 years of age

significant at the 10% level for West Germany. The R^2 for East Germany is only 0.62 compared to 0.97 in West Germany suggesting that there is substantial variation left in the skill premium after accounting for a linear trend and relative supply changes. The implied elasticity of substitution between low- and medium-skilled workers is with $\sigma_{ML} = 10(1/0.106)$ for East Germany and $\sigma_{ML} = 15(1/0.065)$ for West Germany very high in both regions suggesting that low- and medium-skilled workers are very good substitutes.[24] The estimates in columns (2) and (4) on the relative supply and college and (composite) non-college labor are actually positive and very similar for East and West Germany, which indicates that relative supply shifts cannot explain the movements in the skill premium of high-skilled workers.

In sum, figure 3.B2 and the evidence in table 3.5 suggest that the slow growth in high-skilled supply is an important driver for the fast rise in the skill premium of high-skilled workers. At the same time, supply-side changes alone cannot account

[24]The CES production function combining low- and medium-skilled labor into one production factor as in table 3.5 might be misspecified. Alternatively, we combine high- and medium-skilled labor into one production factor and low-skilled labor into a second. Estimating this specification in one step, we obtain coefficients that have the same sign and are of similar magnitude than the estimates reported in columns (1) and (3) in table 3.5. Hence, that alternative specification does not provide any additional insight.

for the evolution of the skill premium because the skill premium (net of a linear, skill-biased demand shift) rose even as relative supply expanded over our sample period. Therefore, sizable changes on the demand side are needed to reconcile the growth in the relative supply of high- and medium-skilled workers with rises in the skill premiums prior to 2010.

3.6.2 Polarization and Routinization

While the demand for skilled, and esp. college-educated labor has been rising, a substantial share of the rise in top wage inequality in figure 3.10 occurs within education and age groups. Several authors have argued that technological change is no longer skill-biased in favor of highly educated workers (Autor and Dorn, 2013; Goos et al., 2009, 2014). Rather, technological change has increasingly automated routine tasks, which are easily codifiable and repetitive. As routine tasks are typically performed in the middle of the skill distribution, routine-replacing technological change reduces employment and possibly wages in the middle of the skill distribution. Employment and possibly wages rise, in turn, at the top, where workers perform complementary, non-routine tasks, and at the bottom, where workers perform manual and interactive tasks – resulting in a polarization of the wage distribution.

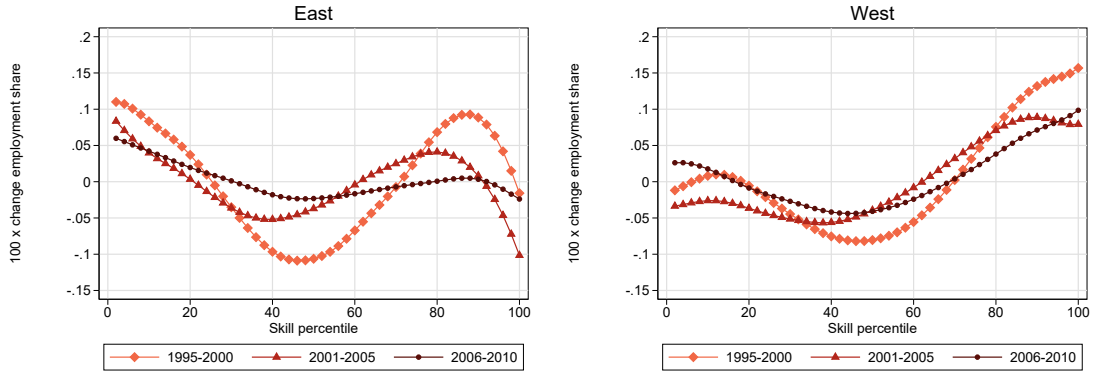
To assess the role of routine-replacing technological change for top wage inequality, we first track the task content of occupations along the skill distribution. To this end we use task data from the 1997/98 wave of the Qualification and Career Survey (BiBB), which has been previously analyzed by Spitz-Oener (2006), Dustmann et al. (2009) and Gathmann and Schönberg (2010). We classify tasks into analytic, interactive, non-routine manual and routine tasks and calculate separate measures for East and West Germany.[25] Sorting occupations by their experience-adjusted median wage in 1995 and dividing them into equal-sized skill groups, we plot the smoothed task intensity for the different tasks in figure 3.B3. In East Germany,

[25] We use the criterion-validated task measures following Rohrbach-Schmidt and Tiemann (2013). The four task measures are calculated from twelve questions on the task content of jobs. Analytical tasks include organizing, conducting research and measuring. We classify training, consulting, buying, advertising and negotiating as interactive tasks. Non-routine manual tasks are repairing and nursing, while routine manual tasks include monitoring and producing.

routine tasks are important between the 10th and 50th percentiles of the wage distribution; in West Germany, they are important for all occupations up to the 60th percentile. Routine tasks are least important at the top of the skill distribution in both parts of the country. Analytical skills, in turn, increase, while manual tasks decrease monotonically across the skill distribution. Interactive tasks are highest at the top and lowest in the middle of the skill distribution.

If labor demand for routine tasks declines, but increases for non-routine manual and analytical tasks, we should observe employment gains at the top and employment losses at the lower and middle part of the skill distribution. We next plot smoothed changes in employment for each skill percentile (defined by the experience-adjusted median wage of an occupation in 1995) in East and West Germany separately for the 1995-2000, 2001-2005 and 2006-2010 period.[26] Figure 3.12 suggests some

Figure 3.12: Occupational Shifts Along the Skill Distribution



NOTE.- The figures plot the relative changes in employment share for occupations along the skill distribution.
Source: 2% SIAB Sample for male full-time workers between the ages of 20 and 62.

employment polarization in East (shown in the left panel) and West Germany (shown in the right panel). Employment polarization is strongest during the 1995-2000 period (the orange line) and starts to level off across the skill distribution in later periods (the dark red lines). Figure 3.12 also shows important differences between East and West: in East Germany, employment at the bottom and top

[26] We limit the time period to the period from 1995 to 2010 for two reasons: first, we want to understand the rise in top end wage inequality, which reversed since 2010. Second, by restricting the period of analysis until 2010, we avoid inconsistencies in the reporting of occupations in 2011.

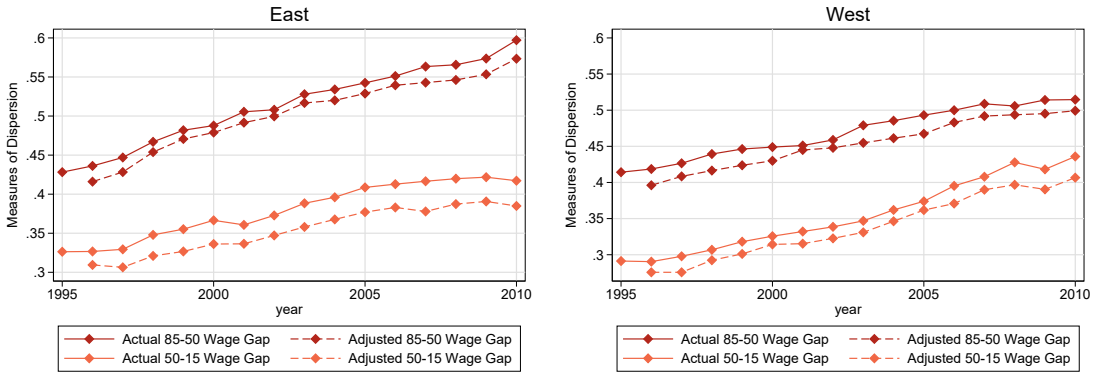
of the skill distribution grow by 10% between 1995 and 2000 and declines by the same amount in the middle. After 2000, there is little employment growth at the top of the skill distribution in East Germany. In West Germany, in turn, there is no employment growth at the bottom, but strong employment growth (by 15%) at the top between 1995 and 2010.

We next investigate whether there is any corresponding polarization in wages that follows the same pattern as employment. Hence, we compute the change in experience-adjusted occupational median wages for the three time periods along the occupational skill distribution and plot them in figure 3.B4 in the appendix. We find no evidence for wage polarization, neither in East, nor in West Germany during the 1995-2010 period (see also Antonczyk et al. (2011) for a similar result). Rather, wages at the bottom show modest losses in all of Germany between 1995 and 2010, while wages at the top grow, especially between 1995 and 2005. In line with the strong growth in the 85-50 wage gap documented in Section 3.3 above, the wage gains at the top are much more pronounced in East Germany: wages at the top grow by 7.5% between 1995 and 2000 and still around 6% between 2001 and 2005. In West Germany, wages grow only by 3-4% on average over the same period.

Can we explain the rise in top wage inequality before 2009 by the employment changes across occupations observed in figure 3.12? To investigate this question, we again use the DiNardo et al. (1996) approach to re-weigh occupational employment to their 1995 shares.[27] The approach again relies on the assumption that the observed employment shifts between 1995 and 2010 do not affect skill returns seems to be satisfied given that we find no wage polarization (see figure 3.B4). Figure 3.13 plots the actual 85-50 and 50-15 log wage gaps and the counterfactual wage gaps based on the re-weighted occupational structure. Changes in the occupational structure play a minor role for the lower end of the wage distribution, esp. in East Germany. Even more importantly, figure 3.13 shows that changes in the

[27] An alternative approach would be to reweigh by task intensities or a combined routinization index (as introduced by Autor and Dorn, 2013). As these alternative measures are calculated at the occupation level, occupations with the same task intensities would be assigned identical weights in the DFL approach. Our approach is more flexible as we allow separate weights for each occupation.

Figure 3.13: Log Wage Gaps - DFL Reweighting to 1995 Occupational Structure



NOTE.- The figures plot the wage gap between the 85th and 50th percentiles and the wage gap between the 50th and 15th percentiles. The solid lines show the observed wage gaps, while the dashed lines show the counterfactual wage gaps when the occupational employment shares are re-weighted to their 1995 level. The left panel refers to East Germany, the right one to West Germany.
Source: 2% SIAB Sample for male full-time workers between the ages of 20 and 62.

occupational structure play no role for the rise in top wage inequality in East and West Germany between 1995 and 2009. Hence, routine-biased technological change and the employment polarization of the wage distribution cannot explain the rising wage inequality at the top prior to 2009.

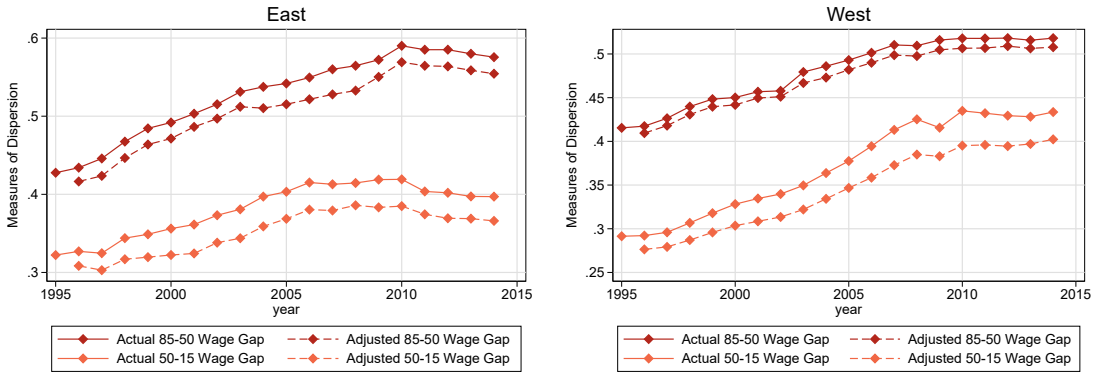
3.6.3 The Role of International Trade

Figure 3.10 indicates that between-industry shifts account for a sizable share of the rise in top wage inequality in East Germany. Globalisation with its accompanying reorganization of production and cross-border value chains might be an important driver of top end inequality during the 1990s and 2000s. Specifically, Eastern Europe and especially China have been integrated into the world market over our sample period. As a result, Germany's open economy experienced an increased demand for export goods and increasing competition from imports (e.g. Dauth et al., 2014, 2017b). Further, international trade affected some industries much more than others: export-oriented industries (like automobiles or machinery, for examples) experienced sizable growth, while industries facing import competition (like textiles, furniture or toys) suffered employment declines (see figure 3.B1). While East Germany had

traditionally a smaller export sector, employment shares in manufacturing, services and construction have moved closer to West German levels over time.

To examine the impact of sectoral employment changes on wage inequality, we re-weigh industry employment shares to their 1995 values using the DiNardo et al. (1996) approach. We present both actual and adjusted 85-50 and 50-15 log wage gaps in figure 3.14. Shifts in industrial employment shares have only a small effect

Figure 3.14: Log Wage Gaps - DFL Reweighting to 1995 Industry Structure



NOTE.- The figures plot the wage gap between the 85th and 50th percentiles and the wage gap between the 50th and 15th percentiles. The solid lines shows the observed wage gaps, while the dashed lines show the counterfactual wage gaps calculated for the industry shares observed in 1995. The left panel refers to East Germany, the right one to West Germany.
Source: 2% SIAB Sample for male full-time workers between the ages of 20 and 62.

on the 85-50 wage gap. However, the re-weighting procedure abstracts from changes in industry premiums over time. And as industry fixed effects explain a sizable share of upper-tail wage inequality, we next investigate whether trade-related wage premiums changed over our sample period.

To do so, we analyze how industry-level trade exposures to new trading partners in Eastern Europe and China have affected manufacturing wages.[28] Following Dauth et al. (2017b), we define the export exposure of an industry as the annual share of exports to new trading partners in its total exports. Import exposures are define

[28]The countries included for the calculation of the trade exposures are Azerbaijan, Bulgaria, Belarus, China, Czech Republic, Estonia, Georgia, Hungary, Kazakhstan, Kyrgyzstan, Lithuania, Latvia, Moldova, Poland, Romania, Russia, Slovakia, Slovenia, Tajikistan, Turkmenistan, Ukraine and Uzbekistan.

accordingly. Our estimation of the effect of trade exposures on wages is then given by

$$w_{ijt} = \beta_1 \text{ImExp}_{jt} + \beta_2 \text{ExExp}_{jt} + \gamma_i + \phi_t + \psi_J + \varepsilon_{ijt}, \tag{3.5}$$

where w_{ijt} represents log wages of individual i employed in manufacturing industry j in year t relative to 1995. The variables ImExp_{jt} and ExExp_{jt} measure the trade exposure to Eastern European countries and China for each 3-digit manufacturing industry in each year. To control for contemporaneous demand shocks, we implement the instrumental variable approach of Autor et al. (2013) where we instrument Germany's exposure to import and exports from Eastern Europe and China with trade exposures of eight other Western economies.[29] All specifications also control for year fixed effects ϕ_t, person fixed effects γ_i and 2-digit industry fixed effects ψ_J. We cluster standard errors at the industry-year level in all specifications. In

Table 3.6: Effects of trade on manufacturing wages

	(1)	(2)	(3)	(4)	(5)	(6)
		East			West	
	Below 15	15 to 50	50 to 85	Below 15	15 to 50	50 to 85
ImExp	0.044	-0.1885***	-0.2822***	-0.0234	-0.111***	-0.1509***
	(0.0758)	(0.043)	(0.0475)	(0.0275)	(0.0038)	(0.0119)
ExExp	0.1115	0.1704***	0.1633***	0.1464***	0.0587***	0.1615***
	(0.1114)	(0.0453)	(0.0564)	(0.038)	(0.0142)	(0.0187)
Emp. Share Export Mft.	10.01%	11.22%	10.67%	8.82%	12.62%	22.24%
Emp. Share Import Mft.	6.56%	12.77%	11.14%	5.93%	12.52%	15.48%
Year fixed effects	YES	YES	YES	YES	YES	YES
Person fixed effects	YES	YES	YES	YES	YES	YES
2-digit industry fixed effects	YES	YES	YES	YES	YES	YES
Observations	29,604	63,038	69,038	222,170	518,301	518,322
Within R^2	0.0330	0.0824	0.0606	0.0240	0.1129	0.2819

Note.-The table presents regression of log wages on import and export exposures of 3-digit industries. We split the sample along percentile categories and compute separate regressions for workers earning below the 15th percentile ('Below 15'), workers earning between the 15th and 50th percentiles ('15 to 50'), workers earning between the 50th and 85th percentiles ('50 to 85'). We also report the 2014 employment shares of export and import manufacturing for the seperate percentile categories.
Source: 2% SIAB Sample for male full-time workers between 20 and 62 years of age

addition, we have divided the sample into wage percentile categories, which we calculate from the wage percentiles for the entire period. The percentile categories are workers earning below the 15th percentile ('Below 15'), workers earning between

[29] These are Australia, Canada, Great Britain, Japan, Norway, New Zealand, Singapore and Sweden

the 15th and 50th percentiles ('15 to 50'), workers earning between the 50th and 85th percentiles ('50 to 85').

The results of these regressions are shown in table 3.6. Interestingly, import competition has negative wage effects at the top but the effects are much stronger in East Germany than in West Germany. In contrast, important competition has no or smaller wage effects below the 15th and below the 50th percentiles. In contrast, export exposure has strong positive wage effects at the top of the wage distribution in East and West Germany. However, even the East German employee earning between 15th and 50th percentiles benefits from rising exports. Between 1995 and 2014, the average export (import) exposure in East Germany increased by 12.6 (15.5) percentage points while it increased by 14 (16.9) p.p. in the West. Evaluating the effect of trade exposures on East German manufacturing wages between the 15th and 50th percentiles implies a 2.9% decline in wages associated with import exposure and a 2.1 % wage increase related to export exposure. Similarly, wages between the 50th and 85th percentiles, export exposure increases wages by 2.5%, while import exposure reduces wages by 4.3 %.

Overall then, the wage effects of import and export exposure are very similar for East German employees earning between the 15th and 85th percentiles. Furthermore, (export and import) manufacturing accounts for a comparatively small share of employment, the growth in trade with new trading partners do not play an important role for explaining the rise in top end wage inequality.[30]

3.7 Conclusion

Wage inequality has widened in both East and West Germany between 1995 and 2009, with wage dispersion in East Germany exceeding West Germany, especially at the top of the distribution. Since 2009 wage inequality is no longer rising in Germany and has even been declining in East Germany.

[30]Employees earning between the 50th and 85th percentiles in West Germany are much more likely to be employed in manufacturing. Furthermore, trade exposure is associated with smaller wage gains for wages between the 15th and 50th percentiles and large gains for workers earning above the median wage. Thus, trade exposure plays a larger role for upper-tail wage inequality in West Germany than in East Germany.

Compositional changes of the workforce and selection along the employment margin play only a minor role as does the decline of union coverage for the rise in wage inequality. Conversely, the introduction of minimum wages in some industries explains the complete reversal in wage inequality at bottom of the distribution after 2009. Changes on the demand side seem to be at the root of the rise in wage dispersion at the top. More rapid inequality growth in East Germany can be attributed to the slow increase in the number of highly skilled workers, partly due to strong east-west migration before 2010.

We also show that two major demand-side forces do not explain the rise in East German inequality at the top: Even though routine-biased technological change leads to employment polarization, it has little effect on the East or West German wage structure. The trade exposure to Eastern European countries and China, on the other hand, is far less important for the wage structure in the East than in the West, due to the smaller and less export-oriented manufacturing sector in East Germany.

3.A Data Appendix

In addition to our main data from the social security records, we use several additional datasets to characterize the labor demand side and minimum wages in the German labor market.

3.A.1 Sector-specific Minimum Wages

To assess the influence of sector-specific minimum wages on the wage structure, we obtain data on minimum hourly wages listed in generally binding collective bargaining agreements from the Federal Statistical Office (German Federal Statistical Office, 2017). From the reported minimum hourly wages, we then compute minimum daily and monthly wages assuming a 30 hours work week.

The following sectors have introduced sectoral minimum wages: Waste management (January of 2010), education and training services (August of 2012), the main construction industry (January of 1997), mining (November of 2009), roofing (October of 1997), electrical installation (June of 1997), hairdressing (November of

2013), buildings cleaners (June of 2007), scaffolding (August of 2013), temporary agency work (January of 2012), painting and varnishing (December of 2003), the nursing and care industry (August of 2010), chimney sweepers (April of 2014), security services (June of 2011), laundry services (November of 2009), stone masonry and stone carving (October of 2013).

3.A.2 Task-structure of occupations

To track labor demand changes in technology and offshoring, we use information on the task content of occupations from the 1998/99 wave of the BiBB Qualification and Career Survey (also used in Spitz-Oener, 2006; Dustmann et al., 2009; Gathmann and Schönberg, 2010). The survey asks almost 35,000 employees in East and West Germany about the content of their job. The East German sample includes about 7,000 observations, the West German one 28,000. The sample includes all workers aged 15 years and older who work in regular, paid employment for at least 10 hours per week. Apprentices or students working in a company were excluded.

Based on twelve questions about the task content of a job, we classify tasks into routine manual, non-routine manual, analytic and interactive tasks. Analytical tasks include organizing, conducting research and measuring. We classify training, consulting, buying, advertising and negotiating as interactive tasks. Non-routine manual tasks are repairing and nursing, while routine manual tasks include monitoring and producing. We then calculate each task measure using the criterion-validated method following Rohrbach-Schmidt and Tiemann (2013).

3.A.3 Trade Exposure

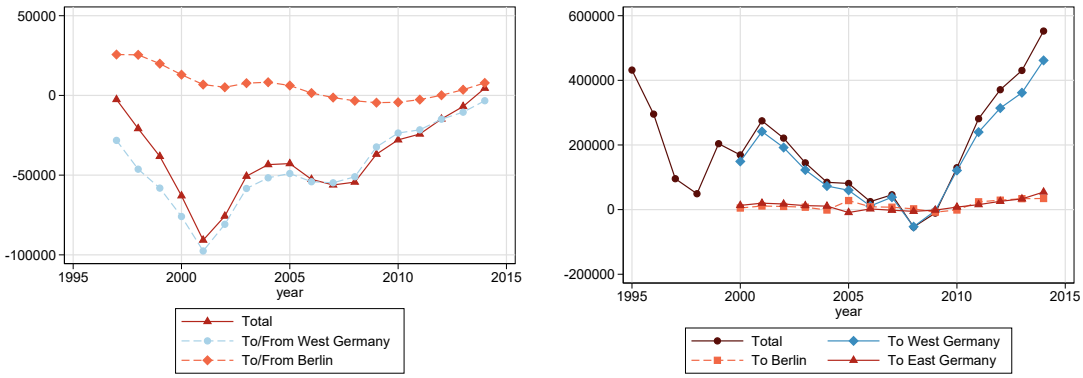
To track the influence of trade on the wage structure, we combine the social security records with aggregate data on trade exposure at the 3-digit industry level. The trade data come from the BACI international trade database (see Gaulier and Zignago, 2010, for a detailed description), a harmonized version of the UN COMTRADE database. The data contain information on trade flows between 150 countries for more than 5000 products using the SITC rev. 2/3 classification. Using

a correspondence table between SITC and 3-digit NACE codes, we then calculate industry-level trade-flows between countries for 92 percent of all products. We focus attention to trade in manufacturing and thus drop trade in raw materials and agricultural goods. All import and export flows are converted to 2014 prices.

We then calculate industry-level export and import intensities for Germany's new trading partners in Central and Eastern Europe whose markets became accessible after the fall of the Iron Curtain. Specifically, the trading partners include: Azerbaijan, Bulgaria, Belarus, China, Czech Republic, Estonia, Georgia, Hungary, Kazakhstan, Kyrgyzstan, Lithuania, Latvia, Moldova, Poland, Romania, Russia, Slovakia, Slovenia, Tajikistan, Turkmenistan, Ukraine and Uzbekistan. The import (export) intensities are defined as the share of annual imports (exports) relative to total industry-specific German imports (exports) (see Dauth et al., 2017b, for details).

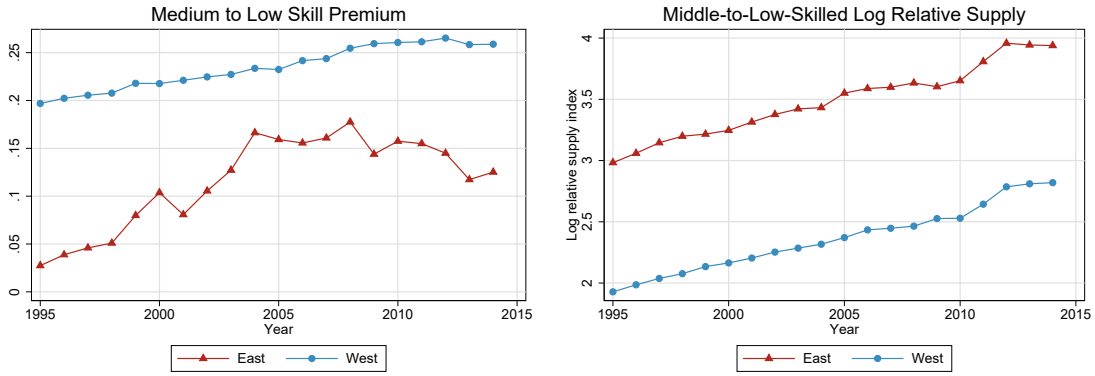
3.B Additional Results

Figure 3.B1: Net Migration between East and West Germany and from Abroad



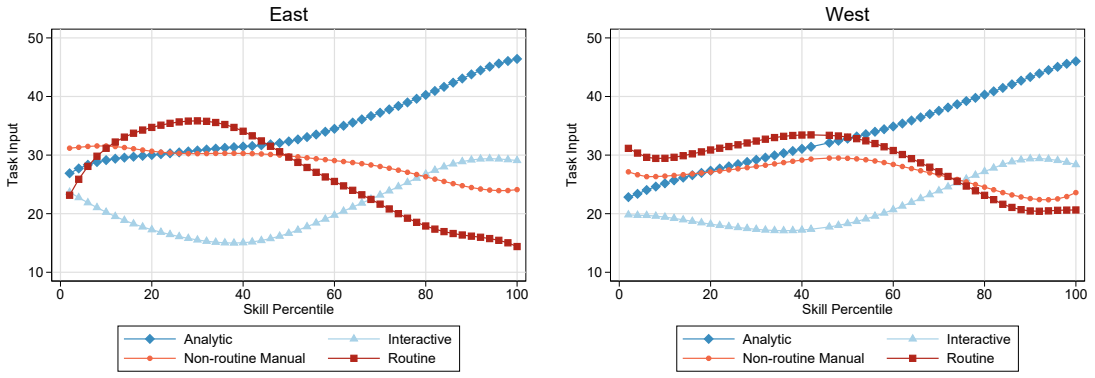
NOTE.- The left panel shows internal net migration between East and West Germany as well as Berlin between 1995 and 2014. The right panel shows net migration to Germany since 1995 and separately to East and West Germany as well as Berlin since 1997.
Source: Federal Statistical Office.

Figure 3.B2: Skill Premium and Relative Skill Supplies of Medium- to Low-skilled Workers



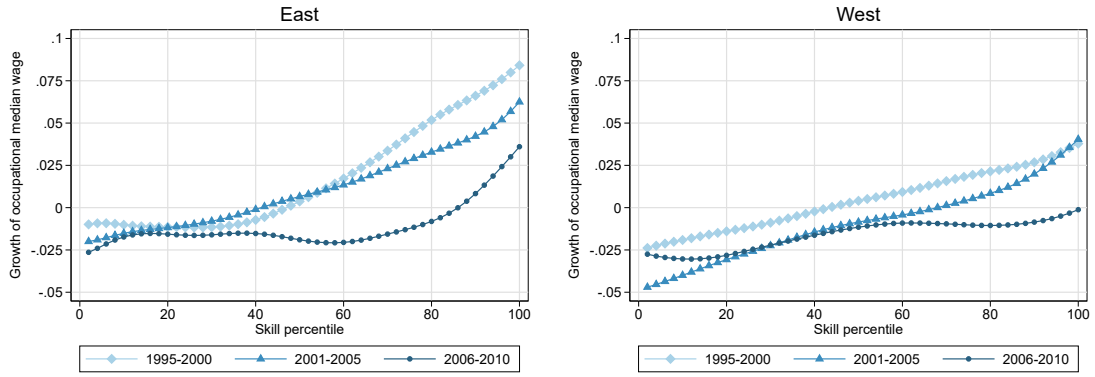
NOTE.- The left panel plot the age-adjusted skill premium between medium- and low-skilled men for East and West Germany. The right panel plots the relative skill supply of medium-skilled relative to low-skilled workers measured in efficiency units.
Source: 2% SIAB Sample for male full-time workers between the ages of 20 and 62.

Figure 3.B3: Occupational task inputs



NOTE.- The figures plots along task inputs across the occupational skill distribution.
Source: 2% SIAB Sample for male full-time workers between the ages of 20 and 62.

Figure 3.B4: Occupational median wage shifts along the skill distribution



NOTE.- The figures plot the change in occupational median wages along the skill distribution.
Source: 2% SIAB Sample for male full-time workers between the ages of 20 and 62.

Table 3.B1: Summary Statistics

	Monthly Wages					
	East			West		
	1995	2004	2014	1995	2004	2014
Mean	2598	2733	2836	3668	3822	3883
Standard Deviation	1069	1403	1470	1427	1844	1955
15-percentile	1696	1593	1620	2492	2345	2220
50-percentile	2350	2368	2400	3334	3368	3420
85-percentile	3606	4039	4290	5045	5473	5700
	Sectoral Employment Shares					
	East			West		
	1995	2004	2014	1995	2004	2014
Export Manufacturing	7.63%	10.14%	10.88%	17.93%	18.04%	17.40%
Import Manufacturing	9.71%	10.50%	10.98%	17.49%	14.72%	13.04%
Services	60.77%	66.24%	66.38%	52.66%	58.42%	61.20%
Construction	17.57%	10.03%	9.03%	9.08%	6.57%	6.30%
Other	4.33%	3.10%	2.74%	2.84%	2.25%	2.06%
	Aggregate Variables					
	East			West		
	1995	2004	2014	1995	2004	2014
GDP per capita	21830	23825	27618	32180	33571	36898
Unemployment rate	13.90%	18.40%	9.80%	8.10%	8.50%	5.90%
Employment rate	43.99%	41.69%	47.77%	44.25%	43.63%	49.77%

NOTE.- The table shows summary statistics of wages, sectoral composition and aggregate indicators of economic activity for East and West Germany in 1995, 2004 and 2014. The employment rate is calculated relative to the total population in each region and year.To do so, we divide the manufacturing sector into export- and import-intensive industries. We subdivide the manufacturing sector into import and export manufacturing. Following Dauth et al. (2017a), we define export manufacturing if the change in net exports to the trading partners from Eastern Europe and China was above the median change for manufacturing as a whole between 1995 and 2014. The new trading partners included are Azerbaijan, Bulgaria, Belarus, China, Czech Republic, Estonia, Georgia, Hungary, Kazakhstan, Kyrgyzstan, Lithuania, Latvia, Moldova, Poland, Romania, Russia, Slovakia, Slovenia, Tajikistan, Turkmenistan, Ukraine and Uzbekistan. Similarly, manufacturing industries are import-intensive if their net exports are below the median change for manufacturing as a whole over the same period.

Source: 2% SIAB Sample for German male full-time workers between 20 and 62 years of age

Table 3.B2: West German Wage Inequality by Education and Age

	Low Education								
	1995		2004		2014		1995	2004	2014
	50-15 gap	85-50 gap	50-15 gap	85-50 gap	50-15 gap	85-50 gap	Employment Share		
20-36	0.32	0.23	0.53	0.34	0.39	0.37	5.0%	2.9%	1.9%
37-47	0.24	0.23	0.34	0.25	0.45	0.32	4.2%	3.6%	1.5%
48-62	0.22	0.23	0.28	0.24	0.39	0.27	4.2%	2.9%	2.3%
All	0.28	0.22	0.45	0.27	0.46	0.33	13.5%	9.4%	5.8%
	Medium Education								
	1995		2004		2014		1995	2004	2014
	50-15 gap	85-50 gap*	50-15 gap	85-50 gap*	50-15 gap	85-50 gap*	Employment Share		
20-36	0.25	0.28	0.33	0.33	0.33	0.35	32.1%	24.2%	21.3%
37-47	0.27	0.35	0.31	0.37	0.37	0.42	23.9%	30.9%	23.2%
48-62	0.28	0.38	0.32	0.43	0.38	0.43	20.1%	20.4%	29.6%
All	0.27	0.36	0.33	0.38	0.37	0.42	76.1%	75.5%	74.1%
	High Education								
	1995		2004		2014		1995	2004	2014
	50-15 gap*	85-50 gap*	50-15 gap*	85-50 gap*	50-15 gap*	85-50 gap*	Employment Share		
20-36	0.29	0.34	0.36	0.34	0.34	0.33	3.8%	3.9%	5.1%
37-47	0.31	0.30	0.36	0.39	0.40	0.36	4.2%	7.5%	7.4%
48-62	0.26	0.28	0.39	0.38	0.42	0.39	2.5%	3.7%	7.7%
All	0.35	0.33	0.39	0.40	0.43	0.40	10.5%	15.1%	20.1%

NOTE.- The table shows 85-50 and 50-15 log wage gaps in West Germany both across and within age and education groups as well their employment shares in 1995, 2004 and 2014. The results are based on imputed and hence, uncensored wages. The star denotes that the the 85th wage percentile for the high-skilled is above the censoring bound.
Source: 2% SIAB Sample for East German male full-time workers between 20 and 62 years of age

Table 3.B3: Log Wage Gaps after Imputation - West Germany

	Imputation of leavers' wages			
	1996		2004	
	50-15 Gap	85-50 Gap	50-15 Gap	85-50 Gap
No Imputation	0.29	0.42	0.36	0.49
Carried-forward wages	0.31	0.42	0.38	0.49
Imputation on observables	0.28	0.40	0.35	0.46
Leaver wages set to 50-pct.	0.27	0.39	0.33	0.45
Leaver wages set to 15-pct.	0.25	0.43	0.31	0.50
	Imputation of entrants' wages			
	2004		2014	
	50-15 Gap	85-50 Gap	50-15 Gap	85-50 Gap
No Imputation	0.36	0.49	0.43	0.51
Carried-backward wages	0.39	0.49	0.45	0.52
Imputation on observables	0.35	0.47	0.41	0.49
Entrant wages set to 50-pct.	0.34	0.45	0.40	0.48
Entrant wages set to 15-pct.	0.32	0.50	0.38	0.54

NOTE.- The table shows 50-15 and 85-50 log wage gaps including individuals who have no full-time job spell next year (top panel); and the same wage gaps when the wages of entrants in the West German labor market are included. Carried-forward wages use the panel structure to impute wages; imputation on observables predicts missing wages based on education, age and year. The last two imputation methods set missing wages to the 50th percentile or the 15th percentile of employees in the particular year.
Source: 2% SIAB Sample for West German male full-time workers between 20 and 62 years of age

Table 3.B4: Demographic Adjustments and Change in Log Wage Gaps

	Change in 50-15 Log Wage Gap					
	Overall Log Wage Gap			Residual Log Wage Gap		
	1995-2004	2004-2014	1995-2014	1995-2004	2004-2014	1995-2014
Actual	0.070	-0.003	0.067	0.046	0.001	0.047
1995 demographics	0.049	0.011	0.060	0.028	0.000	0.028
2004 demographics	0.070	0.009	0.079	0.048	-0.001	0.047
2014 demographics	0.075	-0.016	0.060	0.050	-0.027	0.023
	Change in 85-50 Log Wage Gap					
	Overall Log Wage Gap			Residual Log Wage Gap		
	1995-2004	2004-2014	1995-2014	1995-2004	2004-2014	1995-2014
Actual	0.106	0.047	0.153	0.079	0.025	0.104
1995 demographics	0.087	0.018	0.105	0.065	0.020	0.085
2004 demographics	0.112	0.041	0.153	0.077	0.026	0.103
2014 demographics	0.100	0.021	0.121	0.074	0.015	0.089

NOTE.-The table shows the evolution of total and residual log wage gaps for the periods from 1995 to 2004, 2004 to 2014 and 1995 to 2014. We present both the actual change in wage gaps and changes in wage gaps where demographics are adjusting to the workforce composition of 1995, 2004 and 2014.
Source: 2% SIAB Sample for East German male full-time workers between 20 and 62 years of age

Table 3.B5: Entrant and Leaver Characteristics

	1995				2004			
	Stayers		Leavers		Stayers		Leavers	
	15 to 50 Pct.	Median	Overall	Outmigrants	15 to 50 Pct.	Median	Overall	Outmigrants
Mean Wage	2026	2350	2427	2367	1973	2368	2253	2146
Std. Dev. Wage	180		1116	1177	210		1272	1287
Mean Residual Wage	2309	2539	2472	2611	2205	2598	2301	2370
Std. Dev. Residual Wage	429	529	936	952	454	295	1080	1029
Share Low-Skilled	4.0%	2.7%	5.7%	3.8%	2.4%	1.5%	5.6%	7.0%
Share Medium-Skilled	92.3%	89.1%	77.4%	82.2%	93.4%	96.9%	78.4%	80.8%
Share High-Skilled	3.7%	8.2%	16.9%	14.1%	4.3%	1.5%	16.0%	12.2%
Mean Age	38.31	39.06	42.11	32.14	40.29	40.82	42.90	34.79
Std. Dev. Age	10.46	10.26	12.42	8.95	10.28	8.98	12.25	9.84

	2004				2014			
	Stayers		Entrants		Stayers		Entrants	
	15 to 50 Pct.	Median	Overall	First Entrants	15 to 50 Pct.	Median	Overall	First Entrants
Mean Wage	1973	2368	2079	1761	1986	2400	2286	2088
Std. Dev. Wage	210		1111	989	208		1251	1030
Mean Residual Wage	2205	2598	2167	2309	2210	2563	2307	2443
Std. Dev. Residual Wage	454	295	1006	872	441	476	1087	858
Share Low-Skilled	2.4%	1.5%	3.4%	13.8%	1.6%	2.1%	2.3%	8.6%
Share Medium-Skilled	93.4%	96.9%	83.7%	72.9%	94.0%	90.5%	80.6%	65.2%
Share High-Skilled	4.3%	1.5%	12.9%	13.3%	4.4%	7.4%	17.0%	26.3%
Mean Age	40.29	40.82	40.23	26.73	42.13	42.17	41.16	28.78
Std. Dev. Age	10.28	8.98	10.08	8.56	11.37	11.02	10.80	7.82

NOTE.- The table displays the characteristics of labor market leavers, entrants and stayers. The upper panel compares east German labor market stayers between the 15th and 50th wage percentile and at the median wage with all leavers (who were employed in East Germany in the year t, but not in $t+1$) and with a subgroup of leavers with a new job in West Germany in a later year both for the years 1995 and 2004. The bottom panel compares labor market entrants (who do not have a record in t, but a social security record in $t+1$) in the 2004-2014 to stayers. We distinguish between all entrants and first entrants (who appear in the social security records for the first time).
Source: 2% SIAB Sample for East German male full-time workers between 20 and 62 years of age

Chapter 4

A Novel Approach to Estimate Labor Supply Elasticities: Combining Data from Actual and Hypothetical Choice[1]

4.1 Introduction

The preference for leisure which characterizes how individuals trade off working time and income is a key parameter for economic policy. The income-leisure tradeoff not only determines retirement choices among older workers but also the labor supply responses to social transfers (see Blundell and MaCurdy, 1999). It is also an important parameter to assess the welfare costs of taxation (e.g. Saez, 2002; Prescott, 2004; Alesina et al., 2005)

A vast literature has estimated labor supply elasticities at the extensive and intensive margin using a variety of empirical approaches. By and large, the profession has settled on a value for the compensated intensive margin elasticity close to zero for prime-age males and somewhat higher for married women. There is also a consensus that intensive margin elasticities are smaller than extensive margin elasticities (see e.g.the surveys in Pencavel, 1986; Heckman, 1993; Blundell and MaCurdy, 1999; Meghir and Phillips, 2010; Keane, 2011).

Recently, a number of authors have questioned the above consensus (e.g. Keane, 2011; Meghir and Phillips, 2010; Pencavel, 2016). Building on earlier contributions, they argue that popular empirical approaches may not identify the preference for

[1]This chapter is co-authored with Christina Gathmann

leisure after all. Instead, the estimates identify an equilibrium parameter that is composed of both preferences and frictions. Frictions can emerge both on the supply and the demand side. On the labor supply side, for instance, optimization errors due to rational inattention, inertia or switching costs might reduce the responsiveness of workers to changes in wages or taxes. On the labor demand side, firms might be constrained to offer certain bundles of working hours and wages to their employees. In this last case, the response of one worker to tax or wage changes not only depends on her taste for leisure but also upon the flexibility of the production function.

These frictions drive a wedge between the behavioral elasticity which matters for long-run welfare and the observed elasticity which is often estimated from short-run variation in wages or taxes (e.g. Chetty et al., 2012). Such a wedge has important consequences for policy. The efficiency cost of a tax levied on a worker, for example, then depends not just upon on her labor supply elasticity but also upon the production process in case of demand side frictions. Similarly, the consensus that it is optimal to levy higher tax rates on men than women because they are less elastic (see e.g. Alesina et al., 2011) might no longer hold if the difference in observed elasticities across genders is caused by heterogeneity in occupational frictions on the demand side rather than tastes on the supply side. And yet, most approaches in the literature ignore supply and demand side frictions, leading to downward biased estimates of behavioral elasticities.

Recent evidence suggests however, that frictions are an important part of observed responses to taxes or transfers (see Chetty et al., 2011; Chetty, 2012; Gelber et al., 2013; Kleven and Waseem, 2013). These studies use nonlinearities (kinks) or discontinuities (notches) in the tax system to identify labor supply responses. In most cases however, these kinks or notches (or their disappearance) only affect a relatively small share of the working population. Due to heterogeneity in labor supply responses across individuals, local elasticities may differ from population parameters typically needed to evaluate the steady-state effects of tax and transfer policies at the national level (Manski, 2014; Blundell and MaCurdy, 1999).

In this chapter, we propose a different approach to estimate the preferences for leisure at the intensive margin. We use data on hypothetical labor supply choices to separate true preferences from constraints. As a first step, we pose a variety of hypothetical labor supply choices to a representative sample of respondents in Germany. Each scenario consists of two choices specifying a combination of hours of work, earnings and other income. For example, one choice would offer higher wages and hours while the other would offer both lower wages and hours but similar non-labor income. Respondents then choose which alternative they would prefer.

Our hypothetical decisions approach could provide important insights to supplement existing econometric knowledge with additional information that is not readily available with actual choices (Manski, 2014). Since the hypothetical choice scenarios are not limited by what nature offers, we can elicit choices over a wide range of wage and hours. Estimates from actual choices often rely on small changes in wages or tax rates instead. Even more importantly, kinks or notches often affect a small share of the total population only. If preferences for leisure vary in the population, the elasticities identified at the kink or notch might not be representative of the response in the population (Blundell and MaCurdy, 1999). In addition, our data contain hypothetical choices from workers as well as non-workers. We therefore do not have to worry about the selection problem of assigning wages to non-workers.

We use the data on hypothetical choices from our sample of respondents to estimate the taste for leisure. Our empirical framework relies on a random utility model (McFadden and Train, 2000; Hensher et al., 1999). Specifically, we use mixed logit methods which allow for heterogeneity both along observable and unobservable dimensions (Train, 2003; Revelt and Train, 1998).[2]

We show that there is a lot of heterogeneity in labor supply elasticities. This heterogeneity can only be partly attributed to differences along demographic variables such as gender, age or education. Our estimates are robust to a number of alternative specifications for the underlying utility function or the estimation method.

[2]Hausman (1981) is an early contribution suggesting a random coefficient model to incorporate individual preference heterogeneity (but based on revealed preference data).

Our analysis contributes to two strands of economic literature. Most importantly, we contribute to the vast literature on labor supply elasticities (see Blundell and MaCurdy, 1999; Keane, 2011; Meghir and Phillips, 2010, for recent surveys). Several contributions to this literature have relied on tax reforms to identify the elasticities of interest (see e.g. Eissa, 1995; Eissa and Hoynes, 2004; Meyer and Rosenbaum, 2001; Blundell et al., 1998; Bianchi et al., 2001). More recently, studies have relied on kinks or notches for identification (Chetty et al., 2011; Kleven and Waseem, 2013; Gelber et al., 2013). Most of the literature imposes strong functional form assumptions and assumes that the response to tax or wage changes is the same for everybody (see Burtless and Hausman (1978); Stern (1986); Mroz (1987) for early contributions; Blundell and Shephard (2012); Blundell et al. (2011) for recent examples that allow for heterogeneity across socio-demographic groups).

Second, we contribute to a growing literature that relies on stated preferences to identify behavioral responses (see Barsky et al. (1997) for evidence on risk tolerance and time preferences; Van Soest and Vonkova (2014) for retirement choices; Kapteyn and Teppa (2003) for inter-temporal consumption choices; Blass et al. (2010) for preferences for electricity; or Benjamin et al. (2012) for measures of subjective well-being). We are the first to apply the stated preference approach to labor supply choices. To allow respondents to be uncertain about their choices, we employ probabilistic polling following the suggestion by Manski and Molinari (2010).

The chapter is structured as follows. The next section introduces the representative online panel and our data on hypothetical choices. The third section presents a simple static labor supply model that provides the basic framework for our estimations. Section 4 lays out our empirical strategy based on a random utility framework and mixed logit models to estimate labor supply elasticities from hypothetical choices. Section 5 reports our estimated preferences for leisure and explores their heterogeneity. Section 6 presents a range of robustness checks. Finally, Section 7 discusses the implications of our results and concludes.

4.2 Data Sources

4.2.1 German Internet Panel (GIP)

The data come from a random probability sample of the population in Germany. The survey elicits information from all adult household members aged between 16 and 75.[3] Recruitment was conducted offline with short face-to-face interviews (about 15 minutes), during which respondents were invited to the online panel. To improve the representativeness of the sample, the survey includes respondents without computer and internet access, by providing them with the necessary equipment and training. Eligible persons, who had a computer and a broadband Internet connection at home, were sent an invitation letter together with a login name and a password to the online survey. Households without Internet access and/or computer were also invited to the study and sent login details. Furthermore, they were informed that someone would call them to make an appointment to install the Internet and/or a personal computer.

Comparing the online sample to population distributions of the German census of 2011 shows that the German Internet Panel (GIP) represents basic demographic groups well. Including previously off-line respondents in the panel substantially improves the representativeness of the GIP with regard to both age and gender. However, the GIP somewhat under-represents the oldest age-groups (above 65) but we exclude this group from our sample below. As such, the results from the internet panel are representative of the population in Germany.

Every two months (at the first day of each uneven month), the survey participants receive an invitation (mostly per email) to participate in a new survey wave. Each survey lasts about 20-25 minutes. Participants receive four Euros for participation as well as a bonus payment if they participate in all surveys in a given calendar year. In addition, extensive efforts are made by the survey team to maintain the panel. As a consequence of these monetary and non-monetary incentives, panel participation rates are high: 96% of all participants registered online participated

[3]TNS Infratest (2012) and Blom et al. (2015) provide a more detailed description of the sampling strategy and recruitment in the German Internet Panel.

in the first wave, 80% are still in wave 3 and 73% in wave five. Table 4.A1 provides summary statistics for the sample we use.

4.2.2 Hypothetical Labor Supply Choices

We pose a series of hypothetical decision scenarios to the same sample of respondents at two points in time (March 2013 and January 2014). Each scenario shows the respondent two alternatives: one choice might have high working hours per week and net wages, while the other might offer lower working hours per wages and net wages with similar non-labor income.[4]

The choice scenario is presented in such a way that individuals interpret them as a shift in the wage profile and not just a transitory change in the wage. The scenarios are presented in blocks of four choice questions separated by questions on other topics. Overall, we elicit twelve hypothetical choices on the intensive margin: eight choices in the first survey and four choices in the second wave.

To design the choices, we start from actual working hours, wages and different income measures for each individual ("pivoting"). Pivoting allows us to to present more realistic choice scenarios to each individual which improves the reliability of answers. At the same time, it introduces some correlation between the characteristics of hypothetical choices and unobserved components in the utility function which influences actual choices. In principle then, the starting point of our hypothetical choice scenarios (actual wages and hours) are endogenous as they are based on the outcome of an optimization problem. However, Train and Wilson (2008) shows that under certain assumption, standard methods can still be used to identify the parameters of interest. For individuals who are un- or non-employed, we use the wages and working hours of the last job. For individuals with missing information, we assign average wage based on education (3 groups), age (5 year groups) and gender separately for East and West Germany. We have four questions in each block in each wave. The order of each block within a survey wave and the order of each question in the block is randomized for every individual. In our scenarios, we

[4]See figure 4.A1 for a screenshot of a choice scenario shown to the survey participants.

vary wages from from -30% and +70% of the actual wage in our first survey wave and from from -80% and +60% in our second survey wave. Hours are calculated accordingly by increasing (or decreasing) hours by the same percentage as wages. To introduce additional variation in our data we multiply the hours in the choice scenarios in the first survey wave by a hypothetical elasticity drawn from the interval [-0.4; 1.5]. In the second survey wave we vary hours in 10 hours increments ranging from 0 to 60 hours. Moreover, we include both a question block, where non-labor income does not vary and one where we randomly decrease or increase non-labor income in the given scenario. In our extensive margin scenarios in the first wave, we lower wages 30%, 50% or 70%. Hours are then calculated in the work option as for the intensive choices. In the second wave we adjust wages between -70% and +50% and set hours in the work option are actual hours (or some positive minimum hours if zero). Income in the not working option are adjusted to proxy welfare payments.

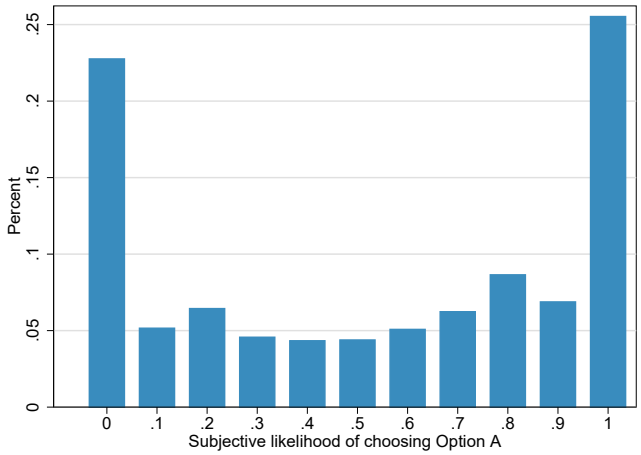
As hypothetical choice models might not provide the full information set as in actual choices (e.g. some context information might be missing), we use probabilistic polling to allow that individuals are uncertain about their decision. We use an 11-point answer scale based on evidence that most people round probability questions to the nearest 10 percent. Therefore, it does not seem informative to include the full 1-100 scale. Manski (2012) argues that one should use probabilistic choices rather than stated choices in lieu of actual choices ("revealed preferences") to allow for measurement error (esp. because individuals might not be sure which choice to prefer).[5].

Figure 4.1 shows a histogram of responses for all scenarios. The responses are strongly bimodal with around 50% of the probability mass concentrated at the two endpoints. The histogram suggests that respondents were certain about their decisions. We therefore collapse the data to binary decisions and use variants of the mixed logit model for estimation.[6].

[5]The idea of measuring choice intentions probabilistically has much precedent, dating back to Juster (1966). Probabilistic polling has recently been used to elicit beliefs about future, uncertain events (see Manski, 2004; Hurd, 2009; Delavande et al., 2014, for surveys)

[6]The less parametric LAD estimator proposed by Manski (1985) does not perform well when the probability mass is concentrated at the extremes.

Figure 4.1: Histogram of choices (overall)



4.3 Theoretical framework

We start out with the simple static labor supply model that we will use to compute elasticities from our estimation of random utility models. Here, we make the standard assumptions that the utility function is additively separable over time which allows us to focus on the allocation of income between consumption and leisure; yet, it rules out dynamic aspects such as human capital investments (Keane, 2011). The utility function of the individual is given by $U_i(c_i, h_i)$ subject to the budget constraint:

$$A_i + w * h_i = c_i \tag{4.1}$$

where we normalize the price of the consumption bundle to 1. Conditional on $h > 0$, we can substitute the budget constraint into the utility function. The first-order condition is then given by:

$$w * U_c(wh_i + A, h_i) + U_h(wh_i + A, h_i) = 0 \tag{4.2}$$

where $U_c = \frac{\delta U}{\delta c}$ and U_h is defined accordingly. Equation 4.2 implies that the wage rate is equal to the marginal rate of substitution between leisure and consumption. Next, we use the the total differential of equation 4.2 to examine, how hours worked

respond to changes in wages and income:

$$(U_c+U_{cc}*wh+U_{hc}*h)dw+(w*(U_{cc}*w+U_{ch})+U_{hc}*w+U_{hh})dh+(U_{cc}*w+U_{hc})dA=0 \tag{4.3}$$

Now, define $B = w*(U_{cc}*w+U_{hc})+U_{ch}+U_{hh}$. The second-order condition for a maximum implies that $B<0$. Furthermore, the quasi-concavity of the utility function also implies that $U_{cc} \leq 0$ and $U_{hh} \geq 0$.

Holding wages constant (i.e. $dw = 0$), the effects of non-labor income on labor supply are given by:

$$\frac{dh}{dA} = \frac{w*U_{cc}+U_{hc}}{-B} \tag{4.4}$$

The denominator is positive (because of the second-order conditions). The nominator and hence, the income effect may be positive, negative or zero. The income effect will be negative (such that hours worked decline when unearned income increases) when $U_{hc}<0$, i.e. if the utility function is such that additional income always raises the marginal disutility of work. It will also be negative as long as U_{hc} is not "too" positive, i.e. as long as additional income does not reduce the marginal disutility of work "too much".[7]

The effects of wage changes on labor supply (holding income constant, i.e. $dA = 0$) are:

$$\frac{dh}{dw} = \frac{U_c+h(w*U_{cc}+U_{hc})}{-B} \tag{4.5}$$

which we can also write as the Slutsky equation:

$$\frac{dh}{dw} = \frac{U_c}{-B}+h(\frac{dh}{dA}) \tag{4.6}$$

The first term is the substitution effect which is always positive. the second represents the income effect and is a-priori indeterminate. Note that if utility is

[7]Note that if the utility function is linear or quasi-linear, the income effect would be zero, since the first term in the denominator would be zero as $U_{cc}=0$. And additive separability between consumption and leisure implies that $U_{ch}=U_{hc}=0$

linear (or quasi-linear in income), the second term in equation 4.6 is zero. We later use the simple model in this section to compute hicksian and marshallian labor supply elasticities for the utility functions we specify in our random utility estimations.

4.4 Mixed Logit Estimation

Having rich data on hypothetical discrete choices, we use a random utility framework (RUM) to represent the choices individuals make (Thurstone, 1927; McFadden, 1974). For each choice c, a decision maker i chooses an alternative j if her utility is larger than the utility of any other alternative. The utility of person i choosing alternative j for choice c is given by

$$U_{ijc} = \beta_i' x_{ijc} + \varepsilon_{ijc} \,, \tag{4.7}$$

where x_{ijc} is a vector of the observed characteristics of an alternative, for example, hours of work, wages and unearned income. ε_{ijc} is an i.i.d extreme value distributed random term. The parameters of interest are the random coefficients β_i [8]. The resulting choice model is a mixed logit model which allows for individual-specific heterogeneity in tastes as well as correlation between alternatives.[9] Furthermore, the mixed logit is flexible enough to approximate any random utility model, given the choice of variables and mixing distribution (McFadden and Train, 2000)see.

The individual-specific coefficients can be specified as $\beta_i = \bar{\beta} + \eta_i$ where $\bar{\beta}$ is the mean preference in the population and η_i describes the individual heterogeneity around the population mean. Due to this heterogeneity, β_i is distributed with some density $f(\beta_i|\theta)$ with parameters θ. This distribution can take different functional forms. We will use a normal distribution, such that $\eta_i \sim N(0, \sigma_\eta)$. However, in our robustness checks we also relax this parametric assumption by using a finite mixture. [10] Moreover, observable heterogeneity along individual characteristics can

[8] With a linear utility function, the β_i would simply be the marginal utilities for the observed x_{ijc}

[9] See Revelt and Train (1998) for an early application of mixed logit using repeated choices for the same individual.

[10] In this case our choice model is a latent-class logit model (Greene and Hensher, 2003)see.

be incorporated in a similar way, such that $\beta_i = \gamma' Z_i + \eta_i$, where Z_i are individual demographics like age and education group, number of children.

Next, we will discuss estimation, for which we will need the probability of observing each individuals choices as function of the mixing distributions parameters θ. Conditional on β_i, the probability of observing a sequence of C choices $c = 1, ..., C$ for decision maker i is given by:

$$S(\beta_i) = \prod_{c=1}^{C} \prod_{j=1}^{J} \left(\frac{exp(\beta_i' x_{ijc})}{\sum_{j=1}^{J} exp(\beta_i' x_{ijc})} \right)^{y_{ijc}}, \tag{4.8}$$

where y_{ijc} indicates whether individual i chose alternative j when facing choice c. Integrating over the distribution of the random coefficient, the probability of observing individual i making the particular sequence of choices is:

$$P_i(\theta) = \int S_i(\beta_i) f(\beta|\theta) d\beta_i \tag{4.9}$$

The distribution parameter θ can then be estimated by simulated maximum likelihood (SML).[11] Based on SML, we obtain estimates for the mean preference in the population ($\bar{\beta}$) and the variation of preferences in the population (σ_η). In addition, we can estimate the preference parameter β_i for each individual from the observed sequence of hypothetical choices and choice characteristics as follows:

$$E[\beta_i|y_i, x_i] = \frac{\int \beta S(\beta_i) f(\beta|\theta) d\beta}{\int \prod_{c=1}^{C} \prod_{j=1}^{J} L_{ijc}(\beta)^{y_{ijc}} f(\beta|\theta) d\beta} \tag{4.10}$$

These individual preference parameters can be used to compute marginal utilities for working hours and consumption for everyone. Depending on the utility function used, marginal utilities are a function of one or more coefficients and the levels of consumption and working hours. [12] We then use the actual values of consumption and working hours to compute the marginal utility and elasticity for each individual in our sample.

[11] Train (2003) provides an overview of the simulation methods available.

[12] With a linear utility function the preference parameters themselves are marginal utilities.

4.5 Preferences from Hypothetical Choices

4.5.1 Baseline Results

We first report on our benchmark results for a quadratic utility function. Stern (1986) compares different utility functions used to analyse the response of hours worked to wages and finds that the quadratic direct utility function offers considerable flexibility. This is because the quadratic utility function can serve as a second degree Taylor approximation to any utility specification. Consequently, our model should be sufficiently flexible to allow for a wide range of possible substitution patterns.

Our basic specification only includes variables for total income (calculated as the sum of monthly wages and non-labor income) and hours worked as well as their quadratic terms. To relax the additive separability assumption for the consumption-leisure decision, we add an interaction term between the two as well. Estimation is based on a mixed logit model and uses a normal mixing distribution to model unobserved heterogeneity.

Table 4.1: Baseline Estimates - Intensive Margin

	(1)	(2)	(3)
Hours	-0.12675***	0.06597***	0.06129***
	(0.0036614)	(0.0084979)	(0.0086008)
sd Hours	0.07421***	0.1076***	0.10819***
	(0.0031812)	(0.0043523)	(0.0043379)
Consumption	0.0049***	0.01054***	0.01033***
	(0.0001538)	(0.0003803)	(0.0003825)
Consumption^2		1.83×10^{-6}***	2.09×10^{-6}***
		(1.09×10^{-7})	(1.32×10^{-7})
Hours^2		-0.00265***	-0.00282***
		(0.000105)	(0.000117)
Consumption × Hours			0.00002***
			(5.40×10^{-6})
Observations	24138	24138	24138
AIC	12656.65	11092.68	11082.19
BIC	12680.92	11133.14	11130.74
Pseudo R^2	0.0521	0.0936	0.0945

Note.- The table reports the baseline mixed logit results for 3 different utility specifications. Robust standard errors are reported in parentheses. The table includes the Akaike information criterion (AIC), the Bayesian information criterion (BIC) and MacFadden's Pseudo R^2 for each specification. $***$ $p < 0.01$

Moreover, we model a random effect on the hours variable, allowing for different leisure-preferences across individuals. For simplicity, we assume a normal

distribution for this coefficient. However, we show below that this functional form assumption is not crucial for our results.

Table 4.1 shows the results for our baseline model. The estimates are consistent with economic theory. While the coefficients for hours in column (2) and (3) are positive, we find negative marginal utilities of working hours and positive marginal utilities of consumption for almost all survey participants. Furthermore, the coefficient for the interaction term in column (3) is statistically significant which suggests that consumption and working hours are not additively separable.

At the bottom of the table, we report common information criteria for our alternative specifications of the utility function for model choice. Our quadratic model (in column (3)) outperforms a simple linear specification (in column (1)) as well as a specification without an interaction term between hours and consumption (in column (2)). The quadratic utility specification has the lowest Bayesian Information Criterion and the highest Pseudo-R^2. Although there seems to be only a minor difference in the criteria between the specifications in Column (2) and (3), the difference is very robust to further extensions of the model. When more coefficients are modeled with a random effect and the parametric assumptions on the distribution of the random effect are relaxed in a latent-class logit model, the difference between a model that allows for non-additive separability and one that does not becomes more pronounced.

From our preferred specification (column (3)), we then calculate compensated and uncompensated labor supply elasticities. We find positive elasticities of hours with respect to wages below 1 for most of our respondents. For less than 1% of our sample, Hicksian labor supply elasticities are negative which would imply a backward bending labor supply curve. We find very high elasticities for about 10% of the sample. The median Hicksian labor supply elasticity for the entire sample is 0.41. Income effects are consistently negative, but small.

4.5.2 Heterogeneity

We next explore how well observable demographics typically employed in the literature can explain the heterogeneity we observe in our data (see e.g. Blundell and Shephard, 2012; Blundell et al., 2011). We add commonly used observable characteristics like gender, age, college education and the presence of small children to our specification. Specifically, we add interaction effects between the choice characteristics and the observed demographics. If observed demographics capture the preference heterogeneity well, the re-estimated parameter of preference variation (σ_η) should be small and possibly no longer statistically significant. As above, we use the estimated parameters from the mixed logit model to predict labor supply elasticities for each individual from her characteristics X_{ic} and observed sequence of choices.

We find that all demographic interactions are highly statistically significant.[13] Hence, preferences for leisure indeed differ for men and women, between low- and high-skilled individuals or for families with or without small children. This result indicates that observed individual characteristics can indeed explain some of the variation in elasticities across individuals. More surprisingly, we find that allowing for observable heterogeneity through socio-demographic characteristics does not affect the mean and standard deviation of the coefficients on working hours much. Hence, a lot of the heterogeneity in preferences for leisure remain even conditional on observable characteristics. In sum, our results show that there is sizable heterogeneity in preferences for leisure which varies both between demographic groups as well as within those groups.

To examine the variation across observed characteristics, we calculate elasticities for every individual and report quartiles of the elasticity distribution for different subgroup of our sample in Table 4.2.

The median Hicksian elasticities are generally close to the sample median of 0.40. The largest difference at the median can be found between East and West Germany. Along the entire distribution, elasticities in East Germany are about

[13] Table 4.A2 in the appendix provides results for both the baseline model and a model with observed heterogeneity.

Table 4.2: Intensive Margin Elasticities

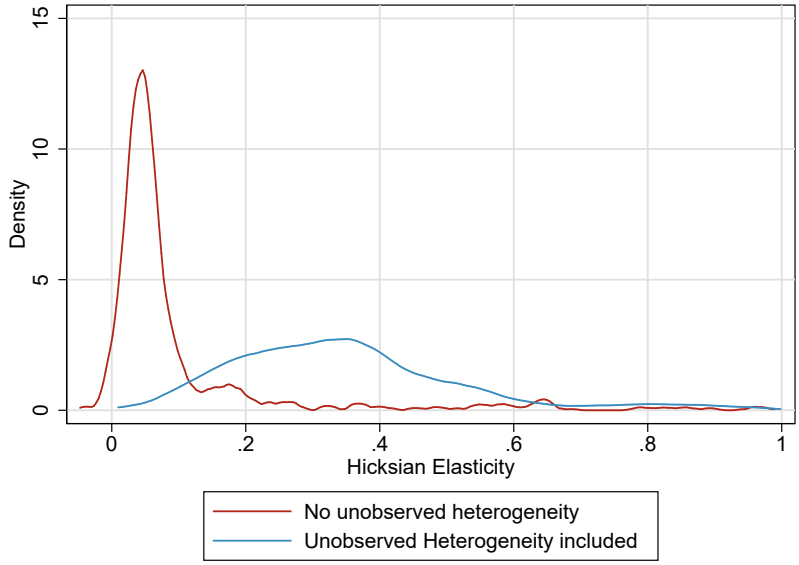
	Gender		Region		Age		
Hickisan Elasticity	Men	Women	East	West	Sub 30	30-50	over 50
1. Quartile	0.27	0.28	0.22	0.31	0.20	0.31	0.28
Median	0.40	0.41	0.30	0.43	0.34	0.45	0.39
3. Quartile	0.50	0.61	0.39	0.57	0.54	0.59	0.50
Marshallian Elasticity							
1. Quartile	0.24	0.23	0.2	0.25	0.16	0.28	0.23
Median	0.34	0.34	0.26	0.36	0.3	0.39	0.32
3. Quartile	0.41	0.53	0.32	0.47	0.41	0.51	0.41
Income Elasticity							
1. Quartile	-0.00044	-0.00051	-0.00038	-0.00048	-0.00045	-0.00047	-0.00047
Median	-0.00026	-0.00032	-0.00025	-0.00029	-0.00025	-0.0003	-0.00027
3. Quartile	-0.00011	-0.00015	-0.00013	-0.00013	-0.0001	-0.00014	-0.00014

NOTE.- These elasticities are predicted using individual coefficients for the mixed logit model that includes observable heterogeneity. Quantiles are calculated from the distribution of the individual elasticity estimates.

0.10 (one standard deviation of the overall elasticity distribution) lower than in West-Germany. That implies that individuals in East Germany respond less to wage and tax changes than individuals in West Germany.

Moreover, we find somewhat less pronounced differences along age groups and gender. Individuals between the ages of 30 to 50 generally seem to have larger elasticities than both younger and older age groups in our sample. Contrary to most of the previous literature, we do not find large gender differences in labor supply elasticities. Women's labor supply elasticities are somewhat higher at the upper tail of the distribution; yet, both the median and the lower quartile estimates are very close to the values for men. These results suggest that tastes for leisure do not seem to be very different for men and women. The observed differences in actual responsiveness to wage or tax changes might therefore be the consequence of different frictions either on the supply or demand side.

Figure 4.2: Intensive Margin Elasticities



Kolmogorov-Smirnov test

Smaller group	D	P-value	Corrected
Actual	0.5360	0.000	
Cumulative:	-0.5694	0.000	
Combined K-S:	0.5694	0.000	0.000

Next, we focus on the relative importance of observed and unobserved heterogeneity in preferences. To do so, we predict two sets of individual elasticities using similar quadratic specifications, once using a mixed-logit model that allows for both observed and unobserved individual heterogeneity and once a simple conditional logit model that only allows for observed heterogeneity.

Figure 4.2 shows the densities of the elasticities estimated from the model that allows both for observed and unobserved individual heterogeneity (blue) and the elasticities based on a model that only allows for observed heterogeneity (red).

The elasticity distributions are drastically different for the two cases. Allowing only for observed characteristics yields a elasticity distribution that is shifted to the left and has a much smaller spread than the distribution that also allows for unobserved heterogeneity. Unsurprisingly, a Kolmogorov-Smirnov test shows that the two elasticity distributions are indeed not identical.

Hence, observable characteristics alone which are typically used in many empirical studies of labor supply elasticities do not capture the full heterogeneity in preferences for leisure.

4.5.3 Comparison to estimates of the existing literature

A comparison of our estimates to those of the existing literature (shown in Table 4.A5) exhibits two interesting patterns: first, our estimates are typically larger than those obtained from tax reforms for identification (see e.g. Bargain et al., 2014). They are also somewhat larger than estimates from structural models for Germany (see e.g. Haan and Wrohlich, 2007; Haan and Steiner, 2005).

The second striking feature is that our elasticity estimates are the same for men and women - in contrast to data from actual choices where elasticities for women are consistently larger than for men. These patterns indicate that frictions which reduce the responsiveness of workers to tax or wage changes might be an important determinant of elasticities derived from actual choices. The comparison also shows that the downward bias from frictions might be much more pronounced for men than for women.

4.6 Robustness

This section presents a range of robustness checks to see whether our estimated preferences for leisure are sensitive to alternative specifications. Most concerns about the validity of our analysis arise from the specification of the random utility model we use and from the choice of the mixing distribution in our mixed logit approach. In addition, the ordering of questions could have an influence on responses if earlier questions act as a prior or respondents learn over the course of the questionnaire. [14]

We address these concerns as follows. First, we check whether the specification of our utility function or mixing distribution has a significant influence on our results. We estimate alternative models using multiple random utility specifications with

[14] Respondent fatigue could also lead to ordering effects.

a discrete mixture as mixing distribution (latent class logit). Second, we analyze whether the order of question blocks or questions within a block influence our results.

4.6.1 Changes in specification

To relax the parametric assumption of a normal distribution of the random coefficient in our model, we allow for a discrete distribution of the random coefficients resulting in a latent class model. In this model every respondent is assumed to belong to one of C pre-specified classes. Then the distributional parameter θ of the mixing distribution $f(\beta|\theta)$ we estimate is the vector of shares of respondents that belong to a specific class. This non-parametric approach can approximate any discrete or continuous mixing distribution.

We estimate latent class models for multiple specifications of the random utility model analyzing different polynomial representations of the utility function up to a fifth-order polynomial. We choose the number of classes C by maximizing the Bayesian information criterion over a range from 1 to 20 classes for each specification. We then compare the elasticity distribution for the different models with our baseline mixed logit quadratic utility model.

Figure 4.3: Elasticity Distribution by choice model

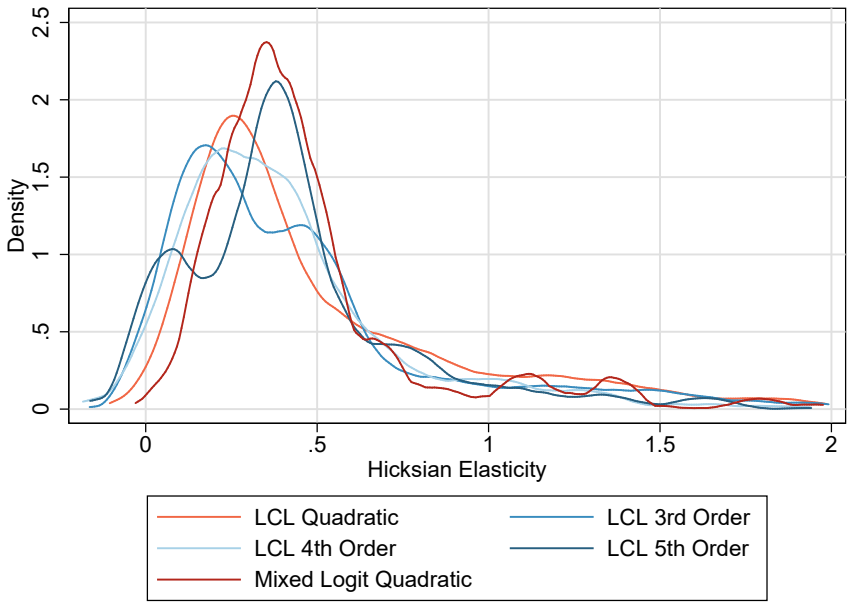


Figure 4.3 presents the densities of the elasticity distributions of the Hicksian intensive margin elasticity for all latent class logit and the baseline mixed logit model. While the more flexible specifications of latent class models with higher order terms, predict less symmetric elasticity distributions with partially different frequencies at specific elasticities , the differences are generally small.
More than 75% of all deviations in the Hicksian elasticities of the alternative models from the baseline quadratic mixed logit specification are smaller than a quarter standard deviation of the baseline elasticity distribution (see Table 4.A3 for a comparison of the model deviation from the baseline).

4.6.2 Ordering Effects

We now also turn to a brief discussion of ordering effects. To examine whether such affects our estimates, we add interactions of the hours term with indicators for the order of question blocks to our baseline model. Then, we estimate elasticities for a model with block ordering controls and compare them to our baseline.

While the ordering coefficients are indeed statistically significant, the effects on response patterns seem negligible.[15] The resulting elasticity distribution for a model with ordering controls is very close to the baseline distribution. This suggests that there is no significant difference in the resulting elasticities. Similarly, we also analyze, how the ordering of questions within question blocks affects our results and also find no significant effects for the resulting elasticity distribution.

4.7 Conclusion

This chapter proposes a new approach to estimate labor supply elasticities. We use information from hypothetical decision scenarios for a representative sample of the German population to estimate a pure preference for leisure that is not constrained by external restrictions.

[15] See Table 4.A4 in the appendix.

We calculate separate labor supply elasticities for each individual in our sample. We find an average Hicksian labor supply elasticities of 0.40, which is larger than most estimates obtained from tax reforms for identification. At the same time, the elasticities we estimate are still in the same order of magnitude as estimates from previous literature. While this suggests that observed elasticities are larger than behavioural elasticities due to frictions, long-term behavioral elasticities are still relatively close to previous measures. Thus the disincentive effects of progressive taxation are in similar order of magnitude even when accounting for potential frictions.

However, frictions play an important role in the differences in labor supply elasticites between subgroups of the population. The most striking difference between our friction-less estimates of the the preference for leisure and previous literature is that our estimates do not vary much between men and women. Consequently, the consensus that it is optimal to impose higher tax rates on men than women since their Labor Supply is less elastic does not hold up to the evidence from hypothetical decision scenarios.

4.A Appendix

Figure 4.A1: Screenshot of a hypothetical choice

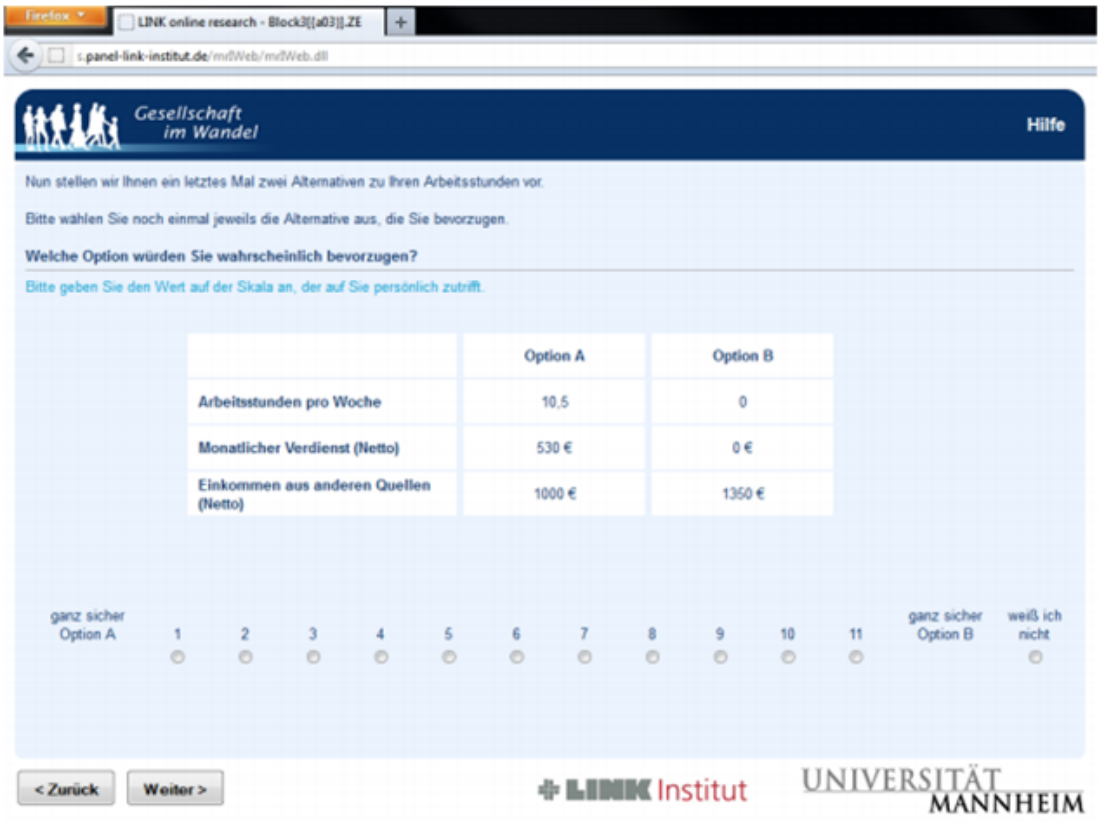


Table 4.A1: Summary Statistics

	Mean	Std. Dev.	Median	Min	Max	Observations
			Summary Statistics: Wave 4			
Actual Wage	1819	1173	1750	0	9000	1118
Actual Other Income	1077	1408	500	0	8750	1118
Actual Hours	34.52	14.19	40	0	99.9	1118
Overtime Hours	5.4	6.7	3	0	50	570
			Summary Statistics: Wave 9			
	Mean	Std. Dev.	Median	Min	Max	Observations
Actual Wage	1852	1225	1750	75	9000	1045
Actual Other Income	1572	1097	1500	0	7500	1045
Actual Hours	37.5	12.6	40	2	90	1045

	Sample Compostion: Gender, Region and Employment					
	Men	Women	East	West	Employed	Non-Emloyed
Frequency	541	559	236	864	733	385
Share	49%	51%	21%	79%	67%	35%

	Sample Composition: Age and Education					
	Age: sub 30	Age: 30-50	Age: over 50	Low Educ.	Medium Educ.	High Educ.
Frequency	181	406	513	210	362	505
Share	16%	37%	47%	19%	33%	46%

	Sample Composition: Family Information				
	No Children	1 Child	2 Children	3 Children	4+ Children
Frequency	372	236	331	110	49
Share	34%	21%	30%	10%	4%

NOTE.- Information about the sample composition was collected in a seperate core wave for both our first (Wave 4) and second (Wave 9) questionnaire. The education categories are based on Germany's three-tiered education system. Low education refers to Hauptschule and below, middle education is for Realschule, and high education indicates a gymnasium or fachgymnsium degree.

Table 4.A2: Intensive Margin Elasticities - Observed Heterogeneity

	(1)	(2)
Hours	0.06129***	0.06214***
	(0.0086)	(0.0093)
sd Hours	0.10819***	0.10426***
	(0.00434)	(0.00447)
Consumption	0.01033***	0.0101***
	(0.00038)	(0.00041)
Consumption^2	-2.09 $\times 10^{-6}$***	-2.05 $\times 10^{-6}$***
	(0.00000013)	(0.00000014)
Hours^2	-0.00282***	-0.0022***
	(0.00012)	(0.00017)
Consumption × Hours	1.92 $\times 10^{-6}$***	2.17 $\times 10^{-6}$***
	(0.0000054)	(0.0000057)
Hours × Age 30-50		-0.00052***
		(0.00014)
Hours × Age 50+		-0.00034**
		(0.00014)
Hours × Children		-0.00031*
		(0.00017)
Hours × Female		-0.00049***
		(0.000097)
Hours × College		-0.00037***
		(0.00011)
Observations	24138	20916
AIC	11082.2	9475.5
BIC	11130.7	9562.9
Pseudo R^2	0.094	0.091

Note.- Standard Errors in Parentheses.$***$ $p < 0.01, **$ $p < 0.05, *$ $p < 0.1$. All interaction terms for the hours variables are interactions of the hours variable with demographic dummies.

Table 4.A3: Absolute Differences to baseline model elasticities

	Quadratic	3rd order	4th order	5th order
1. Quartile	7.81%	6.73%	6.47%	6.87%
Median	15.41%	14.75%	14.50%	15.86%
3. Quartile	28.96%	29.70%	26.31%	28.78%

Note.- This table provides quantiles of the distribution of the absolute differences between the mixed logit models Hicksian elasticities and the Hicksian elasticities of the alternative models. The absolute differences are normalized to the standard deviation of the mixed logit models elasticity distribution and differences are reported as percentages of that standard deviation.

Table 4.A4: Intensive Margin Elasticities - Ordering Effects

	(1)	(2)
Hours	0.06129***	-0.13158***
	(0.0086)	(0.0178)
sd Hours	0.10819***	0.2078***
	(0.00434)	(0.0111)
Consumption	0.01033***	0.02518***
	(0.00038)	(0.0011)
Consumption^2	-2.09 $\times 10^{-6}$***	-4.25 $\times 10^{-6}$***
	1.33 $\times 10^{-7}$***	(3.55 $\times 10^{-7}$)
Hours^2	-0.00282***	-0.00195***
	(0.00012)	(0.0002)
Consumption $\times$ Hours	1.92 $\times 10^{-6}$***	-5.83 $\times 10^{-5}$***
	5.40 $\times 10^{-6}$***	(1.34 $\times 10^{-5}$)
First Block $\times$ Hours		0.03797***
		(0.0087)
Second Block $\times$ Hours		0.05183***
		(0.0098)
Observations	24138	16342
AIC	11082.2	6195.4
BIC	11130.7	6257.1
Pseudo R^2	0.094	0.142

Note.- Standard Errors in Parentheses.$***$ $p < 0.01, **$ $p < 0.05, *$ $p < 0.1$.

Table 4.A5: Comparison to previous literature

Authors	Hicks	Marshall	Population and Variation
Fuest et al. (2008)	-	0.20	SOEP (2004) working age men, structural model
Fuest et al. (2008)	-	0.38	SOEP (2004) working age women, structural model
Steiner & Wrohlich (2004)	-	0.11 - 0.38	SOEP (2002) working age men structural model
Steiner & Wrohlich (2004)	-	0.16 - 0.55	SOEP (2002) working age women, structural model
Keane (2011)	0.31	0.06	Average of a survey of 20 articles
Blundell-Duncan-Meghir (1998)	0.20	0.17	U.K. Women, 1980s Tax reforms
Our estimates	0.40	0.34	Men
Our estimates	0.41	0.34	Women

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