ESSAYS ON SKILLS, WAGES, AND INEQUALITY IN GERMANY

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Für meine Eltern

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Preface

The causes and consequences of inequality in income, wealth and opportunities have long been studied by economists, philosophers and social scientists alike (e.g. Aristoteles 1911; Rawls 1971; Rousseau 1984). The eruption of the Global Financial Crisis in 2007/08 has sparked a renewed interest in inequality, both in the public and academic debate. In fact, inequality has grown in almost every industrialized country over the last three decades according to data from the OECD (2017b). Thomas Piketty's (2014) seminal book "Capital in the 21st Century", Raghuram Rajan's (2011) "Fault Lines" or the global "Occupy" movements and their slogan "We are the 99%" are just three prominent examples that illustrate this revived interest in and concern about rising inequality around the world.

Economists have long and controversially debated about the effect of inequality on economic growth without reaching a clear consensus (Ostry et al. 2014). On the one hand, inequality may encourage investment, innovation and entrepreneurship and provide incentives to invest in education (Kaldor 1957; Lazear and Rosen 1981; Barro 2000). On the other hand, high levels of inequality may impede the accumulation of human capital by exacerbating adverse effects of credit constraints (Galor and Moav 2004) or deter investments due to socio-political instability (Alesina and Perotti 1996). The consequences of inequality, however, might go beyond the economic sphere. In most cases, a concentration of income and wealth leads to a concentration of political power that prevents a fair representation of the population and may ultimately give those at the top an unacceptable degree of control over the lives of others (Robinson and Acemoglu 2012; Scalon 2014).

Irrespective of the normative position taken, providing answers to these important topics requires a sound, evidence-based analysis of the underlying factors. The aim of this dissertation is to contribute to the understanding of the causes and consequences of inequality by studying three aspects of the German labor market. Germany lends itself to an empirical analysis for at least two reasons. First, researchers can draw on high quality, large scale data derived from administrative labor market records – the *Sample of Integrated Labour Market Biographies* (SIAB) – that by now have become one of the most prominent data sets used by labor economists around the world. All of my three chapters base their empirical analysis primarily on these data. Second, Germany, as Europe's largest economy, has undergone two major economic transformations that have generated considerable international interest. Firstly, the reunification of East and West Germany in 1989/90 with its concurrent large influx of migrants from East Germany and ethnic Germans from Eastern Europe into West Germany and, secondly, the so-called "Hartz reforms", a series of labor market reforms implemented between 2003 to 2005. Even more than a decade later, the weals and woes of these reforms are still intensely debated. Critics blame the reforms for anything ranging from increased inequality and the expansion of precarious employment to the shrinking of the middle class. Proponents point out the positive effects of the reforms on the German labor market in the form of lower unemployment and an employment boom that made them a role model for labor market reforms in countries such as France, Greece or Spain.

My dissertation is composed of three chapters. The first two chapters relate directly to these topics. In my first chapter, I provide an empirical analysis of recent trends in income inequality, the wage structure, and labor force participation in Germany. I find that – contrary to common thinking – inequality among full-time employees has been decreasing since 2010. The second chapter starts out from a finding of the first chapter which documents an increase in inequality at the lower end of the distribution. A decomposition of this increase reveals that, in particular, the change in the return to education has contributed to this increase.

The second chapter builds and empirically tests a model that links the supply of differently educated workers to their returns to education and thus helps to explain how inequality, in particular, at the lower end of the wage distribution has changed due to changes in the educational attainment and the migration influx after the German reunification. It shows that contrary to what was previously hypothesized, the widening gap between medium- and low-skilled workers over the 1990s was not primarily driven by the inflow of low-skilled workers from outside of West Germany, but rather by a polarization in the long-run educational attainment of the native West German population.

My third chapter has a more indirect, but nevertheless interesting connection to inequality. It starts with the observation that focusing on inequality in wages might overstate inequality in utility terms due to compensating differentials. A compensating differential is the extra pay required to attract a worker to do a job that is more unpleasant in a certain respect compared to an otherwise similar job. In fact, Sorkin (2018) finds indirect evidence that implies that about 15% of the inequality in US wages can be explained by compensating differentials. The setting I study in the third chapter aims at uncovering *direct* evidence of such compensating differentials, which, until now, has been proven difficult to establish empirically. The setting I study is related to the compensating differential that is expected to accrue to waiters in bars and restaurants that allow their customers to smoke indoors which is associated with considerable health risks for these employees. Using the introduction of smoking bans in the German hospitality sector as a natural experiment, I estimate this compensating differential to amount to some 2.4% of waiters' wages. Taken literally, this finding implies that although inequality among all workers has increased ceteris paribus as a result of hospitality workers' lower wages, inequality in utility terms has remained constant as these workers now enjoy healthier working environments.

The following overview provides a more detailed summary of the three chapters of my dissertation. Each chapter is self-contained and can be read independently from the others.

Chapter 1 provides an empirical analysis of recent trends in inequality and the wage structure in Germany. I find that wage inequality among full-time workers in Germany increased continuously up to 2010. Since then, however, wage inequality has been decreasing again and is now at a level similar to the early 2000s. This evolution holds true in both East and West Germany and within men and women. Furthermore, I find that the decrease since 2010 was driven primarily by a compression of wages at the lower tail of the distribution. A detailed decomposition exercise based on recentered influence functions show that some part of the increase in inequality is mechanically related to the aging and educational upgrading of the workforce. The majority of the change, however, is explained by changes in the return to certain labor market characteristics, in particular the return to working in a specific sector and – at the lower end – education and – at the upper end –

experience (proxied by age). Comparing the changes in employment and wages over 1993 to 2014, I subsequently find strong evidence in favor of task-biased technological progress favoring non-routine occupations. This leads to a pronounced polarization of the labor market as these occupations are mainly concentrated at the lower and upper tail but not at the middle of the wage distribution. Turning to the evolution of the labor force participation since the early 1990s, I find a dramatic increase in the share of the working age population in part-time employment. In contrast to widespread conjecture, this increase in part-time employment seems not to have evolved at the expense of full-time employment but was rather fed by a corresponding pronounced decrease in the share of the inactive or unemployed population. Finally, based on microcensus data, I find – in accordance with the strong increase in the employment rate after 2005 – that net income inequality also decreased starting in 2005 and has since then remained at lower level until 2011.

In CHAPTER 2, which is joint work with Albrecht Glitz, we study the development and underlying drivers of skill premiums in Germany between 1980 and 2008. We show that the significant increase in the medium to low skill wage premiums since the late 1980s was almost exclusively concentrated among the group of workers aged 30 or below. Using a nested CES production function framework which allows for imperfect substitutability between young and old workers, we investigate whether changes in relative labor supplies could explain these patterns. Our model predicts the observed differential evolution of skill premiums very well. The estimates imply an elasticity of substitution between young and old workers of about 8, between medium- and low-skilled workers of 4 and between high-skilled and medium/low-skilled workers of 1.6. Using a cohort level analysis based on Microcensus data, we find that long-term demographic changes in the educational attainment of the native (West-)German population – in particular of the post baby boomer cohorts born after 1965 – are responsible for the surprising decline in the relative supply of medium-skilled workers which caused wage inequality at the lower part of the distribution to increase in recent decades. We further show that the role of (low-skilled) migration is limited in explaining the long-term changes in relative labor supplies.

CHAPTER 3 starts from the observation that although compensating wage differentials are a classic concept in economics, their empirical confirmation has proven surprisingly difficult. To make progress on this front, I use the staggered introduction of smoking bans in the hospitality industry in the German states in 2007/08 as a natural experiment that led to a substantial and lasting improvement of working conditions of workers in bars and restaurants. Using administrative labor market data and employing either a simple difference-in-differences or a triple difference-in-differences approach, I find a 2.4% wage decline associated with the most comprehensive smoking ban. This effect is robust to a battery of robustness and placebo checks and does not seem to be driven by a decline in hospitality revenues or hours worked. I furthermore present evidence that smoking bans changed the selection of workers partly on observable but mostly on unobservable characteristics and that smoking bans increased employment and turnover. All in all, I find that the observed patterns are consistent with a simple model of compensating wage differentials.

Chapter 1

The State of the German Labor Market*

1.1 Introduction

The perception of Germany's economic performance has changed markedly over the past decade. From the "Sick man of the Euro" (The Economist 1999) Germany has become an "Economic Superstar" (Dustmann et al. 2014). Figure 1.1 illustrates this success story by comparing four macroeconomic indicators over 1991 to 2016 of Germany to those of the US, the world's largest economy, and France, Europe's second largest economy. Up until 2005, the unemployment rate in Germany had increased to 11.1%[1], income per capita had fallen to 75% of the US's, and the employment to population ratio was at 52%, a striking 10 percentage points lower compared to the US. Since 2005, however, Germany started an unprecedented economic recovery. Until 2016, unemployment more than halved reaching 4.3%, income per capita increased to 83% relative to the US and the employment share of the adult population increased to 58% being nearly en par with the corresponding share in the US. This development seems all the more impressive when compared to the development of the same indicators in France which saw a deterioration of its economic position since 2005.

One explanation for Germany's new economic "miracle" (The Economist 2013) proposed by Dustmann et al. (2014) was the restrained evolution in unit labor cost illustrated in panel d of Figure 1.1. While labor cost have increased continuously in France and the US since the 1990s and are now some 30% higher than in 1998, unit labor cost in Germany decreased during the 2000s and only started to grow at a similar pace as in the US and France since the late 2002 such that unit labor

* I am grateful to Raphael Guber for providing the sample counts for the GSOEP. All errors remain mine.

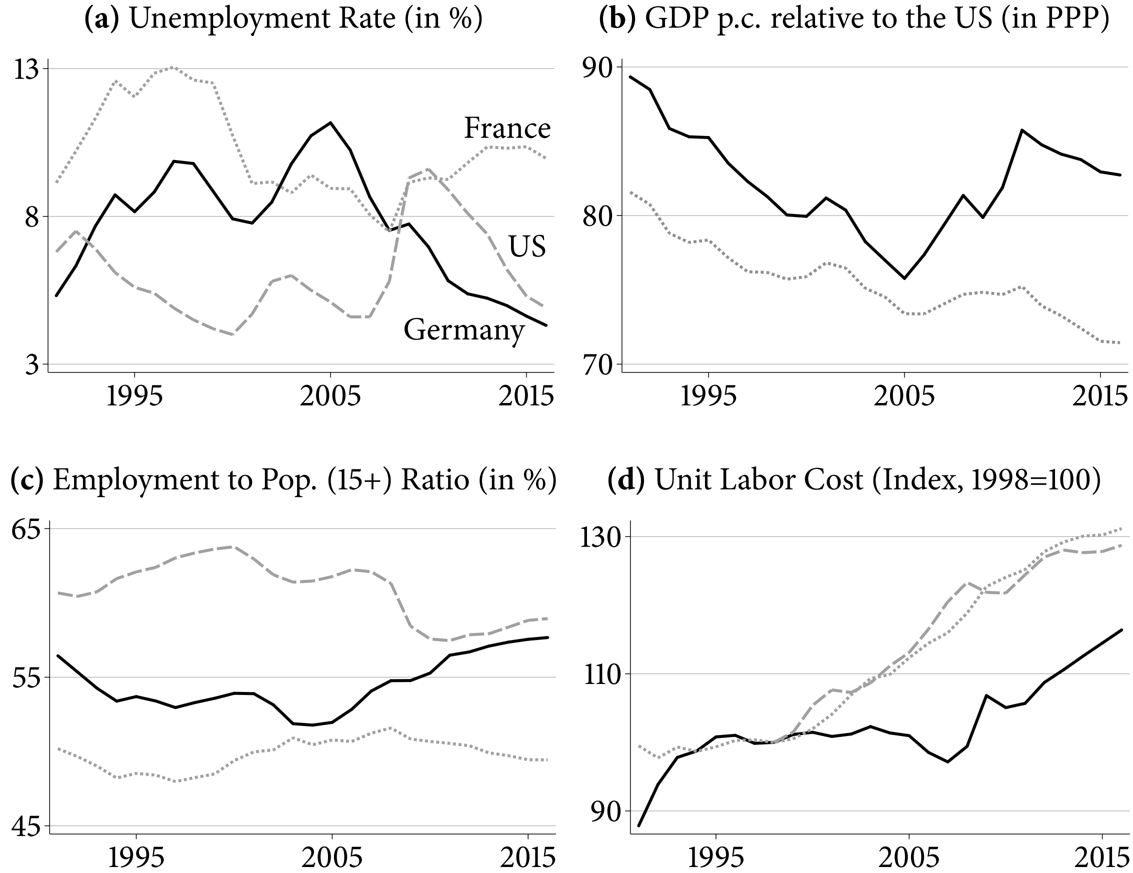
[1] According to internationally comparable data from the International Labor Organization (ILO).

cost in 2016 are only about 16% higher than in 1998. Although falling unit labor cost significantly improved Germany's international competitiveness, many critics have argued that Germany's stellar macroeconomic development was not primarily driven by increased productivity but came at the price of poor wage growth, precarious employment, and increased inequality (see, for instance Bäcker and Schmitz 2016; Fratzscher 2016; Grabka and Goebel 2017).

The goal of this paper is to provide a set of evidence-based insights that I hope can inform an important debate. For this purpose, I review and update a series of facts on inequality and labor market participation. Furthermore, I offer some potential explanations for the observed patterns using the most recent versions of two large-sample, representative micro data sets, namely administrative labor market records provided by the Federal Employment Agency and data from the Microcensus, an official, compulsory population survey. A sober focus on the facts seems necessary when considering the results of Niehues (2014) who shows that there is almost no connection between how people think that incomes in their country are distributed and the actual distribution. In Germany, the perceived income distribution is far more unequal than it actually is. A corresponding puzzling finding is that – as of 2017 – 80% of German think that their personal and the general economic conditions are good or very good, but at the same time 60% think that the distribution of incomes in Germany is unfair (Kramer and Bürckholdt 2017). In light of these results, my two most important findings may offer a more optimistic perspective. First, I find that inequality in gross wages among full-time workers has decreased since 2010 for men and women and in East and West Germany alike and second that the strong increase in part-time employment was largely fed by a decrease (at the aggregate level) in the inactive and unemployed population. Undoubtedly, empirical analyses cannot, by themselves, give an answer to normative questions such as what constitutes a "fair" level of inequality or redistribution. However, a description of the facts that is based on the best data available is crucial when discussing potential policy prescriptions.

Different data sets may lead to different conclusions about the same phenomena. When it comes to measuring inequality, relative poverty rates, the size of the middle class or related issues, the majority of studies are based on the German Socio-Economic Panel (GSOEP). It is the most comprehensive panel survey in Germany

Figure 1.1: Macro Indicators of Germany in International Comparison



Notes: The unemployment rate refers to the share of the labor force that is without work but available for and seeking employment (modeled ILO estimate). The GDP per capita relative to the US is constructed as each country's GDP per capita in purchasing power parity 2011 international dollars divided by the corresponding value for the US in each year. The employment to population ratio is the proportion of a country's population that is employed (modeled ILO estimate). Employment is defined as persons of working age who, during a short reference period, were engaged in any activity to produce goods or provide services for pay or profit, whether at work during the reference period (i.e. who worked in a job for at least one hour) or not at work due to temporary absence from a job, or to working-time arrangements. Ages 15 and older are generally considered the working-age population. Annual unit labor cost are expressed as the ratio of total labor compensation per hour worked to output per hour worked. Sources: World Development Indicators (unemployment, GDP per capita, and employment to population) and OECD (unit labor cost).

in terms of the set of covariates and includes, for instance, a detailed breakdown of (self reported) gross and net individual and household earnings, labor market status, hours worked, or the composition of household members.[2] Despite its many merits and widespread use in the scientific and policy community, in this paper I rely on two other data sets – the Sample of Integrated Labor Market Biographies 1975-2014 (SIAB7514) and the Microcensus – for a number of reasons. First, compared to the GSOEP both the SIAB7415 and the Microcensus feature large sample sizes. For instance, in 2011, in the SIAB there are about 400,000 full-time working individuals aged 21-62 (in East and West Germany, women and men), about 120,000 in the Microcensus, and about 8,500 in the GSOEP. Analyses of subgroups such as men and women or employees in East and West Germany or detailed wage structure decompositions require that the respective cells are filled with a sufficient number of observations. Second, inclusion or participation is compulsory by law in the SIAB and Microcensus assuring their representativeness. In contrast, participation in the GSOEP is voluntary and sample attrition is considerable (Kroh et al. 2015) which may put the GSOEP's overall representativeness into question. Finally, wages are expected to be very precisely measured in the SIAB where wages are automatically reported by employers and used to determine legal claims against the social security system. Misreporting can be severely punished. In contrast, income in the GSOEP (as well as in the Microcensus) are self-reported and thus more likely prone to (systematic) measurement error. In a detailed comparison of the GSOEP and the SIAB, Dustmann et al. (2009, p. 854) conclude that "measures of inequality are very noisily estimated in the GSOEP".

Throughout the main text, I will perform my main analyses jointly for men and women as well as workers in East and West Germany starting in 1993 when records in the SIAB from East Germany are assumed to be complete (vom Berge et al. 2017, p. 24). Many previous studies perform their analyses separately for men

[2] Recent studies using GSOEP data to calculate wage inequality and other related measures of the wage structure in Germany include the latest report of the scientific advisory board of the Federal Ministry of Finance on income inequality and social mobility (Bundesministerium der Finanzen 2017), the Federal Government's official report on poverty and wealth (Bundesregierung 2017), the Federal Ministry for Family Affairs, Senior Citizens, Women and Youth's family report (Bundesminsterium für Familie, Senioren 2017), data for Germany in the OECD Income and Distribution Database (OECD 2012), and many other frequently cited studies and books such as Fratzscher (2016) and Grabka and Goebel (2017)

and women or often exclude females – who make up nearly half of all full-time workers - altogether. I prefer to group men and women as well as East and West German workers together not only to provide a full picture of the labor market but also because – as I will show – many of the most important patterns in the evolution of the wage structure are surprisingly similar across men and women and in both East and West Germany.

My analysis focuses on three main indicators: inequality in gross wages, labor force participation among the working age population and inequality in net incomes. Starting with inequality of gross wages among full-time workers based on the SIAB7514, I find that after it had continuously increased over the 1990s and the 2000s, since 2010 inequality has *decreased* considerably reaching a level comparable to the beginning of the 2000s. Thus – at least in terms of market wages earned by full-time workers – the commonly expressed concern about an ever widening gap between the rich and the poor is not supported by the data. A closer analysis reveals that the decrease in the dispersion of wages is related, in particular, to a compression of wages at the lower end of the distribution. Between 2009 and 2014, wages below the 30th percentile have seen considerable gains and strongly outperformed wage growth at higher percentiles. A decomposition of the longer run change in wage inequality over 1993 to 2014 using a recentered influence function approach following Firpo et al. (2009) shows that some part of the increase in inequality is mechanically related to the fact that the workforce becomes older and better educated. Another considerable part of the increase is explained by changes in the return to working in a certain sector and an increase in the return to education and experience (proxied by age). The analysis also yields some evidence for a decrease in the gender pay gap. I also find clear evidence – once also including part-time workers – in favor of a polarization in employment and wages over the period 1993 to 2014 along the occupational skill distribution. The observed patterns are strongly in line with task-biased technological change that – due to the decreasing cost in the automation of routine tasks – favors jobs with a higher content of non-routine tasks such as manual, lower-skilled service jobs at the lower end and analytical and interactive jobs at the upper end of the wage distribution.

In a second part of the paper based on data derived from the microcensus, I review changes in the labor market participation. Compared to 1993, the share of the working age population aged 15 to 64 years in part-time employment – mostly a female phenomenon – increased more than twofold. However, contrary to common stereotypes, this growth seems mostly fed by a decrease in the inactive and unemployed population and not by a corresponding decrease in regular full-time employment. While in 1993 about 35% of the population either did not participate in the labor market or was unemployed, this share dropped to 27% by 2014 – a decrease of about 5.1 million individuals. The years 2004/05 seem to constitute a watershed in this context. Since then, the share of full-time employed in the total working age population increased by 4 percentage points while the share of the inactive and unemployed dropped by 6 and 2.5 percentage points, respectively. Finally, I find that the importance of so called "mini jobs" – a form of part-time employment with reduced tax- and social security contributions – reduced in total part-time employment has decreased over the last decade while the share of long term unemployed in all unemployed individuals remained roughly constant since 2000. In a last part, based on Microcensus data, I compute inequality among the entire working age population and find that the strong increases in labor force participation led to a decrease in overall inequality in post-transfer net incomes since 2005. Since then, net income inequality has remained roughly constant.

This paper is not the first to study wage and income inequality in Germany. As mentioned before, most existing studies are based on the GSOEP (for instance Goebel et al. 2015; Fratzscher 2016; Bundesministerium der Finanzen 2017; Bundesminsterium für Familie, Senioren 2017; Bundesregierung 2017; Grabka and Goebel 2017). Compared to these studies, I use two different data sets that – as I argue above – may be better suited to study questions related to wage and income inequality in terms of sample size and representativeness. The paper by Dustmann et al. (2009) was one of the first and most prominent papers to use IAB data to analyze the German wage structure.[3] As their data extends up to 2004, my paper can also be understood as an update and extension of their work. Differently from them, my analysis extends up to 2014 and I do not restrict attention to West German men only, but include women as well as workers in East Germany. Other papers have used IAB data reaching up until 2010 to study, for instance, plant-level heterogeneity and rising assortativeness between workers and firms (Card et al.

[3] Fitzenberger (1999) is an important, earlier study using IAB data.

2013), the role of exporting establishments (Baumgarten 2013), collective bargaining, technology, and worker characteristics (Antonczyk et al. 2010b; Battisti et al. 2016; Baumgarten et al. 2016), and domestic outsourcing (Goldschmidt and Schmieder 2017) in explaining wage inequality as well as to study the changing situation of labor market entrants (Reinhold and Thomsen 2017). One difference compared to these studies is that I use the most recent version of the SIAB reaching up to 2014. Crucially, the last four years since 2011 are particularly interesting as they yield a turnaround in the tale of inequality in Germany that up to 2010 was a story about ever increase inequality. To the best of my knowledge, the only other study using the most recent IAB data up to 2014 is Möller (2016), who also finds a trend break in the evolution of wage inequality around 2010. Finally, other studies have also analyzed wage and/ or employment polarization (Dustmann et al. 2009; Antonczyk et al. 2010a; Senftleben and Wielandt 2013) but did not include part-time and/or female workers. As I will show, including these two groups yields much more pronounced polarization patterns than when these groups remain excluded.

The remainder of the paper is organized as follows. Section 1.2 studies inequality in gross wages among full-time workers looking at overall inequality, as well as differences at the lower and upper tail of the distribution. Section 1.3 then asks how much of the observed change in inequality is due to composition versus wage structure effects. Section 1.4 studies the polarization of wages and employment providing evidence that is in line with technological progress that favors non-routine tasks. Section 1.5 extents the view to the entire population studying the overall labor force participation, while Section 1.6 looks at inequality in net incomes. Section 1.7 concludes.

1.2 Inequality in Market Wages of Full-Time Workers

In this section, I present an overview of the evolution of inequality in gross wages based on various measures. The data is derived from administrative labor market records contained in the *Sample of Integrated Labour Market Biographies* (SIAB) provided by the by the *Institute for Employment Research* (IAB) and covers all individuals subject to social security between 1975 and 2014. Thus, it does not include civil servants, self employed, while mini job workers were only included starting from April 1999 onwards. For my analysis, I include male and female workers aged 21-62 living in East and West Germany.[4] Wages are censored above the upper social security income threshold which was at 71,400 euros in 2014 affecting about 7-9% of full-time workers across the years since 1993. I impute these censored wages assuming that wages are log normally distributed. Dustmann et al. (2008) and Glitz and Wissmann (2017) show that the upper tail of the wage distribution is well approximated by this imputation approach. More details on the sample preparation and imputation of censored wages are given in section A.2 in the Appendix.

For most of the analyses, I will focus on full-time workers as the wage structure of these individuals best represents "market inequality", i.e. inequality that results from the market renumeration of (scarce) skills such as education or experience, the ability to perform certain tasks, or is related to secular forces such as technological progress or international trade. Any redistribution via the tax system, unemployment insurance, or joint household consumption has to start from these market incomes. Another reason to focus on full-time wages is that – since the hours worked are not directly observed in the SIAB – inequality measures are far less confounded by variation in hours worked than compared to when including spells of part-time and mini jobs workers as well.[5].

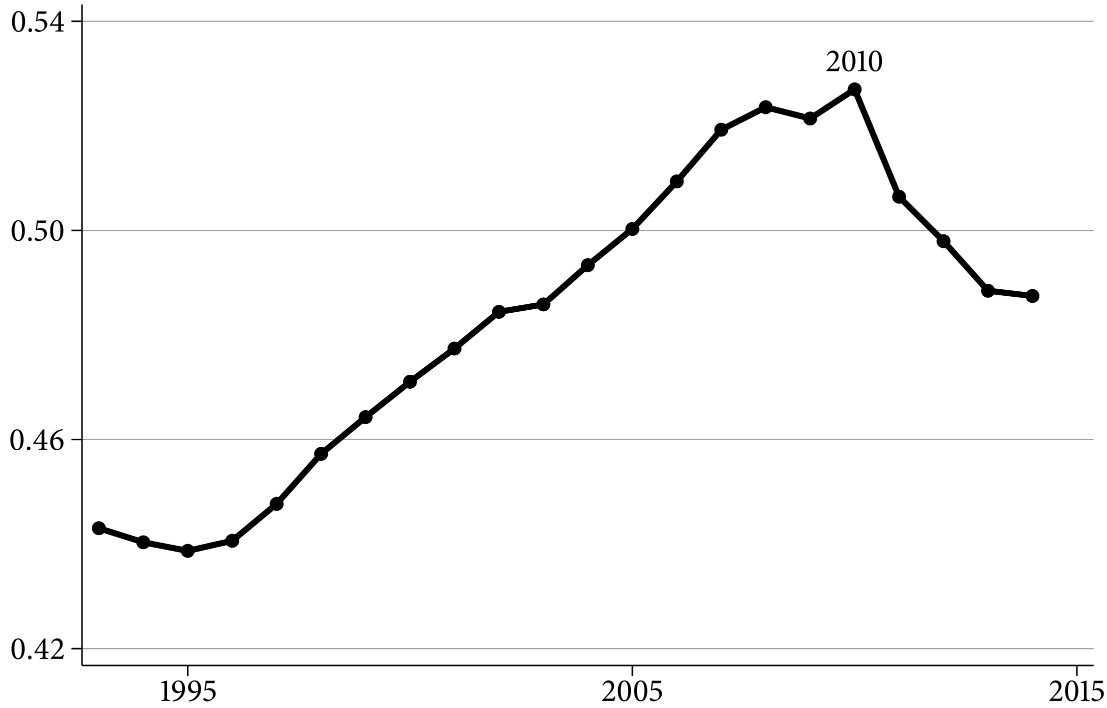
Figure 1.2 plots the evolution of the standard deviation of log wages of all full-time workers in Germany, including workers from both East and West Germany as well as men and women between 1993 and 2014.[6] Inequality has risen substantially since the beginning of the 1990s until 2010. Since 2011, however, inequality has been decreasing and is now at about the same level as in 2004. Figure 1.3 shows

[4] Workers in the SUF SIAB7514 are only observed until the age of 62.

[5] Wanger et al. (2016, 227, Figure 4) show the hours worked by full-time employees have remained basically constant over 1991-2014 and Dustmann et al. (2008, p. 9), based on GSOEP data, find that "using hourly as opposed to monthly wages does not have a large impact on measured inequality". Thus, inequality calculated using full-time wages as observed in the SIAB is likely to be a good proxy for inequality based on hourly wages.

[6] In my baseline calculations, I use the duration of a spell measured in days as weights. In Figure A.1, I present a number of alternative methods to compute inequality including the Gini coefficient, the standard deviation of log wages not weighted by spell duration and the standard deviation of log wages weighted by spell duration but based on imputed wages that – unlike in the baseline – assume a different variance for each education and age group. All of these alternative measures show a very similar evolution of inequality compared to my baseline specification.

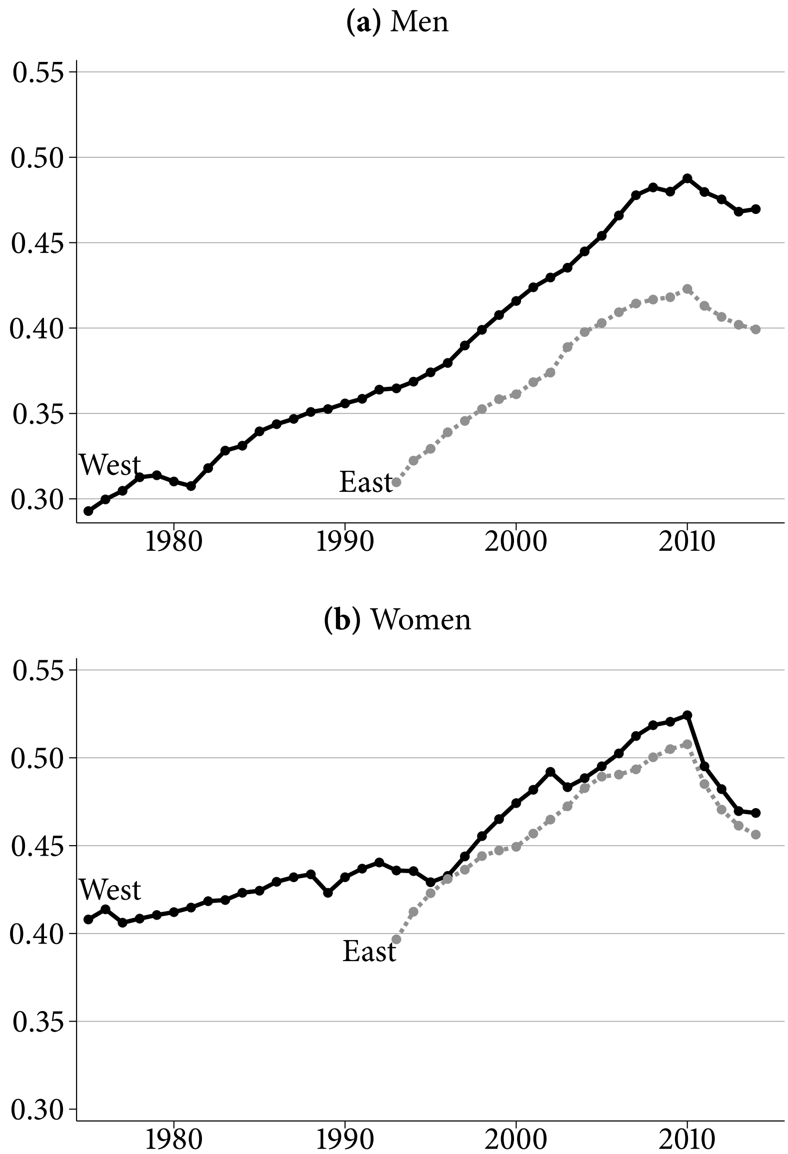
Figure 1.2: Evolution of the Standard Deviation of Log Earnings (Full-Time Workers, Germany)



Notes: This figure plots the evolution of the standard deviation of log real gross wages of all full-time workers in Germany (East and West, men and women) based on the SIAB7514. Censored wages are imputed using a fully saturated specification of age and education indicators separately for each year, East and West Germany, and men and women assuming log-normally distributed error terms with a constant variance across education and age groups. All calculations are weighted by spell duration. More details on sample restrictions and the imputation of censored wages are given in subsection A.2 in the Appendix.

that – maybe surprisingly – the main patterns of the evolution of inequality do not differ much for men and women in East or West Germany. For men, inequality is at lower levels in East Germany but the evolution of inequality among men in the east follows that of men in the west very closely (Figure 1.3a). For women, the evolution of inequality is practically the same in East and West Germany apart from some initial differences and a much smaller level difference than in the case of men. The decrease in inequality since 2011 is more pronounced for women but still apparent across all groups. Looking at the longer run (West Germany only), inequality has

Figure 1.3: Standard Deviation of Log Full-Time Wages (Separately for Men and Women in East and West Germany)



Notes: This figure plots the evolution of the standard deviation of log gross wages of full-time workers aged 21-62 years in East and West Germany separately for men (panel a) and women (panel b) based on SIAB7514. For more details see notes for Figure 1.2.

been steadily rising since 1975 with the increase being more pronounced for men than for women until the mid 1990s. The longer run also reveals that the decrease in inequality since 2011 is remarkable as it is the only sustained decrease after a monotonous increase inequality over the last three decades.

Overall inequality may rise when incomes at the lower end of the distribution grow slower than those in the middle and the top or when incomes at the top grow faster than the rest. To better understand which parts of the distribution were driving the change in overall inequality, Figure 1.4a plots the evolution of the 15th, 50th, and the 85th percentile of the wage distribution (based on real wages) of all full-time workers in Germany between 1993 and 2014.[7] The reason to focus on the 85th percentile is that it is uncensored throughout the sample period and is thus independent from the specific imputations method used (similar to Dustmann et al. 2009; Card et al. 2013). Wages at the 85th percentile have been steadily rising and, compared to their level in 1993, were about 19% higher in 2014. In contrast, real wages at the middle were barely higher in 2009 than in 1993 and have only started to grow in recent years resulting in an overall growth of 6% since 1993. Wages at the bottom grew even slower until 2009, plummeting to a level that was 7% lower than in 1993. Since then, however, they have been growing faster than wages in the middle leading to a compression of the wage distribution at the lower half in recent years. Still, compared to their level in 1993, real wages at the 15th percentile were only some 3% higher in 2014. Figure 1.4b depicts these developments in an alternative way by showing the 50th to 15th (85th to 50th) percentile ratio representing the lower (upper) tail of the wage distribution. The figure shows that inequality was growing at the same rate at the lower and upper tail of the distribution until about 2007. Since then the growth of the 85/50 gap has flattened while after 2009 the 50/15 gap has been shrinking again.

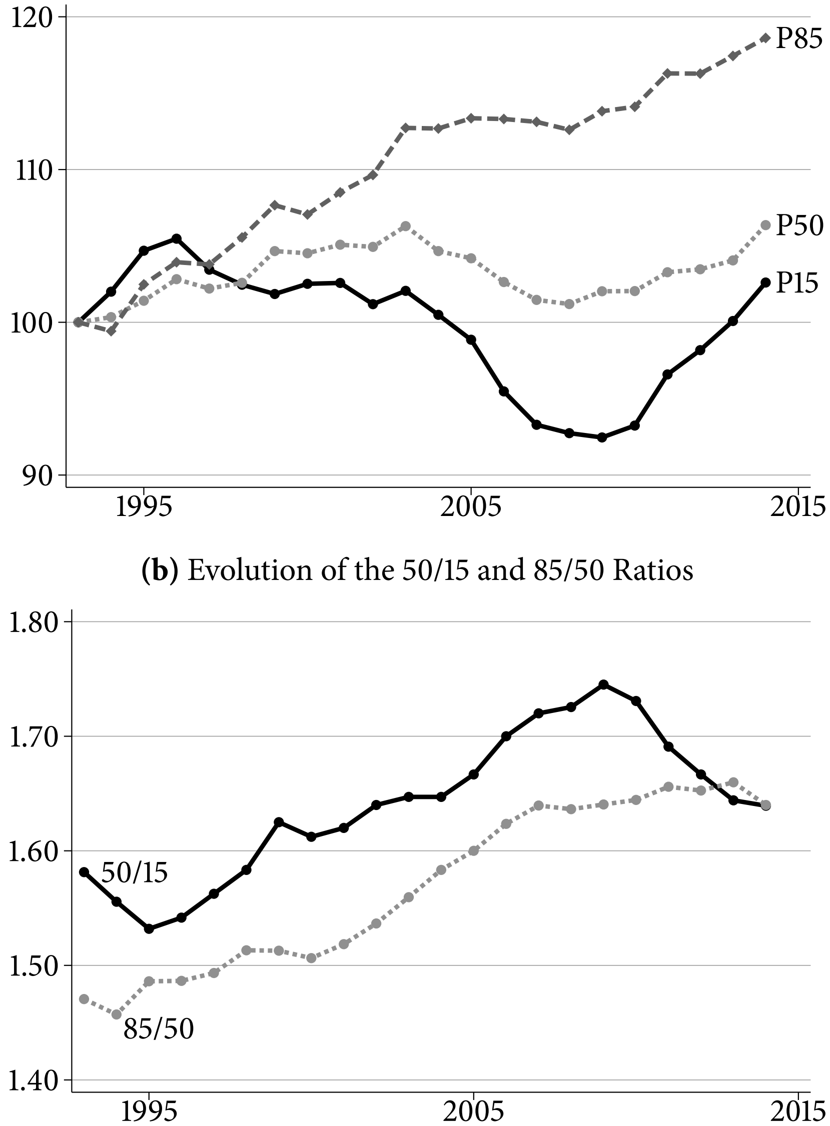
Figure 1.5 shows that these conclusions also hold when looking at the growth rates along the entire (uncensored) distribution between the periods 1993 to 2009 and 2009 to 2014. Wage growth between 1993 and 2009 was virtually linearly related to the initial percentile in the wage distribution with real wages at the lowest

[7] Figure A.2 plots the evolution of the 15th, 50th, and 85th separately for West Germany (since 1975) and East Germany (since 1993). Again, the general patterns for entire Germany are closely resembled in both East and West Germany.

Figure 1.4: Inequality at the Lower, Middle and Upper Part of the Distribution

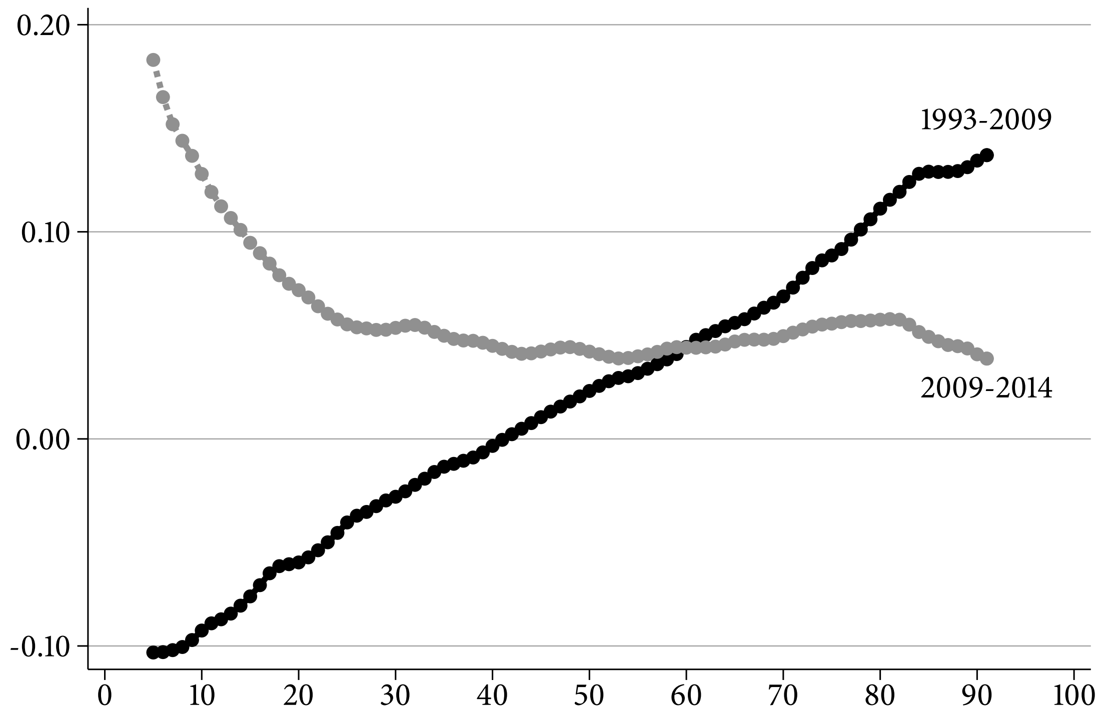
(a) Evolution of the 15th, 50th, and 85th Percentile (Index 1993=100)

(b) Evolution of the 50/15 and 85/50 Ratios



Notes: This figure plots the evolution of the 15th, 50th, and 85th percentiles of the distribution of real gross wages of all full-time workers in Germany aged 21-62 years based on the SIAB7514 in two different ways. Panel (a) plots the index growth of the three percentiles (1993=100) while panel (b) plots the evolution of the 50/15 and 85/50 percentile ratios. All calculations are weighted by spell duration.

Figure 1.5: Change in Log Real Wages Along the Distribution



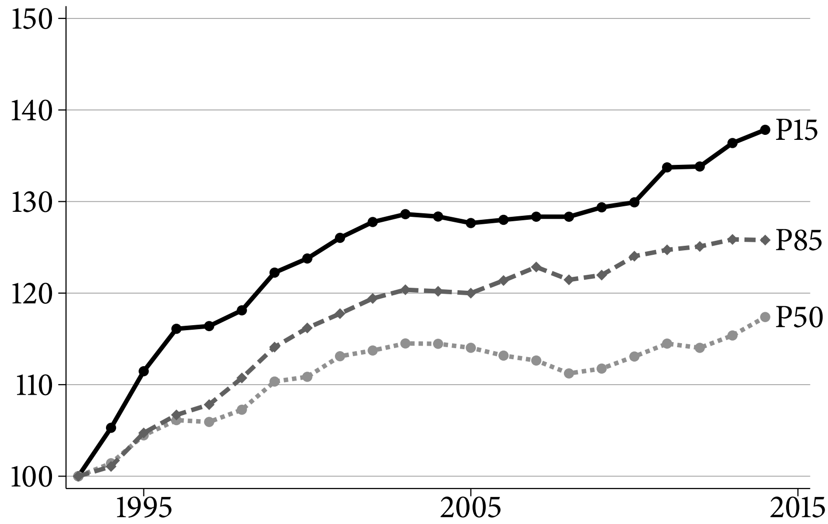
Notes: This figure plots the growth between 1993-2013 (solid black line) and 2014-2009 (gray line) for each uncensored percentile of the distribution of real gross wages of all full-time workers in Germany aged 21-62 years based on the SIAB7514. Plots are smoothed using a locally weighted regression with a bandwidth of 0.1. All calculations weighted by spell duration.

percentiles decreasing (in real terms) by up to 10% while wages at top percentiles grew by nearly 15%. Between 2009 and 2014, however, wages at the lowest percentiles grew fastest by up to 20%, while the growth rate fell exponentially until the 30th percentile from where it remained basically constant at 5%. Taken together, the decrease in overall inequality starting after 2010 shown in Figure 1.2 was driven partly by a slowdown of the divergence at the upper tail but mainly by a convergence of wages at the lower tail. This already point to some form of polarization of the wage structure which I will study in more detail in section 1.4.

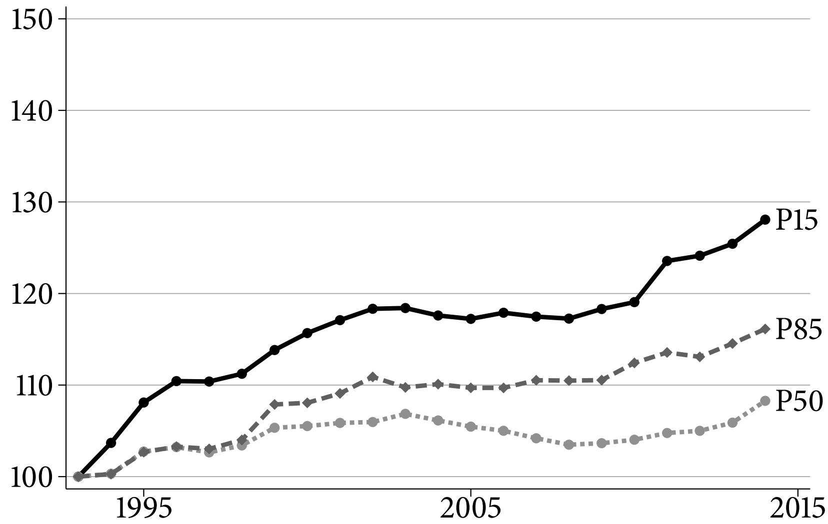
When studying the evolution of different percentiles of the wages distribution over time as in Figure 1.4a, it is important to bear in mind that these percentiles do not refer to a fixed group of individuals but are computed over a changing set of

Figure 1.6: Evolution of Percentiles for Fixed Groups of Full-Time Workers

(a) Ranked by Position in Population Distribution in 1993



(b) Ranked by Position in Peer Distribution in 1993



Notes: This figure plots the indexed evolution of real gross wages for full-time workers aged 30-35 years in 1993 based on the SIAB7514 (1993=100). In panel a) workers are grouped by their position in the distribution of all full-time workers aged 21-62 years in 1993. In panel b), workers are grouped by their position in the distribution of all full-time workers aged 30-35 years in 1993. A bandwidth of 1 percentage point around each given percentile is chosen to group workers. All calculations are weighted by spell duration.

workers. For instance, younger workers will typically start their careers at lower percentiles and will move up along the wage distribution when increasing their experience and skills. Furthermore, workers who have previously been unemployed or inactive might join the labor force earning below average wages which will also drive down wages at the lower end of the distribution. Finally, due to transitory one-time-payments such as bonuses or extra hours, some individuals might find themselves at top percentiles of the wage distribution in one year and return to a lower percentile in another. Thus, a decline in the 15th percentile by 7% between 1993 and 2008 does not mean that an actual group of workers saw their real wages decline by this amount. To illustrate this point, I plot the evolution of real wages for a fixed group of full-time workers who were between 30 and 35 years old in 1993 in Figure 1.6. In panel a, I divide these workers into three groups according to their position in the overall wage distribution of all full-time workers in 1993.[8] In panel b, I group these workers according to their position in the wage distribution of their 30 to 35 year old peers instead. Independent of the grouping, both panels show that real wages have grown for all groups. What maybe surprising at first glance is that real wages for the group of 30 to 35 year olds who found themselves at the 15th percentile in 1993 grew the fastest (by up to 38% depending on the grouping), while wages for workers at the median grew the slowest with wage growth of workers initially at the 85th percentile in between. Although more research is needed on this point, this may suggest a kind of mean reversion and considerable wage mobility at lower percentiles while steeper age earnings profiles for high-skilled workers may explain the accelerated wage growth at the top compared to the middle.[9]

[8] To increase sample sizes, I include in the 15th percentile workers from the 14th and 16th percentile and accordingly for the 50th and 85th percentile. Increasing or decreasing this bandwidth has little influence on the results.

[9] Another explanation for the observed pattern might be selection, i.e. "survival" of the best workers while less productive workers leave the full-time labor force along the way. Figure A.3 shows a version of Figure 1.6a for a strictly balanced panel of workers who are observed in each year between 1993 and 2014. Wage growth is more pronounced for every percentile group but the ordering regarding their growth rates remains the same as in the panels of Figure 1.6.

1.3 Decomposing Inequality in Full-Time Wages

A further step in understanding inequality relates to the question of how much of the change in inequality across two periods can be explained by compositional changes in the workforce. Table 1.1 summarizes the workforce along its demographic and main sectoral composition in 1993, 2009, and 2014 along with the respective changes. 1993 and 2014 mark the beginning and end of my data while 2009 – according to Figure 1.4 – represents a break in the wage evolution in particular of the 15th percentile that resulted in a subsequent decrease in overall inequality. Between 1993 and 2014, workers have become older (by about 3.6 years on average) and much more educated with the share of high-skilled workers doubling. The share of females and East Germans decreased somewhat while the share of workers with German nationality remained basically constant.[10] As shown before, the overall variance in wages in 2014 is higher than it was in 1993 but lower than in 2009 and wage growth at the 15th, 50th, and 85th percentile follows the patterns depicted in Figure 1.4a. Regarding the sectoral composition, the employment share of the service sector has grown from 64% in 1993 to 69% in 2014 while the employment shares of the primary and manufacturing sector remained constant at 2% and decreased from 33% to 29% in 2014, respectively, although the decrease in manufacturing in Germany has been lower than in other OECD countries.[11] A more detailed sectoral breakdown is presented in Table A.1.

Given that the dispersion of wages is higher within the group of older, high-skilled workers than within young, low-skilled workers, an aging and better educated workforce will mechanically be associated with higher inequality.[12] To analyze such effects more carefully, I use a recentered influence function (RIF) regression approach proposed by Firpo et al. (2009) to perform a detailed decomposition of the change in the variance and the 15th, 50th, and the 85th percentiles between

[10] When including part-time employment (excluding mini jobs), the share of women increased from 0.42 in 1993 to 0.46 in 2014.

[11] According to data from the OECD (2017a), in 2014 the share of manufacturing employment in total employment (including part-time employment with a much higher share of services) was 0.20 for Germany, 0.12 France, and 0.10 for the US.

[12] For instance, among full-time workers in 2014, the standard deviation of log real wages was 0.45 for low-skilled workers between 21 and 32 years while it was 0.54 for high-skilled workers between 45 and 56 years.

Table 1.1: Summary Statistics of Demographic Characteristics (1993, 2009, 2014)

	Means			Δ		
	1993	2009	2014	1993 -2009	2009 -2014	1993 -2014
Demographics						
Age (in years)	38.97	41.85	42.53	2.88	0.68	3.56
21-26 years	0.14	0.09	0.09	-0.05	0.00	-0.05
27-32 years	0.20	0.14	0.15	-0.06	0.01	-0.05
33-38 years	0.17	0.14	0.14	-0.03	-0.00	-0.03
39-44 years	0.16	0.20	0.14	0.04	-0.05	-0.02
45-50 years	0.13	0.20	0.19	0.07	-0.01	0.06
51-56 years	0.15	0.15	0.18	0.01	0.03	0.03
57-62 years	0.05	0.08	0.11	0.03	0.03	0.06
Female	0.35	0.34	0.32	-0.01	-0.02	-0.03
German[a]	0.90	0.92	0.90	0.02	-0.02	-0.00
East Germany	0.19	0.15	0.15	-0.03	-0.00	-0.04
Education Shares						
Low-Skilled	0.12	0.07	0.07	-0.05	-0.00	-0.05
Medium-Skilled	0.78	0.76	0.73	-0.02	-0.03	-0.05
High-Skilled	0.10	0.17	0.20	0.07	0.03	0.11
Log Real Wages						
Variance	0.20	0.27	0.24	0.08	-0.03	0.04
15th Percentile	4.02	3.94	4.05	-0.08	0.10	0.03
50th Percentile	4.48	4.50	4.54	0.02	0.04	0.06
85th Percentile	4.87	4.99	5.04	0.13	0.04	0.17
Main Sectors						
Agriculture[b], Mining, Quarrying	0.02	0.02	0.02	-0.01	-0.00	-0.01
Manufacturing[c]	0.33	0.29	0.29	-0.03	-0.01	-0.04
Services	0.64	0.68	0.69	0.04	0.01	0.05

Notes: This table presents summary statistics of full-time working individuals aged 21-62 years as observed in the SIAB7514. Statistics are weighted by spell duration. [a]refers to West Germany only. [b]includes forestry and fishing. [c]defined as NACE section D.

1993 and 2014 into a part explained by changes along the demographic, sectoral and regional composition of the workforce (*compositional effects*) and a part that reflects the changes in how these characteristics are priced (*wage structure effects*). The main advantage of a RIF approach over a re-weighting approach such as DiNardo et al. (1996) or a conditional quantile regression approach such as Machado and Mata (2005) is that is allows for a detailed decomposition of both the compositional and the wage structure effect and that it is computationally less demanding (see Fortin et al. 2011, for a detailed review of decomposition methods).

Table 1.2 shows the results of this decomposition exercise.[13] The overall variance of log real wages between 1993 and 2014 increased by 4.1 log points (the estimated increase reported in the table is virtually identical to the actual increase). Compositional effects alone, i.e. changes in the observable characteristics of the workforce, would have led to an increase of 2.9 or some 70% of the overall effect. This is sizable. The detailed decomposition in panel B reveals that changes in the education of workers as observed in Table 1.1 are the most important driver behind this effect. Thus, had the educational composition of workers in Germany remained at its 1993 level, the increase in inequality would have been lower by 1.3 log points or about one third of the overall increase. Both, the contributions of changes in the age structure and sectoral composition are also sizable but smaller while changes in the share of women, German workers or the regional allocation had virtually no effect. Panel C shows that changes in the return to education also contributed to increased inequality. Glitz and Wissmann (2017) show that this is, in particular, due to a rise in the medium- to low-skilled premium of young workers explained by their increasing relative scarcity (also compare results in column 3). Note that an important drawback of a decomposition approach such as the one presented here is that it cannot account for general equilibrium effects, for instance a change in the return to education resulting from a change in the relative supplies of differently skilled workers. Furthermore, the change in the return to working in a specific sector and being female both had a compressing effect on overall inequality though.

[13] Coefficients are reported in log points, i.e. coefficients are multiplied by 100. In the interest of space, I do not report standard errors, but significance levels only. Table A.2 shows the same table including bootstrapped standard errors.

This is an interesting point. In the case of the female coefficient it suggests that the (conditional) gender pay gap has decreased between the two periods.

The decomposition of overall inequality measured as the 85th to 15th percentile difference (85-15 difference in the following) yields somewhat different conclusions. Here, only 34% of the overall effect is explained by compositional changes while the majority or 66% is attributable to wage structure effects.[14] The most important effects are changes in the return to sectors (-37% of the overall effect) exerting a compressing effect, while the return to education (17%), and the sectoral composition (17%) contributed to increasing inequality. As the controversy between Autor et al. (2005) and Lemieux (2006) highlights, the focus on overall inequality can, however, mask considerable heterogeneity within the lower and upper tail of the distribution.[15] At the lower tail (measured as the 50-15 difference), 44% of the overall change is estimated to be due to compositional effects. Here, the biggest significant effects stem from changes in the return to working in a specific sector (108% of the overall effect), the return to education (55%), and the return to being female (-33%) while compositional changes are mainly driven by changes in the sector (32%) and age (26%) composition. At the upper tail, only 30% of the increase in the 85-50 difference is related to compositional changes. Changes in the return to specific sectors exert a strong compressing effect on upper tail inequality (-108%)[16] while changes in the return to age (17%) and changes in the education structure (18%) had a smaller but increasing effect on inequality.

My findings are in line with a number of other studies that have also analyzed the determinants of inequality. For instance, Autor et al. (2005) decompose wage

[14] One reason for this might be technical, i.e. the decomposition of the variance relies in part on the imputation of top coded wages where education and age are used in the imputation. Another reason might be that wage structure effects go in different directions at different parts of the distribution and thus are setting each other off to some extent.

[15] Lemieux (2006) focusing on the overall change in inequality in the US concluded that most of the growth in US residual wage inequality between 1973 and 2003 is due to spurious composition effects. In contrast, Autor et al. (2005) studying the same period find that compositional effects only played a major role at explaining changes in lower tail inequality masking considerable countervailing price compressions. In addition, according to their results, changes in the inequality at the upper tail are almost exclusively driven by price changes.

[16] Since some factors have a positive, and some a negative effect on the overall change, the estimated effects of some factors can be larger than the overall effect. At the end, the impact of all compositional and wage structure effects sum up to 100%.

Table 1.2: Decomposition of the Change in Inequality over 1993-2014

	(1) Variance	(2) 85-15	(3) 50-15	(4) 85-50
Panel A: Overall Effect				
Δ	4.13***	16.92***	5.56***	11.36***
Compositional Effects	2.87***	5.95***	2.55***	3.40***
Wage Structure Effects	1.27***	10.97***	3.01***	7.96***
Panel B: Composition Effects Attributable to				
Education	1.31***	2.38***	0.37***	2.01***
Age	0.93***	1.91***	1.43***	0.48***
Female	−0.06***	−0.13***	−0.19***	0.05***
German	0.01***	0.02***	0.02***	0.00
Region	−0.20***	−1.06***	−0.84***	−0.22***
Sector	0.88***	2.83***	1.75***	1.08***
Panel C: Wage Structure Effects Attributable to				
Education	1.18***	2.85***	3.07***	−0.22
Age	−0.74***	2.23***	0.31	1.92***
Female	−2.10***	−2.11***	−1.83***	−0.28
German	0.07	1.49***	0.70***	0.80***
Region	4.91	1.04	11.17	−10.14
Sector	−2.74***	−6.30***	6.00***	−12.30***
Constant	0.68	11.77	−16.41	28.18***

Notes: This table presents decomposition results using RIF regressions (Firpo et al. 2009). Calculations are based on full-time working individuals aged 21-62 years in the SIAB7415. Regressions are weighted by spell duration. Coefficients multiplied by 100 for better readability. Coefficients in 1993 are used as the reference category. ***/**/* indicate significance at the 1%/5%/10% level. Empirical p-values based on bootstrapped standard errors with 200 replications used.

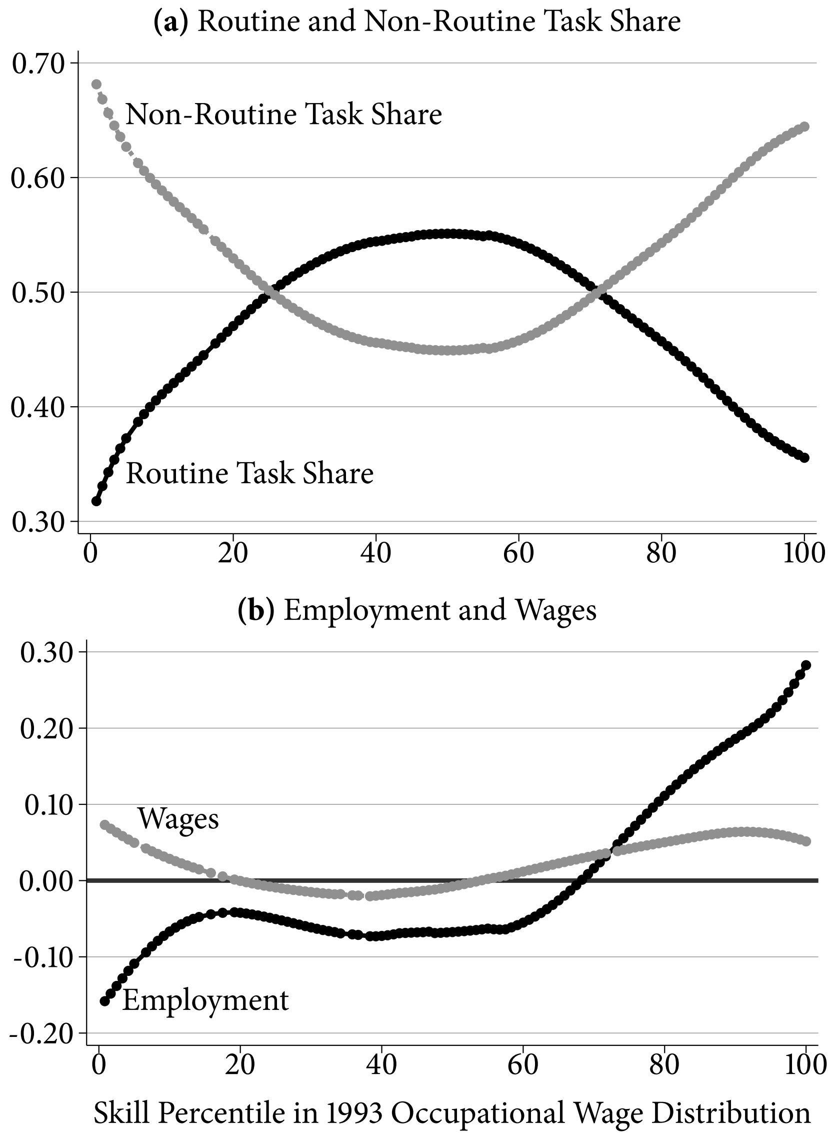
inequality in the US over the period 1973-2003 and find that compositional effects play a more important role at the lower end while wage structure effects are the main driver of the increase in upper tail inequality which is in line with the findings presented in Table 1.2. Baumgarten et al. (2016) study the changes in wage inequality of full-time employed men in the manufacturing sector in West Germany (about 20% of the overall full-time employed German labor force) between 1996 and 2010. They find that changes in the return to education and the decline in collective bargaining coverage are the two most important drivers of increased inequality among these manufacturing workers.

Despite the heterogeneous patterns at the lower and upper tail, two insights emerge from Table 1.2. First, the aging and higher educational attainment of the workforce in Germany has led to a mechanical in increase in inequality. Second, wage structure effects play an important role, in particular changes to the return to working in a specific sector. Studying the reasons behind these price change is an important step forward in understanding the ultimate drivers of inequality. For instance, technological progress and the automation of routine tasks might have a heterogeneous effect on the wages earned in different sectors or occupations. The next section will present evidence related to this reasoning.

1.4 Polarization

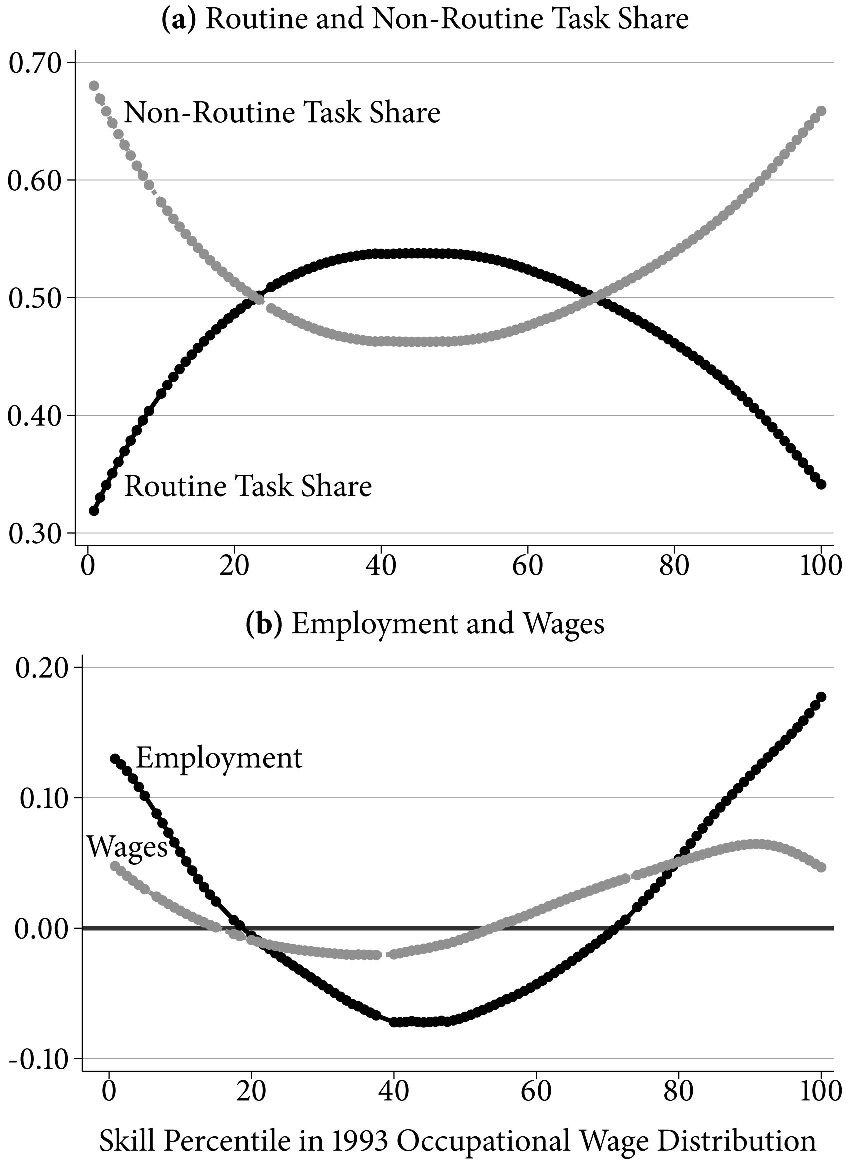
The previous analyses showed that inequality has decreased in recent years and that this was, in particular, due to a compression of wages at the lower end while inequality at the upper end has continued to increase although more moderately in recent years. I also presented evidence that changes in the return to working in a specific sector had a significant effect on inequality. A prominent explanation for such a polarization – i.e. an improvement of the two ends of the distribution for instance in terms of wages and employment relative to the middle – stipulates that the decrease in the cost of automating routine, codifiable tasks over the last years coupled with the fact that these tasks are concentrated in jobs in the middle of the wage distribution leads to a polarization of wages and employment at the tails (Goos and Manning 2007; Autor et al. 2009; Autor and Dorn 2014). In Figure 1.7 I assess the polarization hypothesis based on full-time workers. The panels of

Figure 1.7: Polarization Patterns for Full-Time Workers (1993-2014)



Notes: This figure plots two illustrations of the polarization of the labor market in Germany based on the SIAB7514. The x-axis of both panels is obtained by first computing the mean log real gross wage for each of 120 occupation groups in 1993. Occupations are then ranked by their percentile in the mean wages of all occupations ("skill percentile"). Panel a) plots the routine task (non-routine) share against skill percentile as the sum of the cognitive and manual routine tasks (analytical, interactive, and manual non-routine tasks), each smoothed using a locally weighted regression with a bandwidth of 0.8. Task data is taken from Dengler and Matthes (2014). For more details see Section A.2. Panel b) plots the change in the share in total employment scaled by 100 and the log wage difference between 1993 and 2014 each smoothed as in panel a). Calculations are based on all full-time spells in Germany between 1993 and 2014 weighted by spell duration.

Figure 1.8: Polarization Patterns for Full- and Part-Time Workers (1993-2014)



Notes: See notes for Figure 1.7. Calculations are based on all full-time and part-time spells in Germany between 1993 and 2014 weighted by spell duration.

Figure 1.7 are based on individual wage spells that are aggregated at the level of the 120 occupations observed in the SIAB. All occupation cells are sorted according to their mean log real wage in 1993 approximating the average skill level in each occupation.[17] Typical occupation at the lower end include hair dressers, waiters, cleaning and housekeeping occupations, or cooks. In the middle, masons, road workers, mechanics, or audio equipment mechanics are common occupations, and at the top one finds economists, doctors, managers, data and IT specialists, architects, scientists, and engineers. Using the classification by Dengler and Matthes (2014), I calculate the share of routine and non-routine tasks for each occupation (see section A.2 in the Appendix for more details).[18] Figure 1.7a shows quite strikingly that the share of routine tasks is highest for jobs in the middle of the skill distribution and much lower at either extreme while the picture is reversed for the share of non-routine tasks.[19] Figure 1.7b shows some moderate polarization in wages. However, in terms of employment, the picture is less clear. While there is a dramatic employment increase at the top of the initial occupational wage distribution starting above the 60th percentile, there is a moderate decrease in the middle (with the exception around the 15th percentile) and a strong decrease at the bottom percentiles. Thus, it seems that polarization in wages and in particular in employment is not borne out for th case of Germany.

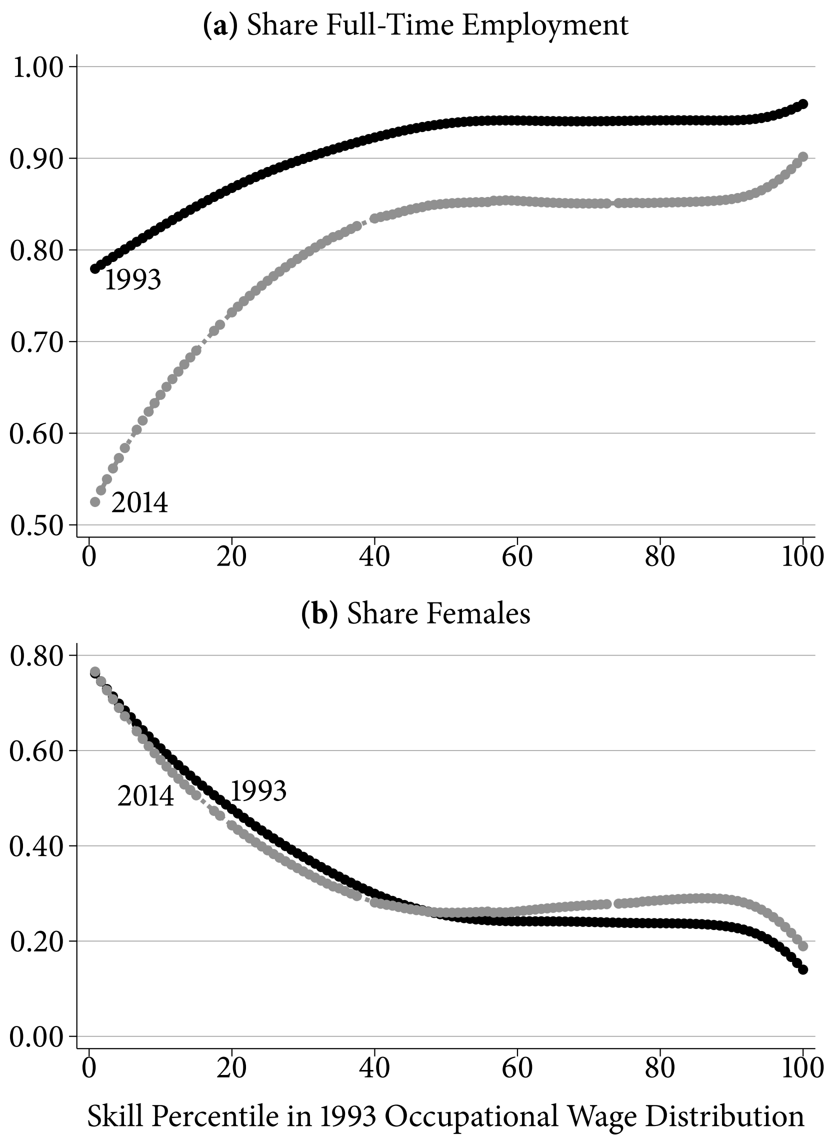
The focus on full-time workers, however, might be incomplete. As I will show in more detail in section 1.5, there was a dramatic increase in part-time employment over the last two decades. Furthermore, as Figure 1.9a suggests, part-time employment is particularly widespread in low-skilled service jobs at the lower end of the occupational wage distribution. For instance, in 2014, nearly 50% of all workers (not including mini job workers) worked part-time at the bottom of the initial occupational wage distribution while at the top, the corresponding figure was more 90%. Therefore in Figure 1.8, I replicate the calculations from Figure 1.7 but

[17] Sorting occupations according to median wages instead leaves the results virtually unchanged.

[18] The classification by Dengler and Matthes (2014) has been used by other papers before, for instance by Reinhold and Thomsen (2017). Like the O*NET classification for the US used by, for instance, Autor et al. (2003), this classification is based on expert knowledge about the specific tasks that are usually performed in a given occupation. Another classification used in earlier work is based on the German Qualification and Career Survey (BIBB) which classifies task to occupations based on answers from employees instead of experts (see Spitz-Oener 2006, for more details).

[19] This is mechanic as the non-routine share is 1 minus the routine share.

Figure 1.9: Share of Full-Time Employment and Females along the Occupational Wage Distribution (1993 and 2014)



Notes: This figure plots the share of full-time workers (panel a) and females (panel b) in all employment (full- and part-time) of a given occupation. The x-axis refers to the percentiles in the occupational wage distribution in 1993 constructed in the same way as in Figures 1.7 and 1.8. Each plot is smoothed using a locally weighted regression with a bandwidth of 0.8.

also include regular part-time workers.[20] Figure 1.8a is very similar to Figure 1.7a showing the same patterns regarding the concentration of routine task intensive jobs in the middle of the distribution. Figure 1.8b, however, is clearly different from Figure 1.7b. When also including part-time workers, a striking polarization pattern emerges. Jobs with a higher share of routine tasks, i.e. those in the middle, show the weakest employment and wage growth while non-routine intensive jobs, i.e. those at the two extremes of the distribution, show strong employment and (somewhat weaker) wage growth.

The evidence in favor of strong employment and wage polarization in Germany presented in Figure 1.8 is in line with findings for the US by Autor and Dorn (2014) who also find a pronounced U-shaped pattern in employment and wage growth along the occupational skill distribution between 1980 and 2005. Dustmann et al. (2009) look at West-German full-time working men over the periods 1980, 1990, and 2000 and find more of an L-shaped pattern similar to Figure 1.7b with substantial employment growth at top percentiles, a decline for the middle and no strong gains or losses at the bottom[21] One reason for not finding stronger employment and wage growth at the lower end is likely the exclusion of part-time employment as demonstrated above. Another related point is the exclusion of women as also Senftleben and Wielandt (2013) point out. Based on a sample including male and female worker (but still restricted to full-time employment) in West Germany, they find stronger polarization in employment growth for different sub-periods between 1979 and 2006 highlighting that the employment growth at lower-skilled occupations is almost exclusively driven by female employees in service jobs. Figure 1.9b confirms that the share of women is considerably (up to four times) higher at the lower tail of the initial occupational wage distribution. Taken together, this shows that when studying polarization – at least in Germany – it is crucial to include both women and part-time workers in the analysis for a comprehensive view.

[20] Regular part-time jobs are scaled to full-time equivalents using a weight of 2/3 following Dustmann et al. (2009). Ideally, I would also like to include so called “mini jobs”, i.e. jobs below the marginal part-time income thresholds that are exempted from most taxes and social security contributions, but these are only recorded since 1999 in the IAB data.

[21] Different from the approach followed here, they define skill percentiles according to the mean years of education.

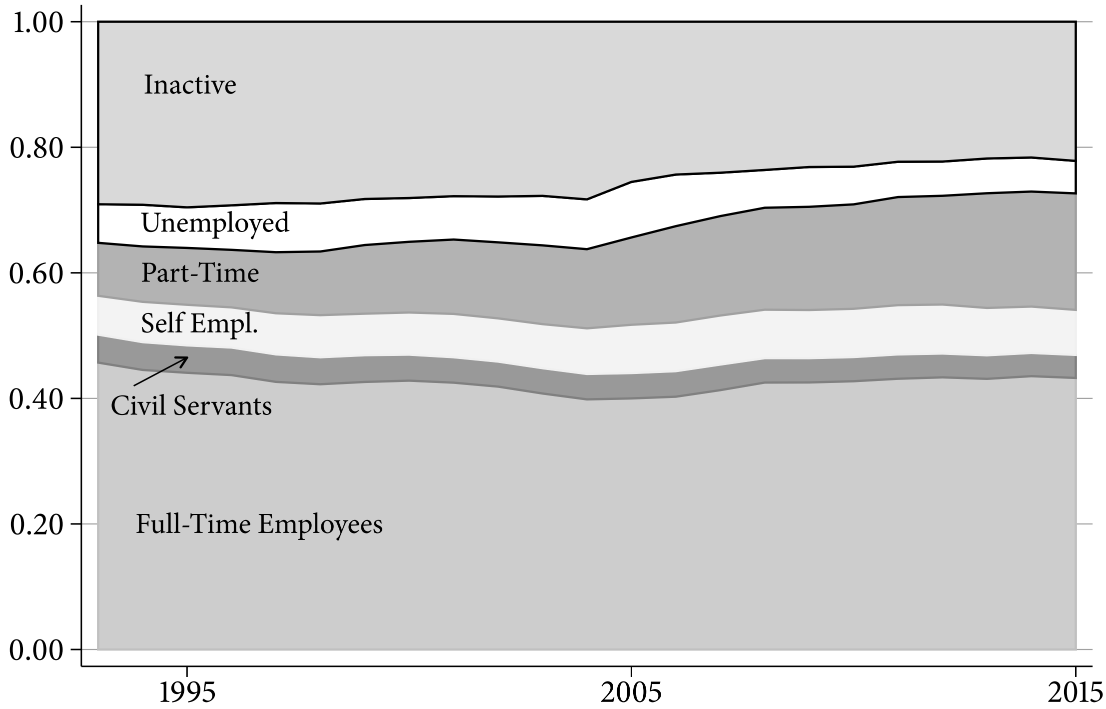
1.5 Labor Market Participation

The discussion so far has mainly focused on full-time employees, a common sample restriction of papers studying wage inequality and other labor market issues based on social security data in Germany (for instance Dustmann et al. 2009; Card et al. 2013; Baumgarten et al. 2016; Goldschmidt and Schmieder 2017). As outlined above, this best allows isolating wage changes from changes in the hours worked when using the SIAB. However, one of the most remarkable developments of the German labor market is related to changes in the participation of the population. Figures 1.10 and 1.11 illustrate this point in two different ways. Both figures are based on data from the microcensus, a 1% compulsory survey of the population (employment status), data from the Federal employment agency, and official population figures from the Federal Statistical Office. Figure 1.10 depicts the composition of the workforce with respect to the extent and type of labor market participation (Table 1.3 lists these shares for selected years). In 2014, the majority of the working-age population (15-64 years)[22] are full-time employees (44%) followed by the group of inactive individuals (22%) such as students, stay-at-home parents, the retired or disabled individuals, part-time employees (18%), self-employed (7.4%), unemployed (5.4%), and civil servants (3.7%).

The remarkable point is how this composition has evolved since the early 1990s. This is better seen in Figure 1.11 which shows the growth rates of the different shares indexed to 1993. The share of part-time employees saw a dramatic increase. Compared to its level in 1993 it has more than doubled and this rise was rather linear since the beginning of the 1990s with the exception of a more rapid increase in 2005 and 2006. No other components has experienced such a pronounced change. A frequently voiced concern is that part-time employment has crowded out full-time employment. Although the time series data presented here does not allow for a casual analysis, the data also suggest a different interpretation: The growth in part-time employment was largely fed by a decrease in the share of the inactive and unemployed. The main demographics groups responsible for this decrease were – according to Burda and Seele (2017) – previously inactive West-German

[22]The population between 15 and 64 years is generally considered the working-age population by institutions such as the World Band, the OCED and also the Federal Statistical Office ("Kernerwerbstätige").

Figure 1.10: Composition of the Population Aged 15-64 (1993 - 2015)

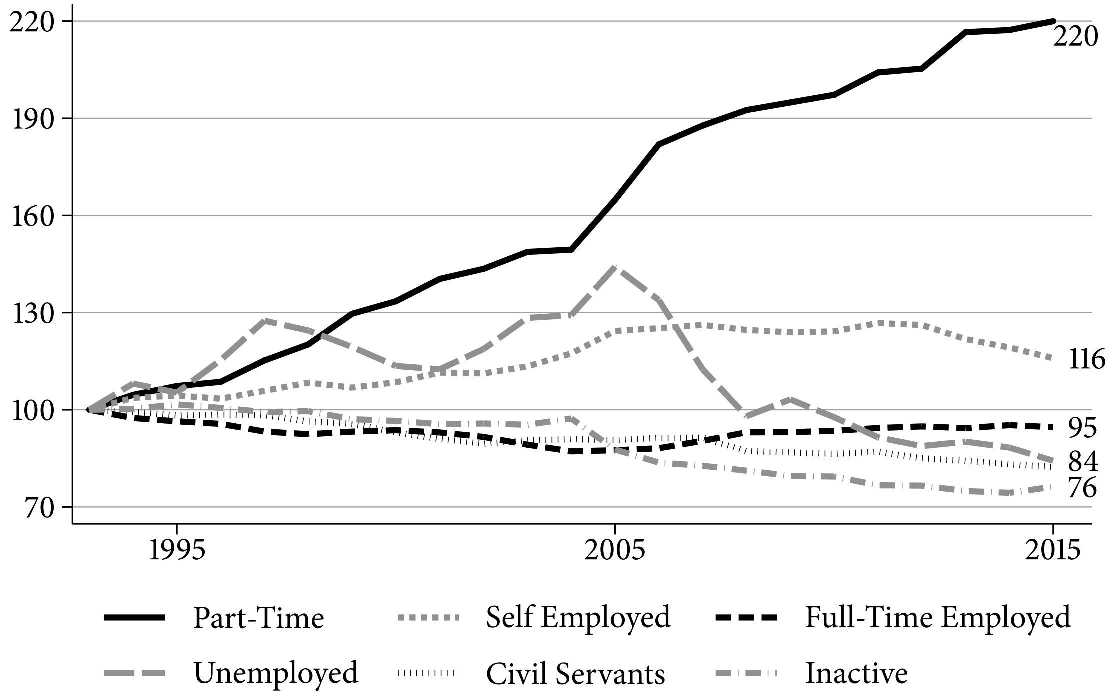


Notes: This figure plots the composition of the working age population (15-64 years) in Germany as cumulated shares. The total number of persons in employment by occupational status (employees, self-employed, and civil servants) and the shares of part- and full-time employees are calculated based on data from Statistisches Bundesamt (2017a,b) and is based on microcensus data. The total number of unemployed is taken from Statistik der Bundesagentur für Arbeit (2017c). The population aged 15-64 is taken from Statistisches Bundesamt (2017c).

women and older West Germans, and previously unemployed male and older East-Germans.[23] The share of full-time employees has decreased only slightly from 46% in 1993 to 44% in 2014 with a low of 40% around 2004 after which it increased (in terms of percentage points of the total population) nearly as much as the share of part-time employment. An important point is to compare the different forms of labor market participation as a percentage of the total working-age population so

[23] Burda and Seele (2017) also find that despite the large increase in the number of employed individuals, the total number of hours worked in Germany has only increased by 1.9% between 1993 and 2016, i.e. there was a large redistribution of working hours between full-time and part-time workers up until about 2010 since when full-time employment is also growing again.

Figure 1.11: Indexed Growth of the Composition of the Working Age Population (1993=100)



Notes: This figure plots the growth in the elements of the composition of the working age population (15-64 years) indexed to 1993. See notes for Figure 1.10 for details on the variable construction and data sources.

as to include the inactive and unemployed as well. Other studies compare shares and growth rates among the employed only which – given the strong increase in part-time employment – will mechanically drive down the share of full-time in overall employment (for instance Bäcker and Schmitz 2016; WSI 2017). Furthermore, comparing the absolute number of hours worked or individuals in full-time employment over time (such as Odendahl 2017) without taking the demographic changes in the working-age population into account might be misleading. As panel A of Figure 1.10 shows, the total working age population in 2014 was more than two million people (or 4%) smaller than in 1993.

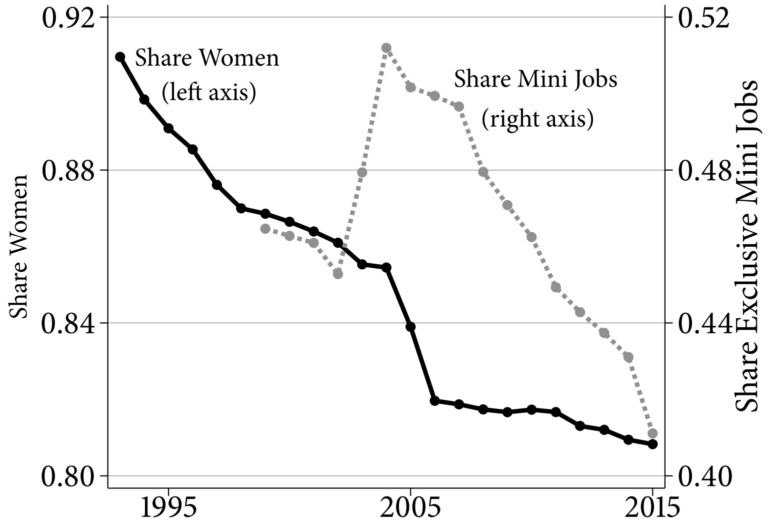
Table 1.3: Participation Shares of the Working Age Population

	1993	1999	2004	2009	2014
Panel A: Working Age Population (in 1,000s)					
Population 15-64 years	55,670	55,915	55,209	53,878	53,422
Index (1993=100)	100	100.4	99.2	96.8	96.0
Panel B: Composition of Working Age Population					
Full-Time Employed[a]	0.46	0.43	0.40	0.43	0.44
Civil Servants[b]	0.045	0.043	0.040	0.039	0.037
Self Employed	0.062	0.066	0.073	0.077	0.074
Part-Time[c]	0.084	0.11	0.13	0.16	0.18
Unemployed	0.061	0.073	0.079	0.063	0.054
Inactive	0.29	0.28	0.28	0.23	0.22
Panel C: Composition of Part-Time Employment					
Share Women	0.91	0.87	0.85	0.82	0.81
Share Exclusive Mini Jobs[d]	–	0.46	0.51	0.47	0.43
Panel D: Composition of Unemployed					
Share Long Term Unemployed[e]	0.28	0.37	0.38	0.33	0.37
Share Receiving ALG II[f]	–	–	–	0.65	0.68

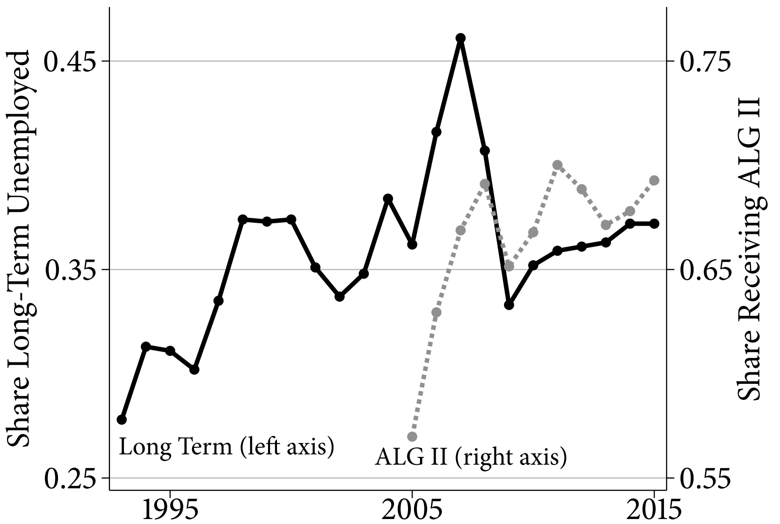
Notes: This table lists the shares in the total working age population (15-64 years) by occupational status. For details on the data and variable construction see notes for Figure 1.10. [a] includes those in vocational training. [b] includes part-time employed civil servants. [c] includes marginally employed (i.e. mini job) employees. [d] sums to one with regular part-time employment. [e] sums to 1 with short term unemployed. Long term unemployed defined as being unemployed for one year and longer. [f] sums to 1 with those receiving ALG I.

Figure 1.12: Composition of Unemployed and Part-Time Employment

(a) Share of Exclusive Mini Jobs and Women in Total Part-Time Employment



(b) Share of Long Term Unemployed and Share Receiving ALG II in Total Unemployment



Notes: This figure plots in panel a) the share of exclusive mini jobs (i.e. mini jobs that are not held as secondary jobs along with a regular part or full-time job; it is based on official data from Statistik der Bundesagentur für Arbeit (2017a,b) and crossed check with data from WSI (2017).) and the share of women in total part-time employment. Panel b) plots the share of long term unemployed (solid black line) and the share of unemployed receiving ALG II (dashed gray line). The share of long-term unemployed is computed as the share of individuals unemployed for more than one year divided by the total number of all officially registered unemployed individuals. Data is taken from Statistik der Bundesagentur für Arbeit (2017a).

Figure 1.12a suggests that the growth in part-time employment was not mainly driven by females (solid black line). Although part-time employment is predominantly female, the share of women in total part-time employment has decreased from 91% in 1993 to 81% in 2014. Another point regarding the composition of part-time employment is related to the extent in the hours worked or income earned of different part-time jobs. In Germany, part-time employment can take two different legal forms: part-time employment with regular earnings of more than 450 euros per month and so called "mini jobs" with regular earnings below 450 euros and subject to reduced tax payments and social security contributions.[24] Figure 1.12a shows that the recent growth in part-time employment was not mainly driven by mini jobs (dashed gray line). The share of exclusive mini jobs, i.e. mini jobs not held as secondary jobs, in total part-time employment has in fact decreased since 2005 meaning that regular part-time employment has grown more rapidly than mini jobs employment since then.

The share of self-employed has also increased considerably and a large share of this change was driven by a rise in the number of self-employed individuals who do not employ any other workers, the so called "solo self-employed" (Mai and Marder-Puch 2013). Many of these solo self-employed are suspected to be in fact employees of a single company misclassified as independent contractors (*Scheinselbständigkeit*, see Dietrich et al. 2017 for an empirical assessment). Finally, the share of civil servants has also decreased considerably since 1993 which is largely due to the privatization of former state-owned companies such as the national railways (*Deutsche Bundesbahn*) and the mail and telecommunication company (*Deutsche Bundespost*).

Anther issue that comes up regularly in discussions about the German labor market is related to the share of unemployed that are out of work for more than one

[24] Specifically, mini jobs are part-time jobs that either regularly do not pay more than 450 euros per month (threshold since 2013) or that last less than two months (short term employent/ *kurzfristige Beschäftigung*). Non short term mini jobs make up the majority of all marginal part-time employment (the general term for low-pay employment, *geringfügige Beschäftigung*). The term "mini job" was introduced in 2003 as part of the Hartz reforms. The regulations and earnings thresholds governing mini jobs have been subject to changes and were recorded in the IAB data since April 1999. There are also so called "midi jobs", part-time jobs that regularly pay between 450.01 and 850.00 euros per month that are subject to reduced social security contributions for the employer. Their share in total part-time employment, however, is marginal.

year. Figure 1.12b plots the share of these long-term unemployed in all unemployed individuals. About 1/3 of all unemployed are long-term unemployed. This share was lower at the beginning of 1990s, spiked in 2005 and 2006, and has been increasing slightly since. In general, however, there is no change in levels since the end of the 1990s.[25] A related issue concerns the share of unemployed workers who receive "ALG II" or often called "Hartz IV", a transfer at the household level for those who exhausted their regular unemployment benefits.[26] After a phase-in period since its introduction in 2005, the share of the unemployed receiving ALG II has fluctuated around 68% without a clear trend.[27]

Summarizing, the most important trend in labor market participation since Germany's reunification was a pronounced increase in part-time employment whereas the shares of the inactive and unemployed have decreased considerably and the share of full-time employment remained practically constant. The year 2005 seems to constitute a particular turning point. The share of unemployed and inactive individuals started to decrease strongly while the growth in part-time employment accelerated. This is also the point stressed by Burda and Seele (2016) who document a strong increase in total hours worked after the implementation of the Hartz reforms in 2003 which they show was mainly driven by an increase in part-time employment and not by demand factors.

1.6 Inequality in Equivalized Net Incomes

The activation of formerly inactive or unemployed individuals can be expected – *ceteris paribus* – to decrease inequality between individuals when also including the inactive and unemployed in the calculations. Since in the SIAB data only employed individuals are included and part-time employed as well as unemployed workers are not consistently recorded over the years[28], I use (self-reported) household income

[25] A regression of the share of the long-term unemployed on a linear time trend for the years since 1998 yields a coefficient very close and insignificantly different from zero.

[26] As of 2017, a recipient living in a one-person household received 409 euros per month plus a rent payment of up to about 700 euros including heating (in Munich).

[27] Again, a regression of the ALG II share on a linear time trend for the years since 2007 does not yield a coefficient significantly different from zero.

[28] Some of the problem are: (i) Mini job employment is only recorded since April 1999; (ii) due to a change in the reporting standards, starting in 1999 there is an increase in the number of part-time

data from the microcensus to study this question. In the microcensus, household income is recorded as net income accruing from all sources including interest payments, rents, transfers and benefits. A caveat of this data is that it is recorded in discrete brackets and that the number and range of these brackets changes across years. Therefore, I impute continuous household incomes by running a series of generalized Tobit regressions assuming that incomes are approximately normally distributed. Note that due to the varying brackets, the share of right censored incomes also changes considerably across years. It is 7-8% in 1993 to 1995, around 1.5% between 1996 to 1999, and less than 0.3% over 2000 to 2012. Thus, for the years starting in 1996, the extent of censoring is much less of an issue in the microcensus than in the SIAB. Finally, to account for economies of scale when living in the same household, I divide household incomes by the OECD-modified scale (OECD 2011) to obtain individual equivalized net incomes (See Section A.3 for more details on the data preparation). Microcensus data has not been used frequently for analyses of income inequality.[29] The GSOEP is still the most widely used data set for inequality analyses in Germany. This may be surprising as the large sample sizes and the high representativeness of the microcensus make it at least as suitable.[30] Also, as Stauder and Hüning (2004) point out based on a comparison of continuously measured incomes from the income- and consumer survey (*Einkommens- und Verbraucherstichprobe*, EVS), the recording in brackets in the microcensus has virtually no effect on estimated means or measures such as the Gini coefficient or the standard deviation of log wages. They conclude that it is not only valid to use microcensus data to measure income inequality, but – due to its large sample sizes and non-existent survey selection bias – also sensible to do so.

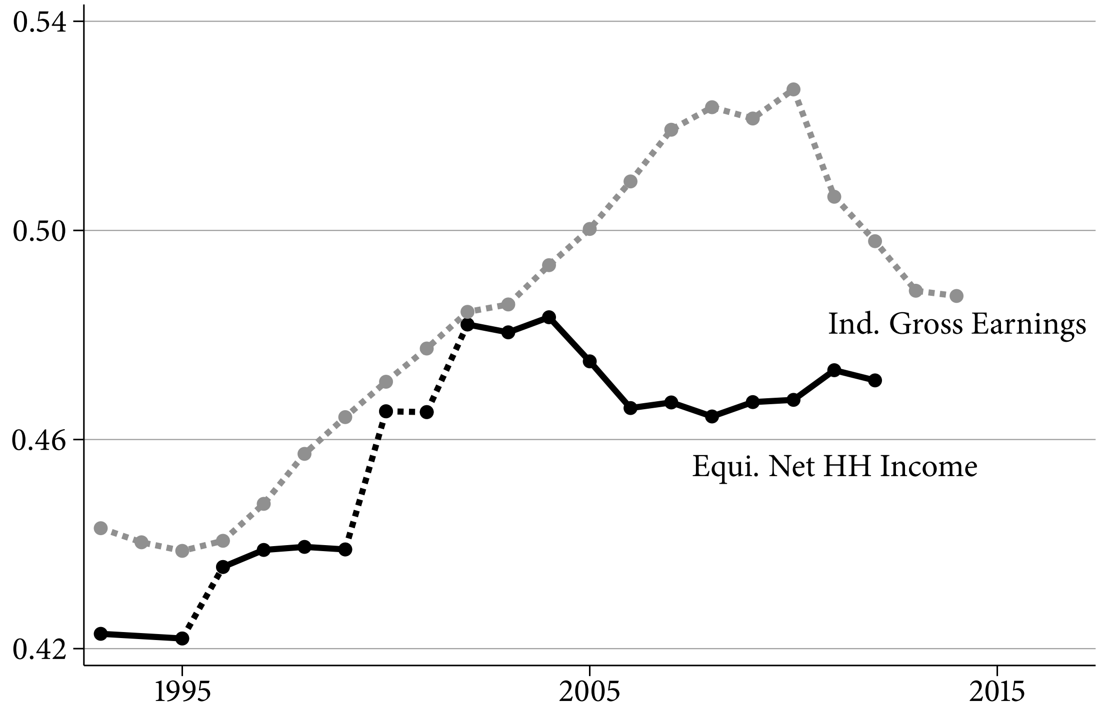
Figure 1.13 plots the evolution of the standard deviation of log equivalized real net incomes across time. To highlight that comparability between two adjacent

spells that is to some part a statistical artifact, in 2011/12 there is an increased number of missing values regarding part-time status. (iii) in 2005 and 2006 unemployed individuals not receiving ALG I are not recorded, see vom Berge et al. (2017).

[29] A few exceptions are Beznoska et al. (2016) and Statistisches Bundesamt (2017d).

[30] One disadvantage of the microcensus is that incomes are available only in aggregated form and no detailed breakdown of its subcomponents are available. Also, the framing of the income question might lead respondents to focus on regular and larger income components while smaller and transitory incomes might be left unreported. Still, the income concept is consistent across years and thus should allow for a valid comparison over time.

Figure 1.13: Evolution of the Standard Deviation of Equivalized Household Net Incomes (1993-2012) and Individual Gross Earnings (1993-2014)



Notes: This figure plots the evolution of the standard deviation in imputed log equivalized household real net incomes for the population aged 21-62 years (solid black line). OECD-modified equivalence scales used OECD (2011). Calculations based on microcensus waves 1993, 1995-2012 using survey weights. For details on the sample restrictions and imputation of incomes see section A.3. The dashed gray line replicates the standard deviation in log individual gross real wages based on the SIAB7514 shown in Figure 1.2.

years with different income brackets might not be fully warranted, such years are connected with a dashed instead of a solid line. In the same figure, I also plot the evolution of the standard deviation of log full-time gross wages based on SIAB data presented in Figure 1.2. This serves as a reference for how market incomes before taxes and transfers have developed.[31] Figure 1.13 shows that inequality in equivalized net incomes has increased since the beginning of the 1990s until it

[31] Ideally, I would plot individual gross market earnings to compare redistribution of the tax and transfer system and redistribution among household member, but unfortunately, these data are not observed in the microcensus.

reached its peak in 2002-2004. Since 2005, however, inequality has decreased again and since 2006 has stabilized at a lower level. The evolution of the equivalized net income inequality based on microcensus data shown in Figure 1.13 is by and large in line with comparable calculations based on GSOEP data. For instance, both Grabka and Goebel (2017, 78, Figure 8) and Bundesregierung (2017, p. 21, Figure 8) find a strong increase in the Gini coefficient up until 2005, a decrease up until 2009 and an increase again up until 2014. Given the relatively small sample sizes of the GSOEP, most of the changes after 2005 are, however, within the 95% confidence bands.[32] This is why Beznoska et al. (2016), also based on GSOEP data, conclude that inequality in equivalized net incomes has not increased significantly since 2005. They note that this holds even more when taking into account that – due to the need of regularly including refreshments samples given the relatively high attrition rates of the GSOEP – since 2013 a migrants subsample was included in the GSOEP. Once excluding this subgroup, they show that the increase in the Gini coefficient starting in 2013 is much less pronounced. It will be interesting to see how inequality measured in the microcensus has evolved after 2012 once these data will be available. Overall, the evolution of net income inequality shown in Figure 1.13 is consistent with the strong increase in the labor market participation since 2005. It also highlights that inequality among the entire population might decrease even when inequality in (full-time) market incomes is increasing.

1.7 Conclusion

This paper used highly reliable wage data from administrative records – the SIAB7514 – to study inequality in gross wages among full-time workers and data from a compulsory population survey – the microcensus – to study inequality in net incomes among the population. Inequality in gross wages has increased substantially since the early 1990s until 2009/10. Since then, wage inequality has been decreasing again. This is mostly driven by a compression of wages at the lower tail of the distribution. Decomposing the longer run increase in inequality between 1993 and 2014 shows

[32] Due to its large samples size, confidence bands are extremely tight in the microcensus which is why I do not report them here. For instance, the standard deviation of log equivalized household net incomes as shows in Figure 1.13 in 2007 is 0.465 with corresponding bootstrapped standard errors (200 replications) of 0.0010.

that the majority of this increase is due to wage structure effects, i.e. changes in the return to certain characteristics. In particular, changes in the return to working in a specific sector are important at both ends of the distribution while changes in the return to education (age) plays a more important role at the lower (upper) tail. Furthermore, I show that there is a clear negative relationship between the routine-task content of an occupation and its subsequent employment and wage growth indicating a strong polarization of the labor market in Germany over 1993 to 2014.

Apart from a trend break in full-time wage inequality since 2010, a second remarkable fact about the German labor market is the strong increase in labor market participation and the corresponding growth in part-time employment. While the share of inactive or unemployed individuals in the total working-age population decreased from 35% in 1993 to 27% in 2014, the share of part-time employment increased from 8% to 18% over the same period. The share of full-time employees today, in contrast, is only slightly lower than some two decades ago. The large scale activation of previously inactive or unemployed individuals – in absolute terms some 5 million individuals - that took place in particular since 2005 is also reflected in a decrease in overall net income inequality in the population.

Many important questions related to the inequality of incomes remain unanswered. For instance, it is common practice to use a common deflator to compute real wages. However, the cost of living can vary significantly across regions or worker types. Moretti (2013) shows that the wage differences between college and high school graduates in the US are much less pronounced once accounting for city-specific consumer price indices. Another highly relevant issue is the mobility of individuals in the income distribution. The same level of inequality measured in one year might be perceived differently depending on how easy or difficult it is for individuals at the bottom of the distribution to improve their position in the following years. Based on GSOEP data, Peichl and Ungerer (2016) calculate the so called "equality of opportunity", i.e. the chances to gain a higher income based on personal effort, and find that this index increased hand in hand with overall inequality since 1991 such that the equality of opportunity remained basically constant over this period. In a series of influential papers, Raj Chetty and coauthors using large scale administrative records find that in the US, intergenerational mobility, i.e.

the chances of children to move up in the income distribution (compared to their parents' position), has remained fairly stable for cohorts born 1971-1993 (Chetty et al. 2014b) but that it varies substantially across areas within the US Chetty et al. (2014a). In contrast, absolute income mobility, i.e. the chances of children to earn more than their parents, has fallen sharply from about 90% for the 1940 cohort to 50% for children born in the 1980s (Chetty et al. 2017). Studies of similar scope are so far lacking for Germany.

A crucial point in all empirical analyses of income inequality is the availability of representative, large scale and promptly available data. Many controversies and speculations about such important topics as income inequality, mobility, or equality of opportunities are – apart from different methodologies – related to the fact that different researchers use different, often incomplete data sets or that appropriate data is missing altogether. For instance, as mentioned before, comparable studies regarding intergenerational mobility are mostly lacking for Germany due to the absence of appropriate, long run panel data sets. Researchers and policy makers should make a joint effort to improve data quality and availability. In particular, the access to administrative, large scale data sets should be expanded and data provision should be made easier and faster (see the open letter by Card et al. 2010, referring to the situation in the US).[33] Many records are already collected by the authorities, but access to these data for research purposes remains limited and fragmentary. A step even further would be – for instance by means of a clearing house and state-of-the-art anonymization techniques – to match and harmonize data from different administrative sources.[34] An integration, for instance, of tax payer data, social security records and Microcensus data on a continuous basis would significantly improve data quality and thus also help to derive better and more reliable policy conclusions.

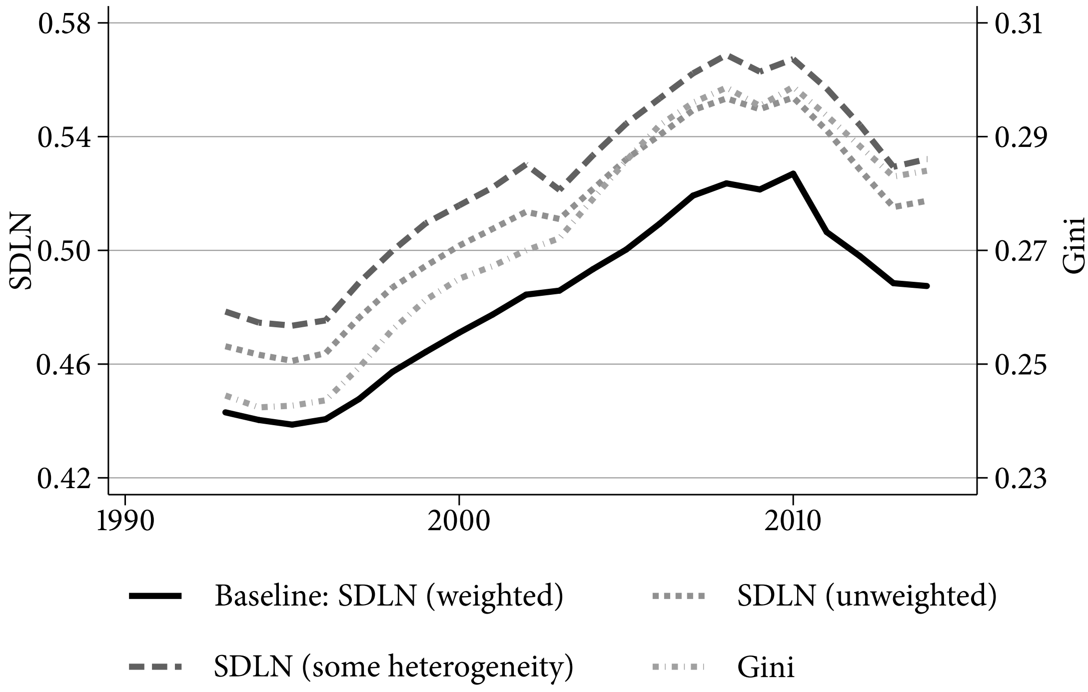
[33] For instance, there is a delay of up to six years between the reporting year of the latest available data set and the current calendar year for data sets such as the microcensus or the SIAB.

[34] A proposal along these lines concerning the measurement of high incomes and wealth in Germany was recently made by Bonin et al. (2016).

Appendix A

A.1 Additional Figures and Tables

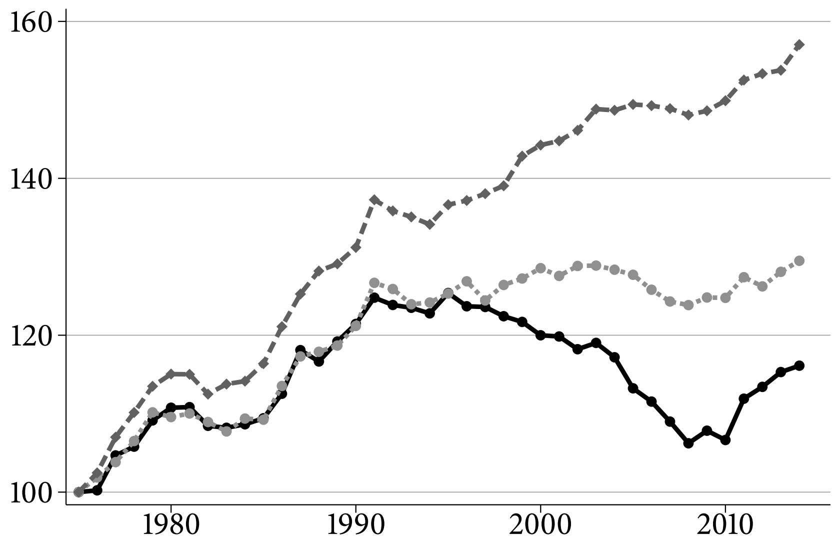
Figure A.1: Comparison of Different Methods to Measure Inequality



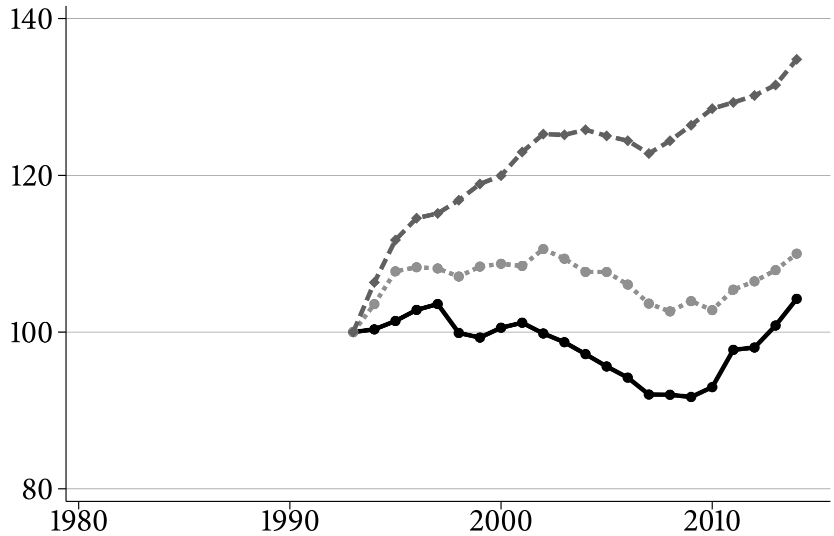
Notes: This figure plots the evolution of the standard deviation of log gross wages of full-time workers aged 21-62 years based on imputed wages assuming no heterogeneity in the variance of the error term across age and education groups weighted by spell duration (solid black line), not weighted by spell duration (gray short-dashed line), assuming heterogeneity in the variance of the error term across age and education groups weighted by spell duration (dark gray dashed line), and the Gini coefficient based on unweighted real earnings (gray dot-dashed line). Also see notes for Figure 1.2.

Figure A.2: Evolution of the 15, 50, 85 Percentiles of the Full-Time Wage Distribution

(a) West (1975=100)

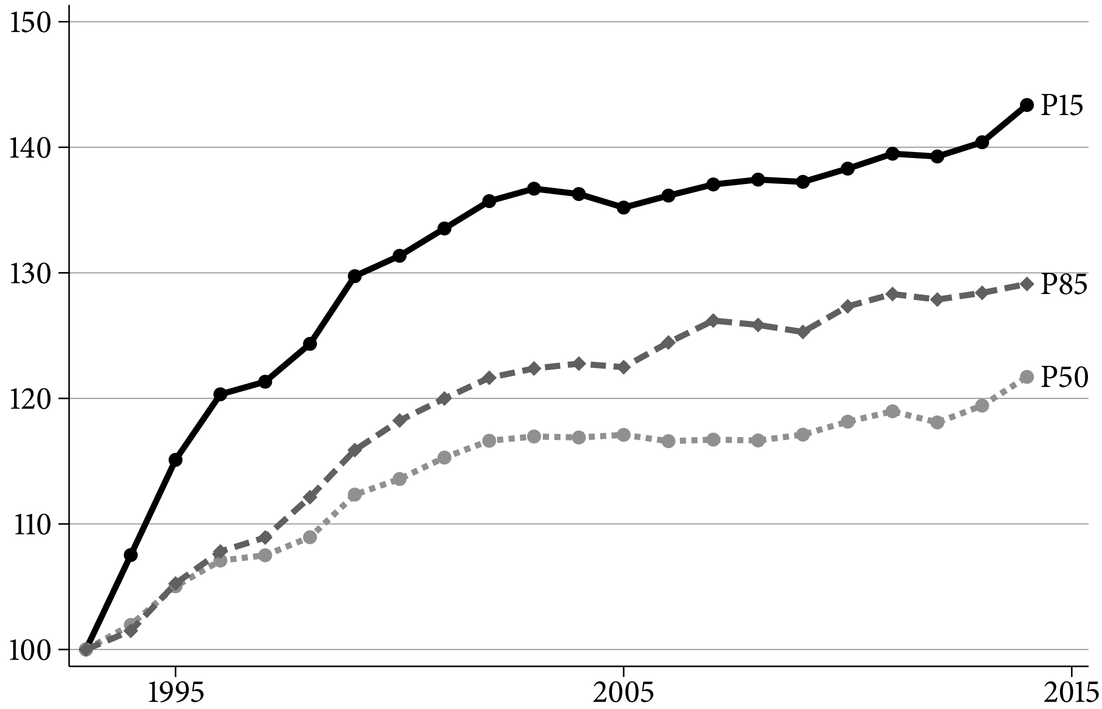


(b) East (1993=100)



Notes: This figure plots the evolution of the 15th, 50th, and 85th percentiles of the distribution of real gross wages of all full-time workers in Germany aged 21-62 years based on the SIAB7514 in panel a) for West Germany indexed to 1975 and in panel b) for East Germany indexed to 1993.

Figure A.3: Evolution of Real Wages for Fixed Panel of Full-Time Workers Aged 30-35 Years in 1993 and Observed without Gap through 2014 (Index 1993=100)



Notes: This figure plots the evolution of real gross wages for full-time workers aged 30-35 years in 1993 who were observed in the SIAB7514 in each year between 1993 to 2014 without interruption. Workers are grouped by their position in the distribution of all full-time workers aged 21-62 years in 1993. A bandwidth of 1 percentage point around each given percentile chosen to group workers. All calculations are weighted by spell duration.

Table A.1: Summary Statistics of Sectoral Composition (1993, 2009, 2014)

	Means			Δ		
	1993	2009	2014	2009 -1993	2014 -2009	2014 -1993
1. Agriculture and Mining	0.023	0.016	0.016	-0.007	-0.000	-0.007
2. Wood, Petroleum, Chemical, and Plastic Products	0.048	0.042	0.042	-0.006	-0.001	-0.007
3. Metal Products; Machinery and Equipment	0.100	0.097	0.097	-0.003	-0.000	-0.003
4. Electrical, Optical, and Transport Equipment	0.084	0.082	0.084	-0.002	0.002	-0.000
5. Food and Beverages; Textile, Paper Products; Publishing and Printing	0.094	0.070	0.064	-0.023	-0.007	-0.030
6. Hotels and Restaurants	0.022	0.024	0.023	0.002	-0.001	0.002
7. Construction	0.101	0.067	0.068	-0.035	0.001	-0.034
8. Wholesale and Retail Trade, Repair of Motor Vehicles; Personal and Household Goods	0.137	0.138	0.137	0.001	-0.001	-0.000
9. Transport, Storage and Communication	0.057	0.063	0.064	0.006	0.001	0.007
10. Financial Intermediation, Real Estate; Renting and Business Activities	0.109	0.178	0.193	0.069	0.015	0.084
11. Electricity, Gas and Water supply; Social and Personal Services	0.051	0.052	0.049	0.001	-0.003	-0.002
12. Education, Health and Social Services	0.088	0.109	0.106	0.021	-0.003	0.018
13. Public Administration and Defense	0.077	0.052	0.052	-0.025	-0.001	-0.026

Notes: Summary statistics of SIAB7514. Sample restricted to full-time working individuals aged 21-62 years. Statistics weighted by spell duration.

Table A.2: Decomposition of the Change in Inequality over 1993-2014 *(incl. Standard Errors)*

	(1) Variance		(2) 85-15		(3) 50-15		(4) 85-50	
	$\widehat{\beta}$	SE	$\widehat{\beta}$	SE	$\widehat{\beta}$	SE	$\widehat{\beta}$	SE
Panel A: Overall Effect								
Δ	0.041***	(0.001)	0.169***	(0.002)	0.056***	(0.002)	0.114***	(0.001)
Compositional Effects	0.029***	(0.000)	0.059***	(0.002)	0.025***	(0.001)	0.034***	(0.001)
Wage Structure Effects	0.013***	(0.001)	0.110***	(0.002)	0.030***	(0.002)	0.080***	(0.002)
Panel B: Composition Effects Attributable to								
Education	0.013***	(0.000)	0.024***	(0.001)	0.004***	(0.001)	0.020***	(0.001)
Age	0.009***	(0.000)	0.019***	(0.001)	0.014***	(0.001)	0.005***	(0.000)
Female	-0.001***	(0.000)	-0.001***	(0.000)	-0.002***	(0.000)	0.001***	(0.000)
German	0.000***	(0.000)	0.000***	(0.000)	0.000***	(0.000)	-0.000	(0.000)
Region	-0.002***	(0.000)	-0.011***	(0.000)	-0.008***	(0.000)	-0.002***	(0.000)
Sector	0.009***	(0.000)	0.028***	(0.001)	0.018***	(0.000)	0.011***	(0.000)
Panel C: Wage Structure Effects Attributable to								
Education	0.012***	(0.000)	0.029***	(0.001)	0.031***	(0.001)	-0.002**	(0.001)
Age	-0.007***	(0.002)	0.022***	(0.005)	0.003	(0.004)	0.019***	(0.004)
Female	-0.021***	(0.001)	-0.021***	(0.002)	-0.018***	(0.002)	-0.003*	(0.001)
German	0.001	(0.001)	0.015***	(0.002)	0.007***	(0.002)	0.008***	(0.002)
Region	0.049	(0.040)	0.010	(0.098)	0.112	(0.102)	-0.101	(0.072)
Sector	-0.027***	(0.005)	-0.063***	(0.015)	0.060***	(0.014)	-0.123***	(0.008)
Constant	0.007	(0.040)	0.118	(0.098)	-0.164*	(0.100)	0.282***	(0.074)

Notes: This table presents decomposition results using RIF regressions (Firpo et al. 2009). Calculations are based on full-time working individuals aged 21-62 years in the SIAB7415. Regressions are weighted by spell duration. Coefficients in 1993 are used as the reference category. Bootstrapped standard errors with 200 replications in parentheses. ***/**/* indicate significance at the 1%/5%/10% level. Empirical p-values used.

A.2 Sample Restrictions and Data Preparation (SIAB)

- **Sample Restrictions**: Starting from the universe of spells in the scientific use file of the *Sample of Integrated Labour Market Biographies* (SIAB-R 7514) which contains 51,987,959 records of 1,707,228 different individuals, I first apply some general sample restrictions following common practice when working with the SIAB data. Specifically, I drop spells with missing location information (after imputation, see below), spells of doctors and pharmacists (due to corrupted and missing records, see vom Berge et al. 2017, p. 32), spells that last only one day, spells with statuses "seeking for employment but not registered unemployed", "without status", and "seeking advice", zero wage employment spells, full-time spells with earnings below the marginal earnings threshold, and spells in East Germany before 1993, individuals younger than 21 years. In case of (exactly) overlapping spells, I keep only one of multiple overlapping unemployment spells, one of multiple overlapping full-time spells, and drop unemployment spells that overlap with full-time or regular part-time spells. My baseline sample comprises spells of individuals aged 21-62 years in West Germany since 1975 and spells in East Germany from 1993 and includes full-time and regular part-time spells, as well as ALG I and ALH (until 2004) spells, mini job spells (since April 1999), and ALG II spells (since 2007) contains 37,248,960 spells of 1,475,165 different individuals. My full-time sample excludes vocational trainings spells and contains 19,785,010 spells of 1,264,680 individuals.
- **Daily Wages**: I impute censored wages above the upper earnings threshold for compulsory social insurance (66,000 Euros per year in 2010) using the “no heteroskedasticity” approach by Gartner (2005) and Dustmann et al. (2009). Specifically, I consider wages as censored that were up to two Euros below the maximum wage value observed in each year and then estimate for each year and for males and females separately a censored regression of log wages on indicators of eight age groups, three skill groups and all their possible interactions, assuming that the error term is normally distributed and has the same variance across age and skill groups.
- **Education**: I impute missing education information following Fitzenberger et al. (2006) and group individuals in three categories (low, medium, and high).

Low comprises those with at most a *Realschul* degree, missing education, and those who have not completed any vocational training, *Abitur* (advanced high school degree), or a tertiary degree. Medium contains those with vocational training or *Abitur*. High refers to all those with a completed tertiary degree (*Fachhochschule* or university).

- **Location and Occupation**: Following Glitz (2012), I impute missing location and occupation information with the last non-missing value of each variable.
- **Task Shares:** The task shares are taken from Dengler and Matthes (2014) and comprise the following tasks: 1. non-routine analytical, 2. non-routine interactive, 3. routine cognitive, 4. routine manual, 5. non-routine manual. For more details on the construction and a description of each task see Dengler and Matthes (2014). Task shares are available for each of 334 occupations (*Klassifikation der Berufe 1988* at the 3 digit level) in 2011, 2012, and 2013. I average over these three years and use employment shares of each of the 334 occupations as of 1985 taken from the publicly available frequency counts of the weakly anonymous version of the SIAB7514 to aggregate from the level of 334 occupations in Dengler and Matthes (2014) to the 120 occupation groups available in the SUF of the SIAB7514.
- **Part-Time Spells Weights** Employment per occupation is computed as the sum of all weights per occupation where full-time spells are assigned a weight of 1 and part-time spells a weight of 2/3 and real wages of part-time spells are multiplied by 3/2 before averaging over occupations. All calculations are additionally weighted by spell duration.

A.3 Sample Restrictions and Data Preparation (Microcensus)

- **Sample Restriction**: I use the SUF of the Microcensus waves 1993 and 1995 to 2011 and restrict the sample to individuals in private households (i.e. excluding those in community housing, hospitals, and prisons) at their main place of residence aged 21-62 years.
- **Imputation of Equivalence Household Net Incomes**: Household income in the Microcensus is recorded in discrete income brackets and refers to net income from all sources including incomes from interest payments, rents, and all transfers and benefits. Since the number and range of these income

brackets is not constant across waves[35], I first convert each bracket's lower and upper bound to nominal euros and then divide these bounds for each individual by the OECD-modified equivalence scale, i.e. the first adults is assigned a weight of 1, each additional adult (>14 years) a weight of 0.5 and children (≤ 14 years)a weight of 0.3. I then impute continuous incomes using generalized Tobit regressions assuming that incomes are approximately log normally distributed (similar to the approach followed when imputing censored wages in the SIAB data) and assuming a constant variance across groups. Imputations are performed separately for men and women each in East and West Germany using all possible indicators for each of seven age categories, three education categories, all of their possible interactions, and indicators for having a partner, having children aged 14 or less in the household, and working full-time. Using indicators for education and age only or using more covariates has a negligible influence on resulting inequality measures.

A.4 Participation Shares

- **Employment Types**: The total number of the working population aged 15-64 by type of activity is taken from destatis (table 12211-0007) and is based on Microcensus data. I aggregate groups as follows:
  - self employed = self employed + family workers
  - civil servants
  - employees = white collar workers + blue collar workers + apprentices
- **Full- and Part-Time Shares**: The total number of full- and part-time dependently employed persons is taken from destatis (table 12211-0011) and is based on Microcensus data. It refers to the entire population of dependently employed including white collar workers, blue collar workers, those in vocational training and civil servants and includes workers in marginal part-time (denoted "mini jobs" starting from 2003).

[35] With constant income brackets or within a given year, a midpoint approach, i.e. assigning each individual the midpoint of her respective income bracket and thus implicitly assuming a uniform distribution of incomes within each bracket generally leads to very similar results.

Chapter 2

Skill Premiums and the Supply of Young Workers in Germany*

2.1 Introduction

Income inequality has increased in most OECD countries almost uninterruptedly since the mid 1980s (OECD 2014).[1] With his seminal book, Piketty (2014) returned inequality to the agenda of economists and policymakers alike. As opposed to capital incomes, which were the main driver of inequality at the beginning of the 20th century in the US and Europe, Piketty and Saez (2014) show that the recent increase is mainly driven by inequality in labor incomes.[2] But while there seems to be a consensus on the descriptive facts, there still remains a vigorous debate over the *drivers* of increasing inequality.

In this paper, we study how shifts in the supply of skills can help to understand the evolution of wage differentials between different demographic groups defined

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[1] Kopczuk et al. (2010) using social security records find an increase in earnings inequality in the US since the 1950s which accelerated in the 1970 and 80s and reached its highest level in the 2000s since the start of the records in 1937. Dustmann et al. (2009) show that wage inequality has also increased considerable in (West-)Germany over the last three decades.

[2] In line with this, Biewen and Juhasz (2012) find that the largest part of the increase in overall income inequality in Germany between 1999 and 2005 was due to rising inequality of labor incomes.

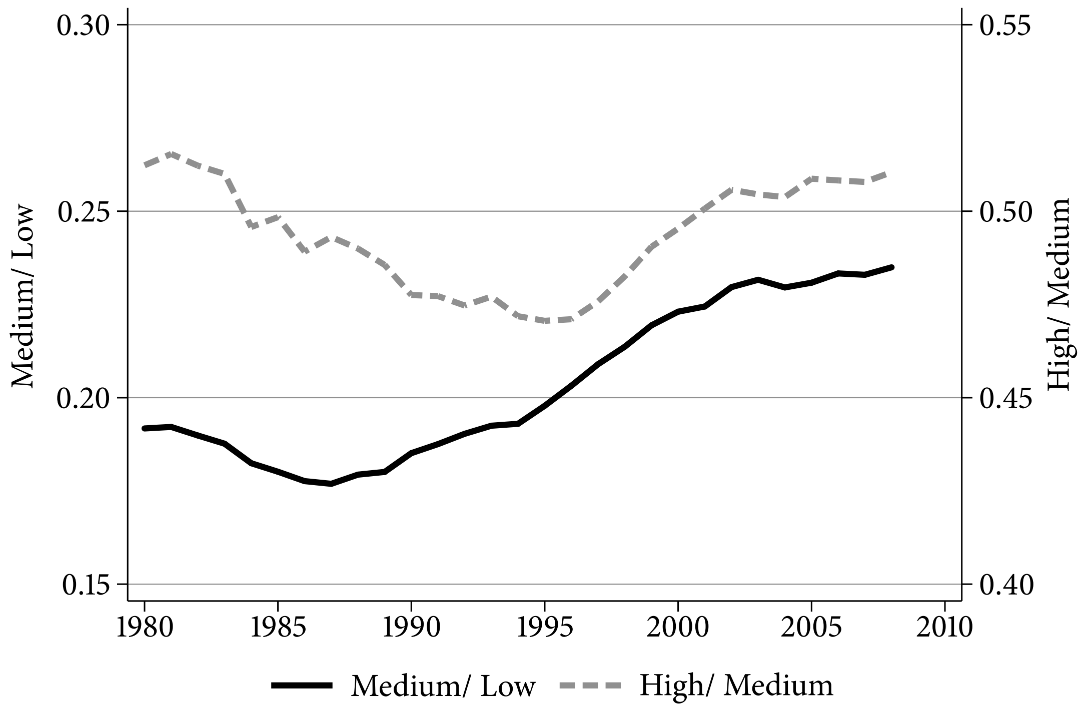
by skill-level and age. These *skill premiums* are an important aspect of inequality.[3] Figure 2.1 plots the evolution of two skill premiums important in the context of Germany's skill structure which, besides college and university education, is characterized by a strong pillar of vocational training. The wage differential between medium (those with vocational training and/or a high school degree) and low-skilled workers (those without a post-secondary degree) decreased slightly over the 1980s and then increased by a third from 18% to 24% since the late 1980s. The high skill premium, i.e. the wage differential between those holding a college or university degree and those with vocational training followed a U-shape pattern over the same period reaching 51% in the early 1980s and late 2000s and about 47% in the mid 1990s.

Our core hypothesis is that differential changes in the supply of skills are responsible for the observed patterns in skill premiums. In particular, we emphasize the role played by imperfect substitutability across age groups and changes in educational attainment across different cohorts. Our framework is a variant of a Tinbergen (1974) *education race* model where increases in the relative supply of more skilled workers and skill-biased technological change work in opposite directions in determining wage premiums. We distinguish between three skill groups (low, medium, and high) and between young (less than 30 years) and old workers, building on previous frameworks by Card and Lemieux (2001), Dustmann et al. (2009), and Goldin and Katz (2009). To illustrate the model's core idea, in Figure 2.2 we plot the skill premiums of both young and old medium-skilled (relative to low-skilled) and high-skilled (relative to medium-skilled) workers against their corresponding relative supplies (both linearly detrended to absorb, for instance, secular skill-biased technological progress). Except for the young high-skilled[4], there is a clear negative relationship – despite many potential rigidities governing the German labor market.

[3] For instance, Goldin and Katz (2007) estimate that the increased return to schooling accounts for about 2/3 of the overall increase in the variance of log hourly wages between 1980-2005 in the US.

[4] The relationship within the group of young high-skilled workers is attenuated due to the pre-unification boom 1987-1990 and in particular by the dot-com/New Economy boom and bust during 1999-2002. Once we exclude these years or allow for separate intercepts for these two periods, the relationship becomes clearly negative as expected, see the discussion in section 2.4.

Figure 2.1: Skill Premiums, Germany (1980-2008)

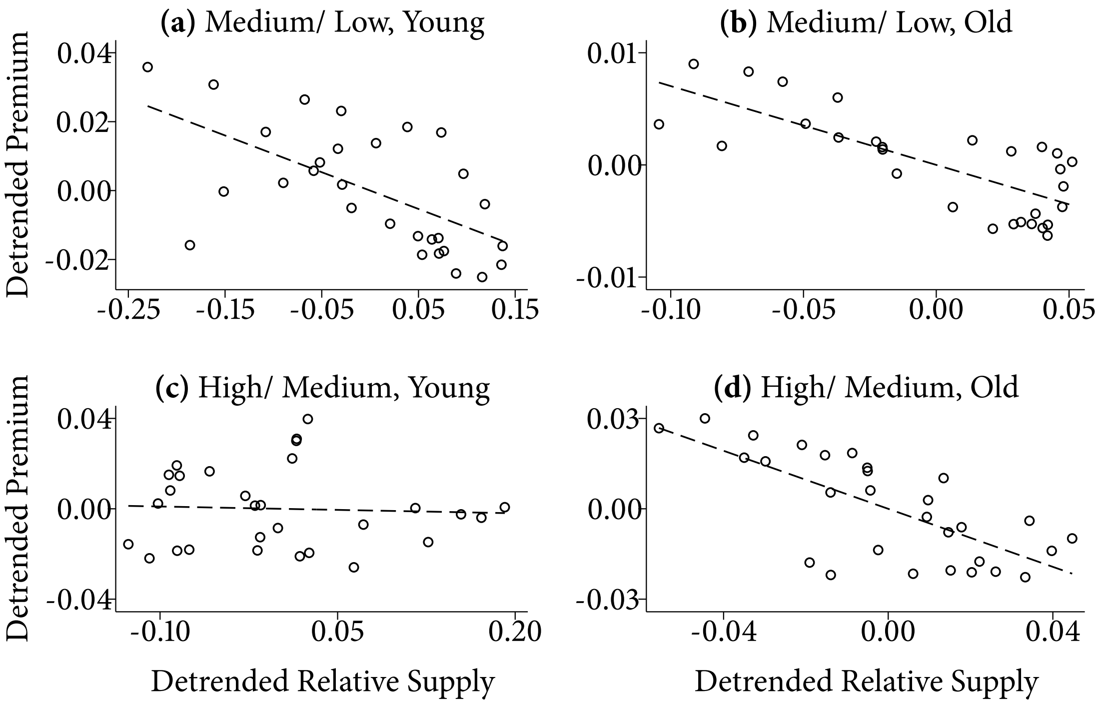


Notes: This figure plots skill premiums defined as log wage differentials between medium and low and high and medium-skilled workers who work full-time, live in West-Germany and have not moved from East to West-Germany between 1980-2008 holding the age- and gender composition constant. For more details on the construction of skill premiums see sections 2.3.3.

Using high quality administrative data for Germany over the period 1980-2008, we first systematically document the evolution of skill premiums along various skill levels and age groups. We show that almost the entire increase in the medium to low skill premium visible in Figure 2.1 is attributable to a pronounced increase in the medium skill premium of *young* workers (aged 30 and below) which increased from about 10% in 1980 to 25% in 2008 – a finding that has not been documented so far in the existing literature.[5] In contrast, wage premiums of older medium-skilled workers and of both younger and older workers holding a university degree have

[5]The only exception is Fitzenberger and Kohn (2006) who apply the CES production framework of Card and Lemieux (2001) to estimate the magnitude of wage changes that would have been necessary to halve unemployment rates in Germany in the mid 1990s.

Figure 2.2: Scatter Plots Premiums vs. Relative Supplies (1980-2008)



Notes: This figure plots skill premiums against their relative efficiency supplies separately for young and old workers. All variables are linearly detrended over 1980-2008. See section 2.3 for a description of skill premiums and efficiency supplies.

stayed remarkably stable. Second, our proposed model which relates skill premiums to shifts in relative skill supplies is able to account well for these differential patterns in observed skill premiums. This is especially true for the medium to low skill premium. Third, we try to be more careful about standard errors than most existing studies. We account for the uncertainty induced by generated regressors as well as serial and contemporaneous correlation of all variables in adjacent years by means of a moving block bootstrap approach (Kunsch 1989). As it turns out, standard errors computed with this method are up to five times larger than those based on conventional methods.

After having established a close link between the supply and the price of skill, we ask in the second part of the paper *why* these shifts in skill supplies occurred. Using data from the German microcensus, we trace out the long-term trends in educational attainment for each cohort born between 1950 and 1981. We show that after the fertility decline starting in 1965, there was a pronounced trend break in the educational attainment of the native (West-)German population: relative to their previous trends, the shares of both high- and low-skilled individuals increased while the share of medium-skilled individuals declined markedly. This observation, again, has gained little attention in the literature studying the evolution of skill premiums and wage inequality in Germany.

Our modeling approach is closely linked to a literature which started with the seminal paper by Katz and Murphy (1992) who use a CES-production function framework to systematically link supply and demand factors to wage premiums. Goldin and Katz (2009) extend their analysis by including historical U.S. wage data from 1890-2005 to understand the evolution of the high school and college premium in the long-term. Dustmann et al. (2009) apply the Goldin and Katz (2009) framework to study the role of supply and demand factors using the same German administrative data as we do. However, they do not allow for imperfect substitutability between young and old workers and find that the two-level CES approach might be "misspecified" (Dustmann et al. 2009, p. 867). Card and Lemieux (2001) introduce imperfect substitutability between young and old workers using data from the US, Canada and the UK. In contrast to these papers, our setting includes three skill groups (such as Dustmann et al. 2009; Goldin and Katz 2009) and (at least) two age groups (such as Card and Lemieux 2001) and we estimate the associated substitution elasticities – key parameters in many theoretical and empirical applications in the context of, for instance, immigration or long-run growth models – consistently in one framework while adjusting standard errors appropriately to the various forms of uncertainty.[6]

[6] In a recent study, Jeong et al. (2015) have proposed an alternative unifying framework to explain key empirical regularities in the US labor market. Based on a model in which workers supply two complimentary inputs, labor and experience, they show that changes in the total supply of experience due to demographic changes can fully explain the strong movements in the price of experience over the last four decades in the US. Moreover, those movements in the price of experience can account for the differential dynamics in the age premiums across education groups and the college premiums across age groups as well as the observed changes in cross-sectional and cohort-based life-cycle profiles. Contrary to the previous literature, they do not find evidence for demand shifts due to skill-biased technological change.

Our paper also relates to a range of studies that have used German administrative labor market data to study the rise in German wage inequality. Antonczyk et al. (2010a) emphasize the role of cohort effects in Germany as an important driver of lower end wage inequality. Card et al. (2013) identify an increasing dispersion in both person- and establishment-specific wage premiums as well as an increasing assortativeness in the matching of workers and establishments as main factors behind rising wage inequality, while Goldschmidt and Schmieder (2017) emphasize the role of domestic outsourcing, calculating that it contributed some 10% to the increase in German wage inequality since the 1980s. Burda and Seele (2016) apply the Katz and Murphy (1992) framework and show that the Hartz reforms implemented in 2003 boosted labor supply and contributed to the recent German employment miracle at the cost of decreasing real wages and increasing wage dispersion. Of particular relevance in the context of our work is the study by Dustmann et al. (2009) who document the recent trends in German wage inequality and perform an extensive analysis of competing explanations, identifying compositional changes (as DiNardo et al. 1996), a decline in unionization (see also Antonczyk et al. 2010b), skill-biased demand shifts favoring in particular the high-skilled, polarization (as proposed by Goos and Manning 2007; Autor et al. 2009; Autor and Dorn 2014) and changes in the supply of skills (similar to Goldin and Katz 2009) as key contributors to German wage inequality. In particular, Dustmann et al. (2009) emphasize that changes in the relative supply of medium-skilled workers are responsible for the significant increase in wage inequality at the lower tail of the wage distribution, attributing this to a deceleration in the rate of decline of low-skilled employment shares in the 1990s. They hypothesize that this deceleration might be due to the "large inflow of [mainly low-skilled] East Germans, Eastern Europeans, and ethnic Germans [...] into the West German labor market" (Dustmann et al. 2009, p. 867). Our findings, however, show that the decline in the relative supply of medium-skilled workers is primarily due to a pronounced and so far undocumented decrease in the share of *native medium-skilled* workers. Our paper thus fills an important gap when it comes to understanding the main drivers of recent changes in wage inequality in Germany.

The rest of the paper is organized as follows. In the next section, we present our model framework relating relative labor supplies to skill premiums. In section 2.3, we describe our data set and the construction of our key variables (skill premiums and efficiency labor supplies) and present graphical evidence on the evolution of skill premiums and efficiency supplies separately for young and old workers. These are the patterns we aim to explain in section 2.4, where we estimate the key structural parameters of our model. In section 2.5, we present our cohort analysis studying the long-term trends in skill attainment. Section 2.6 concludes the paper.

2.2 Analytical Framework

Our modelling approach closely follows previous work by Card and Lemieux (2001), Dustmann et al. (2009) and Goldin and Katz (2009). Suppose aggregate output at each time t is generated by a CES production function depending on college/university (or high-skilled) labor H_t and non-college (or non-high) labor U_t:

$$Y_t = A_t \left[\lambda_t H_t^\gamma + U_t^\gamma\right]^{\frac{1}{\gamma}} ,$$

where A_t denotes total factor productivity and λ_t is a time-varying technology or demand shifter that reflects both the importance of each input and factor augmenting (skill-biased) technological progress. The elasticity of substitution between non-college and college labor is given by $\sigma_{hu} = \frac{1}{1-\gamma} \in [0, \infty]$. If $0 \leq \sigma_{hu} < 1$ the two factors are gross complements. If $\sigma_{hu} \geq 1$ the two factors are gross substitutes and (high-) skill-biased technological progress will increase the wage differential in favor of better skilled workers.[7]

We choose this nesting structure to allow for a different elasticity of substitution between high and non-high and medium and low-skilled workers as do Dustmann et al. (2009). In contrast, Fitzenberger et al. (2006) and D'Amuri et al. (2010) assume the same mutual substitution elasticities between all skill groups, i.e. they assume, for instance, that high- and medium-skilled workers are as substitutable as high- and low-skilled workers which is less flexible than the approach we follow here.

[7] See Acemoglu and Autor (2012, 433ff) for a more careful distinction between demand shifters and factor-augmenting technology terms and on how the effect of skill-biased technological progress on skill premiums depends on σ.

Non-college labor is itself a CES-subaggregate of low- and medium-skilled labor inputs

$$U_t = \left[\theta_t M_t^{\rho} + L_t^{\rho}\right]^{\frac{1}{\rho}}, \tag{2.1}$$

where θ_t represents a demand shifter as above. The elasticity of substitution between medium- and low-skilled labor is given by $\sigma_{ml} = \frac{1}{1-\rho}$ defined analogously as before. Each type of labor in turn is composed of the corresponding supply in different age groups

$$L_t = \left[\sum_j (\alpha_{lj} L_{jt}^{\eta_l})\right]^{\frac{1}{\eta_l}} \qquad M_t = \left[\sum_j (\alpha_{mj} M_{jt}^{\eta_m})\right]^{\frac{1}{\eta_m}} \qquad H_t = \left[\sum_j (\alpha_{hj} H_{jt}^{\eta_h})\right]^{\frac{1}{\eta_h}},$$

which implies that the elasticity of substitution across the different age groups j in skill group s is given by $\sigma_{as} = \frac{1}{1-\eta_s}$. This nesting structure is supposed to reflect the fact that workers within the same skill group but of different ages and thus experience levels are likely to be imperfect substitutes.

Imposing the standard assumption that each labor input is paid its marginal product yields the following wage equations for each skill-age labor type:

$$w_{jt}^{L} = \frac{\partial Y_t}{\partial L_{jt}} = Y_t^{1-\gamma}(1-\lambda_t)U_t^{\gamma-\rho}(1-\theta_t)L_t^{\rho-\eta_l}\alpha_{lj}L_{jt}^{\eta_l-1} \tag{2.2}$$

$$w_{jt}^{M} = \frac{\partial Y_t}{\partial M_{jt}} = Y_t^{1-\gamma}(1-\lambda_t)U_t^{\gamma-\rho}\theta_t M_t^{\rho-\eta_m}\alpha_{mj}M_{jt}^{\eta_m-1} \tag{2.3}$$

$$w_{jt}^{H} = \frac{\partial Y_t}{\partial H_{jt}} = Y_t^{1-\gamma}\lambda_t H_t^{\gamma-\eta_h}\alpha_{hj}H_{jt}^{\eta_h-1} \tag{2.4}$$

Assuming that σ_a is the same in each of the three skill groups, i.e. $\sigma_{al} = \sigma_{am} = \sigma_{ah}$ (we will relax this assumption later) we finally get the following expressions for the medium to low skill premium

$$\omega_{jt}^{M} \equiv \ln\left(\frac{w_{jt}^{M}}{w_{jt}^{L}}\right) = \ln(\theta_t) + \left(\frac{1}{\sigma_a} - \frac{1}{\sigma_{ml}}\right)\ln\left(\frac{M_t}{L_t}\right) + \ln\left(\frac{\alpha_{mj}}{\alpha_{lj}}\right) - \frac{1}{\sigma_a}\ln\left(\frac{M_{jt}}{L_{jt}}\right) \tag{2.5}$$

$$= \ln(\theta_t) + \ln\left(\frac{\alpha_{mj}}{\alpha_{lj}}\right) - \frac{1}{\sigma_{ml}}\ln\left(\frac{M_t}{L_t}\right) - \frac{1}{\sigma_a}\left[\ln\left(\frac{M_{jt}}{L_{jt}}\right) - \ln\left(\frac{M_t}{L_t}\right)\right] \tag{2.6}$$

and the high to medium skill premium

$$\omega_{jt}^{H} \equiv \ln\left(\frac{w_{jt}^{H}}{w_{jt}^{M}}\right) = \ln\left(\frac{\lambda_t}{\theta_t}\right) - \frac{1}{\sigma_{hu}}\ln\left(\frac{H_t}{U_t}\right) + \frac{1}{\sigma_a}\left(\frac{H_t}{M_t}\right) - \frac{1}{\sigma_{ml}}\ln\left(\frac{U_t}{M_t}\right) + \ln\left(\frac{\alpha_{hj}}{\alpha_{mj}}\right) - \frac{1}{\sigma_a}\ln\left(\frac{H_{jt}}{M_{jt}}\right) \tag{2.7}$$

$$= \ln\left(\frac{\lambda_t}{\theta_t}\right) + \ln\left(\frac{\alpha_{hj}}{\alpha_{mj}}\right) - \frac{1}{\sigma_{hu}}\ln\left(\frac{H_t}{U_t}\right) - \frac{1}{\sigma_{ml}}\ln\left(\frac{U_t}{M_t}\right) - \frac{1}{\sigma_a}\left[\ln\left(\frac{H_{jt}}{M_{jt}}\right) - \ln\left(\frac{H_t}{M_t}\right)\right]. \tag{2.8}$$

Given all σ's > 1, the model predicts that over time the premium of medium-skilled workers in age group j, ω_{jt}^{M}, *increases* with θ_t, the rate of skill-biased technological change (or shifts in relative demand in favor of workers with vocational training) and *decreases* with the aggregate and age-group specific relative supply of medium-skilled workers given by $\frac{M_t}{L_t}$ and $\frac{M_{jt}}{L_{jt}}$, respectively. Similarly, the age group specific high to medium skill premium ω_{jt}^{H} depends positively on technological progress favoring the high-skilled relative to the medium-skilled, $\frac{\lambda_t}{\theta_t}$, and negatively on the aggregate relative supply of high to non-high, non-high to medium-skilled labor, and the age group specific relative supply of high-skilled workers denoted by $\frac{H_t}{U_t}$, $\frac{U_t}{M_t}$ and $\frac{H_{jt}}{M_{jt}}$, respectively. These equilibrium equations will guide our empirical analysis in section 2.4.

2.3 Data and Descriptive Evidence

2.3.1 Data Set and Construction of Baseline Sample

To take the model to the data, we need to construct skill premiums and labor supplies for each of the distinct skill-age-groups. We use administrative labor market data provided by the Institute for Employment Research in the *Sample of Integrated*

Labour Market Biographies (SIAB).[8] The SIAB is a 2% random sample of the official records of all employees subject to social security in Germany between 1975 and 2010. It contains the labor market history of about 1.5 million individuals and includes information on daily wages and employment status (full-time, part-time, unemployed, in vocational training) as well as a number of individual characteristics such as age, gender, skill, German nationality, region, occupation, and industry. We restrict the analysis to men and women between 21 and 60 years of age living in West Germany with earnings above the official marginal earnings threshold (400 euros per month in 2010[9]) as marginal part-time spells were only officially recorded from 1999 onwards. In addition, we exclude the years 1975-1979 due to the very high incidence of censoring among the high-skilled and the crisis years 2009/10 such that our final sample covers the years 1980-2008. We also conduct three imputations that are by now common practice when working with IAB data: the imputation of missing education information following Fitzenberger et al. (2006), the correction for the structural break in 1984 according to Fitzenberger (1999) and Dustmann et al. (2009) and the imputation of censored wages above the upper earnings threshold for compulsory social insurance (66,000 euros per year in 2010) applying the "no heterogeneity" approach suggested by Gartner (2005).[10]

2.3.2 Definition of Skill and Age Groups

For our subsequent analysis, we divide workers into low-, medium- and high-skilled. Following Dustmann et al. (2009), we define the low-skilled as those with missing or at most lower secondary education (*Realschule* or less), medium as those with apprenticeships, vocational training, and/or a high school degree (*Abitur*), and high-skilled as those with a tertiary degree (*Fachhochschule* or *Universität*). This grouping differs from many US studies where a distinction is only made between college and non-college labor to study the college premium (Card and Lemieux 2001; Autor 2014). The division into three skill groups in Germany reflects Germany's

[8] Specifically, we use the scientific use file of the SIAB Regional-File 1975-2010, see vom Berge et al. (2013) for a detailed description of this data set.

[9] We convert all monetary values into 2010 euros using the consumer price index of the German Bundesbank.

[10] See Appendix B.2 for a more detailed description of the derivation of our sub-sample and the imputation of censored wages.

strong pillar of vocational training and is also clearly suggested by comparing the wage levels of these groups (see Figure B.1). Along the age dimension, we consider eight different age groups spanning five year intervals for ages between 21-60 years. For most of the graphical evidence and the empirical estimations, however, we just distinguish in each skill group between young (≤ 30 years) and old workers (> 30 years) as these two groups capture well the underlying trends of more finely disaggregate age groups (see section 2.3.5 for more details).

2.3.3 Skill Premiums and Efficiency Labor Supplies

Our objective is to calculate the *pure* price for different skill levels net of any compositional changes due to, for instance, migration or changes in the gender or age group composition of the working population.[11] To keep our premium sample as homogeneous as possible, we restrict the attention to men and women working full-time and are "West German natives", i.e. we exclude those who started their labor market biography in East Germany and then moved to West Germany as well as those with missing or non-German nationality information.[12] We then calculate age and gender composition constant skill premiums similar to Katz and Murphy (1992).[13] Skill premiums can be interpreted as the (approximate) percentage difference in wages between two skill groups. Section B.3 in the Appendix describes the computation of skill premiums in more detail.

Our labor supply measures are based on a broad set of individuals and are expressed in efficiency units which can be understood as productivity adjusted full-time equivalents. To compute efficiency labor supplies, we include full-time,

[11] For instance, Dustmann et al. (2009) show that it is important to account for compositional changes in the workforce but that neither lower or upper tail inequality can be fully accounted for by these compositional changes. Carneiro and Lee (2011) compute skill premiums that are also adjusted for the quality of college graduates.

[12] Ideally, we would also like to exclude ethnic Germans and those East Germans who came to work in West-Germany during 1989-1991 or who started their employment history in West-Germany right away, however, we cannot identify these individuals in the SIAB data. We will identify these groups *as aggregates* using additional data sets when we assess the impact of migration on skill premiums in section 2.5.

[13] Figure B.2 in the Appendix compares "raw", i.e. unadjusted, and composition-adjusted skill premiums for young and old workers. The comparison shows that compositional effects play some role, but in general raw and adjusted premiums follow the same overall patterns.

part-time (but no marginal part-time spells as noted above), vocational training, and unemployment spells of all workers registered in West Germany, i.e. we include West German natives as well as foreigners and those who were first registered in East Germany and migrated to West Germany (we will refer to the latter two groups as “migrants” in what follows). In contrast to our premium data set, we choose such a broad set of workers and work types to mitigate concerns regarding the endogeneity of labor supplies. If we computed labor supplies based on full-time spells only, we would fail to incorporate transitions to and from part-time work or unemployment induced by changes in skill premiums, or any differential effects of the business cycle on the labor supply of different skill or age groups.

Labor supplies need to be measured in efficiency units because the framework outlined in section 2.2 assumes that workers in the same skill-age cell are perfect substitutes. Therefore, we allow productivities (reflected in wages) to differ by age and skill group as well as gender and West German nativity.[14] Note that these productivity measures are time-invariant, i.e. we average wage differences over all sample years to approximate the underlying productivity differences (see Section B.4 in the Appendix for more details). Accounting for productivity differences between natives and migrants is also important to mitigate issues related to potential downgrading of migrants’ education and experience, i.e. the fact that the human capital of migrants is not fully transferable (see for instance Friedberg 2000; Dustmann et al. 2012; Basilio and Bauer 2017).

Finally, we translate spells into full-time equivalents. Since working hours are not readily observable in the IAB data, we approximate them by assigning long part-time spells (i.e. part-time spells with more than half of the hours of a comparable full-time work schedule) a weight of 2/3 and short part-time spells a weight of 1/2 (less than half of a full-time work schedule) following Dustmann et al. (2009). Vocational training and unemployment spells are assigned a weight of 1/3. In our robustness checks, we show that our results are not sensitive to the specific weighting scheme. For instance, it would also be sensible to assign a weight

[14] One reason to allow for different efficiency weights for women is that women – on average – work less hours than men in the same age × skill group, even if both are recorded as working full-time or part-time in the IAB-data. Our results, however, do not depend on allowing for different efficiency weights by gender, i.e. when we assign the same efficiency weight to men and women alike, our estimation results presented in Table 2.4 remain virtually unchanged.

of 1 to those unemployed who worked full-time before. Applying this alternative weighting scheme leaves our estimates basically unchanged. Section B.4 in the Appendix contains more details on the construction of our efficiency supplies.

2.3.4 Summary Statistics

In panel A of Table 2.1, we summarize some characteristics of our wage sample based on which we construct the different wage premiums. Between 1980 and 2008, the West German native full-time workforce became older with the share of young workers below 30 years dropping from around 30% in the 1980s to 19% in 2008. This is the consequence of declining cohorts sizes after the baby boomer generation in the mid 1960s. The share of women working full-time remained remarkably stable over the sample period at around 33%. In contrast, the skill composition of full-time workers changed dramatically: The share of low-skilled workers dropped from 20% in 1980 to 6% in 2008 with the largest decline occurring in the 1980s. The share of medium-skilled workers followed a reversed U-shape reaching 81% in the 1990s and then declined to 79% in 2008. The share of high-skilled workers increased more than threefold since 1980 in a virtually linear fashion reaching 15% in 2008. Wages in all three skill groups grew during the 1980s and the 1990s but then declined in the 2000s. Wage inequality measured as the standard deviation of log real wages remained relatively stable up to the end of the 1990s but increased considerably since then.[15]

Panel B summarizes our supply data. The workforce including part-time, vocational training and unemployment spells is younger and more female. The share of females increased much more than in the sample of full-time workers as the increased participation of women was concentrated mainly in part-time jobs (see also Burda and Seele 2016). The broader set of workers represented in the supply data set is also less well educated. While the share of individuals receiving unem-

[15] This is in line with Dustmann et al. (2009, Figure I, p.850) and Card et al. (2013, Table I, p. 975) who also find an acceleration in the dispersion of log wages in the 1990s for the sample of all full-time West-German workers (including East movers and foreigners) using IAB data. It is also in line with Biewen and Juhasz (2012) who, using SOEP data, find an unprecedented rise in net equivalized income inequality since 1999/2000.

Table 2.1: Summary Statistics of Wage and Supply Sample

	1980	1990	2000	2008
Panel A. Wage Sample (Full-Time Natives)				
Age	39.0	38.4	39.8	41.4
Young (≤ 30 years)	0.29	0.31	0.20	0.19
Female	0.32	0.33	0.34	0.33
Shares:				
Low-skilled	0.20	0.11	0.07	0.06
Medium-skilled	0.75	0.81	0.81	0.79
High-skilled	0.05	0.08	0.12	0.15
Real monthly wage (2010 Euros):				
Low-skilled	2,221	2,429	2,474	2,319
Medium-skilled	2,702	2,926	3,097	3,009
High-skilled	4,491	4,767	5,080	5,033
Std. Dev. log real wages	0.41	0.43	0.45	0.51
Person × spells in year	332,702	371,798	364,347	372,580
Unique individuals	288,358	315,386	288,219	267,028
Panel B. Supply Sample (All)				
Age	38.7	38.2	39.6	40.9
Young (≤ 30 years)	0.29	0.32	0.22	0.21
Female	0.38	0.40	0.46	0.48
German	0.90	0.90	0.87	0.85
Shares:				
Low-skilled	0.26	0.17	0.14	0.13
Medium-skilled	0.70	0.76	0.76	0.75
High-skilled	0.05	0.07	0.10	0.12
Full-time	0.87	0.82	0.67	0.63
Long part-time	0.07	0.09	0.11	0.14
Short part-time	0.02	0.02	0.12	0.15
Vocational/other	0.01	0.02	0.02	0.02
Unemployed	0.03	0.05	0.07	0.06
Person × spells in year	499,280	597,012	853,688	992,925
Unique individuals	382,555	443,838	521,000	531,851

Notes: This table presents summary statistics for the premium and supply data sets. The wage sample consists of full-time employed German individuals aged 21-60 living in West-Germany. Individuals working in West-German who are non-German and/or were first registered in East Germany are excluded. The supply sample consists of full-time, part-time, vocational training, and unemployment spells of all individuals including non Germans and East-West movers. All summary statistics are weighted by spell length.

ployment insurance benefits was just 3% in the 1980s, it more than doubled by the end of the sample period.

2.3.5 Graphical Analysis

Figure 2.3 plots the evolution of our key variables separately for young and old workers using comparable scales.[16] In the top left part, we plot the medium to low skill premiums of young and old workers. While the premium for old medium-skilled workers changed only little over the 1980-2008 period (from 0.23 in 1980 to 0.26 in 2008), the premium of young medium-skilled workers more than doubled over the same period (from 0.11 in the mid 1980s to 0.25 in the 2000s).[17] To put these numbers in perspective, according to Goldin and Katz (2009, Figure I, p. 27) the combined premium of young and old high school graduates in the US (relative to those who only stayed in school until 8th grade) increased from 0.23 in 1980 to 0.29 in 2005. Thus, our medium skill premium is similar in magnitude to the US high school premium.[18]

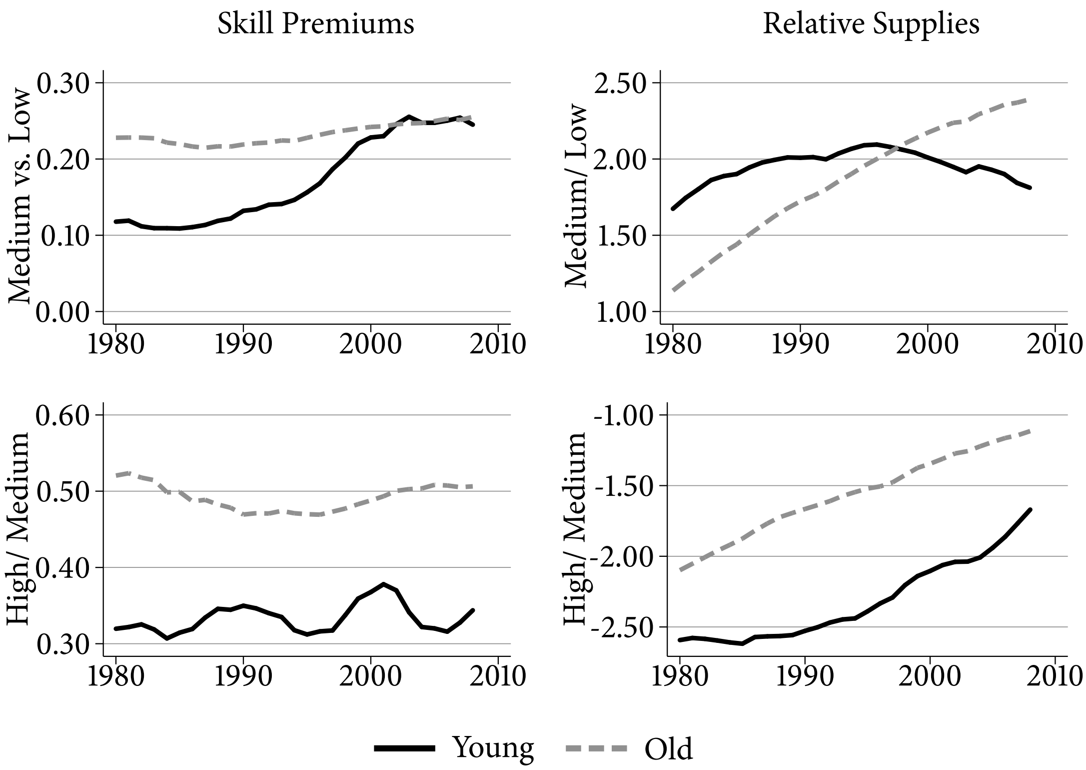
The development of the high-skilled or college premium is depicted in the bottom left part of Figure 2.3. The young high-skilled saw their premium fluctuating around 0.33 with considerable variation while the college premium of old workers followed a soft U-shape pattern starting from 0.52 in 1980, reaching a low of 0.47 during the 1990s to eventually increase to 0.51 in 2008. Since skills premiums are partly based on imputed wages (in particular the high to medium premium of old workers), one might be worried about how accurately they really represent the *actual* high-skilled premiums. In Appendix B.6, we show that there is no systematic divergence over time between the 85th-percentile in our data (which is always uncensored) and various top income fractiles taken from the from the *World Top Incomes Database* (WTID, Alvaredo et al. 2017). Theses comparisons

[16] Figure B.3 shows the evolution of the medium and high skill premium separately for eight different age groups. It shows that those aged above 30 (or 36) and below follow a similar pattern.

[17] These patterns in the skill premiums are also prevalent when looking at men and women separately (see Figures B.4 and B.5). They are somewhat less pronounced for women and more pronounced for men. In both series, the medium premium of young workers has more than doubled over 1980-2008 and has increased much faster than that of old workers. Out main findings also hold when we only use skill premiums of men (see Table 2.5, model 3).

[18] The combined medium premium of young and old workers in Germany increased from 0.19 in 1980 to 0.23 in 2005, see Figure 2.1.

Figure 2.3: Skill Premiums and Relative Supplies



Notes: This figure plots on the left hand side the difference in composition constant mean log earnings between medium- and low-skilled workers (upper left) and high- and medium-skilled workers (bottom left) who work full-time, live in West-Germany and have not moved from East to West-Germany, separately for the young (30 years or below) and old (above 30 years) between 1980-2008. The right hand side depicts the corresponding difference in log supplies in efficiency units of all workers in West-Germany including full-time, part-time, unemployment and vocational training spells but excluding marginal part-time spells. For more details see sections 2.3.3.

make us confident that the skill premiums derived from right-censored SIAB data are indeed representative for the true evolution of the earnings gap between high- and medium-skilled workers.

Our core hypothesis is that differential changes in the supplies of different skill groups are responsible for the observed patterns in skill premiums. To illustrate this, in the right column of Figure 2.3, we plot the relative supplies of medium-skilled (to low-skilled) and high-skilled (to medium-skilled) labor separately for young and old workers. Starting with the top right panel, we see that the relative supply of old medium-skilled workers increased by a factor of 2.5 in an almost linear fashion. In contrast, the relative supply of young medium-skilled increased by some 0.4 log points up to the 1990s, stayed constant and then decreased by 0.2 log points in the 2000s. The relative supply of old high-skilled workers – similar to the old medium-skilled – increased linearly from 1980-2008 while the relative supply of young high-skilled workers increased exponentially.

These figures in combination with the scatter plots presented in Figure 2.2 suggest that wage differentials between different skill groups are systematically related to their relative supplies. In the next section, we will use our analytical framework detailed above to investigate this relationship more rigorously.

2.4 Empirical Estimation

2.4.1 General Estimation Approach and Standard Errors

We now turn to the estimation of the model outlined in section 2.2 using the skill premiums and efficiency labor supplies introduced in section 2.3. We will estimate the model's parameters from bottom to top in three steps: First, using the premium equations 2.5 and 2.7, we will estimate σ_a (the elasticity of substitution between young and old workers) and the efficiency parameters between theses two groups, α_s. With these parameters at hand, we construct the aggregate amounts of L_t, M_t and H_t. Second, using L_t and M_t, we estimate σ_{ml} (the elasticity of substitution between medium- and low-skilled workers) and θ_t (the technology parameter shifting the demand for medium- relative to low-skilled workers) which are needed to construct U_t (the aggregate amount of non-high skilled labor). Finally, in the third step, using the aggregate amounts of the various skill types, we estimate σ_{hu} (the elasticity of substitution between college and non-college labor). This final step yields estimates for the parameters estimated in the previous steps and can thus serve as a consistency check.

Identification of our parameters of interest relies on labor supplies to be *predetermined*, i.e. that labor supplies must not be correlated with any other unobservables that also determine skill premiums and that premiums and supplies are not determined simultaneously. For two reasons we think this assumption is tenable. First, labor supplies are inelastic in the short run and are the result of past human capital investments. Thus, although an individual might invest in vocational training or college education when observing a high premium, skill supplies will only increase with a substantial lag. Second, our labor supply measures are very broad, i.e. they do not only include full-time workers, but also those who work part-time, complete vocational training, or are unemployed. Thus, our supplies capture virtually the entire labor force subject to social security and are considerably less sensitive to changes along the intensive margin (e.g. people might be more likely to work full-time when premiums are high).[19] Still, if labor supplies reacted contemporaneously to skill premiums, this would lead to an underestimation of the negative relationship between premiums and supplies. In that case, different groups of workers would appear more substitutable than they really are, i.e. our substitution elasticities would represent upper bounds.

To compute standard errors, we rely on a moving block bootstrap approach.[20] Bootstrapping standard errors is necessary for at least three reasons. First, the three-step estimation procedure implies that we rely on generated regressors in steps 2 and 3, so we need to take into account the estimation uncertainty induced by the previous step(s). Second, the theoretical model implies that skill premiums at one point in time depend on both, the supply of young and old workers of two adjacent skill groups and thus the two skill premiums are by construction correlated with each other. Third, premiums are serially correlated over time.[21] Thus, the error terms of the premium equations we are going to estimate are correlated contemporaneously across equations as well as serially over time.[22] The moving block bootstrap is a

[19] Fitzenberger et al. (2006) follow a similar approach and use broad measures of skill supplies derived from microcensus data to instrument labor supplies.

[20] The overlapping block bootstrap for time series was first introduced by Kunsch (1989). See Horowitz (2001, 3188ff) for an overview of different bootstrap methods for dependent data.

[21] A simple Wooldridge (2002, ch. 10) test for serial correlation in panel data detects serial correlation in both premium equations.

[22] There is also sampling uncertainty related to the estimation of premiums and supplies. However, given the very large number of observations and the corresponding extremely tight confidence intervals, this uncertainty contributes very little to the overall uncertainty related to our estimations and we will abstract from it in what follows. For similar reasons, we also decided to ignore the uncertainty induced by imputing top coded wages. Effectively, we thus take premiums and supplies as given.

way to account for these various types of uncertainty.[23] It divides observations in $n - b + 1$ blocks or clusters, where b indicates block length. Thus, the first block jointly contains all premiums and supplies of low, medium, and high-skilled workers of both age groups from year 1 through b, the next all observations from year 2 through $b + 1$, and so on. Each bootstrap sample is constructed by drawing (with replacement) k blocks such that the number of observations contained in these k blocks is less than or equal to the number of observations in the corresponding full sample.[24] This bootstrapping procedure is supposed to resemble the underlying data generating process and allows error terms in a given block to be arbitrarily correlated with each other across and over time. The choice of b should mimic the serial correlation of the error terms. Following the suggestions of Hall et al. (1995) we choose $b = 3$.[25] Hence, when using the full sample period 1980-2008, each bootstrap sample consists of $k = 9$ randomly drawn blocks of length $b = 3$ resulting in $n_{bs} = 54$ observations, 4 less than when using the full data where $n = 58$.

Since our parameters of interest (e.g. $-\frac{1}{\sigma_a}$) are non-smooth functions of estimated parameters (discontinuous at zero), they cannot be bootstrapped directly. Therefore, the standard errors of the parameters of interest are calculated using the delta method. We use 500 repetitions for all bootstraps. Whenever we estimate two premium equations jointly, we use a seemingly unrelated regression framework to account for error correlations across equations and to impose parameter constraints across equations.

Previous related work did not consider the various sources of uncertainty in computing standard errors. For instance, Card and Lemieux (2001) and Goldin and Katz (2009) estimate similar frameworks as ours but only report conventional standard errors. D'Amuri et al. (2010) also estimate a similar framework to study the impact of immigration to West Germany over the period 1987-2001. They

[23] Lahiri (1999) compares different block bootstrap methods and finds that in terms of asymptotic efficiency, the block bootstrap (fixed block length) performs better than the stationary bootstrap (random block length). Furthermore, Hall et al. (1995) show that overlapping blocks (as we use here) provide somewhat higher efficiency than non-overlapping ones (but that the efficiency difference in likely to be small in practical applications).

[24] Formally, choose $k = \lfloor (t_{end} - t_0 + 1)/b \rfloor$ such that $n_{bs} = 2(k \times b) \leq n$ and where the 2 is coming from the two groups (young, old).

[25] We also used a more conservative block length of 5 and all results remained significant at least at the 10%-level.

cluster standard errors at the education-experience level even when estimating the elasticity of substitution between different skill groups and thus ignore the potential correlation between education and experience groups. A comparison between different standard errors in our setting shows that standard errors obtained from a moving block bootstrap are up to five times larger than conventional standard errors obtained from a seemingly unrelated regression using a small sample adjustment. Thus, using block bootstrapped standard errors is crucial for correct inference in our setting.

2.4.2 Estimating σ_a

We apply our simple model setting $j = \{\text{young} \leq 30, \text{old} > 30 \text{ years}\}$ for the period 1980-2008 using composition constant skill premiums and efficiency skill supplies as described above. To estimate the elasticity of substitution between young and old workers, σ_a, we absorb the first two terms of equation 2.5 and the first three of equation 2.7 with a linear time trend or time fixed effects, and the terms containing the α's by age group fixed effects. This yields the following estimation equations which allow us to recover the σ_a's as $\beta_a = -\frac{1}{\sigma_a}$:

$$\omega_{jt}^{M} = \text{time}_t^{ML} + \text{age}_j^{ML} + \beta_a \ln\left(\frac{M_{jt}}{L_{jt}}\right) + \varepsilon_{jt}^{ML} \tag{2.9}$$

$$\omega_{jt}^{H} = \text{time}_t^{HM} + \text{age}_j^{HM} + \beta_a \ln\left(\frac{H_{jt}}{M_{jt}}\right) + \varepsilon_{jt}^{HM} \tag{2.10}$$

As mentioned above, we estimate the two premium equations jointly in a seemingly unrelated regression framework to account for possible correlation of the error terms ε_{jt}^{ML} and ε_{jt}^{HM} across equations. In Table 2.2, we present three different models where in each model we restrict the elasticity of substitution between the two age groups to be the same across the three skill groups. Model 1 assumes linear time trends for time_t^s. This relatively simple model already fits the data very well with an R^2 above 0.95 for both premium equations. Model 2 allows for more flexibility by including time dummies for each year. The parameter of interest β_a increases slightly (in absolute terms) compared to the simple linear trend model. In model 3, we only use the years 1980-90 with a linear time trend as a kind of *pseudo out-*

of-sample exercise. Reassuringly, the parameter of interest changes very little. Our preferred estimate of model 2 corresponds to an elasticity of substitution between young and old workers of 8.2, which is somewhat higher than the comparable estimates by Card and Lemieux (2001) of around 5 for the US and 6 for Canada.[26]

In section B.7 in the Appendix, we allow σ_a to differ across skill groups. According to this more flexible approach young and old workers are found to be closer substitutes within the group of low skill workers (σ_{al} = 14.7) than in the groups of medium and high skill workers (in both σ_a is about 7). For the sake of simplicity and since equality of σ_{al}, σ_{am}, and σ_{ah} cannot be rejected statistically, we will continue to assume a common σ_a across all skill groups in the following sections.

To estimate σ_{ml} in the next step, we also need to estimate the efficiency parameters α_s. Section B.8 in the appendix contains the details related to this step. The estimated α_s do not differ by much whether we assume σ_a's to be constant across skill groups or not and suggest that one unit of young low skilled labor is about 73-78% as efficient as one unit of old low-skilled labor while the corresponding ratios are 68-69% for medium-skilled and 52-54% for high-skilled labor. The different efficiency ratios are consistent with the different age earnings profiles of the three skill groups that are much steeper for high-skilled workers than for medium- or low-skilled workers.

2.4.3 Estimating σ_{ml}

To estimate the elasticity of substitution between the aggregate amounts of low- and medium-skilled labor corresponding to equation 2.1, we construct the aggregate amounts of L_t, M_t (and H_t for later) using a model where we restrict the elasticity of substitution between age groups to be the same across skill groups and which includes time fixed effects.[27] We then estimate variants of the following equation (note that ω is not indexed by j and thus refers to the *aggregate* medium skill

[26] Card and Lemieux (2001) use 7 different age groups in 5-year intervals instead of only 2 as in our models. Estimates are similar to the ones presented in table 2.2 (yielding a slightly higher σ_a) if we use 8 different 5-year interval age groups or if we re-define young as 35 years and younger.

[27] That is, we use σ_a from model 2 of Table 2.2 and the α_s from model 1 of Table B.4. All subsequent estimates remain virtually identical when we use alternative parameters from models including a linear time trend only or when allowing the σ_a's to vary flexibly across skill groups.

Table 2.2: Estimating the Elasticity between Young and Old Workers σ_a (*Constant Across Skill Groups*)

	(1) Linear Trend (1980–2008)		(2) Time FEs (1980–2008)		(3) Linear Trend (1980–1990)	
	ω^M_{jt}	ω^H_{jt}	ω^M_{jt}	ω^H_{jt}	ω^M_{jt}	ω^H_{jt}
Age Group Specific Relative Supply	-0.112***	-0.112***	-0.123***	-0.123***	-0.129**	-0.129**
	(0.013)	(0.013)	(0.011)	(0.011)	(0.050)	(0.050)
Young	-0.051***	-0.243***	-0.050***	-0.250***	-0.047**	-0.262***
	(0.004)	(0.011)	(0.004)	(0.008)	(0.022)	(0.031)
Time	0.007***	0.004***			0.006*	0.002
	(0.001)	(0.001)			(0.003)	(0.004)
Constant	0.343***	0.256***	0.370***	0.258***	0.373***	0.243**
	(0.017)	(0.030)	(0.022)	(0.024)	(0.055)	(0.118)
Time FEs			✓	✓		
σ_a	8.9	8.9	8.2	8.2	7.7	7.7
	(1.1)	(1.1)	(0.8)	(0.8)	(3.0)	(3.0)
Observations	58	58	58	58	22	22
R^2	0.958	0.952	0.990	0.984	0.997	0.987

Notes: The coefficients of the age group specific relative supplies, $\ln(M_{jt}/L_{jt})$ and $\ln(H_{jt}/M_{jt})$, are restricted to be the same in each model's pair of equations, i.e. by assumption $\sigma_{al} = \sigma_{am} = \sigma_{ah}$. Estimates are obtained using a two-step seemingly unrelated regression framework. The number of observations refers to the full sample, n. Young is an indicator for age ≤ 30 years. Moving block bootstrap standard errors with block length 3 and 500 replications in parentheses. ***/**/* indicate significance at the 1%/5%/10% level.

premium):

$$\omega_t^M = \ln\theta_t - \frac{1}{\sigma_{ml}}\ln\left(\frac{M_t}{L_t}\right).$$

In column 1 of Table 2.3, we regress the medium- to low-skilled premium on the aggregate relative supply of medium- to low-skilled labor $\ln\frac{M_t}{L_t}$ and a linear time trend. This model has a comparatively poor fit and the coefficient of the relative medium to low supply is imprecisely estimated. In column 2, we exclude all years after 1990 and do a pseudo-out-of-sample prediction which is visualized in Figure

Table 2.3: Estimating the Elasticity between Medium- and Low-skilled Labor σ_{ml}

	(1) Simple 1980-2008 ω_t^M	(2) Simple 1980-1990 ω_t^M	(3) Simple 1980-2001 ω_t^M	(4) Trend Break 2002 ω_t^M	(5) Full Trend Break 2002 ω_t^M
Aggr. Medium/ Low Rel. Supply	-0.103	-0.275	-0.267***	-0.262***	-0.259***
	(0.090)	(0.169)	(0.061)	(0.060)	(0.064)
Aggr. Medium/ Low Rel. Supply × Post 2002					0.001
					(0.002)
Time	0.006*	0.014	0.013***	0.013***	0.013***
	(0.003)	(0.010)	(0.002)	(0.002)	(0.002)
Time × Post 2002				-0.008***	-0.008***
				(0.001)	(0.002)
Constant	0.306**	0.520***	0.510***	0.502***	0.498***
	(0.122)	(0.199)	(0.080)	(0.080)	(0.086)
σ_{ml}	9.7	3.6	3.7	3.8	3.9
	(8.5)	(2.2)	(0.9)	(0.9)	(1.0)
Observations	29	11	22	29	29
R^2	0.902	0.850	0.966	0.983	0.983

Notes: This table presents regressions results of the aggregate medium skill premium ω_t^M on the aggregate relative supply of medium- to low-skilled workers $\ln\left({}^{M_t}/_{L_t}\right)$. M_t and L_t are constructed using the σ_a obtained from a corresponding estimation sample in step 1 where the elasticity of substitution between young and old workers is restricted to be the same across all three skill groups using time FEs (model 2 of Table 2.2. The number of observations refers to the full sample, n. Young is an indicator for age ≤ 30 years. Moving block bootstrap standard errors with block length 3 and 500 replications in parentheses. ***/**/* indicate significance at the 1%/5%/10% level.

B.6. This model predicts the medium skill premium for the years 1991–2001 very well, but does a poor job from 2002 onwards. Actual premiums in 2002–2008 are much lower than predicted. In column 3, we exclude the years 2002-2008. The estimates become highly significant and are very similar in magnitude to those in column 2. To account for the different regimes, in column 4 we allow for a trend break in the demand for medium- relative to low-skilled labor in 2002.[28] This

[28] A formal structural break test (Quandt-LR) confirms 2002 as the break year.

improves the model fit significantly and yields a highly significant point estimate for the relative supply of -0.262, very similar to the point estimates in columns 2 and 3. The estimates in column 4 imply a substantially decelerated growth in the medium to low premium after 2002 (the combined demand trend is 62% lower than before 2002). Finally, in column 5, we also allow the substitution elasticity to change in 2002 but find no evidence that this parameter has changed after 2001.

The observed pattern of a decreased relative demand for medium workers after 2001 are consistent with increasing polarization at the beginning of the 2000s along the lines of Autor and Dorn (2014) implying a decreasing medium to low premium due to increasing computerization of medium-skilled tasks and a relative increase in low-skilled wages. It could also be related to the implementation of the Hartz reforms in 2003 (coupled with some anticipation effects). For instance, Launov and Wälde (2013) find that the Hartz reforms had a more adverse effect on medium-skilled workers: while *increasing* benefits and thus reservation wages for most low-skilled workers, the reforms *decreased* reservations wages for medium-skilled workers. Furthermore, Hirsch and Schnabel (2014) find a marked drop in union power at the beginning of the 2000s which could further explain decreasing wages of medium-skilled workers as coverage rates were particularly high among this group of workers (Kohaut and Schnabel 2003).[29]

Our preferred specification 4 implies a σ_{ml} of 3.8 which is somewhat lower than the elasticity of substitution between high school graduates and high school dropouts in the US of about 5.3 (for the post 1949-period) estimated by Goldin and Katz (2009, Table 8.4). Arguably, high school graduates and high school dropouts are closer substitutes than those with a completed vocational training specialized in a specific occupation and those without such a training holding at most a general schooling degree (at most *Realschule*). Our estimate of σ_{ml} is also lower than the estimate of around 5 obtained by Dustmann et al. (2009, Table V) for Germany who, however, only consider men during the period 1975-2004.

[29] Burda and Seele (2016) find a pronounced positive effect of the Hartz reforms on labor supply. Our labor supply measures, however, do not show a significant change in *relative* labor supplies during this period (compare Figure 2.3). One reason is that our supply measures also include the unemployed and much of the change in labor supplies due to the Hartz reforms occurred as a shift from unemployment to part-time employment (Burda and Seele 2016).

2.4.4 Estimating σ_{hu} and the Full Model

Using the estimates of the previous step, we can now construct U_t, the aggregate amount of non-high skilled (or non-college) labor.[30] Using equations 2.6 and 2.8, we can then estimate σ_{hu}, the elasticity of substitution between college and non-college workers and, at the same time, assess the ability of the overall model to explain the differential evolution of the skill premiums of the different skill and age groups – the primary interest of this paper.

In Table 2.4, we jointly estimate the medium to low and high to medium skill premiums for each age group in a seemingly unrelated regression framework as before, this time using equations 2.6 and 2.8. These equations state that the age-specific skill premiums do not only depend on the corresponding age-specific relative labor supplies ($\ln \frac{M_{jt}}{L_{jt}}$ for the medium to low premium and $\ln \frac{H_{jt}}{M_{jt}}$ for the high to medium premium) but also on the *aggregate* relative supplies ($\ln \frac{M_t}{L_t}$ and $\ln \frac{H_t}{M_t}$, respectively). Equation 2.8 also implies that the age-specific high to medium premium depends on the aggregate relative supplies of high to non-high and non-high to medium labor. The coefficients on these aggregate supplies yield an estimate for the elasticity of substitution between high and non-high (σ_{hu}) and medium- to low-skilled (σ_{ml}) labor, respectively. In the following, we impose equality of the coefficients on the age-specific supplies of medium to high and high to medium labor (implying the same elasticity of substitution between young and old workers across all three skill groups, σ_a) and of the aggregate medium to low and non-high to medium supply (thus yielding the same σ_{ml} in both equations) as implied by equations 2.6 and 2.8.

For the medium to low premium we allow for a trend break in 2002 as before. The technology-related parameter corresponding to the high to medium premium in model 1 of Table 2.4 is assumed to follow a linear trend throughout the whole sample period representing a linear shift in the demand for high-skilled workers. The estimates of model 1 yield a coefficient of -0.120 for the age group specific

[30] To construct the aggregate amount of non-college labor U_t we use the estimates of model 4 of Table 2.3. Apart from σ_{ml} we also need an estimate for the demand shifter θ_t which is recovered from the estimated coefficients as $\widehat{\theta}_t = \frac{\exp(B)}{1+\exp(B)}$ where $B = \widehat{\beta}_{time} \times time + \beta_{posttime} \times posttime$, where $posttime$ is 0 in the years before the break year, 1 in the break year and increasing by one in each subsequent year after the break year.

Table 2.4: Estimating the Elasticity between High- and Non-High-Skilled Labor σ_{hu} and the Full Model

	(1) Baseline		(2) High Young Intercepts		(3) 1980-1990 only	
	ω_{jt}^M	ω_{jt}^H	ω_{jt}^M	ω_{jt}^H	ω_{jt}^M	ω_{jt}^H
Aggr. Medium/ Low Rel. Supply	-0.223***	-0.223***	-0.266***	-0.266***	-0.237	-0.237
	(0.085)	(0.085)	(0.086)	(0.086)	(0.167)	(0.167)
Aggr. High/ Non-High Rel. Supply		-0.250		-0.611**		-0.201
		(0.319)		(0.305)		(0.372)
Age Group Specific Rel. Supplies	-0.120***	-0.120***	-0.121***	-0.121***	-0.097***	-0.097***
	(0.010)	(0.010)	(0.013)	(0.013)	(0.027)	(0.027)
Young	-0.050***	-0.248***	-0.050***	-0.261***	-0.062***	-0.252***
	(0.003)	(0.008)	(0.004)	(0.009)	(0.012)	(0.017)
Time	0.012***	0.009	0.014***	0.023*	0.012	0.003
	(0.003)	(0.012)	(0.003)	(0.012)	(0.010)	(0.018)
Time × Post 2002	-0.008***		-0.009***			
	(0.002)		(0.002)			
Constant	0.478***	-0.041	0.533***	-0.689	0.503***	0.068
	(0.117)	(0.576)	(0.118)	(0.550)	(0.194)	(0.743)
Young $\times I(1987-1990)$				✓		✓
Young $\times I(1999-2002)$				✓		
σ_{ml}	4.5		3.8		4.2	
	(1.7)		(1.2)		(3.0)	
σ_{hu}		4.0		1.6		5.0
		(5.1)		(0.8)		(9.2)
σ_a	8.4	8.4	8.2	8.2	10.3	10.3
	(0.7)	(0.7)	(0.8)	(0.8)	(2.8)	(2.8)
Observations	58	58	58	58	22	22
R^2	0.980	0.948	0.982	0.973	0.998	0.996

Notes: The coefficients on the aggregate relative supply of medium- to low-skilled workers $\ln\left({}^{M_t}/{}_{L_t}\right)$ and the aggregate relative supply of non-high to medium-skilled workers $\ln\left({}^{U_t}/{}_{M_t}\right)$, i.e. σ_{ml}, as well as the coefficients on the age group specific supplies (i.e. σ_a) are restricted to be the same in each model's pair of equations. The number of observations refers to the full sample, n. Young is an indicator for age ≤ 30 years. Moving block bootstrap standard errors with block length 3 and 500 replications in parentheses. ***/**/* indicate significance at the 1%/5%/10% level.

relative supply which is almost identical to the corresponding estimate for the elasticity of substitution between young and old workers obtained in column 2 of Table 2.2 (-0.123). Thus, concerning σ_a the estimates based on equations 2.6 and 2.8 are consistent. However, model 1 yields a coefficient of the aggregate medium to low supply of -0.223 which is somewhat different than the corresponding estimate of Table 2.3 of -0.262. According to the model, these two estimates should be the same. The point estimate of the aggregate high to non-high supply is -0.250 but is imprecisely estimated. These discrepancies suggest that the model – in particular the specification for the high to medium premium – might be misspecified. In particular the high to medium premium of *young* workers exhibits "bumps" that are unrelated to supply changes.[31] As it turns out, the wages and thus the premium of young high-skilled workers show a strong co-movement with the business cycle (see Figure B.8) – something that to this extent cannot be observed for the remaining three premiums. In particular, the premium of young high-skilled workers is amplified and detached from its underlying supply during the pre-unification boom (1987-1990) and the boom and bust of the dot-com bubble (1999-2002, Burda and Seele 2016, p. 5).

Therefore, in model 2, we include two separate intercepts for these two periods interacted with the young indicator to account for the two biggest "bumps" in the high to medium premium of young workers. The coefficient on the aggregate medium to low supply now changes to -0.266 and is thus very close to the corresponding estimate in Table 2.3 as it should be. The coefficient on the aggregate amount of high to non-high labor changes to -0.611 and becomes significant.[32]

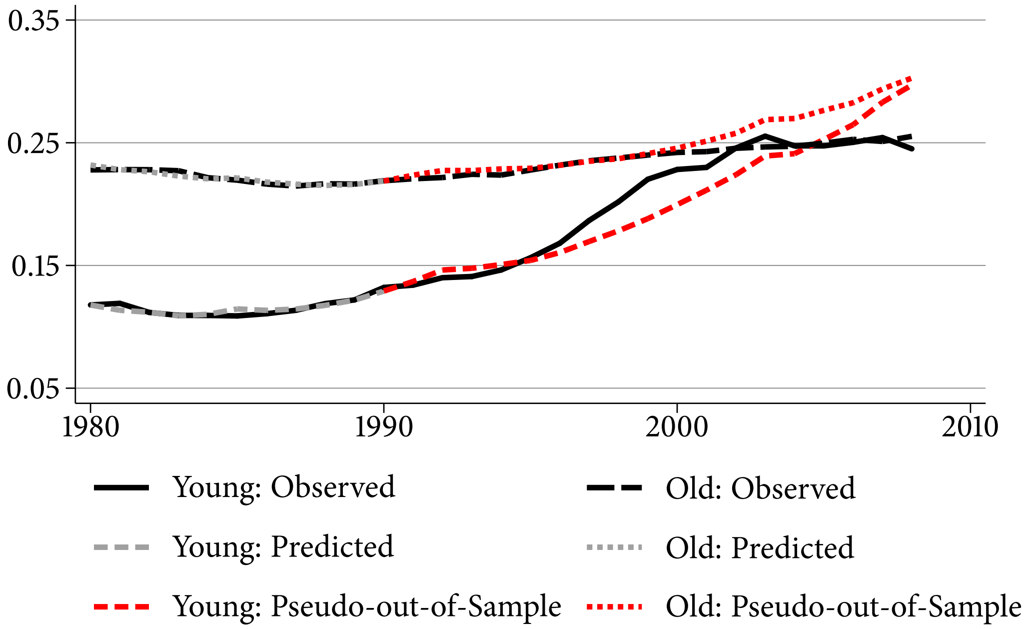
The estimates of our preferred specification (model 2) imply an elasticity of substitution between college and non-college labor of 1.6. This happens to be identical to the elasticity of substitution between college and high school labor in the US estimated both by Goldin and Katz (2009, their Table 8.2) and Card and Lemieux (2001, Table VI). D'Amuri et al. (2010) and Fitzenberger et al. (2006) both do not estimate σ_{ml} and σ_{hu} separately but impose equality of these two parameters in their estimations (i.e. they assume that the elasticity of substitution is the same between, say, high- and low-skilled labor and high- and medium-skilled labor).

[31] As shown in the appendix, these bumps are not a peculiarity of the SIAB data (e.g. due to censoring) as similar patterns can be observed in the (virtually uncensored) microcensus (see Figure B.7)

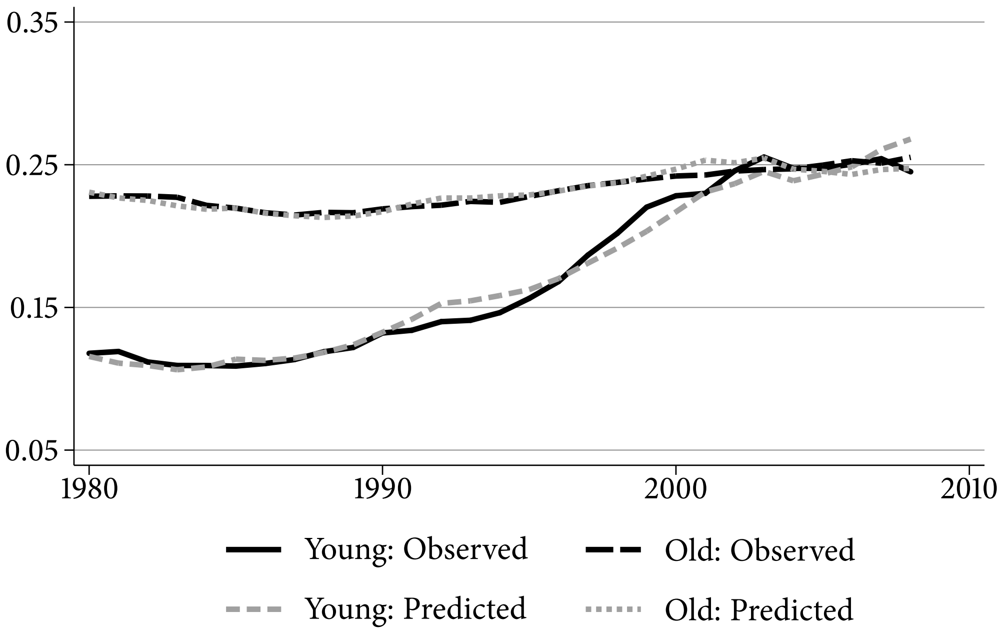
[32] Including other sets of interacted intercepts or dummies in the high to medium specification leads to no major changes of the estimates.

Figure 2.4: Predicted vs. Observed Medium Premiums

(a) Pseudo-out-of-Sample Prediction after 1990 (Model 3 of Table 2.4)



(b) Prediction based on Full Sample 1980-2008 (Model 2 of Table 2.4)



This simplifying assumption is not supported by our estimation results, i.e. σ_{ml} and σ_{hu} are significantly different from each other. Bearing that in mind, D'Amuri et al. (2010, Table 7 column 3 and 4) estimate an elasticity of substitution between any two skill groups of 2.9 which is right between our corresponding elasticities of 3.8 (σ_{ml}) and 1.6 (σ_{hu}). Fitzenberger et al. (2006, Table 1) estimate a σ_s between 4.9 and 6.9 (their preferred IV estimates) and note that their estimates "imply a rather high degree of substitutability compared to findings in the related literature".

To get an impression of the model's out-of-sample predictive power, we plot the observed and the predicted medium to low premium separately for young and old workers in Figure 2.4. The prediction in panel a) is based on the estimates of model 3 in Table 2.4 where we exclude all years after 1990. Although we lose statistical power due to the smaller sample size, the coefficients related to the medium to low and the age group specific supply measures remain comparable in magnitude. The figure shows that the model based on only the observations from 1980–1990 is able to predict the differential evolution of the medium to low premium of young and old workers during the 1990s up until the early 2000s. In panel b), we use the estimates of model 2 and the prediction is very close to the observed premium. The model is also able to predict the high skill premium reasonably well – even without accounting for the peculiarities in the premium of young college graduates (Figure B.9).

2.4.5 Robustness Checks

How robust are our estimates regarding the construction of premiums and supplies? We compare alternative premium and supply measures to our preferred estimates from above which are restated in model 1 of Table 2.5.

Premiums do not only depend on supplies but are likely also influenced by the business cycle. To capture fluctuations around the underlying longer-term trends, we include GDP growth in model 2 (and also in step 2). This leaves our estimates basically unchanged and GDP growth turns out insignificant in both premium equations.

So far, we used composition constant skill premiums that included both men and women. In model 3, we compute wage premiums of men only (holding their age composition constant as before) and re-do our previous estimation steps. Using premiums of men only yields similar results with a somewhat lower degree of

Table 2.5: Robustness Checks of CES Models

	(1) Preferred Specification		(2) Baseline + GDP Growth		(3) Premiums of Men		(4) Weighting $w_{voc} = w_{ue} = 1$		(5) Supplies excl. Voc. Training & Unemployed		(6) Supplies as Head Count	
	ω_{jt}^M	ω_{jt}^H	ω_{jt}^M	ω_{jt}^H	ω_{jt}^M	ω_{jt}^H	ω_{jt}^M	ω_{jt}^H	ω_{jt}^M	ω_{jt}^H	ω_{jt}^M	ω_{jt}^H
Aggr. Medium/ Low Rel. Supply	-0.266*** (0.086)	-0.266*** (0.086)	-0.277*** (0.089)	-0.277*** (0.089)	-0.334*** (0.114)	-0.334*** (0.114)	-0.307*** (0.078)	-0.307*** (0.078)	-0.218** (0.091)	-0.218** (0.091)	-0.188*** (0.057)	-0.188*** (0.057)
Aggr. High/ Non-High Rel. Supply		-0.611** (0.305)		-0.612** (0.297)		-0.569* (0.318)		-0.626** (0.287)		-0.547* (0.289)		-0.336*** (0.112)
Age Group Specific Rel. Supplies	-0.121*** (0.013)	-0.121*** (0.013)	-0.121*** (0.012)	-0.121*** (0.012)	-0.132*** (0.018)	-0.132*** (0.018)	-0.111*** (0.012)	-0.111*** (0.012)	-0.132*** (0.014)	-0.132*** (0.014)	-0.105*** (0.010)	-0.105*** (0.010)
Young	-0.050*** (0.004)	-0.261*** (0.009)	-0.050*** (0.004)	-0.261*** (0.009)	-0.009 (0.006)	-0.239*** (0.014)	-0.068*** (0.003)	-0.254*** (0.009)	-0.034*** (0.005)	-0.268*** (0.010)	-0.070*** (0.003)	-0.224*** (0.007)
Time	0.014*** (0.003)	0.023* (0.012)	0.014*** (0.003)	0.023** (0.012)	0.017*** (0.004)	0.022* (0.012)	0.014*** (0.003)	0.023** (0.011)	0.012*** (0.004)	0.021* (0.011)	0.008*** (0.001)	0.011*** (0.004)
Time × Post 2002	-0.009*** (0.002)		-0.009*** (0.002)		-0.010*** (0.003)		-0.010*** (0.002)		-0.007*** (0.002)		-0.007** (0.003)	
Real GDP Growth			0.047 (0.077)	-0.054 (0.146)								
Constant	0.533*** (0.118)	-0.689 (0.550)	0.545*** (0.120)	-0.694 (0.535)	0.560*** (0.155)	-0.684 (0.583)	0.584*** (0.105)	-0.744 (0.523)	0.470*** (0.126)	-0.547 (0.522)	0.393*** (0.066)	-0.374 (0.279)
Young $\times I(1987-1990)$		✓		✓		✓		✓		✓		✓
Young $\times I(1999-2002)$		✓		✓		✓		✓		✓		✓
σ_{ml}	3.8 (1.2)		3.6 (1.2)		3.0 (1.0)		3.3 (0.8)		4.6 (1.9)		5.3 (1.6)	
σ_{hu}		1.6 (0.8)		1.6 (0.8)		1.8 (1.0)		1.6 (0.7)		1.8 (1.0)		3.0 (1.0)
σ_a	8.2 (0.8)	8.2 (0.8)	8.2 (0.8)	8.2 (0.8)	7.5 (1.0)	7.5 (1.0)	9.0 (1.0)	9.0 (1.0)	7.6 (0.8)	7.6 (0.8)	9.5 (0.9)	9.5 (0.9)
Observations	58	58	58	58	58	58	58	58	58	58	58	58
R^2	0.982	0.973	0.982	0.973	0.960	0.948	0.985	0.973	0.976	0.972	0.980	0.983

Notes: See notes for Table 2.4.

substitutability between medium- and low-skilled workers and a slightly higher one between college and non-college labor.

A possible concern is that our results depend on the specific weighting scheme used to construct the efficiency supplies. In particular, we assigned a "spell type weight" of 1/3 to vocational training and unemployment spells which we think is a reasonable assumption. One could argue, however, that these two groups of workers are (in their great majority) willing to work full-time and thus should be assigned a spell type weight of 1. This is what we do in model 4. Re-weighting of this kind makes the estimates slightly more pronounced but the differences to the estimates in model 1 are small. Thus, our results are not driven by the particular weighting scheme (we experimented with other weighting schemes as well and results remain robust). The same is true when we completely exclude vocational training and unemployment spells from our efficiency supply measures (model 5). σ_{ml} and σ_{hu} increase slightly likely because the degree of substitutability in the group of those working full- or part-time is higher than in the group that also includes those in vocational training and currently unemployed.

When constructing supplies not based on efficiency units, i.e. not taking productivity differences into account, but rather do a simple head count (model 6, but still weighted by spell duration) similar to the approach followed by D'Amuri et al. (2010) the estimates are more attenuated towards zero but the overall patterns continue to hold.

2.5 Determinants of Supply Changes

After having demonstrated that the heterogeneous evolution of age-specific skill premiums depicted in Figure 2.3 can be readily explained by a relatively simple supply and demand framework, we now turn to the potential reasons for the underlying age and education specific changes in labor supply. We assess the importance of two main potential explanations. First, we look at the role of immigration. The relative decrease in the supply of medium-skilled workers, in particular among young workers, could be driven by a large inflow of mainly low-skilled migrants after the fall of the Berlin Wall as hypothesized, for instance, by Dustmann et al. (2009). To evaluate the effect of migration, we compute supply measures excluding migrants and simulate the counterfactual evolution of skill premiums under this "no-immigration" scenario. Second, we investigate the role of more fundamental shifts in the educational attainment of native Germans. To assess this alternative channel, we perform a cohort analysis based on the German microcensus to understand the dynamics of educational attainment of the native West German population.

2.5.1 The Role of Migration

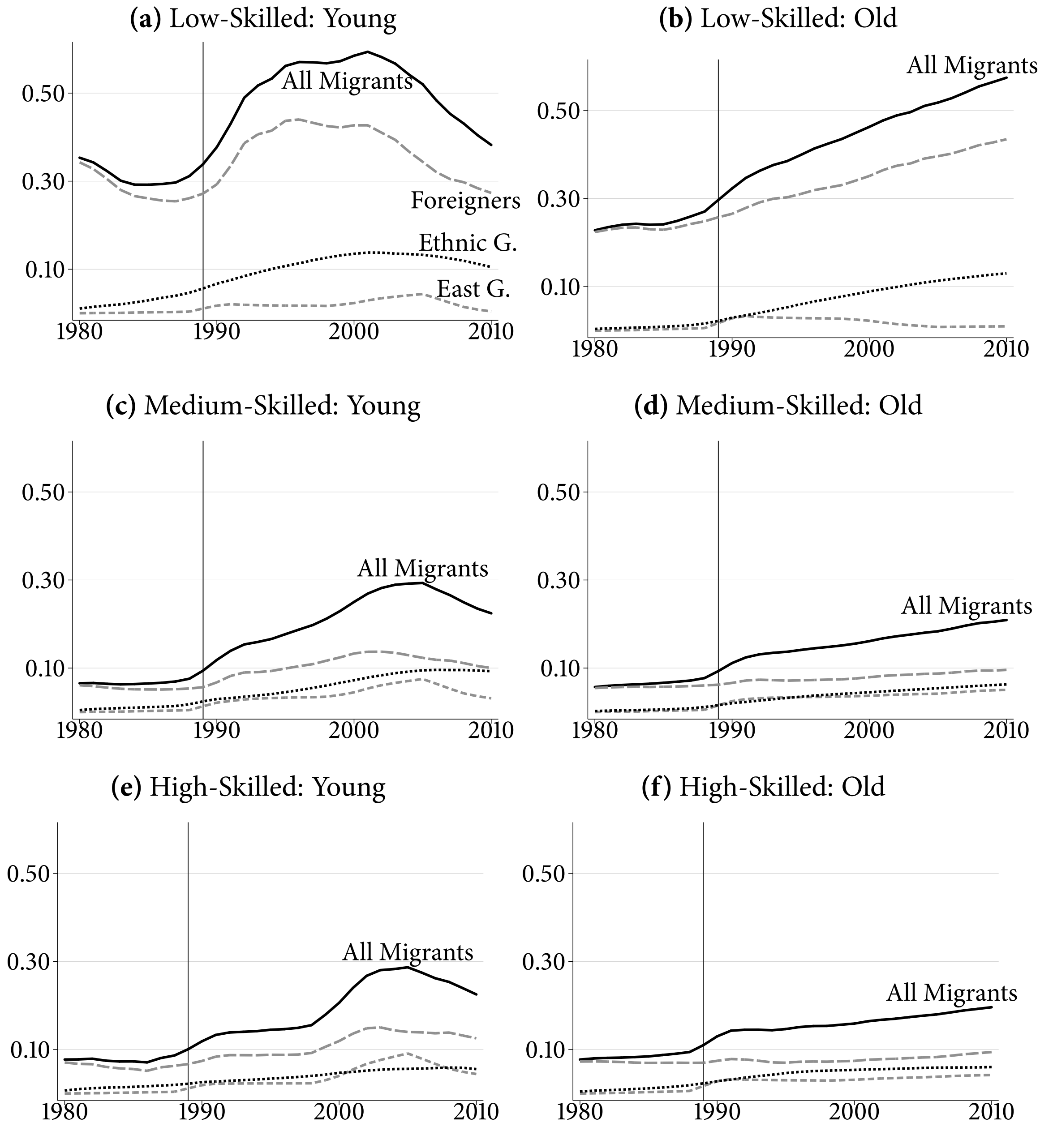
After the fall of the Berlin wall in 1989, West Germany experienced large migration inflows from essentially three groups: (i) East-Germans, (ii) ethnic Germans from Eastern Europe and the former Soviet Union, and (iii) foreigners immigrating from other European countries or parts of the world. Within 15 years after the fall of the Berlin wall, about 1.5 million East Germans, 2.7 million ethnic Germans and 2.7 foreigners migrated to (West) Germany which had an initial population of about 60 million in 1989.[33]

In Figure 2.5, we plot the share of different migrant groups in the total efficiency supply of each age-skill group.[34] Foreign workers can directly be identified at the individual level in the IAB-labor market data. This is not the case for East- and ethnic Germans. Their aggregate shares are derived using data from the Qualification and Career Survey (East Germans) and the microcensus (ethnic Germans). For more details on the construction of these migrant shares see section B.9 in the appendix. As the figure suggests, the migration inflows after the fall of the Berlin wall into the West German labor market were substantial. During its peak in the mid 1990s and

[33] These figures are calculated by summing up the corresponding flows over 1989-2003 as follows: (i) East Germans: net migration from East to West Germany (inflows minus outflows) taken from Statistisches Bundesamt (2014); (ii) ethnic Germans: inflows from Bundesverwaltungsamt (2016); (iii) foreigners: net inflows from Statistisches Bundesamt (2016) minus inflows of ethnic Germans. Using gross inflows for ethnic Germans seems justified as "only a negligible number of [ethnic Germans] have later left Germany, rendering the selection on return migration a non-issue" as Hirsch et al. (2014, p. 213) point out.

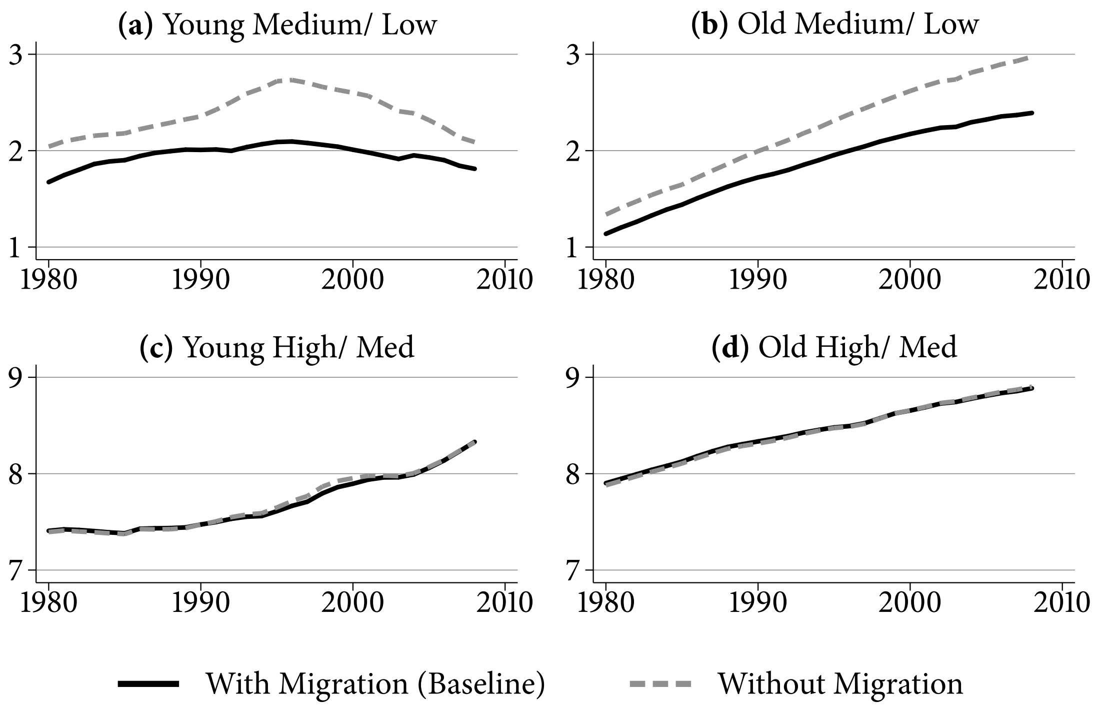
[34] Note that the official inflows do not necessarily translate into corresponding shares in labor supplies due to different participation rates of the different migrant groups. Children, students, pensioners, and other non-working migrants are included in the official figures but do not contribute to the migrant labor supply. Furthermore, some shares might seem high at first glance, but they occur in subgroups (low-skilled and/or young workers) that make up only a smaller share of total labor supply which is why the corresponding shares in *total* labor supply amount to only 4.3% (East Germans), 6.8% (ethnic Germans), 11.8% (foreigners), and 23.0% (all migrants) in 2008.

Figure 2.5: Share of Different Migrant Groups in Total West Germany Supplies



Notes: This figure plots for each education group and separately for young (≤ 30 years) and old workers (>30 years) the share of different migrants groups in efficiency supplies.

Figure 2.6: Relative Efficiency Supplies with and without Immigrants



early 2000s, more than half of the efficiency supply of young low-skilled workers was supplied by migrant workers with foreigners making up the largest part. The share of East Germans is similar across the different age-skill groups at about 3-6%.[35] Ethnic Germans and foreign migrants are mostly concentrated in low-skilled labor. All in all, migration affects low-skilled labor supplies the most, but are still sizable in the groups of medium- and high-skilled supplies.

How did these migration flows affect relative labor supplies - the quantity our model links to skill premiums of natives? Figure 2.6 depicts relative supplies with (baseline) and without migration. Migration of the three groups left the relative supply of high-skilled labor basically unchanged (panels c and d), which is why we will focus on the medium- to low-skilled supplies and premiums in what follows. Without migration, medium-skilled labor would be more abundant both for young

[35] In line with this, Prantl and Spitz-Oener (2014, p. 5) note that "[t]he German-German migration wave [..] does not include workers of any education class over-proportionally. Hence, the educational distribution of German workers in West Germany remained stable".

and old workers as less low-skilled labor would be available under this scenario (panels a and b). However, migration would not have changed the general patterns in the evolution of relative medium supplies - a reverse U-shape pattern for the young and a continuous increase for the old.

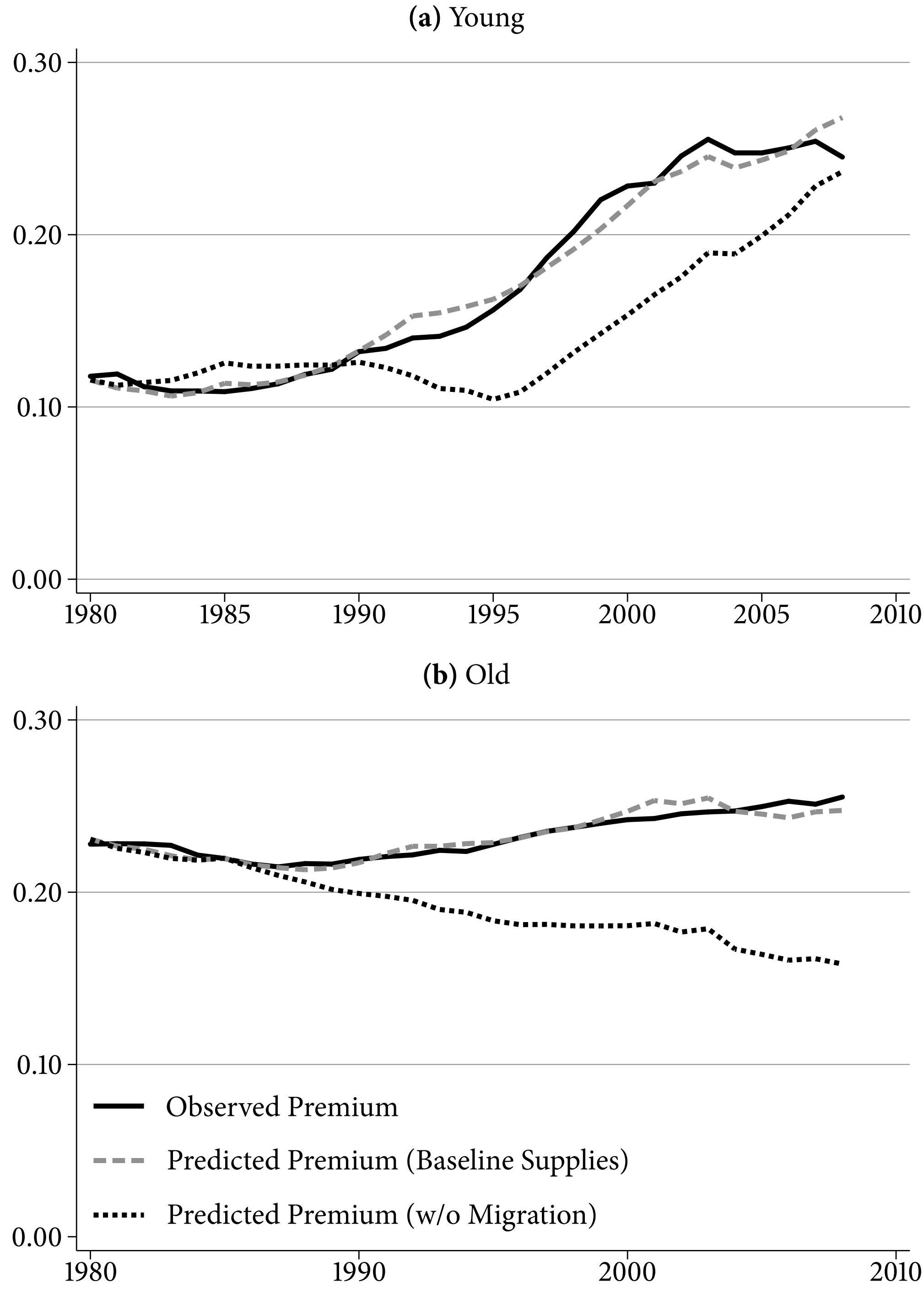
Given these migration flows and changes in the relative supplies - how would skill premiums have developed in the absence of migration?[36] In particular, is (low-skilled) migration responsible for the pronounced increase in the medium to low premium of young workers? To answer these questions, we use our preferred parameter estimates (model 2 of Table 2.4) to simulate the counterfactual evolution of skill premiums in the absence of migration. We feel comfortable doing this as the underlying structural parameter estimates are very similar in magnitude when using the full period or only data over 1980-90, a period of no or only incipient immigration flows.

It should be noted that, given our analytical framework, we implicitly assume perfect substitutability between migrants and natives within a given age-skill cell. This assumption, if incorrect, would lead to an *overestimation* of the impact of migration on native wage premiums. Since our results show that migration is *not* the main driver of rising inequality at the lower end of the German wage distribution, ignoring the issue of potentially imperfect substitutability is inconsequential for the main qualitative finding of the paper. Furthermore, remember that we account for the different productivities of natives and migrants when constructing labor supplies, thus natives and migrants are not treated identically in this respect. Finally, substitutability between migrants and natives is likely to be relatively high in the German context given that East and ethnic Germans were more similar to natives in terms of language and culture than the typical foreign immigrant. In line with this, D'Amuri et al. (2010) estimate a rather high elasticity of substitution of around 25 between migrants and natives in Germany.

Figure 2.7 shows the results of our counterfactual exercise. Without migration, the young medium to low premium would have first declined slightly into the mid 1990s to then strongly increase to the same level as the actual premium in 2008. Thus, migration seems to have advanced the divergence in wages between young

[36] Of course, this is a static counterfactual exercise, native labor supplies could have developed differently had the Berlin wall not come down.

Figure 2.7: Observed vs. Predicted Medium to Low Premiums with and without Migration



medium- and low-skilled workers by around 5 years. Its strong increase, however, would have occurred even without the large migration flows of the 1990s. The conclusion is somewhat different for the medium to low premium of older workers. Here, migration seems to have kept that premium at a rather stable or slightly increasing path which in the absence of migration would have decreased by some five percentage points compared to its 1990 level. All in all, migration did have a considerable impact on wage premiums of medium-skilled workers, but cannot explain the strong increase in the skill premium of young workers that eventually occurred over the 1990s and 2000s. In the next section, we will therefore turn to the educational attainment of *native* workers as an alternative source of supply changes.

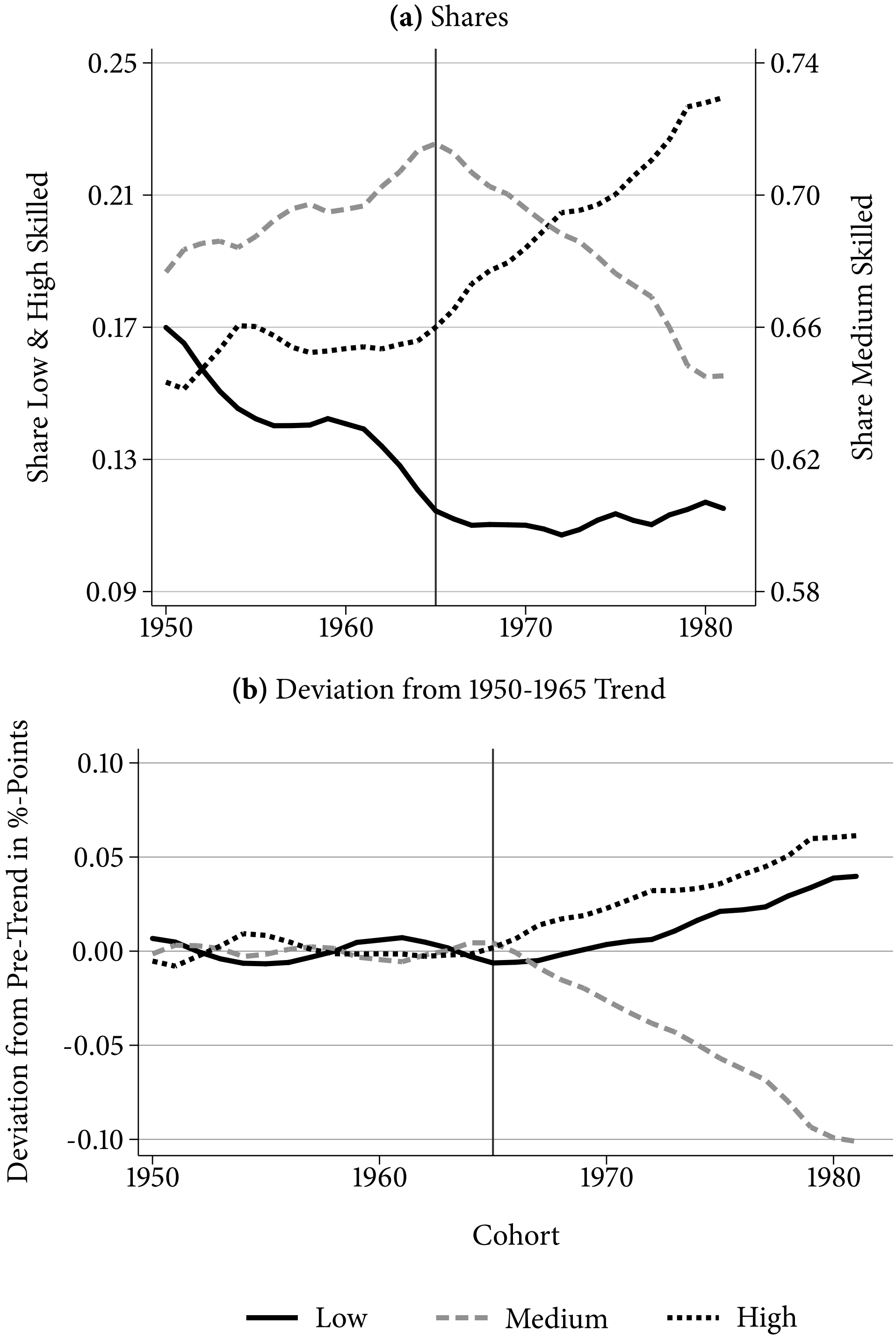
2.5.2 Cohort Analysis of Skill Acquisition

To proceed in understanding the drivers of the observed supply changes, we use data from the German microcensus, an officially conducted yearly survey based on a 1% random cross-section of the German population similar to the US Current Population Survey (CPS). Participation in the microcensus is compulsory and non-compliance can be fined or even punished. Most official German population and labor market statistics are based on the microcensus. We pool microcensus waves 2005-2011 and restrict the sample to individuals residing in West-Germany at the time of the interview. In the following, we focus on native West-Germans by excluding individuals who were born or migrated from outside Germany, who have a non-German nationality, have been naturalized, or obtained a school degree from former East Germany. We furthermore consider only individuals who are at least 30 years old to make sure they have finished their formal education.[37] We group individuals in the same three education groups as defined in our SIAB sample.

Using this sample of native West Germans with completed education, we plot for each birth cohort the share of low-, medium- and high-skilled individuals in Figure 2.8a. We focus on cohorts born between 1950-1981 as these are the rele-

[37] Unlike in some previous years, answering the question about the highest formal occupational degree is mandatory for all age groups from microcensus wave 2005 onwards. See Fitzenberger et al. (2004) for an imputation method of the education information in case the related questions are voluntary for some age groups and thus suffer from potential selection bias.

Figure 2.8: Educational Attainment by Cohorts



vant cohorts determining the inflows of young workers over our study period.[38] The figure reveals a striking pattern which, to the best of our knowledge, has not been documented in the literature so far. The share of individuals with completed vocational training, i.e. the medium-skilled, shows a reversed U-shaped pattern with the turning point occurring at the peak baby boomer cohort around 1965. In the 15 years up to that point, this share was increasing from 67% to 71% but then started to decrease quite rapidly reaching only 64% in the 1981 cohort, a share comparable to that of the 1940 cohort (not shown). At the same time, the share of low-skilled stopped its continuous decrease over the previous decades to stabilize at around 11%. Finally, the share of individuals holding a university degree started to increase strongly after it had stayed virtually flat throughout for most of the 1950-1965 cohorts. The break in educational attainment of native West Germans around the 1965 cohort becomes even more salient in Figure 2.8b where we estimate a linear trend for the cohorts 1950-1965 and plot the deviation from this trend.[39] This plot reinforces the impression from the previous figure. The evolution of the educational attainment of natives is characterized by a sort of "polarization", i.e. a marked drop in the share of those acquiring vocational training on the one hand, and an accelerated increase in tertiary education and a relative increase in the share of the low-skilled on the other. The figures also show that while low-skilled immigration played some role, the major force behind the overall decrease in the relative supply of young medium-skilled workers in the 1990s and 2000s was due to a strong reversal in the trend towards medium-skilled education of natives around the 1965 cohort.

More research is needed to understand the reasons behind the break in educational attainment that decisively influenced labor supplies and thus, via wage premiums, inequality of labor incomes in Germany. Here we can only offer some speculative explanations. One potential reason could be the so-called "educational expansion" (*Bildungsexpansion*), a series of educational reforms implemented in

[38] These time series are smoothed using a moving average including one lag, the current value and one lead for illustrative purposes. Non-smoothed series look very similar and are available from the authors.

[39] A structural break test (maximum F-value) picks 1965 (low-skilled), 1965 (medium), and 1966 (high) as the break points. We also estimated a linear trend using the cohorts 1945-1965 or allowed for a quadratic pre-trend with similar results.

West Germany during the 1960s and 1970s with the primary objective of increasing university access.[40] However, the 1965 cohort took the decision of whether to do a vocational training or go to university in the early 1980s, some ten years after the educational expansion started to take effect. Thus, these reforms should have already affected cohorts born before 1965 and are therefore unlikely to serve as the primary explanation for the documented trend break in the educational attainment of the West-German native population.

Another potential explanation could be related to cohort sizes. Cohort sizes increased gradually in Germany in the post-war period, reaching their peak in 1964 with 1.35 million individuals. After that, cohort sizes decreased rapidly to 0.8 million in the mid 1970s. While cohorts became smaller, university capacity continued to increase. Thus, for the post baby boomers, it might have been easier to get into college and university. Other possible reasons may include societal changes in the 1960s that shifted parents' preferences away from traditional vocational careers for their children towards more academic university education, or a signaling story along the lines of Bedard (2001) in which wider access to universities reduced the incentive for individuals to "pool" in the medium education group to take advantage of high-ability individuals who are constrained from entering university. Finally, it could also be that due to the smaller cohort sizes and, consequently, smaller families, parents had more resources to invest in the education of each of their children (quality-quantity trade-off), pushing them increasingly into the tertiary education track.

2.6 Conclusion

The rise in inequality in many OECD countries over the last decades has triggered a rich body of academic work. Scholars agree in general that recent changes in inequality are mainly driven by inequality of labor incomes which in turn are closely related to skill premiums. This is certainly true in Germany, where the medium to low skill premium closely tracks the evolution of inequality at the lower end of the wage distribution (measured as the 50th to 15th percentile gap). In this paper,

[40] These reforms included, for instance, the foundation of new universities such as Augsburg, Bamberg, Bochum, Bielefeld, or Passau and the introduction of a federal student loans and grants program (*BAföG*).

we ask whether skill-biased technological change and, in particular, shifts in the supply of different skill groups – both along the age and the education dimension – can explain the observed evolution of skill premiums in Germany over the last three decades.

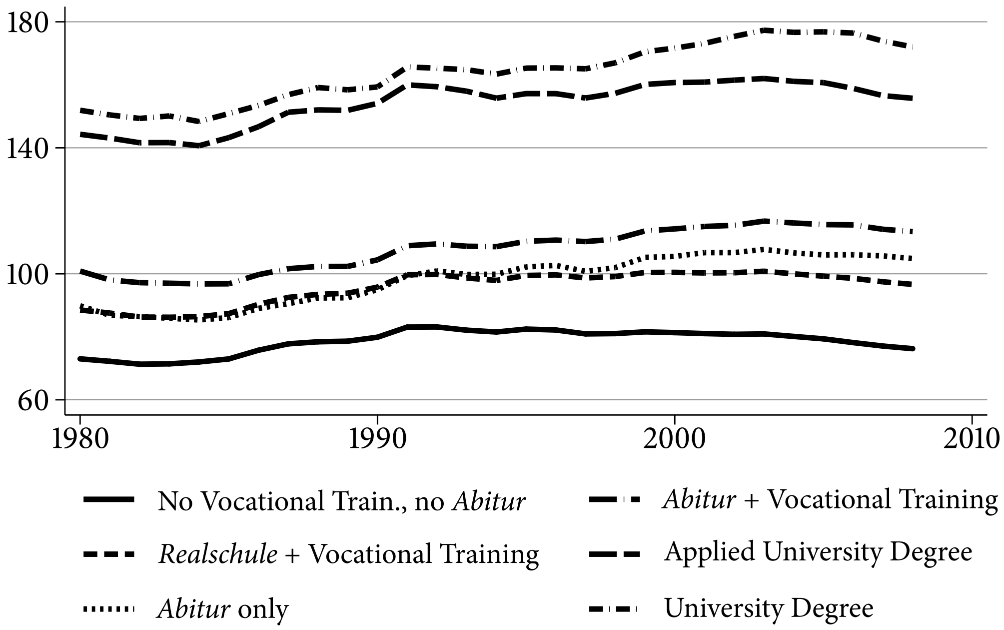
Our estimations based on a model comprising three skill and two age groups show that linear technological progress (up to 2002) and observed changes in skill supplies go a long way in explaining the peculiar patterns of skill premiums in Germany. In particular, our model is able to explain the pronounced increase in the wage premium of young medium-skilled worker from 10% in the 1980s to 25% in the 2000s very well. Premiums for both young and old high-skilled workers show no systematic upward or downward trend despite a pronounced increase in their relative demands. Our framework suggests that this was because the corresponding supply of high-skilled workers has kept pace with increased demand. The share of high-skilled workers among all full-time workers has tripled from 5% at the beginning of the 1980s to 15% at the end of the 2000s and continues to increase.

Our cohort analysis suggests that the rapid increase in the skill premium for young medium-skilled workers is rooted in a pronounced change in the educational attainment of the native (West-) German population that occurred for cohorts born after 1965 and which reversed previous trends in the acquisition of different types of education. The share of individuals with completed vocational training decreased strongly and was as large for the 1980s cohorts as it was for the 1940s cohorts while the share of individuals with tertiary education increased to unprecedented levels and the long-term decline in the share of low-skilled individuals came to a hold. All in all, our study suggests that a considerable part of recent changes in earnings inequality between different skill groups in Germany are the result of longer term educational choices of the population and hence, ultimately, driven by labor supply.

Appendix B

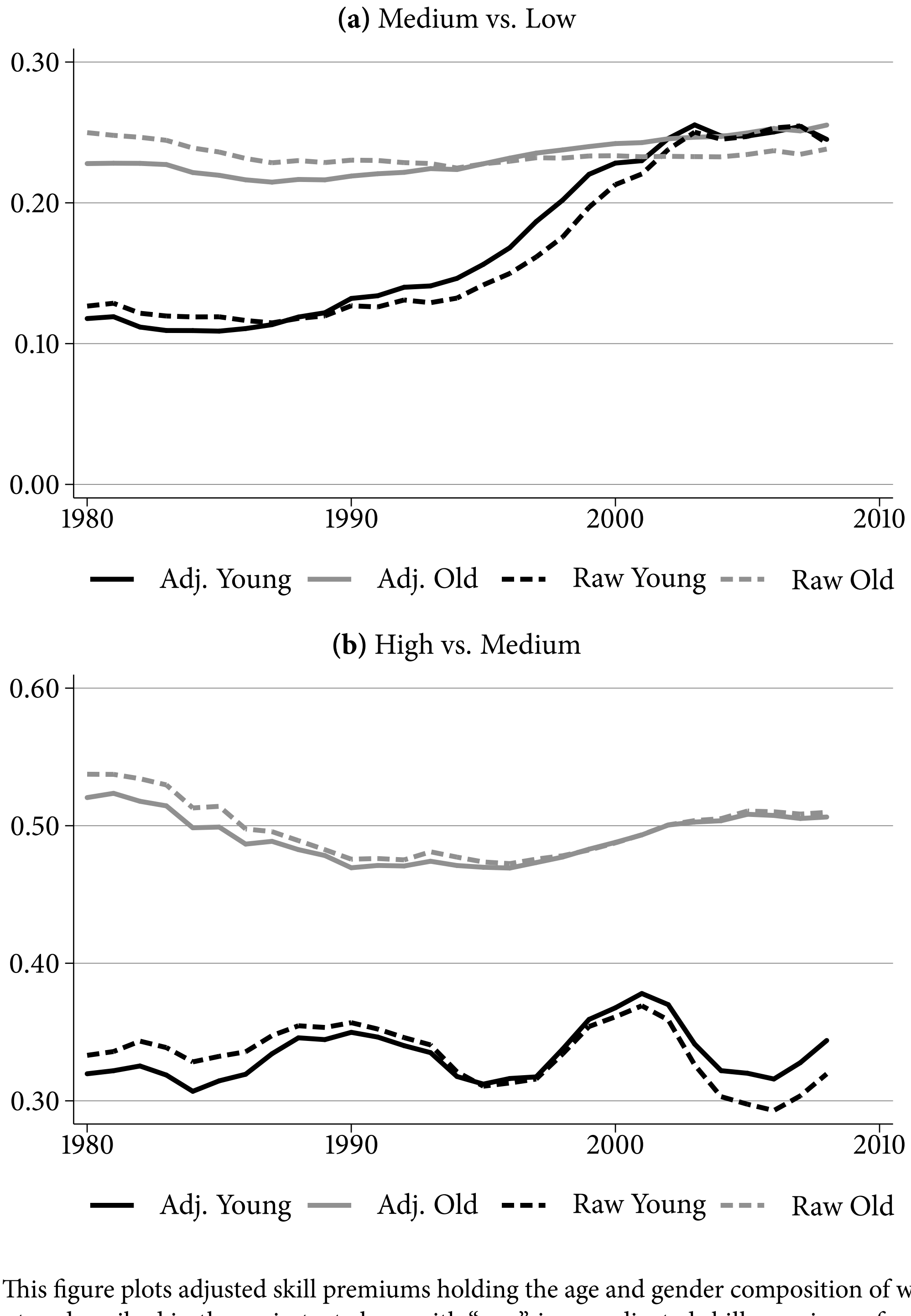
B.1 Additional Figures

Figure B.1: Mean Real Daily Wage By Disaggregated Education Groups



Notes: This figure plots the mean real daily wage aggregated by six different educational attainment categories. *Realschule* denotes an secondary degree after ten years of schooling (ISCED level 2), *Abitur* denotes an advanced secondary degree after 12 or 13 years of schooling (ISCED level 3), an applied university degree corresponds to a degree from a *Fachhochschule* (ISCED level 5a).

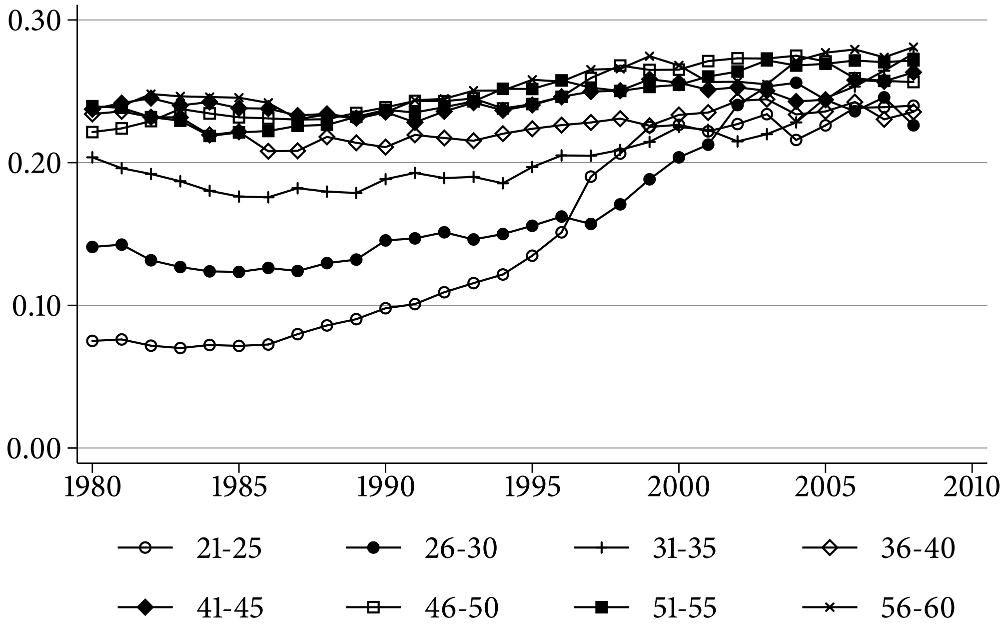
Figure B.2: Raw and Composition Adjusted Skill Premiums



Notes: This figure plots adjusted skill premiums holding the age and gender composition of workers constant as described in the main text along with "raw", i.e. unadjusted skill premiums, for young and old workers.

Figure B.3: Skill Premiums by Eight Different Age Groups

(a) Medium to low



(b) High to Medium

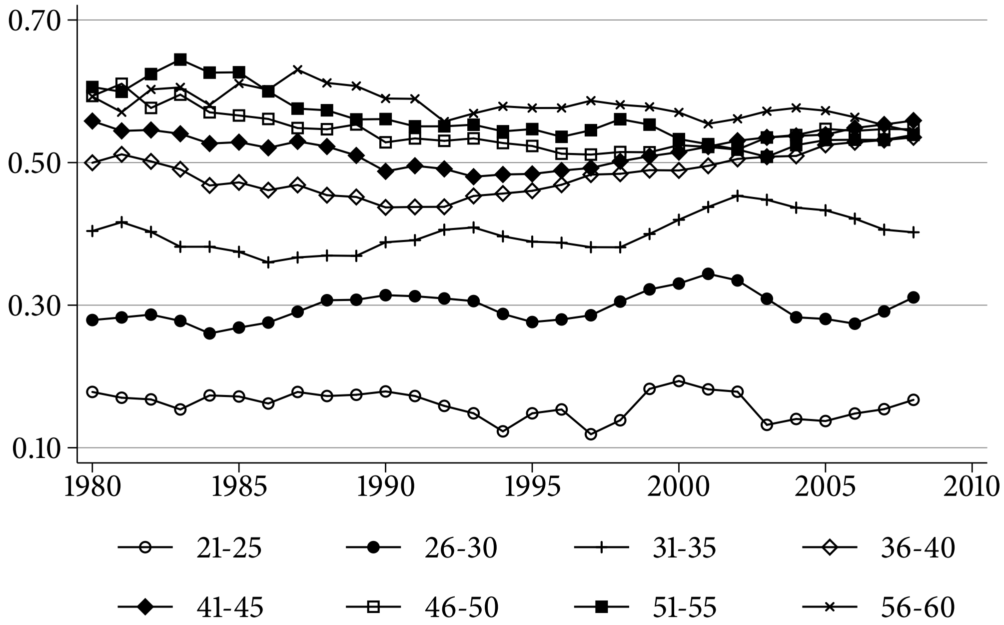
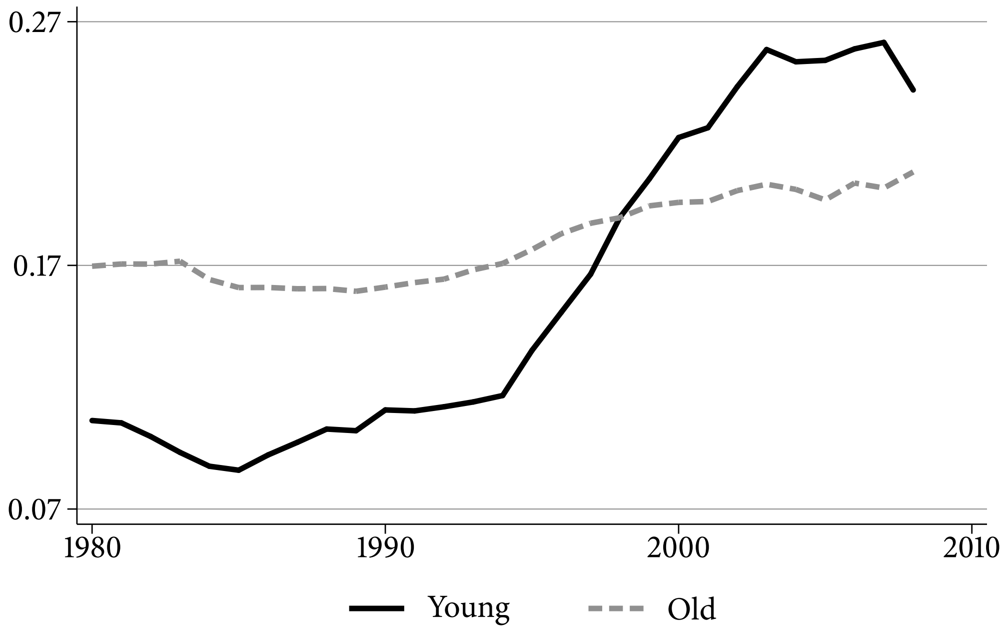


Figure B.4: Medium to Low Skill Premiums Separately for Men and Women

(a) Men



(b) Women

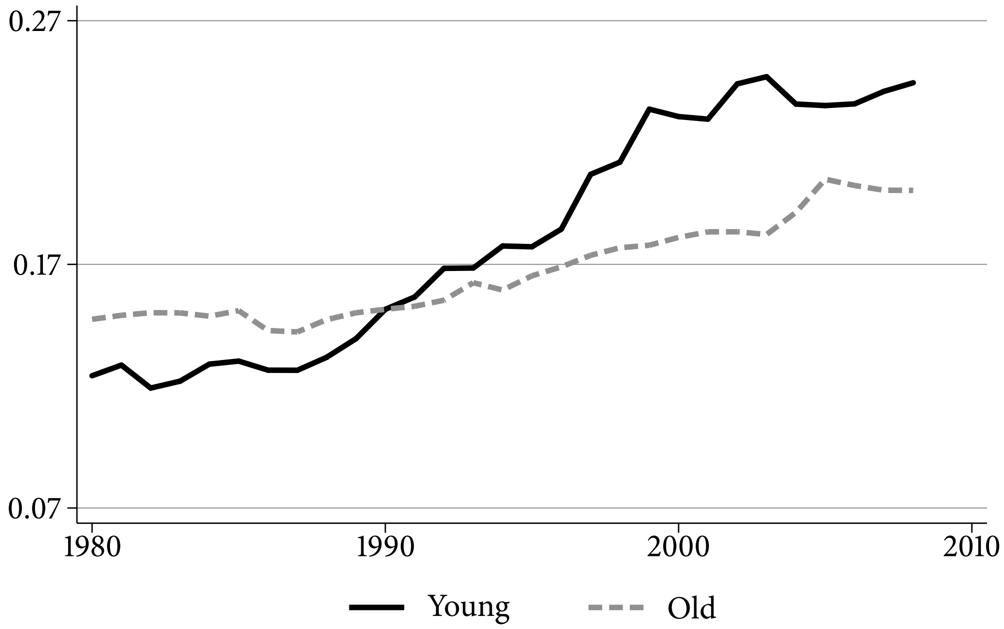
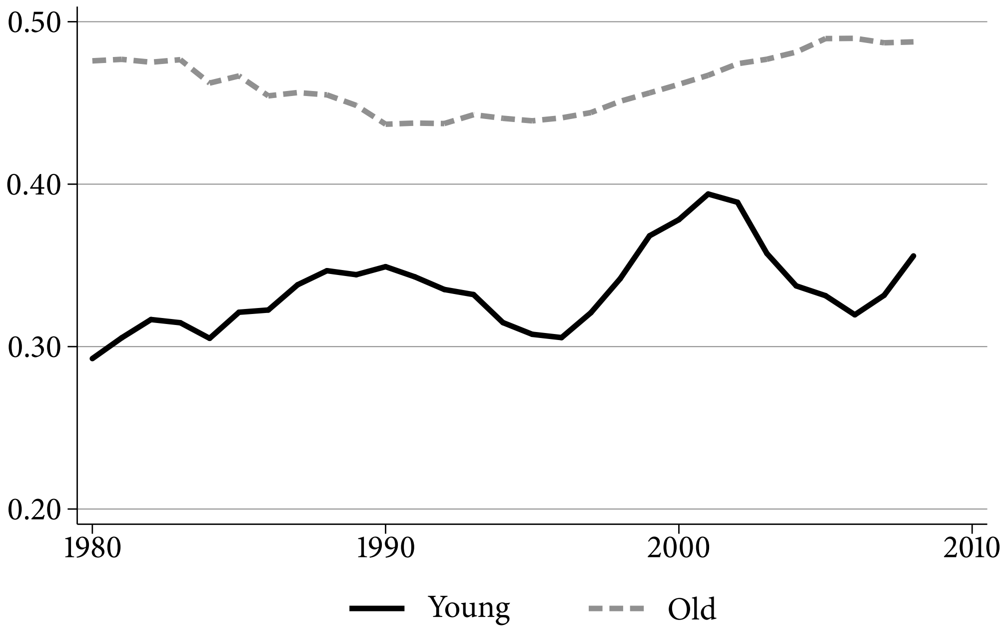


Figure B.5: High to Medium Skill Premiums Separately for Men and Women

(a) Men



(b) Women

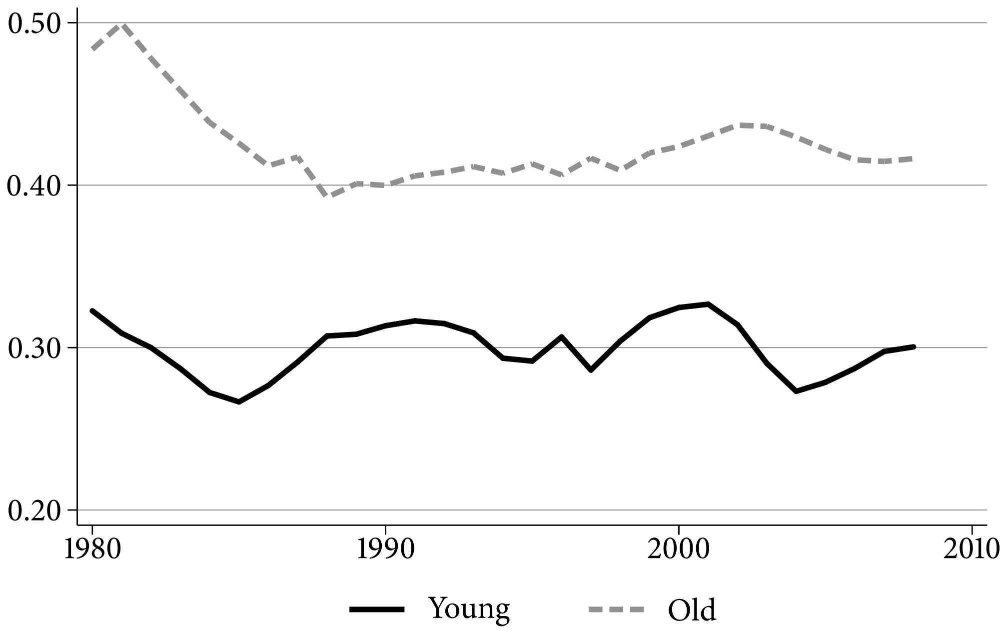


Figure B.6: Observed vs. Fitted Aggregated Medium- to Low-Skilled Premium (Corresponding to Model 2 of Table 2.3)

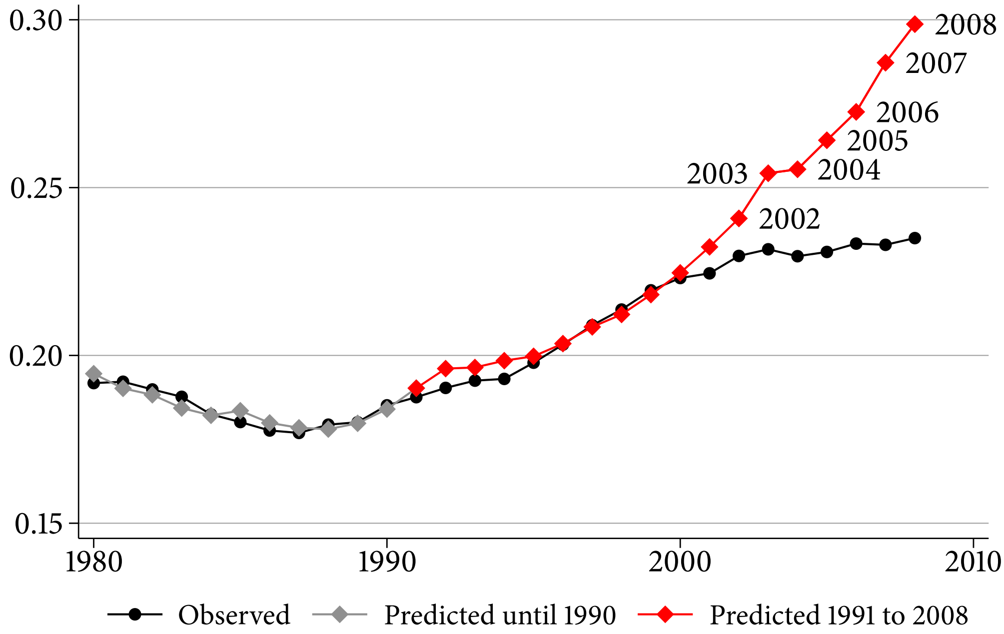


Figure B.7: Comparison of Young High to Medium Premiums (SIAB vs. Mikrozensus)

Figure B.8: Co-Movement of the High-Skill Premium of Young Workers and GDP Growth

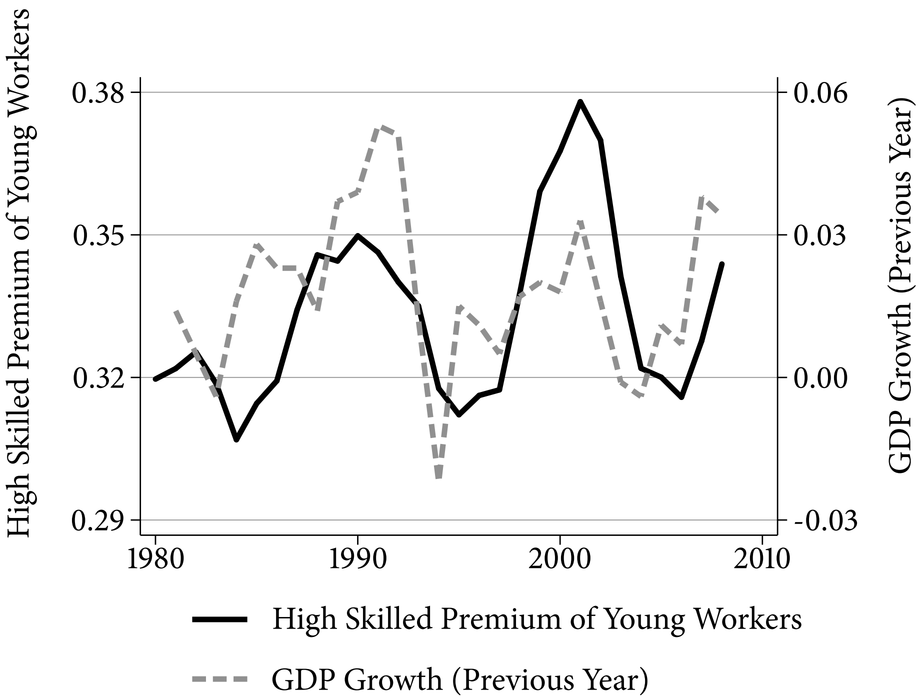
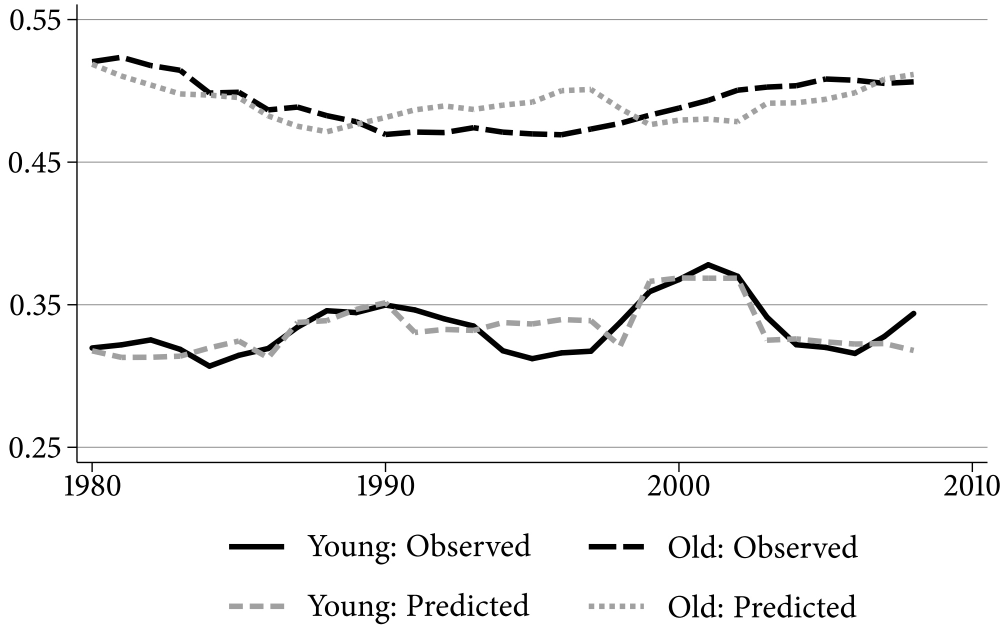
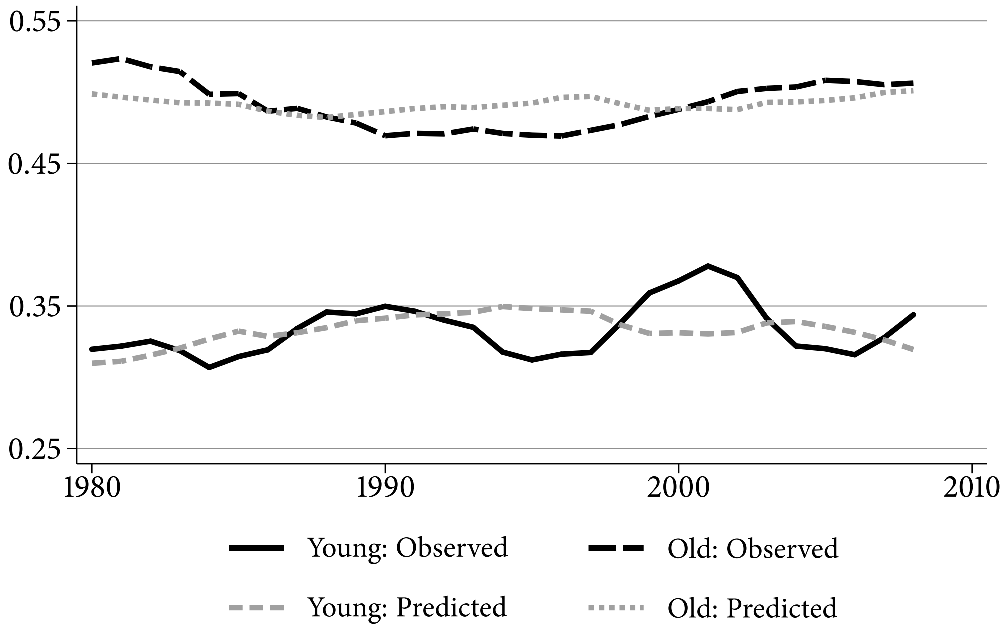


Figure B.9: Predicted vs. Observed High-Skilled Premiums

(a) High vs. Medium: all years 1980-2008 (model 2 of Table 2.4)



(b) High vs. Medium: all years 1980-2008 (model 1 of Table 2.4)



B.2 Data Preparation and Sample Restrictions

- **Imputation of Missing Values** Using the universe of spells in the *Sample of Integrated Labour Market Biographies*, we impute missing education information following Fitzenberger et al. (2006). For each individual we also impute missing location with the last non-missing location information. We impute missing German nationality and gender information by first computing the minimum and maximum of these dummy variables by each individual. If these two values are the same, then all missing values of a given individual are replaced by his/her unambiguous value of the variable. If the two do not agree, no imputation is performed.
- **Correction of Structural Break 1984** From 1984 onward the IAB wage measure also includes bonuses and other one-time payments. We correct for this structural break following the non-parametric method proposed by Dustmann et al. (2009) (which builds on Fitzenberger 1999).
- **Imputation of Censored Wages** We impute censored wages above the upper earnings threshold for compulsory social insurance (66,000 euros per year in 2010) using the "no heteroskedasticity" approach by Gartner (2005) and Dustmann et al. (2009). Specifically, we consider wages as censored that were up to two euros below the maximum wage value observed in each year and then estimate for each year and for males and females separately a censored regression of log wages on indicators of eight age groups, three skill groups and all their possible interactions, assuming that the error term is normally distributed and has the same variance across age and skill groups. We also imputed wages assuming different censoring limits and assumptions on the variance of the error term but found the "no heteroskedastity" approach to be more robust with respect to different censoring limits and the share of censored observations (confirming Dustmann et al. 2008, who imputed wages over 1975-2004 using the "no heterogeneity" approach to calculate and analyze skill premiums). Both imputation methods, however, yielded implausibly high wages (e.g. compared to series derived from the Mikrozensus) for high-skilled workers between 1975-1979 (as also noted by Dustmann et al. 2008, 2009). This is likely because of the high share of censored wages in these years (up to 18% after the structural break correction as compared to

around 10% from 1980 onwards). This is why we exclude observations from 1976-1979.

- **Sample Restrictions** We then drop all individuals living in East Germany and those younger than 21 and older than 60 years. Following common practice, we also exclude spells that start and end on the same day (2.1% of all initial spells in West Germany), spells that overlap with one or more parallel full-time spells (~1.4%), spells of doctors and pharmacists (~0.8%) as their records are corrupted and missing between 1996-1998 (see vom Berge et al. 2013, for further details), and spells of individuals who are registered as "not unemployed, but registered as a job seeker with the BA", "without status", or "seeking advice".
- **Exclusion of Crisis Years 2009/10** A closer examination of the data suggests that the years 2009-10 are unusual, in particular for old medium-skilled workers who see an abnormal depression in their wages. This is likely to be related to the global financial crisis that started in 2007/08. Although unemployment in Germany did not increase during the financial crisis, many workers – in particular medium-skilled worker in manufacturing – had to go on short-term work which was associated with temporary wage cuts (supplemented by public transfers). We therefore exclude observations from 2009 and 2010. Estimates including these crisis years are slightly lower but all main conclusions continue to hold.

B.3 Skill Premiums

Our skill premiums are based on a sample restricted to native West-Germans (i.e. excluding those ever reported to be non-German or have missing nationality information and those first registered in East Germany). To compute the price of skills not confounded by changes in the age and gender composition within skill groups, we proceed as follows. First, we calculate the mean log real wage in each skill-age-gender-year cell (cell-specific wages) weighted by the share of days worked per year. Second, in each year we calculate the share of each cell in the total supply of a corresponding skill group measured as days worked and then average these shares for each cell over all years (fixed cell weights). The composition constant log real wage of a given skill-age group is then calculated as the weighted average of all corresponding cell-specific wages using the fixed cell weights as weights. For instance, the composition constant log wage of low-skilled workers at time t is calculated as $low_t = \sum_{a=1}^{8}\sum_{g=0}^{1} \ln wage_{s=low,a,g,t} \cdot weight_{s=low,a,g}$ where a denotes one of eight different age group (the young comprise age groups 1 and 2, the old 3 to 8) and g gender. Note that the weights are not indexed by time meaning that they are constant over time. Finally, the medium to low (high to medium) skill premium are calculated as the difference between the composition constant log real wage of medium- and low-skilled (high- and medium-skilled) workers. Thus, skill premiums can be interpreted as the percentage difference in wages between two skill groups. Age group specific premiums are calculated by restricting the above calculations to the corresponding age groups of young (age groups 1 and 2) and old workers (age groups 3 to 8).

B.4 Efficiency Labor Supplies

The efficiency labor supply of a specific skill-age group is calculated as the number of spells in that group weighted by the spell length, the approximate hours of work, and the efficiency weight. The efficiency weight is time-constant and calculated based on full-time spells as the normalized wage of a skill-age-gender-nativity group relative to a baseline wage averaged over all years. Specifically, the efficiency weights are computed by first aggregating full-time wages by year, skill, age, West German nativity and gender. Analogously to our wage sample, we classify all individuals who ever report to be non-German or have missing nationality information and/or those who started their first spell in East Germany as non West German natives. These cell averages are then divided in each year by the corresponding baseline wage of West German native male medium-skilled workers aged 36-40. Thus, women and men as well as West German natives and non-natives in the same skill-age group are assigned different efficiency weights. Then, we average these weights over the entire sample period for each group. Table B.1 lists the full set of efficiency weights used to construct our baseline efficiency supplies. In an alternative approach, we allowed the productivity of women and non-natives to be time-varying relative to native men. This, however, has only a minor effect on our estimates. Spells are further weighted by their approximate hours of work (or spell type specific weights) which are listed in Table B.2. Expressed more formally, the supply of skill group

s in age group a in year t is computed as the weighted sum of all spells i in that cell where h denotes spell-type (full-time, part-time, vocational, unemployed), g gender and m West German nativity:

$$\text{Supply}_{sat} = \sum_{i \in Cell_{s,a,t}} \text{spell-length}_i \cdot \text{spell-type-weight}_h \cdot \text{efficiency-weight}_{sagm}.$$

For instance, medium-skilled native men aged 31-35 working full-time all year long supply exactly one unit of efficiency labor in each year, while a high-skilled native female aged 41-45 working long part-time for half of the year supplies 0.41 units (= 0.5 (half a year) × 2/3 (spell type weight long part-time) × 1.22 (efficiency weight high-skilled females aged 41-45)) and a low-skilled non-native men aged 26-30 who is unemployed half of the year and full-time employed the other half supplies 0.49 (= 0.5 (half of the year) × [1/3 (spell type weight unemployed) + 1 (spell-type weight full-time)] × 0.74 (efficiency weight non-native low-skilled men aged 26-30)) units of efficiency labor.

Table B.1: Efficiency Weights for Baseline Supplies

	Low		Medium		High	
	(1) Native	(2) Foreign/ East German	(3) Native	(4) Foreign/ East German	(5) Native	(6) Foreign/ East German
Panel A: Men						
Age 21-25	0.68	0.66	0.76	0.74	0.93	0.98
Age 26-30	0.77	0.74	0.89	0.84	1.21	1.23
Age 31-35	0.84	0.79	1.00	0.90	1.49	1.45
Age 36-40	0.88	0.83	1.06	0.93	1.69	1.61
Age 41-45	0.89	0.85	1.10	0.95	1.81	1.70
Age 46-50	0.90	0.86	1.11	0.94	1.86	1.73
Age 51-55	0.90	0.87	1.11	0.94	1.88	1.74
Age 56-60	0.89	0.85	1.09	0.93	1.86	1.74
Panel B: Women						
Age 21-25	0.56	0.53	0.65	0.61	0.78	0.83
Age 26-30	0.63	0.58	0.75	0.71	1.00	1.02
Age 31-35	0.64	0.60	0.78	0.74	1.15	1.16
Age 36-40	0.64	0.61	0.77	0.74	1.20	1.22
Age 41-45	0.64	0.62	0.78	0.74	1.22	1.26
Age 46-50	0.65	0.63	0.79	0.75	1.25	1.23
Age 51-55	0.66	0.65	0.79	0.75	1.27	1.22
Age 56-60	0.65	0.64	0.79	0.75	1.29	1.23

Notes: This table shows the full set of efficiency weights for each of the 96 gender × West German nativity × skill group × age group cells. Each entry corresponds to full-time year-round spells. The baseline group with an efficiency weight of 1 are medium-skilled native men between 31-35 years.

Table B.2: Spell Type Specific Weights

	Spell Type Weight	
Spell Type	Baseline	Alternative
Full-Time	1	1
Long Part-Time	2/3	2/3
Short Part-Time	1/3	1/3
Trainees & Unemployed	1/3	1

Notes: This table shows the different spell type specific weights to construct efficiency supplies.

B.5 Imputation of Missing Unemployment Spells

In our baseline efficiency supplies, we also include unemployment spells. These include ALG, ALH, and ALG II spells. ALG II spells are missing in 2005/06. We therefore linearly interpolate aggregated unemployment spells in these two years separately for each skill and age groups. Also note that the number of unemployed drops between 2003/04 which leads to the bump in the medium to low-skilled supply of young workers visible in the top right part of Figure 2.3. This is likely due to a change in the data collection procedure of the IAB (compare vom Berge et al. 2013, p. 30).

B.6 Robustness of High to Medium Premium

We present two different pieces of evidence that corroborate the robustness of the high to medium premium derived from SIAB data. First, Dustmann et al. (2008) perform an extensive evaluation of various imputation methods. They take an uncensored distribution of wages available for 2001[41], artificially censor it at the same thresholds as in the SIAB data and compare several statistics of the imputed distribution with the true counterparts from the uncensored distribution. Their comparisons show that the "no heterogeneity" imputation approach (which we also use here) matches the standard deviation and in particular the high to medium skill premium of the uncensored distribution very well (true 0.472, no heterogeneity 0.471). This shows that the imputation method works well in a particular year (2001).

Second, we compare the evolution of the 85th percentile (of gross earnings) observed in the SIAB which is always uncensored in 1980-2008 with the top fractiles (of labor incomes) from the WTID.[42] If the top 15% of the income distribution systematically diverged from the bottom 85% and assuming that most individuals in the top 15% are high-skilled, we would underestimate the high to medium premium.

[41] This uncensored wage distribution comes from the GSES a survey of 27,000 establishments with compulsory participation conducted by the German Federal Statistical Office. For more details see Dustmann et al. (2008, section 2, pp. 6f).

[42] The WTID data is based on the incomes of all individuals who file an income tax report and thus also includes self-employed, civil servants, members of the armed forces, and other who are not observed in the SIAB.

Figure B.10: Log Difference Between the 85th Percentile (SIAB) and the Average Income (WTID) of...

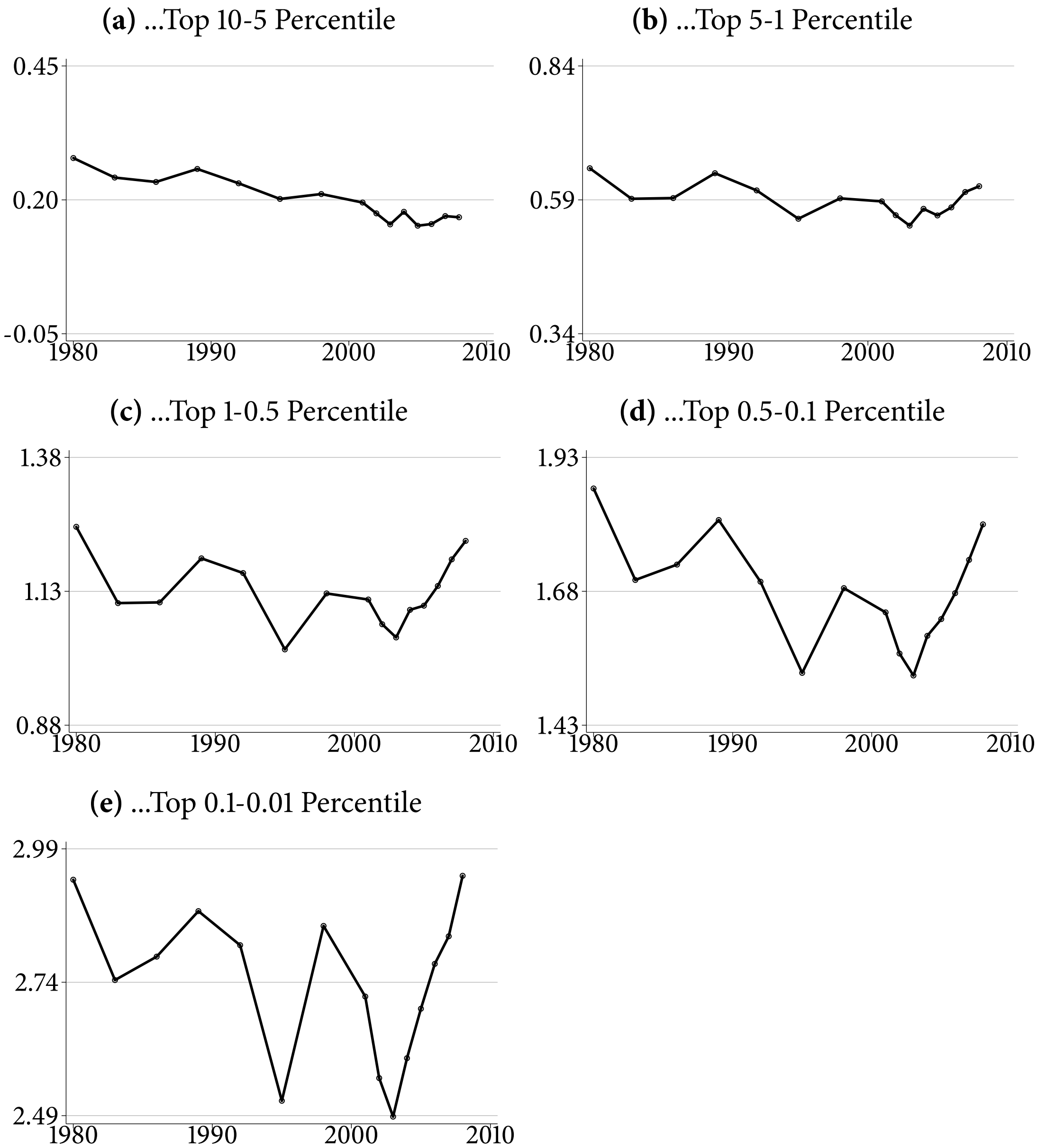


Figure B.10 shows that this is not the case. It depicts the log difference between the average incomes of the five top fractiles observed in the WTID and the 85th percentile observed in the SIAB. Although there is considerable variation in these gaps, there is no clear upward trend in neither of them. All gaps stayed roughly the same or even decreased somewhat (or even considerably in case of the difference to the top 10-5 fractile, see panel a of Figure B.10).

B.7 Flexibly Estimating σ_a

In our main analysis, we assume that the elasticity of substitution between age groups, σ_a, is identical for low-, medium- and high-skilled labor. We can relax this assumption and allow σ_a to differ within each skill group. By substituting in for the different σ's, premium equations 2.5 and 2.7 can be expressed as

$$\omega^M_{jt} = \ln\theta_t + \rho\ln\left(\frac{M_t}{L_t}\right) - \eta_m\ln M_t + \eta_l\ln L_t + \ln\left(\frac{\alpha_{mj}}{\alpha_{lj}}\right) - \left(\frac{1}{\sigma_{am}}\right)\ln M_{jt} - \left(\frac{1}{\sigma_{al}}\right)(-\ln L_{jt}) \tag{2.11}$$

$$\omega^H_{jt} = \ln\lambda_t - \ln\theta_t + \gamma\left(\frac{H_t}{M_t}\right) + \rho\left(\frac{U_t}{M_t}\right) - \eta_h\ln H_t + \eta_m\ln M_t + \ln\left(\frac{\alpha_{hj}}{\alpha_{mj}}\right) - \left(\frac{1}{\sigma_{ah}}\right)\ln H_{jt} - \left(\frac{1}{\sigma_{am}}\right)(-\ln M_{jt}). \tag{2.12}$$

In Table B.3, we estimate this system of equations, again using a seemingly unrelated regression framework. Similar to above, we replace the two last terms with the skill and age group specific labor supplies in each year, $\ln\left(\frac{\alpha_{mj}}{\alpha_{lj}}\right)$ with an indicator for the young age group and absorb the remaining terms using time dummies.[43]

The model implies that the coefficients on M_{jt} should be the same. To see if this is also implied by the data, in model 1 of Table B.3, we do not restrict the coefficients on M_{jt} in the two premium equations to be identical and test for the equality of the two coefficients. It turns out that the two coefficient on the age specific supply of medium-skilled workers are indeed similar and insignificantly

[43] Note that the coefficients on $\ln M_{jt}$ in equations (2.11) and (2.12) should be the same except for the minus sign. This is why we use $-\ln M_{jt}$ as a regressor in equation (2.12) and $-\ln L_{jt}$ in equation (2.11) to make coefficients comparable across equations. The minus sign is omitted for simplicity.

Table B.3: Estimating the Elasticity between Young and Old Workers σ_{as} (*Flexible Across Skill Groups*)

	(1) Unrestricted		(2) Restricted	
	ω_{jt}^{M}	ω_{jt}^{H}	ω_{jt}^{M}	ω_{jt}^{H}
$\ln L_{jt}$	-0.069**		-0.068**	
	(0.030)		(0.029)	
$\ln M_{jt}$	-0.141***	-0.130	-0.140***	-0.140***
	(0.011)	(0.094)	(0.009)	(0.009)
$\ln H_{jt}$		-0.138		-0.146***
		(0.111)		(0.036)
Young	-0.142***	-0.272**	-0.142***	-0.274***
	(0.040)	(0.121)	(0.035)	(0.061)
Constant	0.503***	0.251	0.501***	0.227***
	(0.052)	(0.160)	(0.042)	(0.022)
Time FEs	✓	✓	✓	✓
H_0: $\sigma_{al} = \sigma_{am}$ (p-value)	0.24	0.18	0.22	0.22
H_0: $\sigma_{al} = \sigma_{ah}$ (p-value)	0.12		0.16	
H_0: $\sigma_{am1} = \sigma_{am2}$ (p-value)	0.91			
H_0: $\sigma_{am} = \sigma_{ah}$ (p-value)	0.98	0.81	0.85	0.85
σ_{al}	14.6		14.7	
	(6.4)		(6.3)	
σ_{am}	7.1	7.7	7.1	7.1
	(0.5)	(5.6)	(0.5)	(0.5)
σ_{ah}		7.2		6.9
		(5.8)		(1.7)
Observations	58	58	58	58
R^2	0.993	0.985	0.993	0.985

Notes: The coefficients on the age group specific supply of medium-skilled workers, $\ln M_{jt}$, are restricted to be the same in model 2's pair of equations, i.e. by assumption $\sigma_{am1} = \sigma_{am2}$. The number of observations refers to the full sample, n. Young is an indicator for age $\leq$ 30 years. Moving block bootstrap standard errors with block length 3 and 500 replications in parentheses. ***/**/* indicate significance at the 1%/5%/10% level.

different from each other (p-value of equality is 0.96). Therefore, in model 2, we constrain this coefficient to be the same across the two premium equations. Our estimates remain stable and the coefficients of the age-specific relative supply of high- to medium-skilled workers ($\ln H_{jt}$) becomes highly significant.[44] The magnitude of the coefficients are in line with expectations. Within the group of low-skilled workers, the young and old are close substitutes with an estimated σ_{al} of nearly 15. Medium- and high-skilled workers of the two age groups are estimated to be imperfect but relatively close substitutes with an elasticity of around 7 in both groups.

Our estimates on the medium- and high-skilled age specific relative labor supplies of about -0.14 are close to -0.16 which Card and Lemieux (2001) obtain for both for Canada (their Table III columns 5-6) and the US (their Table V column 1) when using a broader measure of college labor similar to ours[45] or when they allow the elasticities to be different for college and high-school labor (-0.18, their Table VII, column 2). D'Amuri et al. (2010) also use German IAB data to estimate the impact of immigration on native wages and employment. Instead of age groups they use potential experience along with the same three skill groups as we do here. Their comparable estimate of the education-experience specific labor supply is about -0.30 (their Table 7, columns 1-2) implying an elasticity of substitution between different *experience* groups of about 3.2, somewhat lower than our estimates. Fitzenberger et al. (2006) estimate σ_{al} between 8.7-10.3, σ_{am} 5.3-6.0, and σ_{ah} 8.5-20.1. Our elasticities are thus slightly higher for low- and medium-skilled workers and somewhat lower for high-skilled workers.

[44] The large standard errors of the coefficients of the high to medium premium equation in model 1 are due to some extreme (and positive) estimates in some of the bootstrap samples.

[45] In their broad measure, Card and Lemieux (2001) include those with 16 and more years of education opposed to only those with exactly 16 years which is similar to our measure of high-skilled labor that includes all individuals with a tertiary degree (college, university, or PhD) and not just those with say a university degree.

B.8 Estimating α_s

Using the estimates for σ_a, we can back out the age group specific efficiency parameters α_{st} by rewriting equations 2.2-2.4 as follows:

$$\begin{aligned}
\widetilde{w}_{jt}^{L} &= \ln w_{jt}^{L} + \frac{1}{\sigma_{al}} \ln L_{jt} &&= \ln \alpha_{lj} + \ln \left[Y_t^{1-\gamma} (1-\lambda_t) U_t^{\gamma-\rho} (1-\theta_t) L_t^{\rho-\eta_l} \right] \\
\widetilde{w}_{jt}^{M} &= \ln w_{jt}^{M} + \frac{1}{\sigma_{am}} \ln M_{jt} &&= \ln \alpha_{mj} + \ln \left[Y_t^{1-\gamma} (1-\lambda_t) U_t^{\gamma-\rho} \theta_t M_t^{\rho-\eta_m} \right] \\
\widetilde{w}_{jt}^{H} &= \ln w_{jt}^{H} + \frac{1}{\sigma_{ah}} \ln H_{jt} &&= \ln \alpha_{hj} + \ln \left[Y_t^{1-\gamma} \lambda_t H_t^{\gamma-\eta_h} \right].
\end{aligned}$$

The terms on the left hand sides can be computed using the estimated σ_{as} either assuming that they are constant (Table 2.2) or allowing them to differ across skill groups (Table B.3). The α_{st}'s can be recovered from regressions of the above equations where the first terms on the left hand side are captured by a dummy for being young and the second terms by a set of time dummies. This is done in Table B.4. Our moving block bootstrap takes account of the uncertainty due to the generated regressors. We interpret the results in the main text.

Table B.4: Estimating the Efficiency Parameters α_{sj}

	(1) Constant σ_a			(2) Unrestricted σ_{as}		
	$\widetilde{w}_{jt}^L$	$\widetilde{w}_{jt}^M$	$\widetilde{w}_{jt}^H$	$\widetilde{w}_{jt}^L$	$\widetilde{w}_{jt}^M$	$\widetilde{w}_{jt}^H$
Young	-0.318***	-0.368***	-0.618***	-0.247***	-0.389***	-0.663***
	(0.018)	(0.016)	(0.020)	(0.030)	(0.017)	(0.063)
Constant	4.461***	4.831***	5.089***	4.381***	4.882***	5.109***
	(0.027)	(0.043)	(0.034)	(0.033)	(0.035)	(0.036)
Time FEs	✓	✓	✓	✓	✓	✓
α_s	0.73	0.69	0.54	0.78	0.68	0.52
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.03)
Observations	58	58	58	58	58	58
R^2	0.982	0.988	0.994	0.968	0.986	0.994

Notes: $\widetilde{w}_{jt}^S = \ln w_{jt}^S + {}^1\!/_{\sigma_{as}} \ln S_{jt}$. The α_s's are the exponentiated coefficients of the young indicator. The standard errors of the α_s are put in parentheses below. The number of observations refers to the full sample, n. Young is an indicator for age ≤ 30 years. Moving block bootstrap standard errors with block length 3 and 500 replications in parentheses. ***/**/* indicate significance at the 1%/5%/10% level.

B.9 Construction of Migrants' Age-Skill Shares in Labor Supplies

- **Foreign Workers** In the IAB-data, German nationality can be directly observed. We define as foreigners all individuals who are at least once either classified as non-German or have missing nationality information. The shares of foreigners in each age-skill group are then directly computed from the IAB-data.
- **East Germans** In previous work (e.g. D'Amuri et al. 2010), East Germans have been identified in the IAB-data by classifying all individuals who are first registered in East Germany. The problem with this approach is that spells in East Germany are only reliably recorded from 1992 onwards (vom Berge et al. 2013, p. 21), but substantial inflows of East Germans already occurred in 1989-1991 (see Figure B.11). To construct the stock of East Germans in the West German labor supply, we therefore rely on external data, namely the 1991/92, 1998/99, 2005/06 and 2012 waves of the BIBB/IAB- and BIBB/BAuA-Surveys of the Working Population, which are representative cross-sectional surveys of the working population in Germany covering about 20,000-30,000 individuals per year. We identify East Germans using the place of birth (wave 1991/92), the region where an individual grew up (wave 1998/99), or information on whether an individual obtained any kind of school or tertiary degree from East Germany (waves 2005/06 and 2012).[46] We can then calculate the share of East Germans in each age-skill cell of the West German labor force. We set the share of East Germans to zero in 1980 and then use the official net-inflow rates in Figure B.11 to interpolate between waves, i.e. we assume that x% of the difference in shares between two BIBB years is closed in the years in which x% of the overall inflow between those years occurred.
- **Ethnic Germans** Ethnic Germans cannot be identified in the IAB-data since, upon arrival, they were given German citizenship and are thus indistinguishable in the data from native West Germans. We therefore use Microcensus waves 2005-11 to calculate the necessary age-skill shares. To identify ethnic Germans, we focus on private households at their main place of residence in

[46] In waves 2005/06 and 2012 we are thus not able to identify individuals who finished their high school degree after German unification and then directly moved to West Germany to work or obtain further qualification.

West Germany who are born outside today's Germany (including East Germany) who have the German citizenship and who have migrated to Germany since 1980. Reassuringly, a comparison of ethnic Germans identified in this way in the Microcensus by year of arrival and official inflow figures from Bundesverwaltungsamt (2016) shows a close correspondence of the two (compare Figure B.12). To then calculate, for instance, the share of young low-skilled ethnic Germans in a given year between 1986-2008, we calculate the number of ethnic Germans who were 30 or younger in that year, had immigrated to Germany between 1980 and the year of interest and are low-skilled, and divide it by the total number of individuals of that same age-skill cell in that year. Thus, migration rates and age-skill shares are obtained retrospectively from individuals living in West-Germany sometime between 2005-11. Since out-migration of ethnic Germans was basically a "non-issue" as pointed out by Hirsch et al. (2014, p. 213), and to the extent that labor force participation and mortality of ethnic and native Germans are comparable, this approach yields reliable estimates of the necessary quantities.

- **Native Efficiency Supplies** Once we obtain the complete time series of all age-skill shares for each of the three migrant groups, we deduct the corresponding portions in each age-skill cell from our total migrant-including efficiency supplies to obtain the native efficiency supplies used in the counterfactual simulations of the no-migration scenario.

Figure B.11: Net Official East-West Migration (Statistisches Bundesamt 2014)

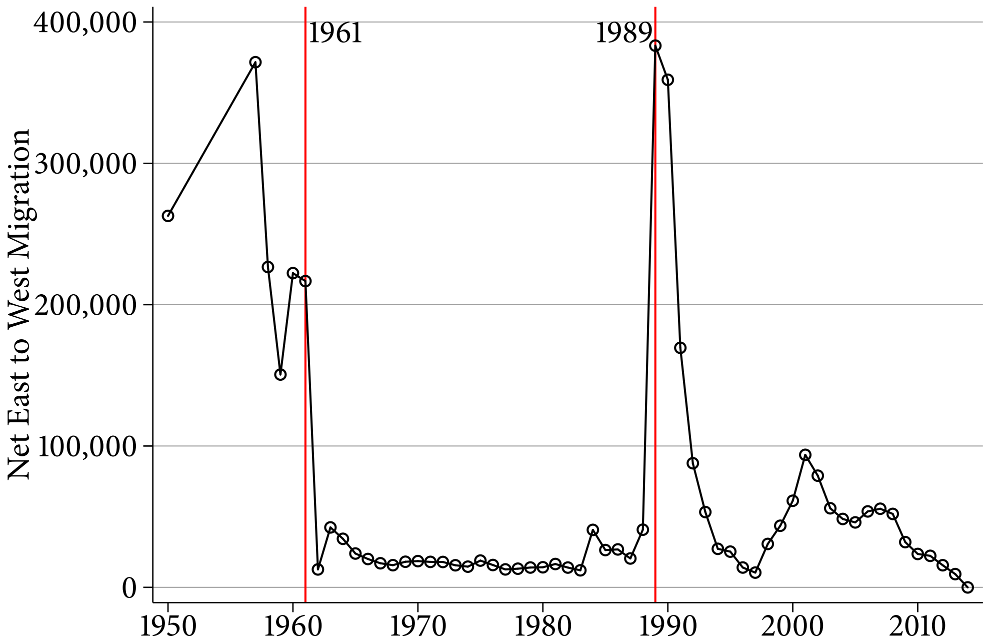
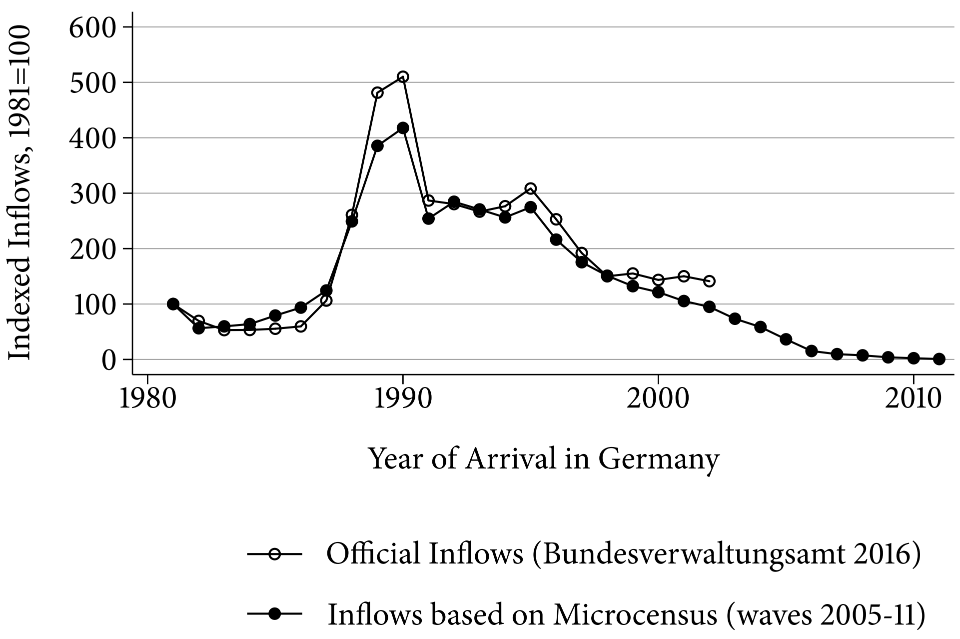


Figure B.12: Yearly Inflows of 18-59 Year Old Ethnic Germans (West Germany w/o Berlin)



Chapter 3

Compensating Differentials and the Introduction of Smoking Bans in Germany*

"The whole of the advantages and disadvantages of the different employments of labour and stock must, in the same neighborhood, be either perfectly equal or continually tending to equality. If in the same neighborhood, there was any employment evidently either more or less advantageous than the rest, so many people would desert it in the other, that its advantages would soon return to the level of other employments."

Adam A. Smith (1776, p. 111)

3.1 Introduction

How do workers' wages react to a sudden improvement in working conditions? The theory of compensating wage differentials (A. Smith 1776; Rosen 1986) suggests that in a competitive labor market workers must be offered a wage premium to offset any disutility associated with unpleasant attributes of a given job relative to another otherwise comparable one. These negative attributes may comprise non-standard working hours, unpleasant tasks, or health related hazards such as being exposed to second hand smoke. This idea also becomes important when understanding inequality: Earnings inequality might significantly overstate utility inequality precisely because some jobs need to pay compensating differentials to compensate for

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certain disamenities. In line with this reasoning, Sorkin (2018) estimates that about 15% of the variation in US earnings can be explained by compensating differentials.

However, despite their importance as a classic concept in economics, establishing empirical evidence for compensating differentials has proven to be challenging. Existing studies are mostly plagued by confounding selection effects that cannot be separated from the effect of interest and the lack of appropriate identifying variation. Thus, it seems as true today as some thirty years ago when Duncan and Holmlund (1983, p. 367) noted that "[Adam Smith's] intuitive statement has [...] shown surprising resistance to empirical confirmation".

In this paper, I exploit a natural experiment that can help to overcome some of the previous challenges in estimating compensating differentials. Guided by a simple compensating differentials framework, I use the introduction of smoking bans in restaurants, bars and clubs in the German federal states in 2007/08 to study their effect on the wages and other labor market outcomes of workers in these businesses. I argue that this setting has several appealing features that facilitate identification. First, smoking bans were highly effective at reducing the amount of harmful airborne particles, a claim backed by representative indoor air quality measurements taken before and after the implementation of smoking bans by the German Cancer Research Center. Second, smoking bans were rolled out over a thirteen-month period across all German states with the introduction dates of individual states being uncorrelated to a host of potential predictors and thus creating arguably exogenous variation in treatment status across time and space. Third, smoking bans also varied in their intensities thereby creating additional variation that I exploit by constructing an index capturing the strictness of different smoking bans. What is more, the intensities of smoking bans in some states were altered from one day to another due to a rather unexpected ruling by the Constitutional Court, thus creating additional, arguably exogenous variation along the intensity dimension. Fourth, the smoking bans of 2007/08 were targeted at restaurants, bars, and dancing clubs while leaving smoking regulations for other occupation groups unaffected. This adds yet another layer of variation which enables me to not only rely on variation across time, space and intensity but also across occupations strengthening the credibility of my findings.

To estimate the effect of smoking bans on the wages of hospitality workers, I use high-quality, large sample administrative labor market data. In most of my analysis, I will focus on workers in so-called *mini jobs* – a flexible part-time contract ubiquitous in the German low wage sector exempted from most social security and tax payments – as I argue that in this less rigid segment of the labor market a new equilibrium can emerge more quickly. Employing either a conventional difference-in-differences (DD) strategy across states and time or a triple difference-in-differences (DDD) approach using unaffected occupations as an additional control group, I find that the most comprehensive smoking ban in the sample led to a 2.4% decline in wages of these workers.

In the following, I address several concerns that could potentially cast doubt on a causal interpretation of my findings. Performing a battery of robustness checks including several placebo and permutation exercises, I find no violation of the parallel trends assumption or any evidence that the effect would be confounded by seasonal effects, a specific choice of the time period, the weights used in the index definition, outliers, foreign or domestic tourist demand, election cycles, or coincidental variation in weather variables. These results suggest that the effect can indeed be interpreted as causal.

I then set out to study potential mechanisms behind the decline in the wages of waiters. One commonly proposed channel is that smoking bans resulted in lower revenues of bars and restaurants which consequently led to lower wages. Using official revenue data from the German Statistical Office, however, I find no evidence for that claim once properly accounting for seasonal variation in the data. The effect also remains virtually unchanged when I control for revenues directly in my wage regressions. Nevertheless, the revenue data might be too noisy or – since only available at the state level – too coarse to accurately reflect changes in demand. Therefore, I test the revenue channel from two different angles. First, a decline in revenues would plausibly also affect the wages of other workers in the hospitality industry. The robustness of the smoking ban coefficient on waiters' wages in my DDD strategy thus provides further evidence against a revenue decline acting as the main channel. Second, if demand after the implementation of smoking bans indeed went down because patronage by smokers went down, we should see a larger decline in states with an initially higher share of smokers. However, including the initial share of smokers in a state interacted with time effects leaves the effect of interest basically unchanged. Further evidence also suggests that there is no increased closure or start-up activity of hospitality establishments associated with the introduction of smoking bans. Taken together, it thus seems unlikely that the effect is driven by a decline in revenues or a change in the business landscape.

Another potential explanation for the decline in wages is a decrease in the hours worked. Since wages in the administrative labor market data are only reported as *daily* wages (i.e. the product of the hourly wage times hours worked) and the hours worked are not observable, I draw on a compulsory labor market survey (Microcensus) in which the hours worked are reported to study the validity of the hours channel. Using either a synthetic control group approach with other occupation defining the donor pool of potential control units or a triple difference-in-differences approach exploiting variation between states and occupation groups I find no support for a decline in the hours worked of workers in bars and restaurants. If anything, the DDD estimates indicate a positive though insignificant increase in hours worked by waiters.

I argue that my findings are consistent with a simple compensating differentials model. If the marginal worker – all other amenities remaining equal – positively values a smoke-free environment, economic theory suggest that she should be willing to give up part of her wage in exchange. Additionally, individuals who previously were not willing to work in smoke-allowed restaurants or bars might now be induced to look for a job in the hospitality sector. Both effects will unambiguously result in lower wages. The effect on equilibrium employment, however, depends on the elasticity of labor demand.

My setting provides an ideal testing ground for these predictions. Due to many non-unionized workers and low qualification requirements on the supply side and many small firms and low entry barriers on the demand side coupled with the absence of a minimum wage, relatively high turnover rates and rather flexible employment regulations, the labor market for waiters in Germany comes close to the textbook case of perfect competition and thus the new equilibrium is expected to emerge quickly. In line with the prediction of such a simple compensating differentials model, I find some evidence for positive selection of workers induced by the introduction of smoking bans. Further analyses suggest that the main bulk of selection is driven by unobservable rather than observable characteristics. Also consistent with the theory of compensating differentials, I find some suggestive evidence for higher turnover and a moderate increase in employment.

This paper contributes to at least two strands of the literature. First and most importantly, I contribute to the literature related to the empirical measurement of compensating differentials. Evidence for compensating differentials have been found in some (specific) cases including shift-work (Kostiuk 1990; Lanfranchi et al. 2002), employer-sponsored health insurance (Kolstad and Kowalski 2016), and fatal and non fatal injury risks (Leeth and Ruser 2003; Galick 2014). However, many studies find insignificant or wrong-signed estimates such that Sorkin (2018, p. 1) concludes that the "conventional view is that... it is hard to find robust evidence that non-pay characteristics are priced in the labor market". In a similar vein, Lavetti (2015) describes the estimation of compensating wage differentials "a classic topic in labor economics that has long been considered notoriously difficult to solve".[1] The most prominent issue noted e.g. by Duncan and Holmlund (1983) and Galick (2014) is self-selection of workers into different jobs.[2] In this paper, I exploit a panel of workers which enables me to control for unobserved fixed worker characteristics, a feature I share with a few other papers (e.g. Duncan and Holmlund 1983; Galick 2014). Identification in these panel studies relies on within-worker job changes. However, as Lavetti (2015) shows, job changes themselves and amenities offered by firms are endogenous which can exacerbate bias in panel studies.[3] In contrast to these papers, I can rely on an arguably exogenous variation in amenities *within* jobs (smoking bans "shocked" existing firm-worker pairs) while still controlling for

[1] For similar assessments see R. S. Smith (1979), Brown (1980), Bonhomme and Jolivet (2009), and Hornstein et al. (2011)

[2] Another important issue are (non-classical) measurement errors of disamenities, e.g. resulting from survey data or low probability events (Black and Kniesner 2003). This issue does not arise in my setting, however, since treatment status is perfectly observable and applies to a large group of workers.

[3] Workers who change jobs in (frictional) labor markets tend to move to jobs that both pay more and offer better non-wage amenities (compare Lavetti 2015). In this context, one might wonder whether the decision to stay at a certain job is also endogenous. However, as Lavetti (2015, 12f) argues, using within-job variation holds latent fixed firm wage effects (the potentially omitted variable) constant thus – mechanically – fixed firm wage effects cannot be correlated with within-job variation in amenities. The decision to leave a job thus affects the representativeness of the sample but not identification.

worker fixed effects.[4] To the best of my knowledge, Lavetti (2015) is the only other paper to apply a similar research design. Relative to Lavetti (2015) who studies compensating differentials related to fatal risks of commercial fishing deckhands in the Alaskan Bering Sea based on survey data, I can rely on a large administrative data set to exploit a relatively broad, economy wide natural experiment studying the compensating differential of a non-fatal health amenity.

A second issue that complicates the empirical establishment of compensating differentials noted by Bonhomme and Jolivet (2009) is the existence of labor market frictions. In particular if job search is costly and plagued by incomplete information related to job-specific (dis-)amenities, compensating differentials might be small or non-existent even when workers exhibit a non-zero marginal willingness to pay for these amenities. In my setting, these concerns are likely to be of less importance. First, as I focus on workers in mini jobs, regulatory frictions and wage rigidities should be less prevalent than in the case of full-time jobs which most previous studies are based on. Second, the existence of smoking bans in bars and restaurants are a very salient and commonly known job feature for any existing or potential worker and thus incomplete information does not constitute a major impediment in the estimation.

Second, my study also relates to a series of papers that evaluate the impacts of smoking bans on various health and economic outcomes. Most epidemiological studies largely agree on the positive impact of smoking bans on air quality and health outcomes of hospitality workers. For instance, Repace et al. (2006) find that Boston's 2003 smoke-free workplace law (granting no exceptions) led to a 95% reduction in respirable particle pollution in bars and pubs while in case of Germany's smoking bans implemented over 2007/08 (granting some exceptions), the German Cancer Research Center (DKFZ 2010) finds a reduction by up to 82% (I will discuss these results in more detail in section 3.3). Goodman et al. (2007), studying Ireland's 2004 complete smoking ban, find that the air quality improvements associated with smoking bans also translate into large and sustainable health improvements of nonsmoking bar staff in terms of the pulmonary function and respiratory and irritant symptoms in the short and longer run. Carton et al. (2016) and Anger et al. (2011) find that smoking bans in the US and Germany,

[4] As a robustness check, I also include worker-firm fixed effects and my results still hold.

respectively, significantly reduced smoking prevalence among specific subgroups of the population such as young or low-income individuals. Kuehnle and Wunder (2013) also find significant health externalities for young non-smokers while Adda and Cornaglia (2010) highlight that public smoking bans may *increase* children's and other non-smokers' exposure to tobacco smoke as smokers shift cigarette consumption to their private homes. Finally, Adams and Cotti (2008) exploit geographic variation in local and state smoke-free bar laws in the US finding that alcohol-related fatal accidents increased as smokers drive longer distances to bars that still allow smoking.

The evidence related to the effect of smoking bans on revenues of bars and restaurants is mixed (Adda et al. 2007; Pakko 2008; Adda et al. 2009; Ahlfeldt and Maennig 2010; Kvasnicka and Tauchmann 2012), with some studies finding negative and some finding insignificant or positive impacts on revenues.[5] More often than not, however, these studies are based on research designs that render a causal interpretation of the findings difficult, for instance by relying on self-reported data by business owners, pure time-series before-after comparisons, or inadequate accounting for seasonal variation in sales data. Finally, Adams and Cotti (2007) find a significant decrease in employment related to the introduction of smoking bans in the US while Thompson et al. (2008) find a significant short-term decrease in employee turnover. To the best of my knowledge, my paper is the first to look at the effect of smoking bans on wages of hospitality workers.

The rest of the paper is organized as follows. Section 3.2 presents a simple compensating differentials framework in the context of introducing a smoking ban. Section 3.3 then provides the institutional and medical background regarding the implementation of smoking bans in the German hospitality industry. Section 3.4 describes the data, explains the identification strategies, and presents the estimation results regarding the effect of smoking bans on waiters' wages along with a set of robustness checks. Section 3.5 discusses several potential channels before section 3.6 concludes.

[5] See Scollo and Lal (2008) for a survey. I will review the related evidence for Germany in more detail in section 3.5.1.

3.2 A Simple Model

In this section, I study the effects of introducing a smoking ban in a simple labor market model characterized by compensating differentials. The aim is to fix intuition, discuss the underlying assumptions, and derive predictions that will inform my empirical analysis. To start, consider a labor market that satisfies the following assumptions:

A.1 Markets are competitive.

A.2 The marginal worker values working in a smoke-free environment.

A.3 The labor demand schedule is fixed.

A.4 Labor supply is not completely inelastic.

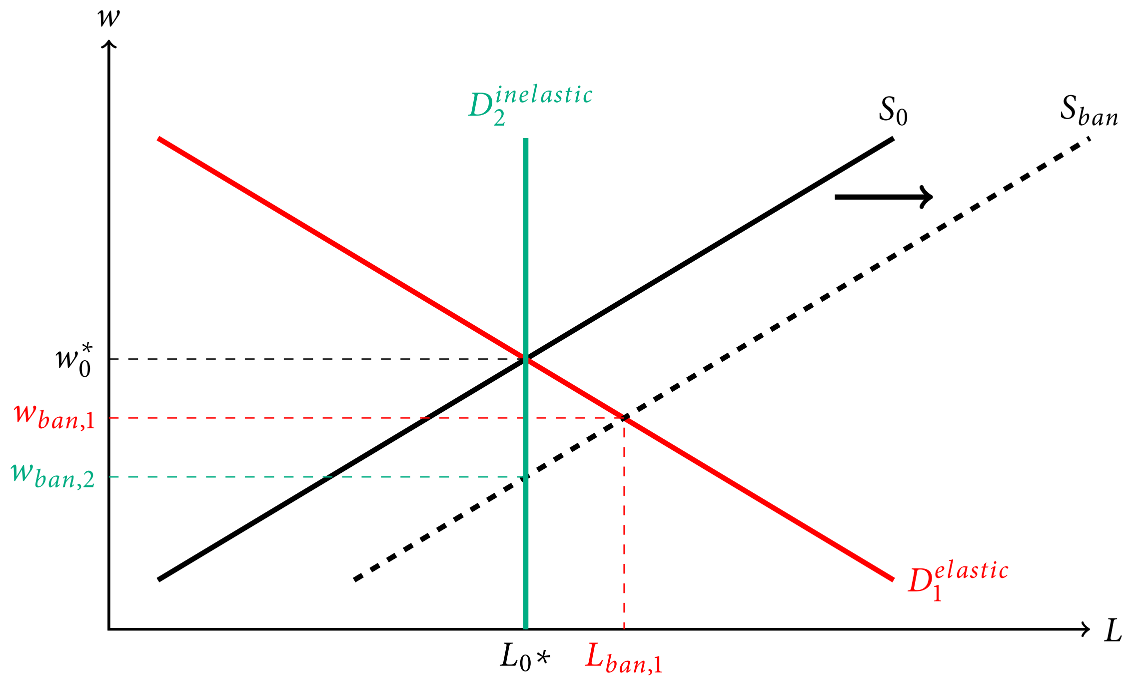
A.5 Individual productivities are held constant.

Given these assumptions, Figure 3.1 illustrates how the introduction of a smoking ban affects a stylized labor market for jobs in hospitality establishments in which smoking is allowed. Initially, there is no smoking ban and the equilibrium wage and employment are denoted by w_0^* and L_0^*, respectively. Given that the marginal workers dislike being exposed to second-hand smoke, the introduction of a smoking ban shifts aggregate labor supply from S_0 to S_{ban} to the right. Working in a bar or restaurant is now less unpleasant and therefore wages are lower than before. This is because with a smoking ban in effect a firm needs to pay the marginal workers a smaller compensation to convince her to work for the firm. The new equilibrium depends on the elasticity of labor demand. Given an elastic labor demand curve $D_{1,elastic}$, wages will decrease to $w_{ban,1}$ and employment will increase to $L_{ban,1}$. If labor demand is inelastic and fixed as $D_{2,inelastic}$ – e.g. in the short run or when product demand remains unchanged[6] – then wages decrease to $w_{ban,2}$ while employment stays constant.[7] Summarizing, given A.1-A.5 the simple model yields two predictions regarding the change in wages and employment:

[6] And given a production function in bars and restaurants that allows for only a limited substitutability between capital and labor.

[7] The assumption of an inelastic labor demand in the short run seems to be in line with the consensus of the minimum wage literature where the elasticity of restaurant employment with respect to the (minimum) wage is small and mostly insignificant (compare Neumark et al. 2014; Neumark 2015).

Figure 3.1: Compensating Differential when Introducing a Smoking Ban



Notes: This figure illustrates a compensating differential as part of waiters' wages and the corresponding wage change when a smoking ban is introduced at their workplace. See main text for a more detailed explanation of the figure.

P.1 Wages will unambiguously decrease.

P.2 The change in employment is non-negative. Whether it remains unchanged or increases depends on the elasticity of labor demand.

How sensible are assumptions A.1 - A.4? I argue that they are a reasonable approximation to the context of the German labor market for workers in the hospitality industry. First, the hospitality industry in Germany consist of a large number of mostly small firms. According to calculations from the Federal Statistical Office (Statistisches Bundesamt 2001), competition between restaurants, cafés, bars, and dancing clubs – the types of establishments I expect most treated workers to work in – is fierce with one of the lowest Herfindahl-indices (a measure for market concentration) of all sectors in the German economy. On the supply side, qualification requirements for hospitality workers are generally low and firms can draw on a large pool of suitable workers such as students or second-income earners. Most of these jobs are mini jobs that allow for a flexible allocation of hours and offer some exemptions from social security contributions and other regulations (see Section 3.4.1 for more details). Furthermore, during the period studied, there was no minimum wage in Germany and unionization rates among hospitality workers were low.[8] Thus, assumption A.1 seems plausible.

Assumption A.2 requires that the marginal worker prefers working in a smoke-free environment over a non smoke-free one. Note that the equilibrium is determined by the *marginal* worker, i.e. the last worker hired, not the average worker. Figure C.1 shows that depending on the state, about 30-39% of the population between 17 and 62 years were regular or occasional smokers in Germany in 2005.[9] Table 3.1 shows smoking behavior in the population and among waiters. Although the share of regular smokers is higher among waiters (43.3% of waiters compared to 29.7% in the general population in 2005), at least about half of all waiters are non-smokers (Panel A). Conditional on smoking, waiters do not differ much in the amount of cigarettes consumed (Panel B). In Table 3.2, I estimate linear probability models of the effect of working as a waiter in 2005 (base category) and 2009 on being a smoker, i.e. being a regular or occasional smoker. I estimate models separately for all individuals (whether employed or not, columns 1 and 2) and for the group of mini job workers (in all occupations, columns 3 and 4). The table shows that the differences in smoking behavior between waiters and the general population do not seem to be driven by compositional differences as the estimates when controlling for a broad set of socio-economic characteristics or not do not differ by much. The table also shows that – if anything – waiters have decreased their gap in smoking propensity relative to non-waiters over the 2005 to 2009 period although this effect is estimated imprecisely. The bottom line is that even though waiters do have a higher likelihood of smoking, still about half of them are non-smokers. If one is willing to assume that smoking behavior is negatively related to the willingness

[8] According to Jacqueline Vogt (2007), the unionization rate in 2007 in the entire hospitality sector was below 10% which includes full-time employees in hotels and catering firms. Focusing on the employees working as waiters, the percentage is likely to be (much) lower, as most of the employees are mini job workers working in small firms (Frese 2015).

[9] For comparison smoking prevalence among males (smoking rates only available by gender) in 2005 was 36% in Germany, 35% in France, 59% in Greece, and 25% in the US according to WHO data (World Health Organisation 2017).

Table 3.1: Smoking Behavior among the Population and Waiters)

	2005			2009		
	(1)	(2)	(3)	(4)	(5)	(6)
	Population	Waiters	Waiters (Mini Jobs)	Population	Waiters	Waiters (Mini Jobs)
Panel A: How Often do you Smoke (%)?						
Regularly	29.7	43.3	42.4	29.6	39.2	38.5
Sometimes	4.8	5.6	8.1	4.8	6.0	6.9
Never	65.5	51.1	49.5	65.6	54.8	54.6
Observations	140,513	1,919	428	188,809	2,207	503
Panel B: How many Cigarettes do you Smoke per Day (%)? (if Smoking)						
1 to 5	13.9	10.6	14.1	14.3	12.7	16.5
5 to 20	70.8	72.9	75.3	72.7	74.2	75.0
12 to 40	14.4	15.3	10.2	12.3	12.4	7.1
41 and more	0.9	1.3	0.4	0.7	0.6	1.3
Observations	45,594	888	209	61,303	962	213

Notes: This table shows descriptive statics regarding the smoking behavior of the general population, waiters, and waiters in mini jobs in 2005 and 2009. The sample is based on Microcensus waves 2005 and 2009 and is restricted to individuals aged 17-62 not in civil service (*Beamte*) and with non-missing values the control variable values used in Table 3.2. Waiters are defined as those working in occupation groups 911 and 912. Mini job holders are those indicating that their main current job is a mini job. The questions regarding smoking behavior are not compulsory in the Microcensus. Statistics are weighted by survey weights.

to pay for a decrease in second hand smoke exposure then it seems plausible that the *marginal* hospitality workers has a positive valuation for a smoke-free work environment as stipulated by assumption A.2.

In the empirical analysis I will provide evidence that support the hypothesis of a constant demand curve (assumption A.3) by looking at revenues of bars and restaurants as a proxy for labor demand.[10]

Although I have no direct evidence to back up assumption A.4 – that labor supply is not completely inelastic – it seems fair to assume that – given the low training requirements and flexible schedules of students or other individuals typically working as (part-time) waiters – labor supply could relatively easily vary along the extensive and intensive margins.

Finally, A.5 requires that individual productivities are held constant. In the empirical estimations, I try to achieve this by including individual fixed effects to hold all time invariant characteristics constant, in particular unobserved traits such as motivation, friendliness, or sales talent that are likely to influence individual wages (apart from tips) and hours worked. Including individual fixed effects thus helps to control for unobserved selection of more able or productive individuals into waiter jobs as a consequence of smoking bans. I will explore the issue of selection in more detail in Section 3.5. It could also be the case that the *same* individual becomes more productive after the introduction of smoking bans. Although I have no way to control for such a time-varying unobserved effect, this effect should lead to *higher* wages and would thus work against me by biasing my estimates upwards.

3.3 Background

According to the American Cancer Society, second hand smoke contains at least seventy substances that can cause cancer and that carry the risk of heart attacks, strokes, and chronic lung diseases. Harmful particles from tobacco smoke stay in rooms and remain a hazard even without anyone present and smoking. Employees working in hospitality establishments not covered by smoking bans are among the most exposed occupation groups and are estimated to have a 50% higher risk

[10]Note that this assumption does not require that the labor demand schedule remains fixed at a certain amount but rather that labor demand does not shift inward or outward. Thus, shifts of the labor supply curve *along* the labor demand curve are possible.

Table 3.2: Linear Probability Models of Being a Smoker

	Dependent Variable: *Smoker Yes/No*			
	All		Mini Jobs	
	(1) No Controls	(2) Controls	(3) No Controls	(4) Controls
Waiters	0.147***	0.131***	0.156***	0.143***
	(0.009)	(0.010)	(0.023)	(0.023)
Waiters × 2009	-0.037**	-0.034*	-0.055	-0.048
	(0.017)	(0.017)	(0.034)	(0.036)
State FEs	✓	✓	✓	✓
Controls		✓		✓
Observations	329,322	329,322	25,233	25,233
Clusters	16	16	16	16
Adj. R^2	0.004	0.055	0.017	0.072

Notes: This table presents linear probability (OLS) models with the dependent variable being a binary indicator that is one if the individual responds to be a regular or occasional smoker and zero otherwise. The sample is based on Microcensus waves 2005 and 2009 and is restricted to individuals aged 17-62 not in civil service (not *Beamte*) and with non-missing values of the control variable values. Waiter is a dummy indicating that the individual is in occupation groups 911 or 912. Controls include dummies for the year 2009, female, having a partner, children under 18 in the household, German, being born in Germany, holding a mini job; indicators for each of three education categories, eight age categories and eight city size categories; the amount of weekly hours, log net income and the state level monthly unemployment rate. Mini job holders are those indicating that their main current job is a mini job. The questions regarding smoking behavior are not compulsory in the Microcensus. Statistics are weighted by survey weights. Standard errors clustered at the census region. ***/**/* indicate significance at the 1%/5%/10% level.

of dying from lung cancer even if they are non-smokers themselves (Siegel 1993). According to Jamrozik (2005), 1.4% of all British non-smoking hospitality workers are estimated to die in the long run due to their exposure to second hand smoke.

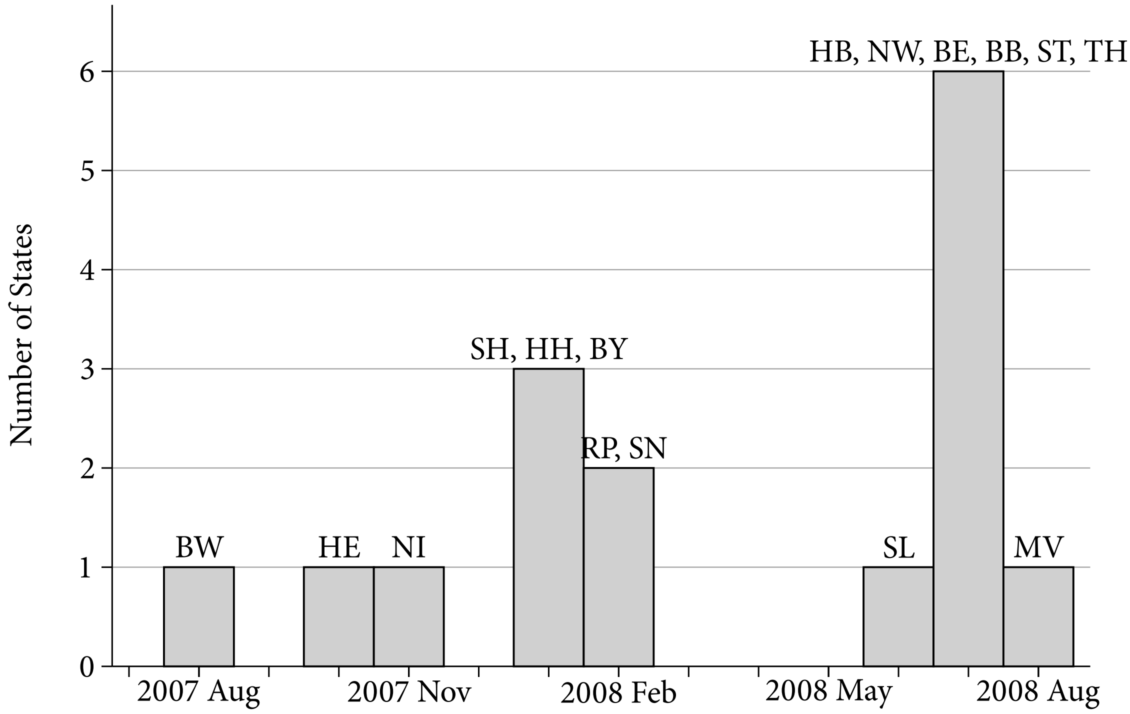
In 2007, the European Cancer League published a report reviewing tobacco control activities in Europe taking into account the price of tobacco products, the protection from second hand smoke via smoking bans, the regulation of advertising, and other indicators. In this report, Germany ranked 27 out of 30 countries and was described as "the biggest problem for tobacco control in Europe [due to its] well established connections with the tobacco industry" (Luk Joossens and Raw 2007, p. 12). In the wake of such reports and a growing number of Western countries implementing smoking bans, anti-smoking sentiment in the general population was growing in Germany. According to a survey conducted by the German Cancer Research Center (DKFZ 2006), in 2006 a majority of 59% was in favor of smoking bans in bars and restaurants. Against this backdrop, in early 2007 the federal states decided to implement smoking bans in public places including bars and restaurants "within the next months" (Bundesrat 2007, p. 4).[11] In doing so, the states had some leeway in deciding *when* and *how strict* a ban they would implement. Subsequently, between August 2007 and August 2008, 16 different smoking bans became effective.[12] Figure 3.2 shows the implementation dates of the different smoking bans.

The smoking bans differed along four components: whether or not (i) restaurants and bars could install a separate smoking room, (ii) dancings clubs could install a separate smoking room, (iii) small pubs could choose to be smoke free or not, and (iv) smoking was allowed in party tents. All states but Bavaria granted larger bars and restaurants the possibility to install a separate smoking room. 10 out of 16 states allowed dancing clubs to install a separate smoking room. Only Rhineland-Palatinate gave small single-room pubs the opportunity to opt out of implementing a smoking ban. A complete overview of the initial regulations in

[11] Anti-smoking regulation in public places is a matter of the states except for regulations concerning public transportation, the workplace and federal buildings which are at the discretion of the federal government.

[12] Throughout, I rely on the *effective* introduction of smoking bans, i.e. the date when a ban was officially enforced by sanctions. In a robustness check, I show that my results remain robust when using the *legal* start of a ban (see Table C.5.

Figure 3.2: Introduction Dates of Smoking Bans in the German States



Notes: This figure plots when and how many states introduced a smoking ban between August 2007 and August 2008 in Germany. The figure shows the *effective* introduction dates, i.e. when violations of a smoking ban started to be sanctioned by law.

the different states along with the introduction dates is given in Table C.1 in the Appendix.

Owners of small bars and dancing clubs challenged some of these regulations claiming they were treated unequally compared to owners of larger bars and restaurants who had the possibility to install separate smoking rooms. In line with their argumentation, on July 30, 2008 Germany's Federal Constitutional Court revoked the respective parts of the smoking ban laws. Minutes after the ruling was made public, restaurants and bars in ten states[13] could return to be smoke-allowed and

[13] The remaining were either not affected (Bavaria and Rhineland-Palatinate) or waited for pending rulings of their respective state courts.

many did so, creating an arguably exogenous variation in the intensity of smoking bans.[14]

To exploit the bans' variation in both time and intensity, I construct an *intensity*-index that aggregates the strictness of the different regulations at different points in time. Specifically, the index is constructed as follows:

$$\text{intensity}_{st} = \mathbf{1}_{st}^{ban}\left[\frac{2 - \omega_{LR}LR_{st} - \omega_{DC}DC_{st} - \omega_{SB}SB_{st} - \omega_{PT}PT_{st}}{2}\right] \in [0,1] \quad (3.1)$$

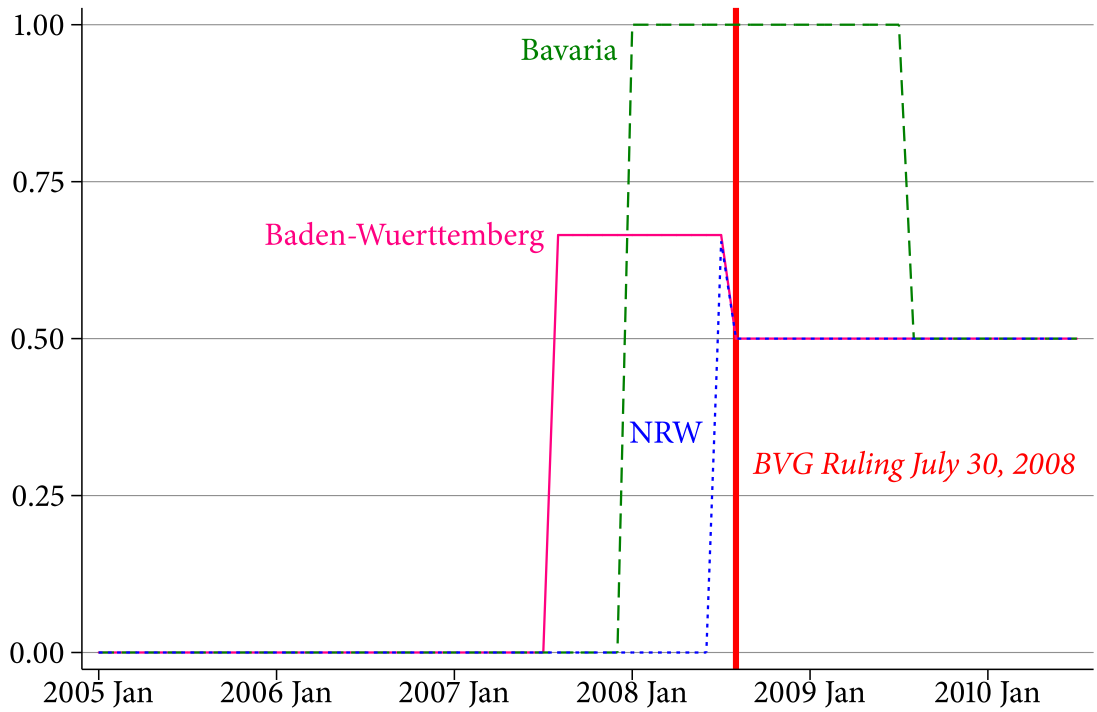
where s refers to state, t to time, and $\mathbf{1}_{st}^{ban}$ is an indicator that is one if a smoking ban is in operation and zero otherwise. $LR_{st}, DC_{st}, SB_{st}, PT_{st}$ are dummies indicating whether or not state s at time t allowed for a separate smoking room in large bars and restaurants (LR_{st}), in dancing clubs (DC_{st}), an opt-out possibility for small pubs (SB_{st}), and party tents (PT_{st}). The ω's denote the corresponding index weights which are derived from the employee shares in the respective establishments in the base year 2007.[15] These index weights are listed in Table C.2. The bulk of workers are employed in large (66% in 2007) and small (30%) bars and restaurants while the share of employees in dancing clubs (3%) and party tents (1%, estimated) is small. Thus, the index will put most weight on the indicators referring to separate smoking rooms and exemptions for small bars. In a robustness check I show that my results are not driven by the specific set of weights but also hold up to using alternative weighting schemes (see Table C.6). By construction, this index is zero if no smoking ban is in operation, 0.5 if a state grants exception in all four categories (Rhineland-Palatinate, weakest ban), and 1 if no exceptions are granted (Bavaria, strictest ban). The intensities of each state's initial smoking ban are tabulated in the last column of Table C.1 and are visualized in a map in Figure C.2. Note that the intensity index is time-varying, i.e. it reflects any changes that occur due to court rulings or the amendments of laws.[16]

[14] See, for instance, a newspaper article by Der Spiegel (2016) which describes how the owner of a pub in Berlin called his employee shortly after the Constitutional Court's decision and told her to put out a "smoking allowed" sign at the pub's door.

[15] For instance, ω_{LR} corresponds to the share of employees in food and beverage serving establishments with 6 or more employees in all employees in food and beverage serving establishments and dancing clubs, see Table C.2.

[16] For instance, Bavaria introduced a strict smoking ban in January 2008 and was not affected by the Federal Constitutional Court's ruling in July 2008. However, on October 1, 2009 the Bavarian

Figure 3.3: Illustration of Variation in Smoking Ban Intensities



Notes: This figure plots the smoking ban intensity index for Baden-Wuerttemberg, Bavaria and Northrhine-Westphalia over the period January 2005 - July 2010. For details on the construction of the intensity index see Section 3.3.

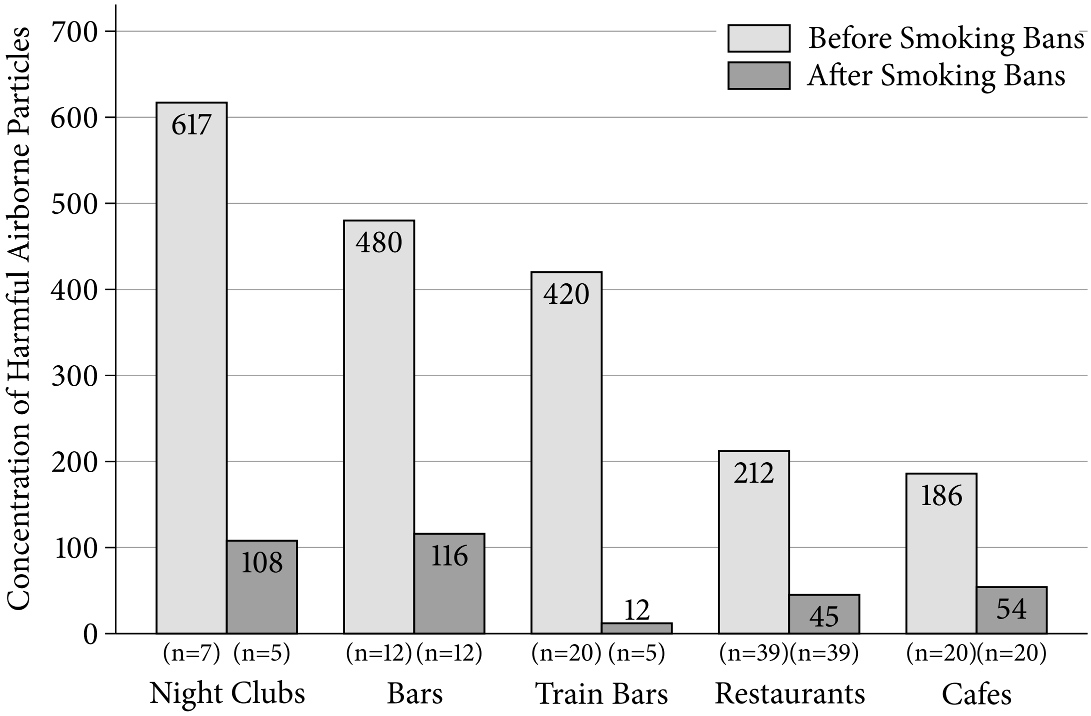
Figure 3.3 illustrates the two dimensions of identifying variation across states for three cases: Baden-Wuerttemberg introduced a moderate smoking ban already in August 2007 and due to the Constitutional Court's ruling had to weaken it in August 2008. Bavaria implemented its strict smoking ban in January 2008, five months after Baden-Wuerttemberg, and its ban remained unaffected by the Court's ruling in July 2008. Finally, North Rhine-Westphalia's ban had to be weakened just a month after its implementation in July 2008.

government weakened parts of the initial ban allowing for separate smoking rooms in restaurants and exempting small pubs altogether as was the case in most other states at that time. This, however, triggered a referendum in favor of an even stricter ban than the initial one which was approved by a 60:40 majority and came into effect August 1, 2010.

As Adams and Cotti (2007) point out, the introduction of smoking bans might be endogenous in the sense that states with a stronger anti-smoking sentiment and a lower prevalence of smokers pass smoking bans earlier in time and choose bans that are stricter. I argue, however, that in my setting these arguments are less of a concern. First, in terms of timing all states agreed to pass a ban within the next months and actually did so over the course of twelve months between August 2007 and August 2008. In fact, as Table C.3 shows, the introduction date is not systematically related to the ban's intensity, the initial share of smokers, the trend in hospitality revenues or hospitality wages, or other potential determinants. Second, as Table C.4 suggests, the intensity of a state's smoking ban, too, does not seem to be significantly correlated with the same set of potential determinants, i.e. in particular states with a higher share of smokers in 2005 did not implement stricter bans. Thus, it seems that the timing as well as the strictness of a smoking ban was rather determined by idiosyncratic factors such as the patterns of parliamentary sessions, administrative concerns, or personal preferences of state legislators.

Were smoking bans in Germany effective, i.e. did they indeed improve air quality in bars, restaurants, and dancing clubs? The German Cancer Research Center measured air quality in a representative sample of hospitality establishments in Germany before and after smoking bans were implemented. The measurements were carried out using an inconspicuous small aerosol monitor during times when clients would typically visit the respective type of establishment. A total of 98 (2005) and 81 (2009) establishments surveyed in 10 cities in 9 states. As Figure 3.4 shows, the amount of harmful particles was reduced dramatically by some 70-80% after the introduction of smoking bans. As smoking was still allowed in (parts of) some establishments, there is still a positive amount of harmful particles left on average. Figure C.3 in the Appendix compares the particle concentration in selected establishments that implemented comprehensive smoking bans granting no exceptions (like Bavaria). In these cases, the amount of harmful particles was virtually completely eliminated. Summarizing, smoking bans in Germany in fact led to a very substantial improvement in the indoor air quality of hospitality establishments and thus effectively reduced the exposure of hospitality workers to harmful particles from second hand tobacco smoke.

Figure 3.4: Air Quality Measurements Before and After the Introduction of Smoking Bans



Notes: This figure compares the average concentration of particle matter (PM) up to 2.5 μm per m^3 measured in the indoor air of five different types of hospitality establishments in Germany before (2005) and after (2009) the introduction of smoking bans. The post measurement for train bars was taken in 2007. Source and more details: DKFZ (2010, 24ff).

3.4 The Effect of Smoking Bans on Waiters' Wages

3.4.1 Data

My prime data source to study the effect of smoking bans on waiters' wages is the *Sample of Integrated Labour Market Biographies* provided by the Institute for Employment Research (IAB). The IAB wage sample is a 2% random sample of the official records of all employees subject to social security and provides data on daily wages and employment status (full-time, part-time, mini job, unemployed, in vocational training) as well as a number of individual characteristics such as age, gender, skill, German nationality, region, occupation, and industry. My baseline samples comprise between 155,000 to 350,000 person-month spells of about 15,000-31,000 different employees aged 17-62 years in East and West Germany between August 2006 and February 2009. Appendix C.2 contains more details on the applied sample restrictions and the construction of variables.

When it comes to studying the wages of hospitality workers it is important to understand the role of tipping. Unlike in the US, where the wage of a typical waiter almost exclusively depends on tips, they are less important in Germany. Customers would typically round up their bill resulting in tips of about 5-10%. Whether waiters can keep these tips or share (parts of) them with their colleagues (e.g. the cooks) varies from establishment to establishment. A common rule seems to be that waiters keep 75% of their tips and share the rest with their non-tipped colleagues. No precise estimate of the share of tips relative to the baseline wage exists, but about 20-30% seems reasonable.[17] In any case, tips are not subject to taxation or social security contributions and thus are thus not recorded in the IAB wage data.[18]

Ideally, I would only select workers who work inside establishments in which smoking is or was formerly allowed (typically these are bars, restaurants, and dancing clubs). However, the data resolution is not fine enough for such an exercise. Therefore, I identify as "waiters" those workers who are employed in the hospitality industry *and* work in "guest attending" occupations. However, this subsample contains some workers such as guest attending workers in youth hostels, ice cream parlors, open-air beer gardens, caterers, or canteens who were likely not exposed to second hand smoke even before the introduction of smoking bans. Similarly, the data does not allow to separately identify hotel and restaurant owners and managers, receptionists, or staff in charge of housekeeping or back office related tasks who are all part of the guest attending occupation group but are unlikely to be affected by the introduction of smoking bans.[19] This will likely result in the attenuation of the estimated treatment effects and thus my coefficients are to be understood as lower

[17] Here, I mainly rely on personal conversations with waiters and restaurant managers, and on information found on the web such as `https://gehaltsreporter.de/gehaelter-von-a-bis-z/hotellerie-gastronomie/Kellner.html` (in German).

[18] Compare Art. 3 Nr. 51 *Gesetz zur Steuerfreistellung von Arbeitnehmertrinkgeldern* as of 08/08/2002.

[19] For instance, the German occupation "Hotelfachmann/ -frau" contained in occupation group 115 (waiters) is related to a wide range of guest attending tasks in hotels including book keeping and other presumably non-second-hand-smoke-exposed tasks.

Table 3.3: Summary Statistics of Individual Wage Data (August 2006 - February 2009)

	(1) All Spells		(2) Regular Jobs		(3) Mini Jobs	
Real Monthly Wage (in 2010 euros)	729.2	[818.2]	1404.2	[875.0]	238.2	[164.3]
Real Wage Growth 2005-07 (in %)[a]	-0.046	[0.0032]	-0.050	-	-0.043	-
Real Wage Growth 2007-09 (in %)[a]	-0.029	[0.0086]	-0.019	-	-0.037	-
Low-Skilled (share)	0.28	[0.45]	0.20	[0.40]	0.33	[0.47]
Medium-Skilled (share)	0.69	[0.46]	0.76	[0.42]	0.64	[0.48]
High-Skilled (share)	0.034	[0.18]	0.032	[0.18]	0.035	[0.18]
Age (in years)	34.0	[11.3]	35.4	[10.9]	33.0	[11.5]
Female (share)	0.71	[0.45]	0.64	[0.48]	0.76	[0.43]
German (share)	0.81	[0.39]	0.75	[0.43]	0.85	[0.36]
East Germany (share)	0.15	[0.36]	0.22	[0.42]	0.11	[0.31]
Tenure (months)	32.4	[40.9]	43.8	[52.2]	24.1	[27.3]
Hospitality Sector Exper. (months)	50.1	[52.3]	71.8	[62.1]	34.4	[36.4]
Regular Job (share)	0.42	[0.49]	1	-	0	-
Mini Job (share)	0.58	[0.49]	0	-	1	-
Persons	21,100		7,976		15,106	
Observations	268,728		113,148		155,580	

Notes: Summary statistics of individual wage data. Standard deviation in brackets. Sample restricted to individuals aged 17-62 years, employed in the hospitality sector and working in guest attending occupations between August 2006 and February 2009. Real euro values are deflated to 2010 using the consumer price index of the German Bundesbank. Censored wages are imputed following Gartner (2005). [a] Aggregated by contract types (regular and mini job).

bounds. However, many of the untreated workers such as hotel managers or back office staff are more likely to work full-time and thus excluding workers in full-time or regular jobs (as opposed to mini jobs) should mitigate the problem of including many untreated workers. Consistent with this line of reasoning, I find much larger and more significant effects when focusing on mini job employees only, the typical contract for waiters in Germany (as Table 3.3 suggests, about 60% of all employees in guest attending occupations in the hospitality sector are employed in mini jobs.) Employees in mini jobs are exempted from regular social security contributions and income taxation while employers only pay a lump sum contribution to social security. During the time of my analysis, workers in mini jobs were allowed to earn up to 400 euros per month on a regular basis.[20] In most of the empirical analyses I will therefore restrict my attention to this group of workers, also because I expect the equilibrium to emerge much faster in this more flexible segment of the labor market.

As mentioned above, I only observe *daily* wages, which are derived from total payments to an employee in a given period divided by the number of days in that period. Therefore, my wage variable is the product of the hourly wage times hours worked. In Section 3.5 when discussing potential channels, I will come back to this point and – using Microcensus data – try to shed more light on whether the observed effect could also be explained by a change in the hours worked.

The individual wage data are summarized in Table 3.3. Employees in the hospitality industry are among the lowest-paid occupations in Germany. Employees in regular jobs receive a gross monthly real wage of about 1,270 euros.[21] Mini job workers, who make up nearly 60% of all hospitality workers, make some 240 euros

[20] Some exceptions from this rule are possible, e.g. for employee in short-term contracts or when smoothing seasonal peaks obeying an annual earnings cap. Only about 3% of mini job employees in my baseline sample earn more than 400 euros per month. All my results are robust to excluding these observations. During the time of analysis, no changes in the income threshold occurred (from April 2003 to January 2013 the threshold remained at 400 euros. On July 1, 2006 the employer contribution to the health and pension insurance was increased to 13% and 15%, respectively. I thus choose to start my baseline analysis in August 2006 which is also 12 months before the start of the first smoking ban.

[21] In the following, regular jobs include full-time and regular part-time part-time (more than half but less than 2/3 of the work hours of a comparable full-time worker) workers. The average monthly real wage for full-time employees is 1,596 euros.

Table 3.4: Summary Statistics of State Level Data
(August 2006 - February 2009)

	Mean	SD	Min	Max
Monthly				
Unemployment Rate (in %)	11.5	[4.23]	4.10	21.2
Revenue Index Restaurants (2005=100)[a]	103.4	[20.6]	57.3	173.7
Revenue Index Bars (2005=100)[a]	91.2	[21.5]	45.1	174.2
Share of Foreign Arrivals (in %)[b]	16.1	[8.13]	2.84	39.1
Temperature (in Degrees Celsuis)	9.46	[6.03]	-3.90	19.1
Rain Amount (in l/m^2)	67.9	[33.9]	1.10	179.2
Sunshine Hours	127.7	[75.5]	18.3	351.3
Yearly				
Population (in Millions)	5.13	[4.70]	0.66	18.0
Share of Smokers in 2005 (in %)	28.5	[2.75]	24.5	33.7
With Election Cycles				
Turnout in State-Level Elections (in %)	58.6	[5.62]	44.4	70.6
Conservative Index	1.05	[0.44]	0.36	2.31

Notes: Summary statistics of state level data between August 2006 and February 2009. Standard deviation in brackets. [a]Data not available for Berlin and Brandenburg. [b]Share of registrations of tourists of foreign nationality in all touristic registrations at accommodation establishments. Sources: GENESIS, MZ 2005, DWD.

a month.[22] This corresponds to a typical student or second-earner part-time job of about 25 hours a month. Wages have substantially declined in the years around the introduction of smoking bans. These mini job workers are relatively young (about 33 years on average) and predominantly female (76%).

To complement my analysis, I use waves 2004-2011 from the German Microcensus, an official yearly survey similar to the US Current Population Survey (CPS). The German Microcensus is based on a 1% random cross-section of German households. Participation is compulsory and non-compliance can be fined. Most population and many labor market statistics are based on the Microcensus. In this data set, I observe the state, the occupation, sector, smoking behavior (only in 2005 and 2009), full- or part-time status and – unlike in the IAB wage data – the usual hours worked per week.

Finally, I include several state level controls. The data on monthly revenues (separately for bars and restaurants) is based on a monthly compulsory survey among an 8% random sample of all establishments in the hospitality sector (about 10,000 businesses per month) with yearly revenues exceeding 50,000 euros and is taken from Ahlfeldt and Maennig (2010).[23] Population, election, and unemployment rates at the state level are taken from Federal Statistical Office (GENESIS). Weather data, i.e. monthly state mean temperature, rain, and hours of sunshine, are derived from the German Weather Service (DWD). Table 3.4 summarizes these variables, all of which vary at the state and month level.

3.4.2 Identification Strategies

In my main approach, I will exploit variation between states and over time in a difference-in-differences (DD) fashion. Specifically, to study the effect of the intensity of a smoking ban on the wages of workers in guest attending occupations in the hospitality industry ("waiters" in the following), I estimate variants of the

[22] As mentioned above, there was no minimum wage during the time of analysis and workers in mini jobs were allowed to earn up to 400 euros a month.

[23] Revenue data is not available for Berlin and Brandenburg due to its underrepresentation in the underlying survey (see Ahlfeldt and Maennig 2010, p. 509).

following OLS specification:

$$\ln \text{wage}_{its} = \beta_1 \text{intensity}_{st} + \alpha_i + \alpha_t + \alpha_s + \alpha_{s,m(t)} + \boldsymbol{\delta}\mathbf{X}_{st} + u_{its} \tag{3.2}$$

where w_{its} denotes the wage of individual i at time t in state s, intensity_{ts} refers to the intensity measure as introduced above, and α_i,α_t, α_s, and $\alpha_{s,m(t)}$ capture individual, time, state, and state ×month fixed effects, respectively. The state × month fixed effects (e.g. Bavaria × October) are supposed to account for the different seasonal patterns of (tourism) demand in each state. $\mathbf{X}_{ts}$ contains further controls that vary at the state-month level such as the current or lags of the state unemployment rate or state specific linear (*pre*-)trends that project the trend in wages from the previous 36 months before treatment into the post-treatment period.[24] The parameter of interest is β_1. It indicates (approximately) by how much percent a waiters' wage changes when a strict smoking ban (equivalent to the intensity of Bavaria's initial smoking ban) is introduced. The estimation period starts in August 2006, i.e. 12 months before the first smoking ban became effective in August 2007 in Baden-Wuerttemberg and ends 6 months after the last ban became effective in August 2008 in Mecklenburg-Western Pomerania. In a robustness check (see Table 3.7), I show that my results do not depend on this specific time choice and are robust to choosing a longer pre-period or a balanced time windows around the treatment. Throughout, I cluster standard errors at the state level, the unit at which the treatment varies.

The identifying assumption underlying this difference-in-difference approach is that wages of workers in restaurants, bars and dancing clubs in more or less treated states would have evolved similarly in the absence of smoking bans, i.e. I rely on a common trends assumption *across* states. A potential threat to this identification strategy would be any kind of unobserved policy or demand change unrelated to smoking bans that affects a state's hospitality sector post treatment. A triple difference-in-differences (DDD) approach is able tackle such a concern by controlling for *two* potentially confounding trends (and not just one as in a DD

[24] Specifically, I recover the coefficient ϕ_s for each state from a regression $\ln \text{wage}_{its} = \sum_{s=1}^{16}(\text{state}_s \times \phi_s \text{time}_t) + u_{st}$ and include $\hat{\phi}_s \times \text{time}_t$ as a new control variable in the main specification thereby projecting pre-treatment trends in the post-treatment period following Repetto (2018). Employing quadratic pre-trends or choosing different lengths of the pre-period leaves my estimates virtually unchanged.

approach): changes in waiters' wages *across* states (unrelated to the introduction of smoking bans) and changes in the wages of other comparable employees in the hospitality industry in the *same* state (possibly related to other state specific changes in tourism demand or the state economy in general). This can be seen more easily in the following regression equation:

$$\begin{aligned}\ln \text{wage}_{itso} =& \beta_1 \text{intensity}_{st} + \beta_2 \text{intensity}_{st} \times \text{waiter}_{io} \\ &+ \alpha_i + \alpha_{to} + \alpha_{so} + \alpha_{s,m(t)} + \boldsymbol{\delta}\mathbf{X}_{st} + u_{itso}\end{aligned} \tag{3.3}$$

where o indexes different occupation groups, waiter_o is an indicator variable that is one if individual i belongs to the occupation group of waiters, α_{so} and α_{to} denote occupation specific time and state fixed effects, while α_i, $\alpha_{s,m(t)}$, and $\mathbf{X}_{ts}$ refer to individual and state × month fixed effects and further state-level controls as before. β_1 corresponds to the effect of smoking bans on wages of all occupations except for the group of waiters. The coefficient of interest is now β_2. It refers to the effect of smoking bans on wages of waiters *net of* secular changes in the hospitality industry within the same state and secular changes in the wages of waiters in non-treated states. In my setting I choose cooks and in another specification all other mini job workers in the hospitality industry as additional control groups. In case cooks are chosen as the additional control group, the DDD approach thus compares the evolution of the difference in wages between waiters and cooks in a treated state with the same difference in an untreated state. The underlying identifying assumption states that the difference in waiters' and cooks' wages in more or less treated states would have developed similarly in the absence of smoking bans.

3.4.3 Estimation Results

Table 3.5 reports estimates of equation 3.2 for workers with different types of work schedules (columns) and using different treatment indicators (panels). In even columns, I present estimation results using a reduced set of controls only (individual-, state-, time, and state × month fixed effects) while in odd columns I include additional controls (linear pre-trends and the current and six lags of the monthly state unemployment rate). The effect of smoking bans – either captured by an indicator for whether a smoking ban is in effect (Panel A) or using the smoking ban intensity as defined above (Panel B) – on workers on all different work schedules is mostly insignificant. Looking separately at the different work schedules, the impact of smoking bans on wages of full-time workers is close to zero (columns 3 and 4) and only slightly more pronounced but still insignificant for waiters' wages working regular part-time (columns 5 and 6). In contrast, for the group of waiters in mini jobs the effect of smoking bans on wages is highly significant, robust in both specifications and sizable (columns 7 and 8). Not accounting for its strictness, the average smoking ban is estimated to reduce wages of mini job waiters by about 1.3% (Panel A), while taking into account the intensity of the different smoking bans yields a decrease of 2.4% (Panel B).[25] This is in line with the reasoning outlined above, i.e. the group of waiters in mini jobs supposedly makes up the overwhelming majority of treated individuals and that due to their flexible contracts are less affected by wage rigidities. Against the backdrop of the results presented in Table 3.5 and the reasoning presented above, in the following I will focus on waiters in mini jobs.

Figure 3.6a plots an event study graph based on a dynamic version of the specification in column 7, Panel A of Table 3.5. Although I control for state × month fixed effects, there is still considerable volatility left in the wage data. The figure shows, however, that there are no trends and that on average, wages are significantly lower after the introduction of a smoking ban (the gray horizontal line and the box indicate the corresponding point estimate of the ban indicator and the 95% confidence interval.) Figure 3.6b depicts the same event study graph for the group of all mini job workers except for waiters in the hospitality industry. Wages of this group (purged of the impact attributable to the set of baseline fixed effects) are similarly volatile (although to a somewhat lesser extent), but in contrast to waiters' wage, are not significantly lower after the introduction of smokings bans (indicated as before with a gray box). The two figures also jointly illustrate the idea of the DDD approach outlined above. The evolution of the wages of all other mini job workers constitute the counterfactual evolution of wages of waiters in the absence of introducing smoking bans.[26]

[25] When jointly including the smoking ban indicator and the intensity index in a regression, both coefficients remain significant thus indicating that it is important not only to account for the existence of a ban by itself but also for its intensity.

[26] Figure C.4 in the Appendix overlay the two plots and shows that the two follow a similar pattern previous to the introduction of smoking bans.

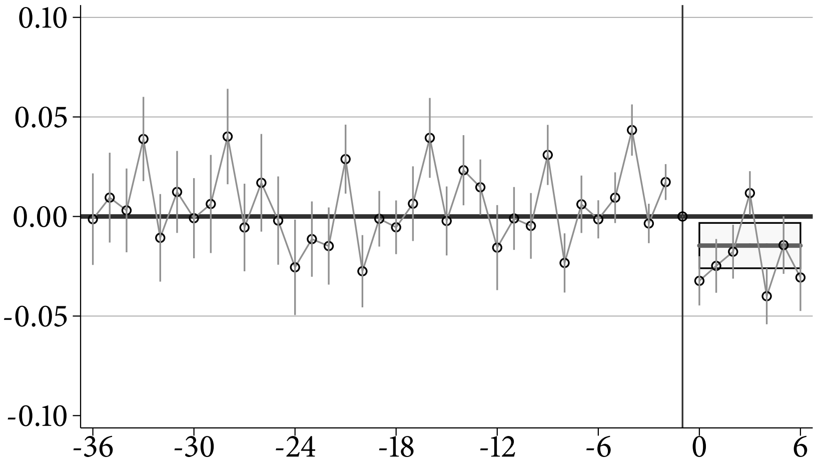
Table 3.5: The Effect of Smoking Bans on Waiters' Wages

	All Workers		Full-Time		Regular Part-Time		Mini Jobs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Smoking Ban Indicator								
Smoking Ban Indicator	-0.009*	-0.004	-0.001	-0.003	-0.010	-0.002	-0.013***	-0.013***
	(0.005)	(0.005)	(0.003)	(0.002)	(0.009)	(0.008)	(0.003)	(0.004)
Adj. R^2	0.918	0.918	0.949	0.949	0.952	0.952	0.869	0.869
Panel B: Smoking Ban Intensity Index								
Ban Intensity	-0.028*	-0.021	-0.001	-0.005**	-0.012	0.000	-0.023***	-0.024***
	(0.014)	(0.012)	(0.003)	(0.002)	(0.011)	(0.008)	(0.006)	(0.006)
Adj. R^2	0.918	0.918	0.949	0.949	0.952	0.952	0.869	0.869
Reduced Controls	✓	✓	✓	✓	✓	✓	✓	✓
Extended Controls		✓		✓		✓		✓
Start	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006
End	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009
Clusters	16	16	16	16	16	16	16	16
Individuals	21,100	21,100	6,066	6,066	2,268	2,268	15,106	15,106
Observations	268,728	268,728	89,811	89,811	23,337	23,337	155,580	155,580

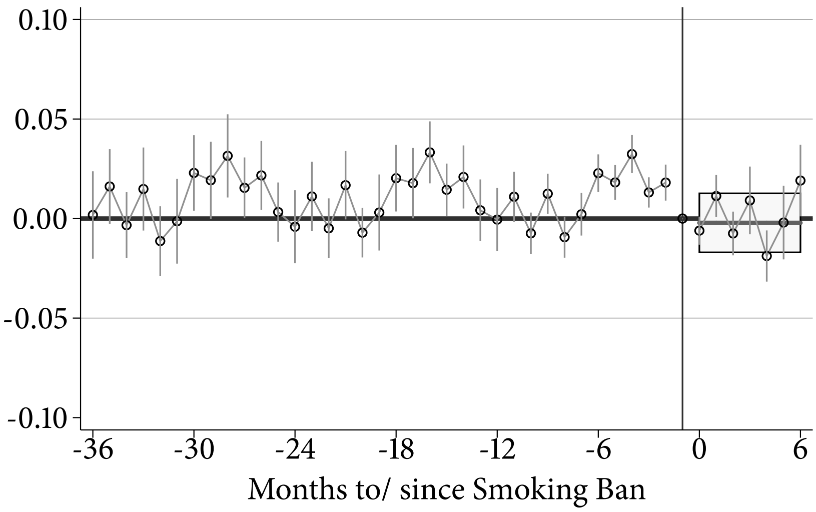
Note: This table shows regression results of the impact of smoking bans on individual log daily wages of different set of waiters in the hospitality sector. The set of reduced controls include person-, time-, state-, and state×month fixed effects. Extended controls include the set of reduced controls and additionally linear pre-trends specific for each set of workers (full-time, regular part-time, mini jobs), and current and six lags of the monthly state unemployment rate. Standard errors clustered at the state level. ***/**/* indicate significance at the 1%/5%/10% level.

Figure 3.5: Event Study Graphs Related to the Introduction of Smoking Bans

(a) Waiters in Mini Jobs in the Hospitality Sector)



(b) All other Mini Job Workers in the Hospitality Sector



Notes: This figure plots the regression coefficients of dummies indicating the months to or since the introduction of a smoking for the period 36 months prior and up to 6 months after the introduction of a smoking ban. The period one month prior to the introduction is the reference period. Regressions are based on a dynamic version of the specification in column 7 in Panel A of Table 3.5, i.e. include individual-, time-, state-, and state × month fixed effects. The point estimate and 95% confidence interval of a regression using a smoking ban indicator corresponding to this sample is shown in row 1 of Table 3.7 and is marked in the figure with a horizontal line and gray box, respectively. The vertical gray lines indicate the 95% confidence intervals of each point estimate. Standard errors clustered at the state level.

3.4.4 Robustness Checks

In Table 3.6, I test the robustness of the baseline intensity effect for mini job workers. Column 1 reproduces the baseline result. In column 2, the intensity measure turns on already at the time of the *legal* introduction of a smoking ban which in some states did not coincide with the time a smoking ban was enforced by sanctions.[27] The estimate – in line with expectations – is slightly lower but remains highly significant and very similar in magnitude. The intensity coefficient also remains unchanged when I include a dummy that indicates whether establishments could avoid imposing a smoking ban by declaring themselves as "smokers clubs" (column 3).[28] Alternatively, in column 4, I reduce the intensity measure by 0.3 for states where smoking clubs could be installed (the same reduction in the index as if smoking was allowed in small bars and restaurants) and again the results remain robust. Furthermore, I show that my baseline results also do not depend on the specific choice of the index weights (Table C.6) and and not driven by a specific state (Table C.7 and Figure C.5).

The baseline estimate also holds up to a battery of additional robustness checks presented in Table 3.7 including using a longer pre-period (from January 2005), a balanced time window equal for all states (including the 24 months before and 6 months after the introduction of a smoking ban in each state), excluding observations from states that introduced smoking bans first (in 2007) or in January or July, i.e. months where the treatment might be particularly prone to be confounded by seasonal effects, relying on observations from West Germany only, excluding the largest and smallest 5% of wages; including weather controls (temperature, rain,

[27] This was the case in Berlin, Brandenburg, Bremen, Mecklenburg-Western Pomerania, Lower Saxony, Saarland, and Saxony-Anhalt. I personally experienced that people still used to smoke in bars in restaurants in Berlin during January and June 2008 when a smoking bans was *legally* in place but violations did not carry any (financial) consequences for establishment owners or guests. This was also the case in Lower-Saxony where smoking was still prevalent in the transition period as mentioned in this newspaper article (in German) `http://www.tagesspiegel.de/berlin/berlin-bis-juli-2008-keine-bussgelder-beim-rauchverbot/1088506.html`.

[28] As Kvasnicka and Tauchmann (2012, p. 4541) point out, this kind of relabeling only developed into a major loophole in Bavaria and North Rhine-Westphalia, consequently the dummy is one for Bavaria between January 2008 and January 2009 and for North Rhine-Westphalia between July 2008 and April 2011, the end dates marking the time when the loopholes were shut down by courts.

Table 3.6: Robustness of the Treatment Effect to Institutional Features of Smoking Bans

	Dependent Variable: *Log Wage*			
	(1)	(2)	(3)	(4)
Ban Intensity	-0.024***		-0.023***	
	(0.006)		(0.007)	
Ban Intensity (not enforced)		-0.022***		
		(0.006)		
Ban Intensity (incl. Raucherclub)				-0.025***
				(0.007)
Smokers Club Permitted			-0.001	
			(0.009)	
Baseline Controls	✓	✓	✓	✓
Start	Aug 2006	Aug 2006	Aug 2006	Aug 2006
End	Feb 2009	Feb 2009	Feb 2009	Feb 2009
Clusters	16	16	16	16
Individuals	15,106	15,106	15,106	15,106
Observations	155,580	155,580	155,580	155,580
Adj. R^2	0.869	0.869	0.869	0.869

Notes: This table shows regression results of the impact of smoking bans on individual log daily wages of waiters in the hospitality sector working in mini jobs. Extended controls include person-, time-, state-, and state×month fixed effects, linear pre-trends, and current and six lags of the monthly state-unemployment rate. Standard errors clustered at the state level. ***/**/* indicate significance at the 1%/5%/10% level.

and hours of sunshine), the monthly state level consumer price index, the months to the next state-level elections, the pre-treatment party vote shares interacted with time fixed effects, proxies for the share of foreign visitors (controlling for a change in the composition of tourists from countries like the US where tipping is more common and generous) and tourism demand, or region and region × month fixed effects to allow for more disaggregated patterns in demand. All of these alternative specifications leave my baseline results virtually unchanged.

Identification in the previous estimations depends on the common trends assumption. A convenient way to test this assumption is a placebo test. If the common trend assumption holds, then shifting the treatment into the past when actually no treatment occurred should yield an insignificant estimate of the treatment coefficient. This is what I do in Figure 3.6. Each point depicts the intensity coefficient obtained from a regression that is identical to the specification using extended controls except that the introduction date of a state's smoking ban policy and the start and end date of the sample are shifted by the number of months indicated on the x-axis into the past/ future. The results of this exercise are in line with the common trend assumption: Pretending that smoking bans were introduced earlier or later than they actually were, yields mostly insignificant coefficients while the strongest and most significant effect is found around the true introduction date.

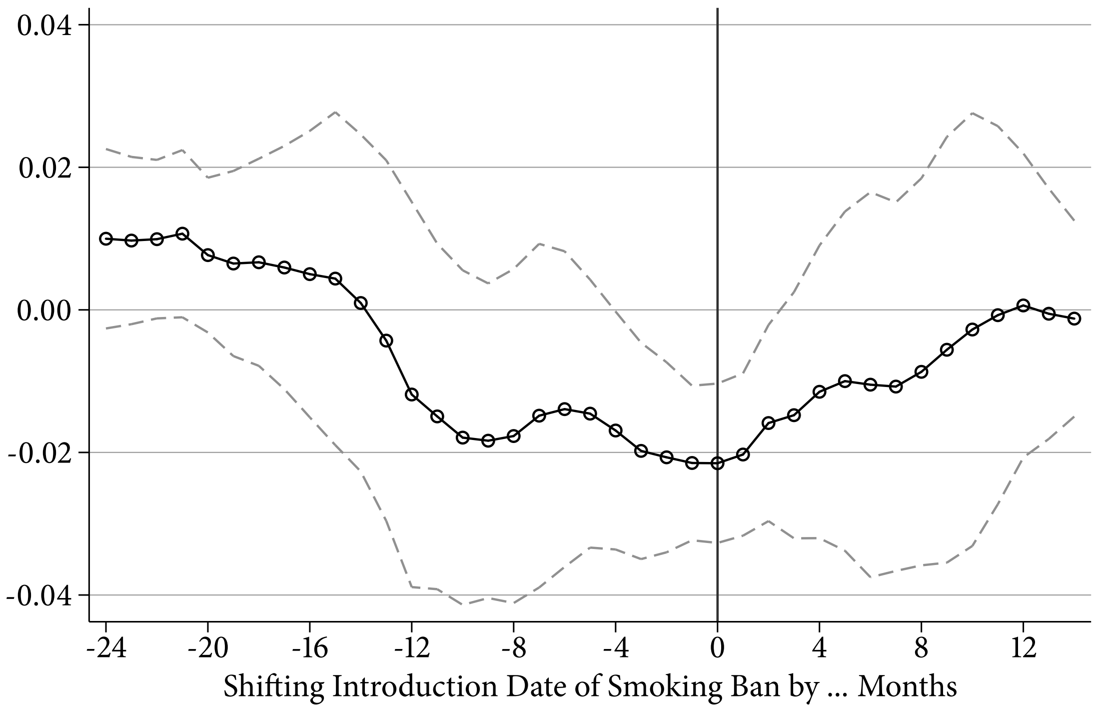
Despite the robustness of the estimate so far, it could still be that the introduction of smoking bans coincided with other idiosyncratic fluctuations that decreased wages at the same time. Furthermore, clustering standard errors at the state level leaves me with 16 clusters, a relatively small number to rely on asymptotic convergence, and clustering at the state level might not capture more complex ways of interdependencies between states. To address such concerns, I run a permutation exercise in which I randomly shuffle polices across states (without replacement) and re-estimate the intensity coefficient for these placebo samples. For instance, in one permutation sample, Bavaria might be assigned the smoking ban introduction date and intensity of Berlin while Berlin is assigned the policy of Baden-Wuerttemberg, and so on for each state. Note that by chance, one or more states might be assigned their actual polices. In each round, I estimate the effect of these placebo policies and repeat this 10,000 times. By comparing the coefficient based on the actual policies with the distribution of placebos estimates, one can compute the percentile

Table 3.7: Additional Robustness Checks

	Intensity Coefficient	Adj. R^2
1. Balanced Time Windows (36 months before, 6 after)	-0.025*** (0.007)	0.847
2. Longer Pre-Period (2005 Jan - 2009 Feb)	-0.025*** (0.006)	0.839
3. Excluding States with Bans Introduced in 2007	-0.032*** (0.007)	0.871
4. Excluding States with Bans Introduced in January 2008	-0.020*** (0.006)	0.871
5. Excluding States with Bans Introduced in July 2008	-0.024** (0.008)	0.868
6. Excluding Spells from East Germany	-0.021*** (0.005)	0.867
7. Excluding Extremes (largest/smallest 5% of wages)	-0.021*** (0.006)	0.851
8. Weather Controls (Temperature, Rain, Sunshine Hours)	-0.026*** (0.005)	0.869
9. Monthly state level CPI	-0.022*** (0.006)	0.869
10. Time to Next State-Level Elections	-0.022*** (0.005)	0.869
11. Initial Party Vote Shares × Time FEs	-0.025** (0.011)	0.869
12. Monthly Share of Foreign Overnight Stays[a]	-0.025*** (0.007)	0.869
13. Monthly Index of Foreign and Domestic Overnight Stays[a]	-0.023*** (0.006)	0.869
14. County and County × Month FEs	-0.023 (0.006)	0.871

Notes: This table shows additional robustness checks of the smoking ban intensity on individual log daily wages of waiters in the hospitality sector working in mini jobs. Each row represents a separate regression with the log daily wage as the dependent variable using extended controls in addition to the indicated specification. The time period covers August 2006 to February 2009 as in the baseline if not indicated otherwise. [a] lags 0-6. Standard errors are clustered at the state-level. ***/**/* indicate significance at the 1%/5%/10% level.

Figure 3.6: Placebo Treatments

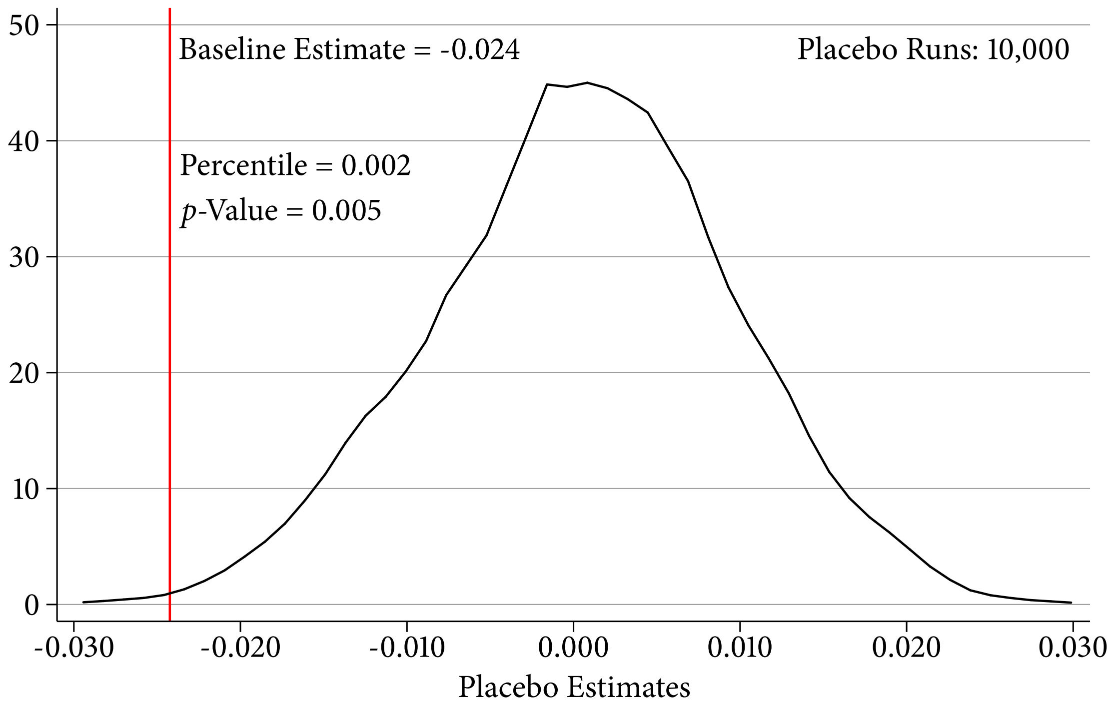


Notes: This figures plots the smoking ban intensity coefficients using a specification with extended controls when shifting the policies by the number of months indicated on the x-axis in the past/ future. The dashed lines indicate the 95% confidence intervals.

in the distribution of outcomes and derive the according *p*-value. The results of this exercise is depicted in Figure 3.7. It turns out that the actual estimate falls in the 0.002 percentile of placebo estimates, corresponding to a two sided *p*-value of 0.005.

As outlined in the identification strategy, the DD-approach maybe confounded if states exhibit diverging unobserved trends such that the evolution of the outcome in control states alone is not an appropriate counterfactual. A DDD-approach helps to overcome this concern by adding a further control group. In table 3.8, I estimate variants of equation 3.3 using cooks (columns 1 and 2) and then all other workers in mini jobs in the hospitality sector (columns 3 and 4) as additional control groups. When using the sample of all other mini job workers in the hospitality sector, I drop occupations with less than 20 observations in any given state. A list of the remaining

Figure 3.7: Kernel Density Graph of Permutation Tests



Notes: This figures plots the kernel density graph of intensity coefficients using a specification with extended controls resulting from a permutation exercise where smoking ban policies are randomly shuffled between states (without replacement) 10,000 times. For more details see section 3.4.4.

occupations and their frequencies can be found in table C.8. These are mainly other guest attending occupations, salespersons, motor vehicle drivers, office workers, as well as housekeeping and cleaning occupations.

The DDD estimation results presented in table 3.8 corroborate the findings using the simpler DD approach from above. The estimation results in column 1 and 2 – using cooks as the additional control group – yield a significant decline in waiters' wages related to the introduction of smoking bans. However, the baseline ban and intensity coefficients are marginally significant and positive indicating that smoking bans might have increased wages for cooks. This might be caused by an increase in food demand in now smoke-free restaurants leading to higher wages for cooks. Thus, a broader control group consisting of all other mini job workers in the hospitality industry might be a better control group. Indeed, in columns 3 and 4 the causal impact of the smoking ban indicator or the intensity index on waiters'

Table 3.8: Triple Difference Estimates

	Cooks		All Other Occupations	
	(1)	(2)	(3)	(4)
Smoking Ban × Waiters Indicator	-0.017***		-0.011**	
	(0.005)		(0.004)	
Smoking Ban Indicator	0.009*		0.000	
	(0.005)		(0.003)	
Ban Intensity × Waiters		-0.031***		-0.024**
		(0.009)		(0.009)
Ban Intensity		0.012*		0.002
		(0.006)		(0.004)
Baseline Controls	✓	✓	✓	✓
Start	2006 Aug	2006 Aug	2006 Aug	2006 Aug
End	2009 Feb	2009 Feb	2009 Feb	2009 Feb
Clusters	16	16	16	16
Individuals	22,150	22,150	31,682	31,682
Observations	231,867	231,867	346,143	346,143
Adj. R^2	0.869	0.869	0.873	0.873

Notes: This table shows triple difference regression results of the impact of smoking bans on individual log daily wages of waiters using wages of cooks (columns 1 and 2) and workers in all other occupations (columns 3 and 4) as additional control groups. The sample is restricted to workers in mini jobs. Extended controls include person-, time × occupation-, state × occupation-, and state×month fixed effects, linear pre-trends for each occupation, and current and six lags of the monthly state-unemployment rate. Standard errors clustered at the state level. ***/**/* indicate significance at the 1%/5%/10% level.

wages are virtually identical to the corresponding DD estimates presented above (compare estimates in column 3 of Table 3.5) while the baseline ban indicator and intensity coefficient are practically zero indicating that smoking bans did not have a significant impact on the wages of other mini job employees working in bars and restaurants such as security personal, cleaning staff and others. That fact that the DD and the DDD estimates coincide makes is unlikely that the DD estimates suffer from a confounding unobserved counterfactual trend. Thus, taken together, the DD and DDD estimates provide strong evidence in favor of a *causal* interpretation of the ban intensity coefficient.

3.5 Channels

In the previous section, I established a negative impact of smoking bans on the wages of waiters in mini jobs working in hospitality establishments. The next step is to ask what lies behind this decline, i.e. what the are the channels responsible for this effect? In this section I will check the validity of four potential channels: (i) a decline in hospitality revenues, (ii) a decline in hours worked, (iii) selection, and (iv) employment effects.

3.5.1 Revenues

A widely held explanation for the negative impact of smoking bans on waiters' wages is that the introduction of smoking bans led to a decline in revenues of bars and restaurants as patronage by smokers decreased. This concern was fiercely advocated by the hospitality industry in their opposition to smoking bans. Even if true, this reasoning seems to neglect that at least part of the supposed revenue decline might be compensated for by an increased demand of non-smokers such as families with children or clients that had a strong dislike for second-hand indoor smoke.

To evaluate the validity of such claims, I perform a difference-in-differences analysis at the state level, separately for the revenues of restaurants (Panel A) and bars (Panel B) in Table 3.9. Note that the revenue data vary at the state $\times$ month level and are only available for 14 out of the 16 German federal states (data for Berlin and Brandenburg are not available for this period). Although the effect of smoking bans on revenues is always insignificant, the results show that if one

Table 3.9: Impact of Smoking Bans on Revenues of Restaurants and Bars

	Dependent Variable: *Log Revenues*				
	(1)	(2)	(3)	(4)	(5)
	Simple DD	+ Linear State Trend	+ State × Month FEs	+ Weather Controls	+ Tourism Demand
Panel A: Restaurants					
Ban Intensity	-0.045	0.012	0.045	0.045	0.051
	(0.033)	(0.045)	(0.068)	(0.063)	(0.066)
Adj. R^2	0.754	0.838	0.855	0.857	0.858
Panel B: Bars					
Ban Intensity	-0.043	-0.001	0.052	0.055	0.056
	(0.058)	(0.048)	(0.086)	(0.080)	(0.080)
Adj. R^2	0.617	0.757	0.813	0.817	0.816
State & Time FEs	✓	✓	✓	✓	✓
Unemp. Rate & CPI	✓	✓	✓	✓	✓
Linear State Trends		✓	✓	✓	✓
State × Month FEs			✓	✓	✓
Weather Controls				✓	✓
Index of Domestic and Foreign Overnight Stays					✓
Start	2005 Jan	2005 Jan	2005 Jan	2005 Jan	2005 Jan
End	2009 Feb	2009 Feb	2009 Feb	2009 Feb	2009 Feb
Clusters	12	12	12	12	12
Observations	576	576	576	576	576

Notes: This table presents regressions of the monthly state-level log revenues in restaurants (panel A) and bars (panel B) on the smoking ban intensity index and further controls. All controls vary at the state-month level. Weather controls include the monthly state mean temperature, rain amount, and hours of sunshine. CPI refers to the monthly state consumer price index. The index of domestic and foreign overnights stays refers to the number of overnights stays by tourists of domestic or foreign origin. Standard errors clustered at the state level. All regressions are weighted by population size. ***/**/* indicate significance at the 1%/5%/10% level.

does not properly account for the seasonal variation in the data (using state × month fixed effects) – crucial when analyzing such seasonal and volatile data as hospitality revenues – the estimations yield a spuriously negative point estimates. Once controlling for seasonal effects, the point estimates, if anything, suggest a positive effect of smoking bans on revenues although the coefficients are estimated imprecisely – even though my preferred specifications including state × month effect explain more than 80% of the variation in the data. A power calculation based on the change in R^2 for the models in column 3 shows that this is not necessarily due to a lack of statistical power. Given the estimated effect sizes in column 3, a power analysis yields values of 0.79 (0.87) and 0.65 (0.76) at a significance level of 5% (10%) for restaurants and bars, respectively. All in all, these results make me reasonably confident that smoking bans did not lead to a significant decrease in revenues of bars and restaurants and that my setting is sufficiently powered to detect a potential decrease. My estimates are in line with a host of studies from the US and other countries that do not find any robust evidence for a negative effect of smoking bans on hospitality revenues (see for instance the review articles by Scollo et al. 2003; Scollo and Lal 2008). They are also in line with Ahlfeldt and Maennig (2010) who perform a similar difference-in-differences analysis based on the same official German revenue data as I do here covering January 2005 and December 2009 and who do not find any statistically significant decline in revenues neither for bars nor restaurants. They suggest that their findings might be explained by increased spending of non-smokers that compensated the reduced spending by smokers or that smokers did not reduce their spending in the first place.[29]

Another issue could be that the average effect masks important heterogeneous effects between different sorts of establishments. For instance, old-school corner

[29] In contrast, Kvasnicka and Tauchmann (2012) use revenue data for the *entire* hospitality industry (thus also including revenues from accommodation businesses such as hotels or camping sites likely unaffected by smoking bans) and find a statistically significant decline in revenues of at some 2%. However, they use data only covering the period January 2007 to September 2008 and thus use considerably less pre- and post-treatment observations than Ahlfeldt and Maennig 2010 and than I do here. They also do not include the monthly state-level consumer price index and do not account for state × month specific seasonal effects which I argue are particularly important in the context of such volatile and seasonal data. For instance, 5 out of 16 states introduced smoking bans in January or February, two months characterized by particularly low demand in some states but not all (e.g. due to winter sports tourism).

Table 3.10: Exploring the Effect of Revenues in Baseline Effect

	(1) Baseline	(2) Initial Share of Smokers	(3) Baseline w/o BE, BB	(4) Revenues (Lags 0-6)
Ban Intensity	-0.024***	-0.020***	-0.022***	-0.023***
	(0.006)	(0.006)	(0.005)	(0.005)
Baseline Controls	✓	✓	✓	✓
Share Smokers 2005 × Time FEs		✓		
Monthly State Bar and Restaurant Revenues (Lags 0-6)				✓
Start	Aug 2006	Aug 2006	Aug 2006	Aug 2006
End	Feb 2009	Feb 2009	Feb 2009	Feb 2009
Clusters	16	16	14	14
Individuals	15,106	15,106	14,312	14,312
Observations	155,580	155,580	148,324	148,324
Adj. R^2	0.869	0.869	0.868	0.868

Notes: This table presents regression results of the impact of smoking bans on individual log daily wages of waiters in the hospitality sector working in mini jobs exploring the role of potential changes in the revenues of bars and restaurants. Extended controls include person-, time-, state-, and state×month fixed effects, linear pre-trends, and current and six lags of the monthly state-unemployment rate. Standard errors clustered at the state level. ***/**/* indicate significance at the 1%/5%/10% level.

bars with a majority of their patronage being smokers might be hit harder and thus are forced to go out of business while modern, more food oriented establishments or trendy coffee shops might benefit from a shift in demand due to smoking bans.[30] However, Kvasnicka and Tauchmann (2012) do not find any significant effect of smoking bans neither on the number of business closures nor business start ups up until December 2008 (i.e. between 5 and 17 months after the introduction of smoking bans).

In Table 3.10, I explore the role of declining demand for wages of mini job employees in bars and restaurants. Column 1 reproduces the baseline estimate. As a first check, in column 2, I interact the share of smokers in each state in 2005, i.e. before any state introduced a smoking ban with time fixed effects. The idea of this exercise is to capture the time-varying effect of having a higher initial share of smokers. If part of the decline in wages is caused by smokers reducing their consumption, then including the time-interacted share of initial smokers in the baseline regression should yield a smaller and/ or insignificant intensity coefficient. The results in column 2 suggest that this is not the case. The intensity coefficient is reduced only slightly and stays highly significant. Next, I directly control for the monthly revenues of bars and restaurants and their lagged values (up to the sixth lag). As these data are not available for Berlin and Brandenburg, column 3 reproduces the baseline estimate excluding these two states. The point estimates remains virtually unchanged. Column 4 includes the revenue controls. Although the revenue variables constitute a set of "bad controls", including them allows to learn something about the mechanism. If the effect of smoking bans on wages worked fully through reduced revenues, then – similar to including the time interacted share of initial smokers – the intensity coefficient should become insignificant and close to zero. Column 4 shows, however, that the role of revenues in explaining the decline in wages is very limited as the intensity coefficient remains unchanged after including revenues as additional regressors.

In Table 3.11, I provide further support that declining revenues are unlikely to serve as the main explanation for declining wages of waiters. In particular, if the decline in waiters' wages was caused by a general decline in revenues of bars and

[30] Kvasnicka and Tauchmann (2012) note that in the sales data some of the very small bars with revenues of less than 50,000 euros per year are excluded. In the IAB wage data, there is no such lower limit censoring and employees of small bars are also sampled proportionally.

Table 3.11: Triple Difference Estimates Controlling for Revenues

	Dependent Variable: *Log Wage*	
	(1) Cooks	(2) All Other Occupations
Ban Intensity × Waiters	-0.030***	-0.022**
	(0.010)	(0.009)
Ban Intensity	0.010	-0.000
	(0.008)	(0.006)
Baseline Controls	✓	✓
Revenues (Lags 0-6)	✓	✓
Start	2006 Aug	2006 Aug
End	2009 Feb	2009 Feb
Clusters	14	14
Individuals	20,920	29,836
Observations	220,355	328,024
Adj. R^2	0.868	0.873

Notes: This table presents triple-difference regression results of the impact of smoking bans on individual log daily wages of waiters in the hospitality sector working in mini jobs. Extended controls include person-, time × occupation-, state × occupation-, and state×month fixed effects, linear pre-trends for each occupation, and current and six lags of the monthly state-unemployment rate. Standard errors clustered at the state level. ***/**/* indicate significance at the 1%/5%/10% level.

restaurants, other occupations would likely also see their wages falling after the introduction of smoking bans. The small and insignificant baseline coefficients of the smoking ban indicator or intensity index in Table 3.8 do not provide support for this hypothesis. Also, explicitly controlling for revenues of restaurants and bars as in Table 3.11 does not yield any different conclusion. Thus, in light of the results presented in Table 3.11 and 3.10 it seems unlikely that a major part of the relative decrease in waiters' wages caused by smoking bans was driven by a decline in hospitality revenues.

3.5.2 Hours Worked

The analyses so far showed that the introduction of smoking bans was associated with lower *daily* wages. However, lower daily wages could either be the result of lower hourly wages or fewer hours worked (or a combination of the two). Ideally, I would like to decompose the total effect into the part that is due to a change in hours worked and the part that is due to a change in the hourly wage. Unfortunately, in the IAB administrative labor market data I do not observe the hours worked (apart from the distinction between different forms of full- or part-time jobs). Therefore, I use data from the Microcensus where individuals report their usual hours worked per week.[31] and construct a sample as similar to the IAB wage sample as possible by selecting all individuals with a mini-job between 17-62 years in East and West Germany. The key drawback using Microcensus data for the purpose at hand is that it comes in *yearly* and not in monthly intervals.[32] Another caveat of the Microcensus data is that – although participation is compulsory – most variables and in particular the hours worked are self-reported and – given the official nature of the survey – it is unlikely that individuals will report informal hours (compare Boockmann et al. 2010, 43f). Bearing these limitations in mind, the Microcensus is still the best available data set to analyze the effect of smoking bans on hours worked by waiters in mini jobs in Germany.

[31] Ideally, I would also re-estimate the effect of smoking bans on *wages* based on Microcensus data, however, one can only observe the net income of a person (or household) including transfers and other income sources but not the wage associated with a specific job.

[32] The German Socio-Economic Panel (GSOEP) would be another data set where the hours of work can be observed. This data set, however, suffers from too small sample sizes to conduct analyses at the level of waiters in mini-jobs.

The yearly data structure and the distinction of a treatment and control groups lends itself to a synthetic control approach pioneered by Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2014). The advantage of this approach compared to a conventional difference-in-differences design is that instead of giving each control unit (implicitly) the same weight, the synthetic control approach assigns data driven weights to construct a control group that optimally matches the evolution of the pre-treatment outcome of the treated unit without relying on post-treatment data. Specifically, I use a synthetic control approach to estimate the effect of smoking bans on hours worked of mini job workers using waiters as the treatment group and all remaining occupations with at least 20 observations per state as the donor pool over 2004-2009, a three-year window around treatment. As pre-treatment predictors I use mean age, the share of females, the share of workers located in East Germany and the hours worked averaged over the pre-treatment period. Including more or less predictors or choosing values of these predictors only in specific pre-treatment periods has little influence on the results.[33]

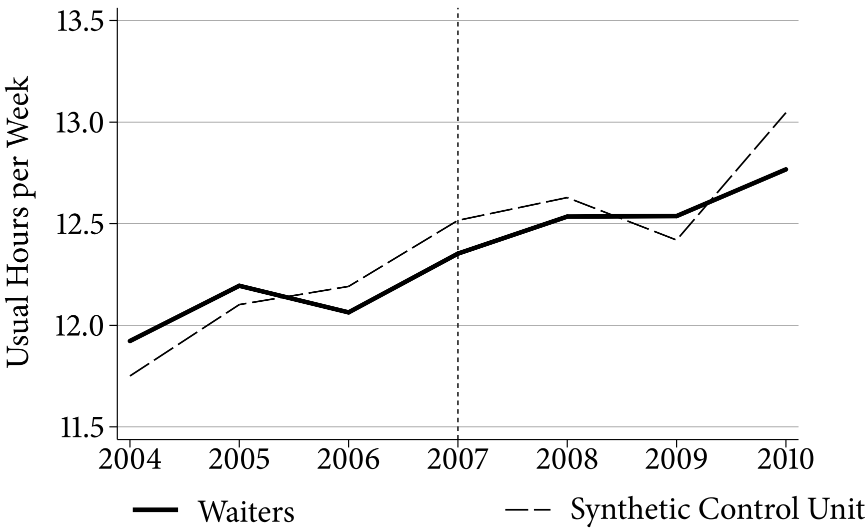
Figure 3.8 shows the results of this exercise. In Figure 3.8a, I use all 16 states and define 2007 as the treatment date since three out of the 16 states introduced smoking bans already in 2007. The synthetic control group in this setting is composed of cashiers (75%) and auxiliary (geriatric) nurses (25%). A full set of donor occupations and corresponding synthetic control group weights is given in Table C.9. A comparison of the usual hours worked per week between the treatment group (waiters) and its synthetic control group shows that the two moved practically in parallel and continue to do so post treatment. There is no evidence that waiters would have significantly reduced their hours compared to their synthetic control group, i.e. there is no evidence that smoking bans led to a decline in hours worked. The same conclusion holds when I restrict attention to only those thirteen out of sixteen states which introduced smoking bans in 2008 (Figure 3.8b).[34] The co-evolution between the treatment and control group is even more evident in this case and again there

[33] Furthermore, I run the fully nested and fully robust (global) optimization procedure that searches among all (diagonal) positive semidefinite V-matrices and sets of W-weights for the best fitting convex combination of the control units (options `nested` and `allopt` of Hainmueller, Abadie, and Diamond's `synth` package for Stata).

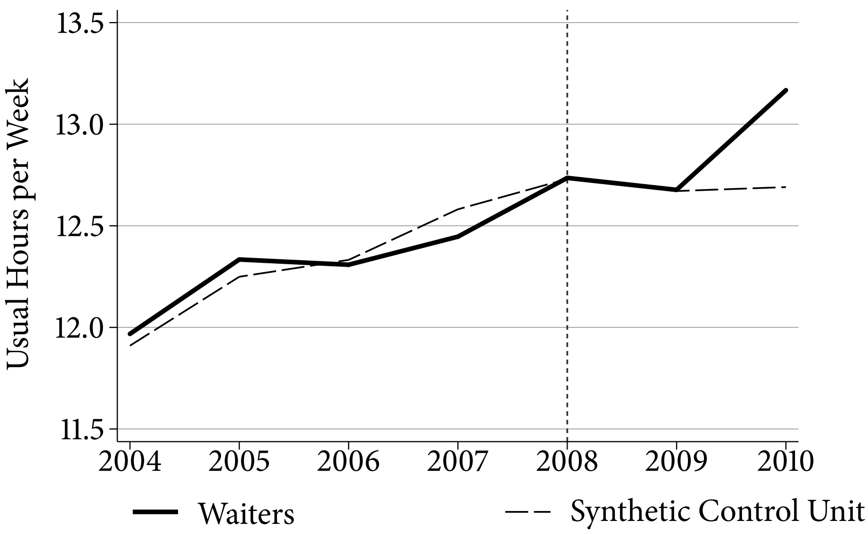
[34] When focusing on only those states which introduced smoking bans in 2007, the synthetic control approach does not converge likely due to smaller sample sizes resulting in an inadequate donor pool.

Figure 3.8: Evolution of Hours Worked (Synthetic Control Group Approach)

(a) All States (16/16)



(b) Only States with Smoking Bans Introduced in 2008 (13/16)



Notes: This figure compares the evolution of the usual hours worked per week of mini job workers employed as waiters to a synthetic control group constructed from a pool of all other mini job workers in occupations with at least 20 observations per state. Data is taken from the Microcensus. The predictor variables are averaged over the entire pre-treatment period and include age, the share of females, and the share of workers in East Germany along with the hours worked.

is no indication for a decline in the hours worked related to the introduction of smoking bans. For completeness, in Figures C.6 and C.7 in the Appendix, I show for each of the two samples two graphs commonly used for inference in a synthetic control setting. These graphs show in two different ways the results of a placebo exercise in which treatment is iteratively re-assigned to each control state in the donor pool and compared to the effect corresponding to the actual treatment. In line with Figures 3.8a and b, these permutation exercises do not show any significant effect of smoking bans on the hours worked by waiters.

Although the synthetic control group approach is well suited in the given setting, I also run a triple difference-in-differences approach at the individual level applying the same sample restrictions as I used in the synthetic control group approach. The two approaches implicitly rely on two different identifying assumptions: While the synthetic control approach requires that in the absence of treatment, the post-treatment outcomes of the treatment and control group would be the same conditional on past-outcomes and observed covariates (*independence conditional on past outcomes*), the DDD estimation identifies the treatment effect based on the parallel trends assumption. A priori it is not clear which assumption is more tenable. It is therefore reassuring that the results of this DDD exercise (see Table C.10 in the Appendix) yield a positive but insignificant effect of smoking bans on working hours and thus confirm the findings of the synthetic control group approach.[35] Summarizing, based on the best data available and using two different methodologies, I find no evidence that smoking bans had a negative impact on the hours worked by hospitality workers.

3.5.3 Selection

If the decline in wages after the introduction of smoking bans is not due to a decline in revenues or hours worked, could it be driven by selection – both on observable

[34] Standard diagnostics suggest to use log hours instead of hours in levels as the dependent variable.

[35] In Table C.10, I present DDD-results using three different treatment indicators: (i) a simple ban dummy that switches on in the year a smoking ban was introduced in a state, (ii) the treatment intensity measure used throughout in the previous analyses, and (iii) an adjusted treatment intensity measured scaled by the share of the year a smoking ban is in place. Given that I only have pooled cross-sections over time and thus cannot include individual fixed effects, I add a rich set of individual and state level (potentially time varying) controls.

and unobservable characteristics? To explore the role of observables, in Table 3.12 I check whether smoking bans led to a significant change in observable characteristics of workers. For instance, it could be that after the introduction of smoking bans individuals who had previously not considered working in a bar take up a job in a now smoke-free bar. Consequently, we should see a drop in tenure or the experience in the hospitality industry. As these characteristics co-move with time, are fixed, or slow moving, estimation is without individual fixed effects. Table 3.12 shows that there is little evidence for a change in observable characteristics associated with smoking bans. In general, using extended controls only explains very little of the overall variation in these variables. In line with this, a power analysis suggests that the ability to detect a true effect given a significance level of 5% in the given setting is far below 70% and often lower than 15%. The exceptions are months since entry, being low-skilled, medium-skilled and whether an individual has previously been unemployed with powers above 80%. Taken at face value, Table 3.12 suggest that after the introduction of smoking bans, individuals tend to have worked less time in jobs covered by social security (proxied by the months since first entry in the IAB-records), are less often low skilled and more often medium-skilled (note that students are recorded as being medium skill) and are less likely to come out of unemployment. A story consistent with these results is that smoking bans changed the composition of workers in bars and restaurants such that now more educated and less experienced individuals work there.

Another way to explore the role of observable and (time-constant) unobservable characteristics is presented Table 3.13. Column 1 reproduces the baseline effect.[36] In column 2, I do not include individual fixed effects. The treatment effect turns insignificant and positive. This is an interesting finding as it suggests that not controlling for time-constant observable and unobservable characteristics, smoking bans did not have a significant effect on waiters' wages. Together, columns 1 and 2 indicate that waiters after the introduction of smoking bans are *positively* selected. Not controlling for this selection, results in an upward biased treatment effect, a bias that plagued some of the previous literature. In column 3 (again estimated without individual fixed effects), I include a host of observable individual characteristics

[36] To increase power for the estimation without individual fixed effects, I use a longer time span starting from January 2005. All conclusions remain unchanged when I use a shorter time period.

Table 3.12: Change in Observable Characteristics due to Smoking Bans

Dependent Variable	Intensity Coefficient	Adj. R^2
1. Experience in Hospitality Industry (in Months)	-0.507 (0.308)	0.014
2. Tenure	-0.396 (0.466)	0.012
3. Occupation Not Waiter (Previous Spell)	0.003 (0.015)	0.020
4. Sector Not Hospitality Industry (Previous Spell)	0.005 (0.015)	0.019
5. Months since First Entry	-2.231* (1.248)	0.017
6. Age	-0.363 (0.218)	0.006
7. Female	-0.001 (0.007)	0.012
8. German	-0.004 (0.008)	0.259
9. Low Skilled	-0.021*** (0.007)	0.016
10. Medium Skilled	0.024*** (0.008)	0.018
11. High Skilled	-0.003 (0.004)	0.003
12. Secondary Job	0.005 (0.012)	0.026
13. Previous Spell: Full-Time	-0.001 (0.004)	0.007
14. Previous Spell: Long Part-Time	0.008** (0.004)	0.001
15. Previous Spell: Short Part-Time	-0.006 (0.014)	0.017
16. Previous Spell: Trainee	0.001 (0.001)	0.002
17. Previous Spell: Unemployed	-0.010* (0.005)	0.017

Notes: This table shows how observable characteristics among employed waiters change with the intensity of a smoking ban. Each row represents a separate regression with the same right-hand side variable specification using extended controls and a different dependent variable indicated in the first column of each row. The sample consists of all spells of waiters in the hospitality sector. All specifications include extended controls. The sample covers January 2005 to February 2009. Standard errors clustered at the state level. Standard errors are clustered at the state-level. ***/**/* indicate significance at the 1%/5%/10% level.

Table 3.13: Exploring the Role of Selection

	Dependent Variable: *Log Wages*				
	(1) Baseline	(2) No Individual FEs	(3) + Individual Controls	(4) Stayers vs. Changers	(5) Worker × Firm FEs
Ban Intensity	-0.025*** (0.006)	0.013 (0.014)	0.018 (0.011)	-0.028** (0.005)	-0.016*** (0.004)
Changed Firm After Ban × Ban Intensity				-0.017 (0.035)	
Baseline Controls	✓	✓	✓	✓	✓
Individual FEs	✓			✓	
Individual Controls			✓		
Worker × Firm FEs					✓
Start	Jan 2005	Jan 2005	Jan 2005	Jan 2005	Jan 2005
End	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009
Clusters	16	16	16	16	16
Individuals	19,538	19,538	19,538	7,924	19,538
Observations	245,145	245,145	245,145	158,253	245,145
Adj. R^2	0.839	0.020	0.082	0.829	0.885

Notes: This table presents various specifications exploring the role of selection. The units of observation are individuals working in guest attending occupations in the hospitality sector. The sample is restricted to employees in mini jobs only. The dependent variable is the log individual daily wage. Individual controls include dummies for being female, German, indicators for each of 9 age- and 3 education categories, hospitality experience, tenure, a dummy for whether the previous spell was not as a waiter, a dummy for whether the previous spell was outside the hospitality industry, the months since the first entry in the IAB data, whether the spell overlaps with another employment spell, and dummies for whether the previous spell was a full-time, part-time, vocational training, or unemployed. Extended controls include person-, time-, state-, and state×month fixed effects, linear pre-trends, and current and six lags of the monthly state-unemployment rate. Standard errors clustered at the state level. Standard errors are clustered at the state-level. ***/**/* indicate significance at the 1%/5%/10% level.

such as age, gender, education, and previous labor market and hospitality industry experience detailed in the table note. Surprisingly, the results barely change and the increase in R^2 is marginal. Even with this extensive set of covariates, time and state fixed effects, I am only able to explain about 8% of the variation in individual wages. This in turn, suggests, that the bulk of selection is due to *unobservable* time-constant characteristics. These could potentially include productivity, friendliness, or appearance for all of which the market is likely to offer higher wages. This reasoning is in line with the results of Table 3.12 where I found little evidence for positive selection in terms of observables. In column 4 I check whether the wage decrease is different for those who stay for the subsequent six months at the same firm they had worked at least one month before the introduction of a smoking ban ("stayers") and those who do not ("changers"). The motivation behind this exercise is that is might be easier to adjust wages for new contracts. Remember, however, that since mini jobs are flexible contracts, hours and wages can potentially be changed every month. There is some evidence that indeed, the effect is larger for "changers", i.e. those changing employers after the introduction of smoking bans. This difference, however, is not significant and a priori it is not clear in which direction the effect would go as "changers" likely act rationally and would only change a job if some kind of gain could be achieved. In line with this and the idea of an unobserved worker-firm specific match component, in column 5 I include worker-firm fixed effects, a rather demanding specification since the treatment effect is now identified only from within spell variation, i.e. from changes in wages of worker who remain at a given firm pre- and post-treatment. The treatment coefficient decreases slightly but remains highly significant. This piece of evidence strongly supports the interpretation of the effect as compensating differentials and overcomes many of the identification issues that plagued previous studies as discussed above.

While not conclusive, the evidence suggests that smoking bans attract a different sort of workers who are positively selected in terms of their unobservables. These unobservables in turn are likely to be correlated with the degree of aversion to second-hand-smoke. Put differently, the new workers are both more productive and likely to have a stronger dislike for second hand smoke. Another piece of evidence consistent with this reasoning may be found in Table 3.2. The regressions presented there can also be interpreted as a difference-in-difference analysis of the effect of smoking bans on waiters' propensity to smoke using individuals in all other occupations as a control group. Although the DD coefficient (Waiters×2009) for the group of mini job holders (columns 3 and 4 of Table 3.2) is estimated imprecisely, the coefficient is sizable and negative implying a drop in the propensity to smoke of about 5 percentage points for the group of waiters in mini jobs relative to the general trend of other workers in mini jobs.[37]

3.5.4 *Employment*

The reasoning presented in the previous section, i.e. that smoking bans led to a positive selection of workers, implies that after the introduction of smoking bans new workers are either substituting existing workers or are added to the set of existing waiters (or a combination of the two). Note that both possibilities are consistent with the simple framework presented in Section 3.2.

To assess the extent of these channels, I first present an event study graph of the probabilities to start and end a job in a given month relative to the introduction of a smoking ban. The regression does a good job at removing trends and obvious seasonal patterns, but there still remains a substantial amount of variation. However, there is no clear change or spike in neither the probability to start nor to end a job around the introduction of a smoking ban. Columns 1 to 3 of Table 3.14 present the corresponding intensity estimates also including turnover, i.e. the probability to start or end a job in a given month based on individual level data.[38] Consistent with the event study graphs, I find no significant effect of smoking ban intensities on turnover or the individual probabilities to start or end a job. The R^2 suggests that I am only able to explain a tiny fraction of the overall variation in turnover which is highly volatile even after controlling for time, and state × month specific fixed effects. A power calculation confirms that the setting is underpowered (<0.10).

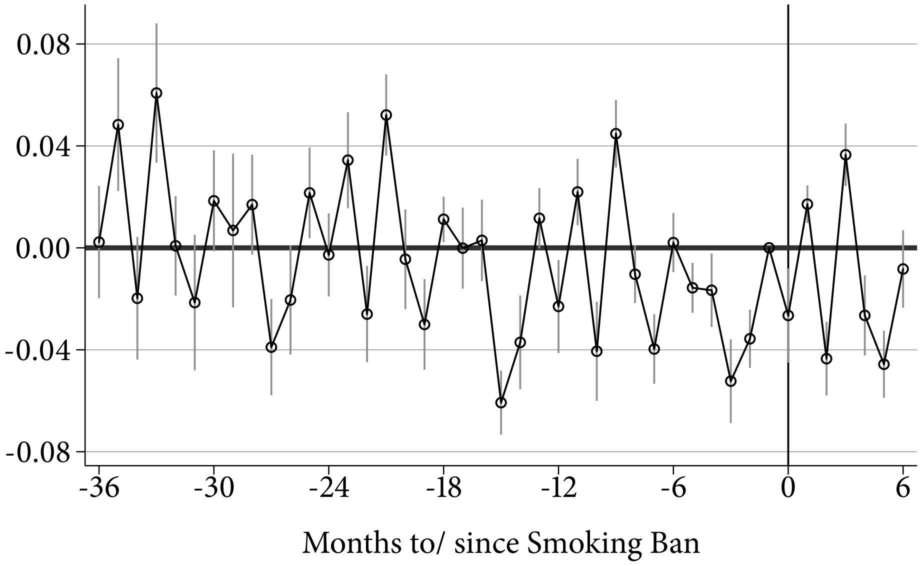
A second approach presented in columns 4 and 5 of Table 3.14 relies on data aggregated at the state level. The coefficients indicate a one percentage point increase

[37] Adding a more extensive set of census region-, occupation-, and sector fixed effects leaves the estimates and standard errors virtually unchanged.

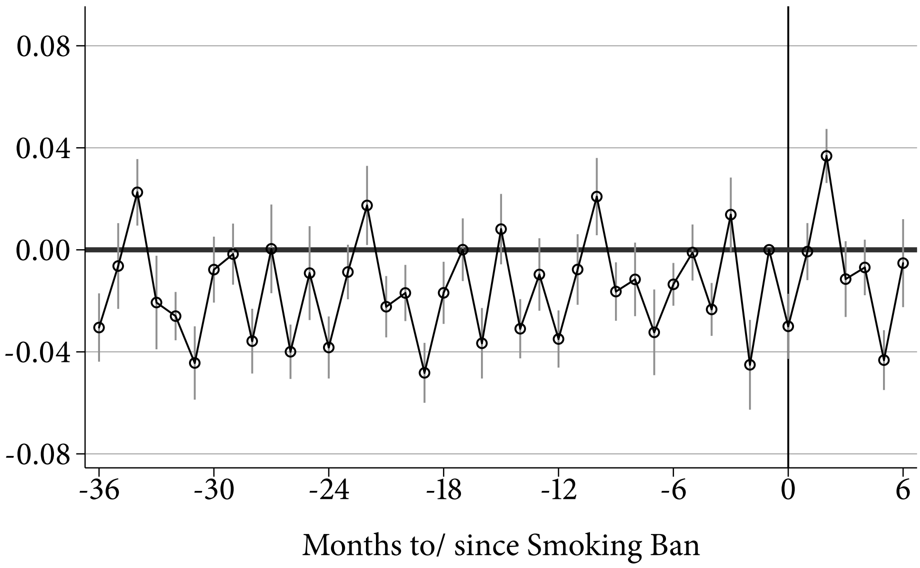
[38] In accordance with the subsequent graph, I choose a bandwidth around treatment of 36 months prior and 6 months after the treatment, the findings do, however not depend on this.

Figure 3.9: Event Study Graph Change in Probability to Start and End a Spell

(a) Probability of Starting a New Spell



(b) Probability of Ending a Spell



Notes: This figure plots the regression coefficients of dummies indicating the months to or since the introduction of a smoking for the period 36 months prior and up to 6 months after the introduction of a smoking ban on a dummy indicating whether a worker started (Panel A) or ended (Panel B) a spell in a given month controlling for individual-, time-, state-, and state × month fixed effects. The period one month prior to the introduction is the reference period. The vertical gray lines indicate the 95% confidence intervals of each point estimate. Standard errors clustered at the state level.

Table 3.14: Turnover and Employment Effects of Smoking Bans

	Probability to … a Job (Individual Level Data)			Employment (State Level Data)	
	(1) Start or End	(2) Start	(3) End	(4) ln(Jobs)	(5) ln(Turnover)
Ban Intensity	0.003	0.002	0.003	0.009	0.055
	(0.005)	(0.003)	(0.004)	(0.026)	(0.048)
Extended Controls	✓	✓	✓	✓	✓
Months included before treatment	36	36	36	36	36
Months included after treatment	6	6	6	6	6
Clusters	16	16	16	16	16
Individuals	17,726	17,726	17,726	-	-
Observations	209,997	209,997	209,997	688	688
Adj. R^2	0.007	0.009	0.016	0.998	0.970

Notes: This table shows results from linear regression models of the impact of the intensity of a smoking ban on various employment indicators among the group of waiters in the hospitality sector working in mini jobs. The units of observation in columns 1-3 are individuals working in guest attending occupations in the hospitality sector and in columns 4-5 these are aggregated at the state-month level. ln(Jobs) is defined as the natural logarithm of the number of (person-month) spells in a given state-month cell +1. ln(Turnover) is defined as the total number of spells starting and ending in a given state-month cell +1. State level regressions are weighted by the number of underlying observations from with the data was aggregated. Standard errors are clustered at the state-level. ***/**/* indicate significance at the 1%/5%/10% level.

in the number of jobs and a 5.5 percentage point increase in turnover related to smoking bans. Although these coefficients are estimated imprecisely – most likely again due to a lack of statistical power – these results can at least be taken as suggestive evidence indicating an increase in turnover and a slight increase in jobs consistent with a compensating differentials mechanism.

3.6 Conclusion

The topic of compensating differentials has recently been re-emerging in the agenda of the economics profession. This is despite – or because – so far it has been hard to find compelling empirical evidence for this intuitive concept first put forward by Adam Smith.

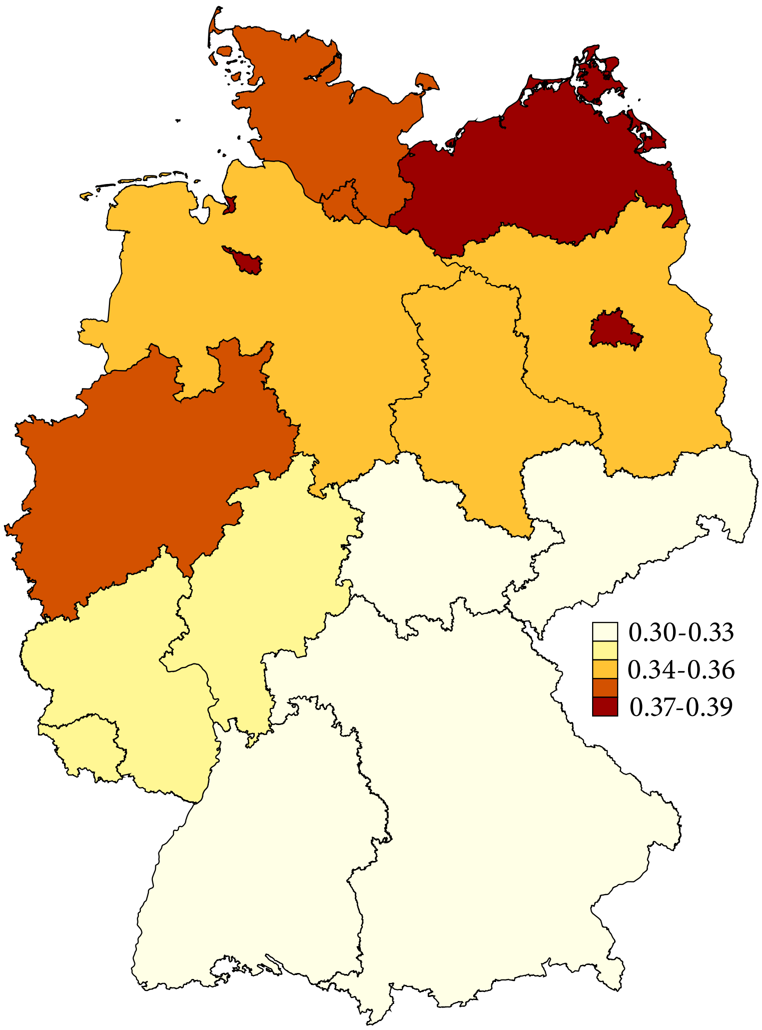
Exploiting a particularly suited policy experiment, I find evidence that strongly suggests that exposure to second hand smoke is priced in the labor market. The main idea of my setting is that in a competitive labor market employers in the hospitality sector have to pay their workers a wage premium to compensate for the exposure to second hand smoke. When a smoking ban is introduced, it is no longer necessary to pay such a premium and we should *ceteris paribus* see a drop in wages that exactly equals the compensating differential of bearing the exposure to second hand smoke. My setting offers three main advantages relative to the previous literature: (i) an arguably exogenous, salient and effective variation in amenities (being exposed or not to second hand smoke) that overcomes the endogeneity of amenities and jobs and their and opaqueness which plagued many of the previous analyses; (ii) the use of panel data which allows holding individual productivities constant; and (iii) a competitive and rather flexible segment of the labor market (hospitality workers in mini-jobs and the absence of a minimum wage) that allows the equilibrium to emerge more quickly and also makes an adjustment via prices more likely than via quantities (in contrast to, e.g. aus dem Moore and Spitz-Oener 2012).

My baseline estimates indicate that introducing a complete smoking ban that grants no exceptions (corresponding to Bavaria's first smoking ban of 2008) leads to a wage decrease of about 2.5%. Performing a battery of robustness checks, I rule out several confounding factors such as seasonal, political or weather effects such that I can put faith in a causal interpretation of my treatment effect. This evidence by itself, however, would not be enough to suggest that this effect can be interpreted as a compensating differential. I present evidence that refutes two main alternative channels, namely a decrease in revenues of bars and restaurants and a reductions of hours worked. I also present suggestive evidence that indicates that smoking bans changed the selection of workers who tend to be more educated and less experienced after their introduction. A major part of selection, however, is occurring on unobservable individual characteristics captured by individual fixed effects. Consistent with the view that the selection of workers has changed, I find some evidence indicating a slightly increased turnover and an increase in employment after the introduction of smoking bans. Taken together, these results are consistent with the predictions of a simple compensating differentials model.

Appendix C

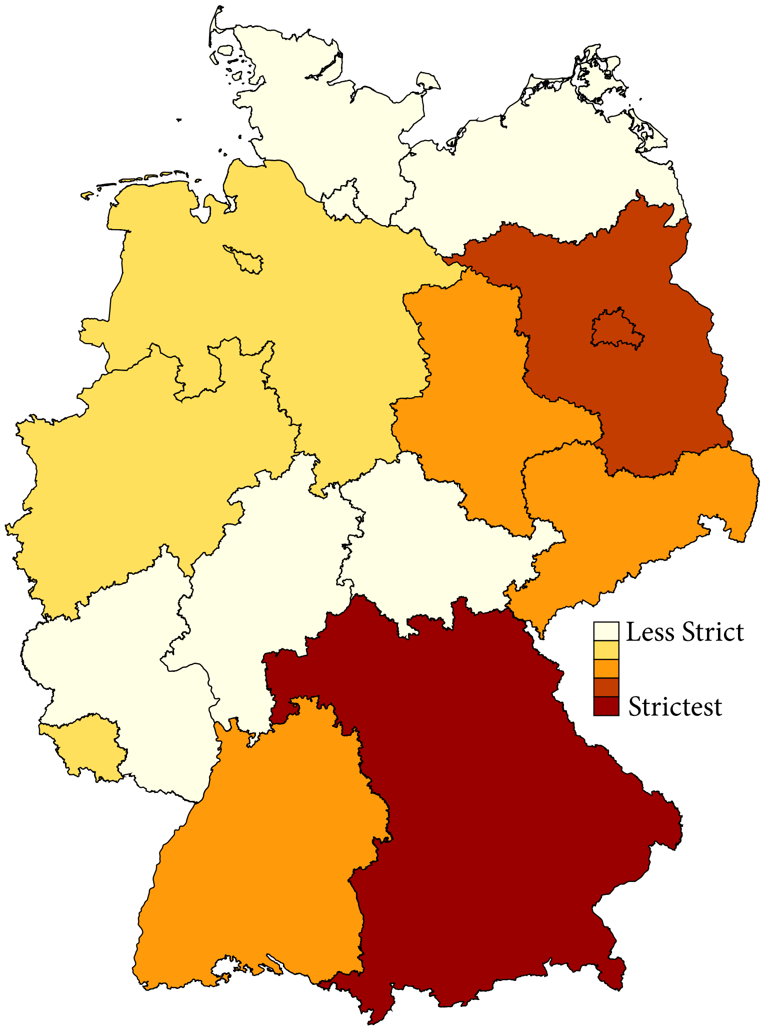
C.1 Additional Figures and Tables

Figure C.1: Map of Share of Population Smoking (Microcensus 2005)



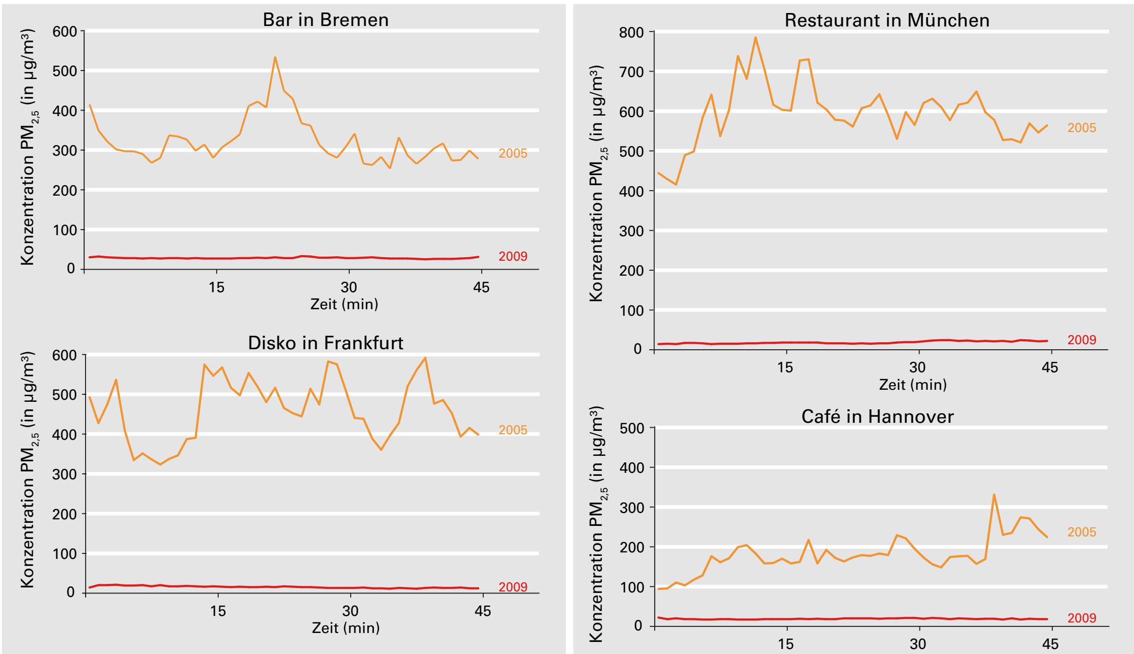
Notes: This map shows the share of smokers in each state in 2005 based on Microcensus data. The sample is based on Microcensus waves 2005 and 2009 and is restricted to individuals aged 17-62 not in civil service (*Beamte*) and with non-missing values the control variable values used in Table 3.2. Statistics are weighted by survey weights.

Figure C.2: Map of Initial Smoking Ban Intensities in Germany



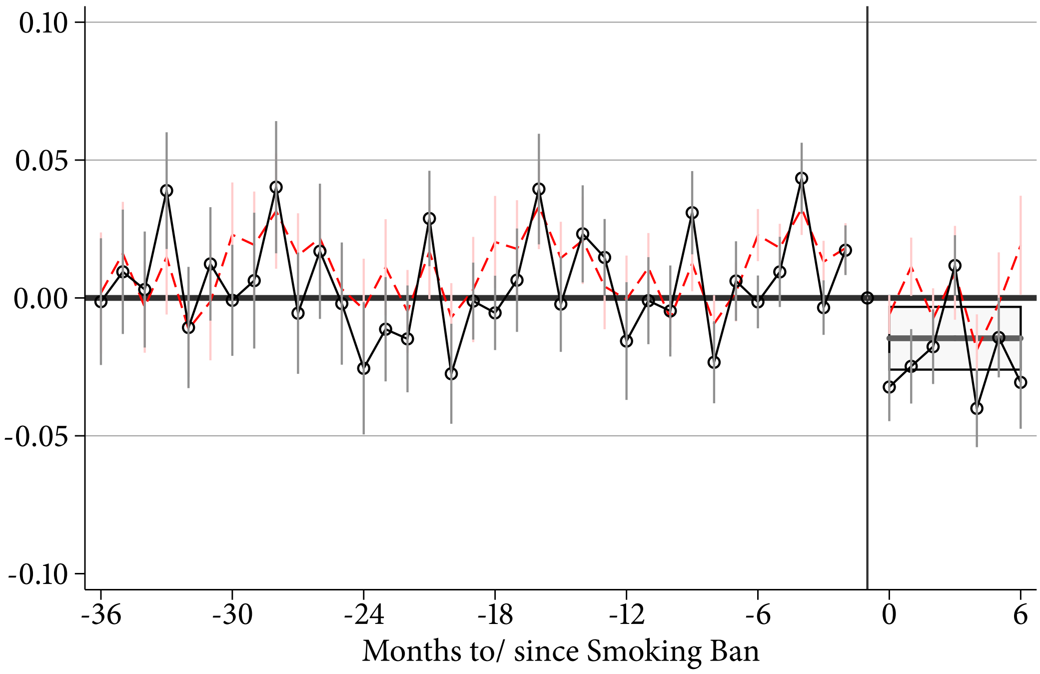
Notes: This map shows the initial intensity of smoking bans according to the index specified in equation 3.1. "Strictest" refers to the strictest ban (corresponding to Bavaria's initial smoking ban, index value 1) and "less strictest" to the less strict ban observed (corresponding to Rhineland-Palantinate, index value 0.5).

Figure C.3: Air Quality Before and After the Introduction of Smoking Bans in German Hospitality Establishments with a Comprehensive Ban



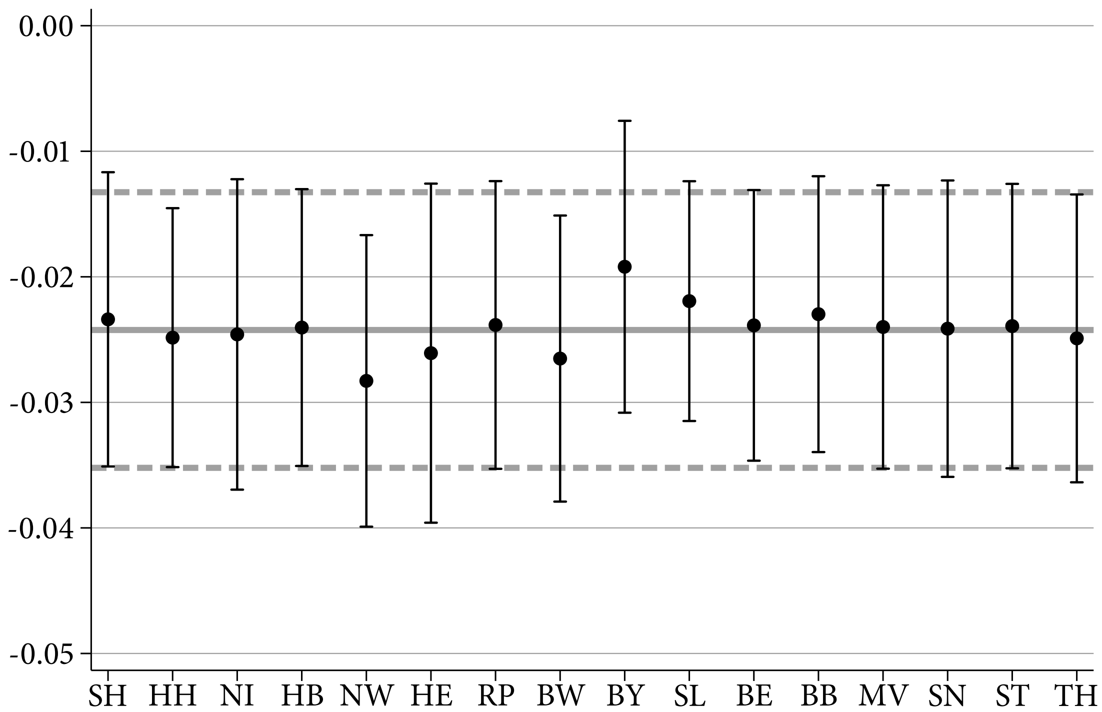
Notes: This figure compares the times series of the average concentration of particles up to 2.5 µm in the indoor air before (dark gray/ red) and after (light gray/ orange) the introduction of smoking bans in hospitality establishments in Germany with a comprehensive smoking ban. Source and more details: DKFZ (2010, 25ff).

Figure C.4: Illustration of DDD Approach



Notes: See notes for Figure 3.5.

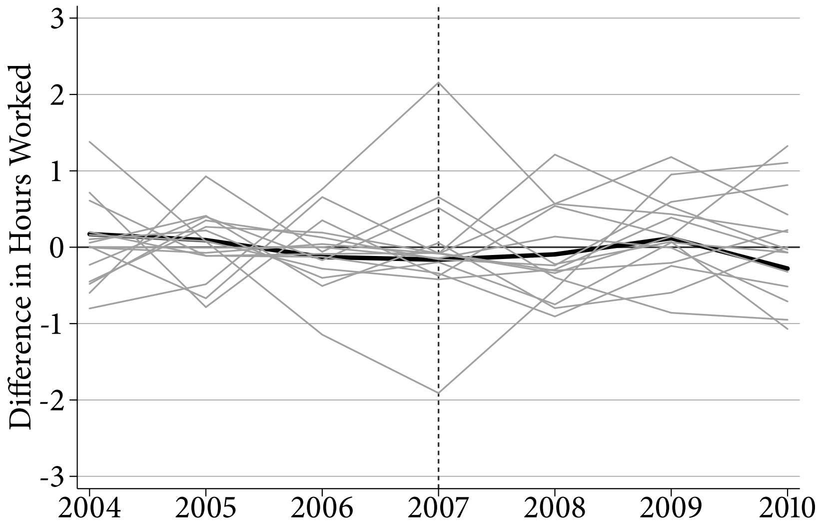
Figure C.5: Leave One State Out at a Time



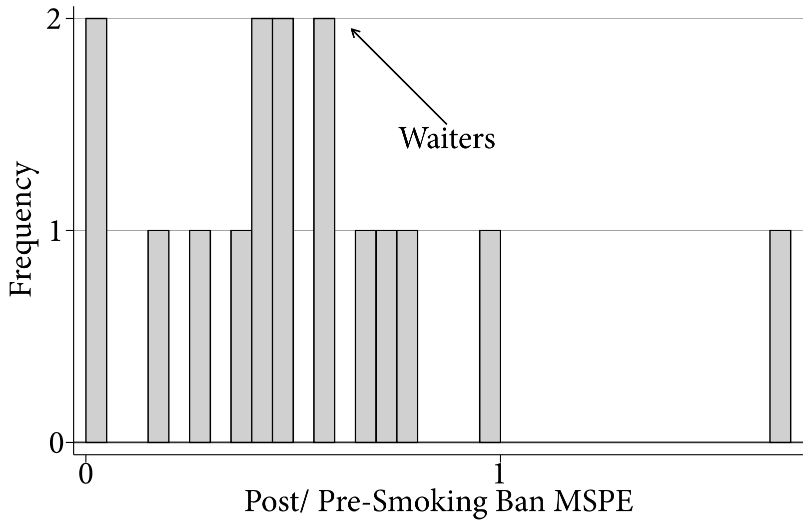
Notes: This figure plots the coefficients (filled black dots) and corresponding 95% confidence intervals (dashed lines) from regressions using extended controls of the smoking ban intensity on waiters' log wages where observations from the state indicated on the x-axis are left out. The solid thick gray line (dashed gray lines) refers to the baseline estimate (95% confidence interval) including observations from all 16 states.

Figure C.6: Synthetic Control Inference Graphs
(All States)

(a) Treatment and Placebo Differences in Hours Worked

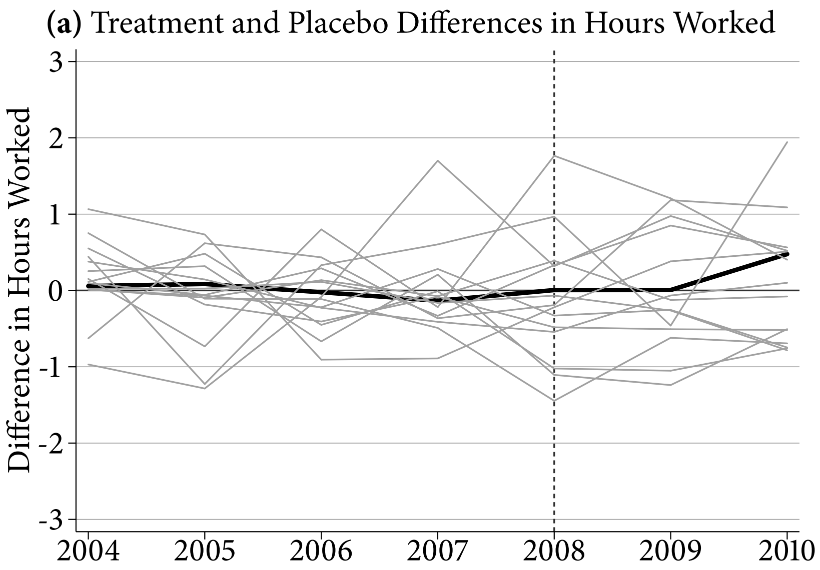


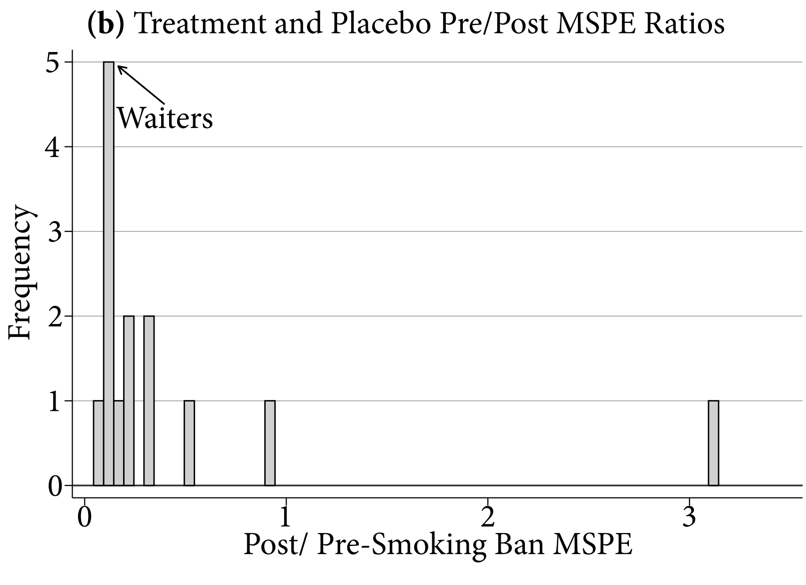
(b) Treatment and Placebo Pre/Post MSPE Ratios



Notes: This figure presents two approaches commonly used to inference in a synthetic control approach. Figure C.6a, shows the result of a placebo exercise in which all occupations in the donor pool are iteratively assigned to be treated while waiters are moved into the control group. Figure C.6b plots the ratios of the pre- and post mean squared prediction errors (MSPE). In neither measure does the occupation of waiters appear to be significantly different from other occupations. For two occupation groups no synthetic control group could be constructed, they remain, however, in the donor pool.

Figure C.7: Synthetic Control Inference Graphs
(Only States with Ban Introduction in 2008)





Notes: This figure presents two approaches commonly used to inference in a synthetic control approach. Figure C.7a shows the result of a placebo exercise in which all occupations in the donor pool are iteratively assigned to be treated while waiters are moved into the control group. Figure C.7b plots the ratios of the pre- and post mean squared prediction errors (MSPE). In neither measure does the occupation of waiters appear to be significantly different from other occupations. For two occupation groups no synthetic control group could be constructed, they remain, however, in the donor pool.

Table C.1: Initial Smoking Ban Regulations in German States (until August 2008)

State	Ban Introduction (Legal)	Ban Introduction (Enforced) *if different*	(i) Smoking Room (Restaurants & Bars) SR1	(ii) Smoking Room (Dancing Clubs) SR2	(iii) Exception for Small Bars SB	(iv) Exception for Party Tents PT	**Intensity** (Baseline)
BW	2007 - 08		✓			✓	0.80
BY	2008 - 01						1.00
BE	2008 - 01	2008 - 07	✓				0.85
BB	2008 - 01	2008 - 07	✓				0.85
HB	2008 - 01	2008 - 07	✓	✓		✓	0.65
HH	2008 - 01		✓	✓		✓	0.65
HE	2007 - 10		✓	✓		✓	0.65
MV	2008 - 01	2008 - 08	✓	✓			0.70
NI	2007 - 08	2007 - 11	✓	✓			0.70
NW	2008 - 07		✓	✓			0.70
RP	2008 - 02		✓	✓	✓	✓	0.50
SL	2008 - 02	2008 - 06	✓	✓			0.70
SN	2008 - 02		✓		✓	✓	0.80
ST	2008 - 01	2008 - 07	✓			✓	0.80
SH	2008 - 01		✓	✓	✓	✓	0.65
TH	2008 - 07		✓	✓		✓	0.65

Notes: Taken from respective state laws from *beck-online*, Ahlfeldt and Maennig (2010, 516ff, table A.1). Ticks in light gray indicate that exception was only granted for owner-operated bars without employees and thus was not considered in the empirical analysis.

Table C.2: Index Weights used to Construct the Intensity Index

Type	Employees	WZ 2008	Weight ω
Restaurants & Bars, large (LB)[a]	567,900	56.1, 56.301, 56.303, 56.304, 56.309	0.66
Dancing Clubs (DC)	26,982	56.302	0.03
Restaurants & Bars, small (SB)[b]	250,428	56.1, 56.301, 56.303, 56.304, 56.309	0.30
Party Tents (PT)[c]	11,590	56.1, 56.301, 56.303, 56.304, 56.309	0.01
Total	856,900	56.1, 56.3	1.00
Other Food Services	91,132	56.2	–
Accomodation	408,599	55	–
Total Hospitality Industry	1,356,631	55, 56	–

Note: [a] 6 or more employees. [b] up to 5 employees. [c] estimated as 1% of employees in large restaurant and bars.
Source: Data refer to the year 2007 and are taken from the Yearly Statistics in the Hospitality Industry (*Jahresstatistik im Gastgewerbe*) published by the Federal Statistical Office (Statistisches Bundesamt 2011).

Table C.3: Potential Determinants of the Introduction Time of a State's Smoking Ban

	Dependent Variable: *Introduction Time of Smoking Ban*										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Ban Intensity	-1.321										64.551
	(1.157)										(44.643)
Share Smokers in 2005 (%)		0.122									-1.391
		(0.191)									(0.771)
Share Foreign Tourists			-4.543								-51.850
			(8.884)								(29.793)
Months to Election				-0.004							-0.155*
				(0.049)							(0.066)
ln(Population)					-0.411						-3.751
					(1.121)						(3.703)
Conservative Index						-1.244					-17.084
						(1.019)					(16.836)
Trend Unemployment Rate 2005-07							-0.310				2.266
							(0.294)				(1.532)
Trend Hospitality Wages 2005-07								-0.686			-10.374
								(1.595)			(8.432)
Trend Bar Revenues 2005-07									0.547		1.389
									(1.283)		(2.826)
Trend Restaurant Revenues 2005-07										1.220	6.212
										(1.362)	(4.041)
Observations	16	16	16	16	16	16	16	16	14	14	14
R^2	0.002	0.013	0.015	0.000	0.016	0.039	0.059	0.007	0.020	0.063	0.672
Adj. R^2	-0.069	-0.057	-0.055	-0.071	-0.055	-0.029	-0.008	-0.064	-0.062	-0.015	-0.423

Notes: This table shows correlations between potential determinants of the introduction date of a state's smoking ban. The dependent variable is the introduction time of a state's smoking ban (measured in Stata's monthly date format, e.g. 571 refers to August 2008). The ban intensity refers to the intensity of the smoking ban in the month it first became effective. The conservative index is defined as the vote shares of CDU/ CSU and FDP over the the shares of SPD, Greens and the Left. The trend variables refer to coefficient from a regression of the state level unemployment rate, the revenues of bars, restaurants, and in the unemployment rate, respectively, on time. Robust standard errors in parentheses. ***/**/* indicate significance at the 1%/5%/10% level.

Table C.4: Potential Determinants of the Intensity of a State's Smoking Ban

	Dependent Variable: *Smoking Ban Intensity Index*										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Ban Introduction Date	-0.002										0.007*
	(0.001)										(0.003)
Share Smokers in 2005 (%)		-0.014									0.015
		(0.011)									(0.008)
Share Foreign Tourists			0.272								0.516
			(0.298)								(0.312)
Months to Election				-0.003							0.002
				(0.002)							(0.001)
ln(Population)					0.044						0.025
					(0.036)						(0.041)
Conservative Index						0.147*					0.309
						(0.073)					(0.090)
Trend Unemployment Rate 2005-07							-0.001				-0.032
							(0.011)				(0.007)
Trend Hospitality Wages 2005-07								0.021			0.088
								(0.032)			(0.093)
Trend Bar Revenues 2005-07									-0.024		-0.019
									(0.038)		(0.037)
Trend Restaurant Revenues 2005-07										-0.051	-0.045
										(0.045)	(0.050)
Observations	16	16	16	16	16	16	16	16	14	14	14
R^2	0.002	0.150	0.043	0.160	0.145	0.441	0.000	0.006	0.032	0.090	0.969
Adj. R^2	-0.069	0.090	-0.025	0.100	0.084	0.401	-0.071	-0.065	-0.049	0.014	0.868

Notes: This table shows correlations between potential determinants of the intensity of a state's smoking ban. The dependent variable is the intensity of a state's smoking ban at the month it first become effective. The conservative index is defined as the vote shares of CDU/CSU and FDP over the the shares of SPD, Greens and the Left. The trend variables refer to coefficient from a regression of the state level unemployment rate, the revenues of bars, restaurants, and in the unemployment rate, respectively, on time. Robust standard errors in parentheses.

Table C.5: The Effect of Smoking Bans on Waiters' Wages (Non-Enforced Intensity Measures)

	All Workers		Full-Time		Regular Part-Time		Mini Jobs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Smoking Ban Indicator								
Smoking Ban	-0.003	0.001	-0.003	-0.003	0.000	0.003	-0.011	-0.011
(not enforced)	(0.004)	(0.005)	(0.002)	(0.002)	(0.007)	(0.006)	(0.004)	(0.004)
Adj. R^2	0.918	0.918	0.949	0.949	0.952	0.952	0.869	0.869
Panel B: Smoking Ban Intensity Index								
Ban Intensity	-0.022	-0.016	-0.003	-0.006	-0.000	0.006	-0.021	-0.022
(not enforced)	(0.015)	(0.013)	(0.002)	(0.002)	(0.009)	(0.008)	(0.006)	(0.006)
Adj. R^2	0.918	0.918	0.949	0.949	0.952	0.952	0.869	0.869
Reduced Controls	✓	✓	✓	✓	✓	✓	✓	✓
Extended Controls		✓		✓		✓		✓
Start	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006
End	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009
Clusters	16	16	16	16	16	16	16	16
Individuals	21,100	21,100	6,066	6,066	2,268	2,268	15,106	15,106
Observations	268,728	268,728	89,811	89,811	23,337	23,337	155,580	155,580

Note: This table shows regression results of the impact of smoking bans (using their legal instead of effective introduction dates) on individual log daily wages of different set of waiters in the hospitality sector. The set of reduced controls include person-, time-, state-, and state×month fixed effects. Extended controls include the set of reduced controls and additionally linear pre-trends specific for each set of workers (full-time, regular part-time, mini jobs), and current and six lags of the monthly state unemployment rate. Standard errors clustered at the state level.
Source: IAB wage data.

Table C.6: Alternative Index Definitions

	(1) Baseline	(2) Alternative Index	(3) Only LR	(4) Only DC	(5) Only SB
Ban Intensity	-0.024***	-0.019***	-0.030***	-0.013	-0.013***
	(0.006)	(0.006)	(0.005)	(0.008)	(0.004)
Baseline Controls	✓	✓	✓	✓	✓
SR1	0.66	0.30	1	0	0
SR2	0.030	0.30	0	1	0
SB	0.30	0.30	0	0	1
PT	0.010	0.10	0	0	0
Start	Aug 2006	Aug 2006	Aug 2006	Aug 2006	Aug 2006
End	Feb 2009	Feb 2009	Feb 2009	Feb 2009	Feb 2009
Clusters	16	16	16	16	16
Individuals	15,106	15,106	15,106	15,106	15,106
Observations	155,580	155,580	155,580	155,580	155,580
Adj. R^2	0.869	0.869	0.869	0.869	0.869

Notes: Marginal part-time employees only. SR1 = separate smoking room in restaurants and bars, SR2= smoking in dancing clubs, SB = smoking in small bars allowed, PT = smoking in party tents. Standard errors are clustered at the state-level. ***/**/* indicate significance at the 1%/5%/10% level.

Table C.7: Leave one State out at a Time

	Intensity
Schleswig-Holstein	-0.023***
	(0.006)
Hamburg	-0.025***
	(0.005)
Niedersachsen	-0.025***
	(0.006)
Bremen	-0.024***
	(0.006)
NRW	-0.028***
	(0.006)
Hessen	-0.026***
	(0.007)
Rheinland-Pfalz	-0.024***
	(0.006)
Baden-Wuerttemberg	-0.027***
	(0.006)
Bayern	-0.019***
	(0.006)
Saarland	-0.022***
	(0.005)
Mecklenburg-Vorpommern	-0.024***
	(0.006)
Sachsen	-0.024***
	(0.006)
Sachsen-Anhalt	-0.024***
	(0.006)
Thueringen	-0.025***
	(0.006)
Berlin	-0.024***
	(0.005)
Brandenburg	-0.023***
	(0.006)

Notes: All regressions replicate the baseline specification but leave out observations from the state indicated in the corresponding row. Marginal part-time employees only. Standard errors are clustered at the state-level. Standard errors are clustered at the state-level. ***/**/* indicate significance at the 1%/5%/10% level.

Table C.8: Control Groups for DDD-Estimations

Occupation Group (KldB 1988)	Observations	Percent
40 Cooks until ready-to-serve meals, fruit, vegetable preservers, preparers	75,810	21.9
56 Unskilled laborer/ assistants (no further specification)	6,860	2.0
73 Salespersons	20,380	5.9
81 Motor vehicle drivers	8,904	2.6
86 Stowers, furniture packers until stores/transport workers	2,333	0.7
93 Office specialists	5,924	1.7
97 Doormen, caretakers until domestic and non-domestic servants	4,378	1.3
116 Others attending on guests	32,272	9.3
117 Housekeeping managers until employees by household cheque procedure	10,852	3.1
119 Household cleaners until glass, buildings cleaners	23,006	6.6
115 Restaurant, inn, bar keepers, hotel proprietors, catering trade dealers until waiters, stewards	**155,561**	**44.9**
Total	346,280.0	100.0

Notes: Frequencies and percentage shares of groups used in the DDD-approach "All Marginal Part-Time Workers". Occupation group identifiers refer to the classification of occupations (version 1988).

Table C.9: Weights in Synthetic Control Approach

	(1) Occupation Group (KldB 1992)	(2) All	Only 2008
5	Gardening occupations (incl. florists)	0	0
41	Cooks	0	0
52	Dispatchers and storekeepers	0	0
53	Unskilled laborer/ assistants (no further specification)	0	0
66	Salespersons	0	0
70	Service salespersons and corresponding occupations	0	0
71	Land transport occupations (incl. Taxi and truck drivers)	0	0
73	Communication services (incl. mailmen)	0	0
74	Stockmen and transport workers	0	0
75	Management and consulting	0	0
77	Cashiers	0.747	0.763
78	Office helpers, secretaries and similar	0	0
79	Guards, doormen, janitors, pool attendants	0	0
85	Other health occupations (doctor's receptionists, massage therapists,...)	0.253	0
86	Social occupations (geriatric nurse, social workers,...)	0	0
87	Teachers	0	0
92	Housekeeper and aids	0	0
93	Cleaning occupations	0	0.100
99	Other worker without explicitly assigned tasks	0	0.137

Notes: This table provides the weights attached to each occupation group in the donor pool used in the synthetic controls approaches based on a sample that includes all stated (column 1) or only those which introduced smoking bans in 2008 (column 2).

Table C.10: Impact of Smoking Bans on Usual Hours Worked

	Dependent Variable: *Log Hours per Week*		
	(1)	(2)	(3)
Panel A: Ban Indicator			
Ban Indicator × Waiters	0.015	0.034	0.035
	(0.058)	(0.068)	(0.069)
Ban Indicator	-0.002	-0.011	-0.010
	(0.010)	(0.007)	(0.007)
Adj. R^2	0.091	0.150	0.151
Panel B: Intensity Measure			
Intensity × Waiters	0.029	0.049	0.052
	(0.067)	(0.080)	(0.083)
Intensity	-0.000	-0.011	-0.005
	(0.010)	(0.007)	(0.008)
Adj. R^2	0.091	0.150	0.151
Panel C: Adjusted Intensity Measure			
Adjusted Intensity × Waiters	0.115	0.124	0.119
	(0.075)	(0.081)	(0.082)
Adjusted Intensity	0.039	0.041	0.032
	(0.022)	(0.022)	(0.020)
Adj. R^2	0.091	0.150	0.151
State × Occup. FEs	✓	✓	✓
Year × Occup. FEs	✓	✓	✓
Individual Controls	✓		✓
Linear Occupation × State Specific Trends			✓
Start	2004	2004	2004
End	2009	2009	2009
Clusters	16	16	16
Observations	60,275	60,275	60,275

Notes: This table shows regression results of the log usual hours worked per week on various smoking bans treatment indicators. The sample is restricted to individuals in mini-jobs. Individual controls include dummies for being female, German, having a partner, children under 18 in the household, and main income source being from own work (opposed to transfers, pensions, interest, and other); indicators for each of three education categories, eight age categories, and eight city size categories; the state level monthly unemployment rate and tenure and tenure^2. Regressions weighted by survey weights. Standard errors clustered at the state level. ***/**/* indicate significance at the 1%/5%/10% level.

C.2 Sample Restrictions and Data Preparation

- **Sample Restrictions**: Following common practice when working with the IAB wage data, I drop spells with missing location information (after imputation, see below), spells of doctors and pharmacists (due to corrupted and missing records, see vom Berge et al. 2013), spells that last only one day, spells with statuses "seeking for employment but not registered unemployed", "without status", and "seeking advice", zero wage spells, spells with missing employment type information, full-time spells with earnings below the marginal earnings threshold, unemployment spells that overlap with non-unemployment spells and unemployment spells that overlap with other unemployment spells (and keep only one of them). This leaves me with a sample of 27,346,345 observations of 1,285,008 individuals. From this "prepared wage sample", I then derive my three baseline samples used in the DD- and DDD-analyses detailed in Tables C.11, C.12, and C.13.

- **Daily Wages**: I impute censored wages above the upper earnings threshold for compulsory social insurance (66,000 euros per year in 2010) using the "no heteroskedasticity" approach by Gartner (2005) and Dustmann et al. (2009). Specifically, I consider wages as censored that were up to two euros below the maximum wage value observed in each year and then estimate for each year and for males and females separately a censored regression of log wages on indicators of eight age groups, three skill groups and all their possible interactions, assuming that the error term is normally distributed and has the same variance across age and skill groups.

- **Education**: I impute missing education information following Fitzenberger et al. (2006) and group individuals in three categories (low, medium, and high). Low comprises those with at most a *Realschul* degree, missing education, and those who have not completed any vocational training, Abitur, or a tertiary degree. Medium contains those with vocational training or Abitur. High refers to all those with a completed tertiary degree (Fachhochschule or Universität).

- **Location**: If missing, location information is imputed with the last non-missing location.

- **Tenure**: For each individual, the number of months at the same employer as observed from his/ her IAB labor market biography are summed up (potentially since 1985).
- **Experience in Hospitality Industry**: For each individual, the number of months in the hospitality sector as observed from his/ her IAB labor market biography are summed up (potentially since 1985).

Table C.11: Restrictions of Baseline DD-Sample
(Waiters only)

Step	Observations		Individuals	
	Remaining	*Change*	Remaining	*Change*
Prepared wage sample	27,346,345	-	1,285,008	-
Drop unemployment spells	21,758,321	*-5,588,024*	1,276,456	*-8,552*
Restrict to spells in hospitality sector	934,871	*-20,823,450*	155,352	*-1,121,104*
Drop homeworkers	934,388	*-483*	155,316	*-36*
Restrict to waiters (occupation group 115)	325,109	*-609,279*	64,271	*-91,045*
Expand to monthly panel	1,752,391	*1,427,282*	64,271	-
Drop shorter overlapping spells in the same month and keep longest	1,686,745	*-65,646*	64,271	-
Restrict to Aug 2006 - Feb 2009	295,710	*-1,391,035*	22,412	*-41,859*
Restrict to marginal part-time jobs	155,580	*-140,130*	15,106	*-7,306*
Baseline DD-Sample (Waiters Only)	**155,580**		**15,106**	

Table C.12: Restrictions of Baseline DDD-Sample 1
(Waiters and Cooks)

Step	Observations		Individuals	
	Remaining	*Change*	Remaining	*Change*
Prepared wage sample	27,346,345	-	1,285,008	-
Drop unemployment spells	21,758,321	*-5,588,024*	1,276,456	*-8,552*
Drop homeworkers	21,737,531	*-20,790*	1,275,875	*-581*
Restrict to marginal part-time jobs	3,337,182	*-18,400,349*	520,523	*-755,352*
Restrict to spells in hospitality sector	349,185	*-2,987,997*	84,094	*-436,429*
Restrict to waiters (occupation group 115) and cooks (occupation group 40)	219,725	*-129,460*	56,380	*-27,714*
Expand to monthly panel	954,128	*734,403*	56,380	-
Drop shorter overlapping spells in the same month and keep longest	902,229	*-51,899*	56,380	-
Keep from August 2004 onwards (36 months before start of first ban)	584,332	*-317,897*	40,503	*-15,877*
Restrict to Aug 2006 - Feb 2009	231,867	*-352,465*	22,150	*-18,353*
Baseline DDD-Sample 1 (Waiters and Cooks)	**231,867**		**22,150**	

Table C.13: Restrictions of Baseline DDD-Sample 2
(Waiters and all other Occupations)

Step	Observations		Individuals	
	Remaining	*Change*	Remaining	*Change*
Prepared wage sample	27,346,345	-	1,285,008	-
Drop unemployment spells	21,758,321	*-5,588,024*	1,276,456	*-8,552*
Drop homeworkers	21,737,531	*-20,790*	1,275,875	*-581*
Restrict to marginal part-time jobs	3,337,182	*-18,400,349*	520,523	*-755,352*
Restrict to spells in hospitality sector	349,185	*-2,987,997*	84,094	*-436,429*
Drop occupation groups with less than 20 observations per state	330,355	*-18,830*	80,177	*-3,917*
Drop observations with missing occupation information	329,715	*-640*	79,937	*-240*
Expand to monthly panel	1,468,045	*1,138,330*	79,937	-
Drop shorter overlapping spells in the same month and keep longest	1,387,942	*-80,103*	79,937	-
Restrict to Aug 2006 - Feb 2009	346,143	*-1,041,799*	31,682	*-48,255*
Baseline DDD-Sample 2 (Waiters and All Other Occupations)	**346,143**		**31,682**	

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