Essays on Employment and Wages in the German Labor Market

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Abstract

This thesis consists of three essays that contribute to the empirical literature on employment and wages in the German labor market. The first essay investigates the impact of a large and unexpected inflow of refugees into the West German labor market between 1988 and 1993 on native wages and employment. The analysis indicates that a one percentage point increase in local immigrant employment reduces average native wages and employment in the short run by about 0.68% and 1.13%, respectively; however, the effect tends to vanish in the longer term. In addition, cross-regional job-to-job moves compensate on average for two-thirds of the negative short run local employment effect. The second essay analyzes the causal effect of a relaxation of the German Protection Against Dismissal Act in 2004 on different labor market outcomes at the firm level. Specifically, the essay exploits a change of the minimum establishment size threshold determining coverage by the employment protection legislation from five to ten employees as a quasi-experiment. The results from the empirical analysis do not provide robust evidence for an effect on overall hiring, separation, job flow, and churning rates as well as wages and temporary employment relations. However, there is some evidence of increases in the gender-specific hiring and job flow rates of women. The third essay studies trends in STEM employment and wages in West Germany between 1980 and 2010. A descriptive analysis indicates an increase in STEM employment and wages in both absolute and relative terms for men and women that coincides with the rise in wage inequality during the same period. Moreover, the essay shows that the increase in the wage differential between STEM and non-STEM workers can be explained by supply and demand factors under a STEM-biased technological change within a CES production framework. Finally, the essay provides an alternative assessment of the STEM premium by exploiting estimates from a model with additive worker and firm fixed effects. Most importantly, estimates from a Gelbach decomposition suggest that the fraction of the STEM premium that is explained by firm effects has increased considerably over time.

Keywords:
Labor economics, employment, wages, immigration, refugees, dismissal protection, worker turnover, temporary employment, STEM, wage differentials.

Zusammenfassung

Diese Dissertation besteht aus drei Aufsätzen, die zur empirischen Literatur über Beschäftigung und Löhne auf dem deutschen Arbeitsmarkt beitragen. Der erste Aufsatz untersucht die Auswirkungen eines großen und unerwarteten Zustroms von Flüchtlingsmigranten auf den westdeutschen Arbeitsmarkt zwischen 1988 und 1993 auf die Löhne und Beschäftigung der einheimischen Arbeitnehmer. Die Analyse zeigt, dass ein Anstieg der lokalen Beschäftigung von Migranten um ein Prozentpunkt die durchschnittlichen Löhne und die durchschnittliche Beschäftigung kurzfristig um etwa 0.68% bzw. 1.13% reduziert, der Effekt langfristig jedoch verschwindet. Darüber hinaus kompensieren überregionale Job-zu-Job-Wechsel durchschnittlich zwei Drittel des negativen kurzfristigen lokalen Beschäftigungseffekts. Der zweite Aufsatz analysiert den kausalen Effekt einer Lockerung des deutschen Kündigungsschutzgesetzes im Jahr 2004 auf unterschiedliche Arbeitsmarktergebnisse auf Firmenebene. Dazu nutzt der Aufsatz eine Änderung des Schwellenwerts der Mindestbetriebsgröße zur Anwendbarkeit des Kündigungsschutzgesetzes von fünf auf zehn Beschäftigte als ein Quasi-Experiment. Die Ergebnisse der empirischen Analyse liefern keine robuste Evidenz für einen Effekt auf die Einstellungs-, Abgangs-, Nettobeschäftigungs- und Churning-Raten sowie auf Löhne und temporäre Beschäftigungsverhältnisse. Dagegen gibt es Evidenz, dass die geschlechtsspezifischen Einstellungs- und Nettobeschäftigungsraten von Frauen zugenommen haben. Der dritte Aufsatz untersucht die Entwicklung der MINT-Beschäftigung und -Löhne in Westdeutschland zwischen 1980 und 2010. Eine deskriptive Analyse deutet auf einen Anstieg der MINT-Beschäftigung und -Löhne in absoluten und relativen Werten für Männer und Frauen hin, der zeitlich mit dem Anstieg der Lohnungleichheit zusammenfällt. Darüber hinaus zeigt der Aufsatz, dass die Zunahme des Lohnunterschieds zwischen MINT und nicht-MINT Arbeitern durch Angebots- und Nachfragefaktoren im Rahmen eines MINT-verzerrten technologischen Wandels auf Basis einer CES-Produktionsfunktion erklärbar ist. Zuletzt bietet der Aufsatz eine alternative Analyse der MINT-Prämie unter Nutzung von Schätzwerten aus einem Modell mit additiven Arbeiter- und Firmeneffekten. Insbesondere deuten die Ergebnisse einer Gelbach-Zerlegung darauf hin, dass der durch Firmeneffekte erklärte Anteil der MINT-Prämie mit der Zeit bedeutend zugenommen hat.

Schlagwörter:
Arbeitsmarktökonomik, Beschäftigung, Löhne, Einwanderung, Geflüchtete, Kündigungsschutz, Arbeitnehmerfluktuation, temporäre Beschäftigung, MINT, Lohnunterschiede.

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1. Introduction

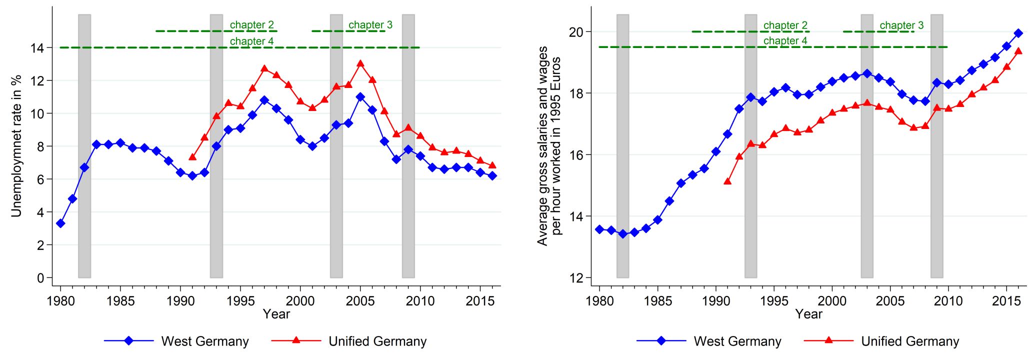
Due to the intricacy of the topic and its significance for economic welfare, the assessment of employment and wages remains central to the academic, political, and public debate. Germany offers an important test case for studying determinants of employment and wages, not only because it has the largest labor force in the European Union, but also since the German labor market was marked by far-reaching economic changes during the last decades. In this dissertation, I study the core topics of employment and wages in Germany in three different periods of time. In chapter 2, I assess the impact of an immigrant-induced supply shock on native wages and employment at the end of the 1980s and beginning of the 1990s. In chapter 3, I analyze the causal effect of a relaxed employment protection legislation that was implemented in 2004 as part of comprehensive social welfare and labor market reforms. Finally, in chapter 4, I document the role of Science, Technology, Engineering, and Math (STEM) occupations in the West German labor market over three decades until 2010.

To provide some temporal perspective, Figure 1.1 displays the evolution of the unemployment rate (panel A) and deflated average gross wage per hour worked (panel B) in West Germany between 1980 and 2016 and unified Germany between 1991 and 2016. West Germany experienced a sharp increase in the unemployment rate in the aftermath of the two oil crises in 1973 and 1980/81, which peaked at 8.2% in 1985. In contrast, the second half of the 1980s and beginning of the 1990s was characterized by a steady expansion of employment, leading to an unemployment rate of 6.4% in 1992, coupled with a continuous growth in average hourly wages. This time was also marked by large inflows of refugees, ethnic Germans (then called Aussiedler), East Germans and other migrants into West Germany (Bundesamt für Migration und Flüchtlinge 2004, 2006). In the years that followed, Germany was first hit by a short-term recession in 1993 and subsequently experienced a large rise in its unemployment rate that climaxed at 12.7% in 1998, while wages remained almost flat. Unsurprisingly, the period around the 1990s was (and still is) at the center-stage of many empirical studies (see, e.g., D'Amuri et al. 2010; Glitz 2012; Prantl and Spitz-Oener 2014; Dustmann et al. 2017).

After a temporary decline of unemployment rates between 1998 and 2001, Germany entered another recession in 2003 after the burst of the internet bubble and the 2001 terrorist attacks (Räth 2009). Eventually, with an unemployment rate of 13% in 2003, its highest level in the German post-war era, policymakers gradually implemented far-reaching social welfare and labor market reforms. However, while unemployment rates indeed diminished following the so-called *Agenda 2010* reforms, researchers have contrasting views on the contribution of the legislative changes to fostering employment (see discussions in, e.g.,

Figure 1.1.: Evolution of Unemployment and Wages in Germany

A. Unemployment rates B. Average gross wages per hour worked



Notes: Unemployment rates refer to the dependent civil labor force. Average gross wages per hour worked are deflated by the German Consumer Price Index (CPI), with 1995 as the base year. Wages of marginal employees are excluded. Data on West German unemployment rates refer to West Germany without Berlin. Data on West German wages refer to West Germany with West-Berlin until 1991 and to West Germany without Berlin after 1991. West German wages between 1992 and 1999 are a linear interpolation of the West German-German wage ratio in the years 1991 and 1999 times the German wage in the years 1992 to 1999. Vertical bars indicate recession years, defined by a reduction in year-on-year GDP growth. Horizontal lines indicate the periods under study in chapters 2, 3 and 4. Data source: German Federal Statistical Office.

Fitzenberger 2008; Dustmann et al. 2014; Burda and Seele 2017).[1] Lastly, even though Germany was not spared by the financial crisis in 2007/08, it only led to a mild deterioration in labor market outcomes, and since 2010 both employment and wages have been growing sustainably. Notably, by 2016, Germany — in the meanwhile by some described as an "economic superstar" — reached the lowest unemployment rate since reunification and the highest average wages ever recorded (Möller 2010; Dustmann et al. 2014).

However, this success story was accompanied by other divisive developments. For one thing, the last decades have been marked by a sharp increase in wage inequality, in particular since the mid-1990s, a phenomenon that has attracted continuing attention from researchers (see, among many others, e.g., Dustmann et al. 2009; Card et al. 2013; Dustmann et al. 2014; Glitz and Wissmann 2017). In addition, numerous studies point out the relative increases in atypical forms of employment (i.e. part-time employment, fixed-term employment, marginal employment, temporary agency work, freelance work) that are often associated with lower wages and higher job insecurity (e.g., Brehmer and Seifert 2008; Keller and Seifert 2013; Eichhorst et al. 2015).

Against the backdrop of these macro trends, this dissertation analyzes three different time periods to provide new insights on factors that affected employment and wages in the German labor market, which may hold lessons in the broader international context as well.

In Chapter 2 of this dissertation, my co-author and I revisit the question of how immigration affects native workers by examining a large and unexpected inflow of refugee migrants into the West German labor market between 1988 and 1993. We use detailed administrative data

[1] For an overview of the literature on the Agenda 2010, see http://www.iab.de/infoplattform/agenda_2010.

to study the short and long run effects of immigration on native wages and employment at the level of local labor markets. Using distance to the southern and eastern German border as instrumental variables, our results indicate that a one percentage point increase in local immigrant employment reduces average native wages and employment by about 0.68% and 1.13% in the first five years after arrival, but tends to have no or even positive effects in the longer perspective. Moreover, we find extensive effect heterogeneity across different groups of the labor market. While short run employment losses are more pronounced for low-skilled and middle-aged workers, short run wage reductions are stronger for skilled and middle-aged workers. Looking across occupations and industries, the estimates further suggest that wage effects are concentrated among workers in simple occupations and in the nontradable sector, whereas employment effects are more important in the tradable sector. In addition, we exploit the panel structure of the data and track transitions between employment and nonemployment as well as cross-regional job-to-job moves. Decomposing the former into inflows and outflows from unemployment, we find that around two-thirds of the net employment decline are attributable to a decrease in inflows, whereas only one-third is attributable to an increase in outflows. In other words, incumbent workers are to some extent protected from the adverse effect of immigration at the expense of unemployed natives who bear most of the short run burden. Finally, turning to cross-regional mobility, we provide evidence that the economy-wide employment decline is substantially smaller than suggested by the local estimates: Notably, even in the short run, on average two-thirds of the local employment effect is compensated for by cross-regional job-to-job moves.

In Chapter 3, I analyze the impact of a change in dismissal protection on different labor market outcomes in small establishments. The identification strategy relies on a quasi-experimental change in the German Protection Against Dismissal Act (PADA) in 2004 that was implemented as part of the Agenda 2010 reform package. Notably, due to a raise of the minimum firm size threshold determining coverage by the PADA, dismissal protection was only relaxed for some establishments. Accordingly, I exploit the temporal and cross-sectional variation in the PADA and apply a difference-in-difference (DiD) approach by comparing the average outcomes of establishments subject to the policy change (treatment group) with establishments that are similar in all dimensions but the exposure to the legislative change (control group) before and after the reform. Specifically, I use matched employer-employee administrative data linked to establishment survey data and estimate the impact of the reform on worker turnover as well as wages and the use of temporary employment. Using different assignment methods to the treatment and control group, I find no robust evidence for an effect on overall hiring, separation, job flow, and churning rates. Moreover, I assess potential heterogeneity in the treatment effects by estimating effects on gender- and age-specific turnover rates as well as differential effects by union status and East-West divide of firms. I only find some evidence of increases in the gender-specific hiring and job flow rates of women which could potentially be explained by more elastic labor supply elasticities. Finally, since treated establishment might have adjusted to the relaxed dismissal protection along other margins, I study the impact of the reform on average wages as well as the establishment-level shares of workers on fixed-term contracts and workers from temporary agencies. Again, I do not find evidence for an adjustment along these alternative margins.

In Chapter 4, I study the role of STEM occupations in the West German labor market and explore potential drivers underlying an increase in wage differentials between STEM and non-STEM workers, which I call the STEM premium. Using detailed administrative data, I first document an increase in STEM employment and wages in both absolute and relative terms for men and women. Moreover, I show that the time pattern between the STEM premium (adjusted for skill-age profiles) coincide with the rapid expansion of wage inequality which suggests that STEM jobs contributed to the accelerated increase in the West German wage inequality since the mid-1990s. Next, I use a CES production function framework in a competitive market environment which allows for imperfect substitutability between STEM and non-STEM workers and show that the rise in the STEM premium can be explained by supply and demand factors under STEM-biased technological change. This finding offers a refined perspective on the often discussed shortage of skilled workers, in particular in the field of STEM occupations. Notably, within the boundaries of the CES model, the results confirm a relative shortage of STEM workers, in particular since the mid-1990s. Finally, I use estimates from a model with additive worker and firm fixed effects from Card et al. (2013) and demonstrate that both male and female STEM workers are clustered at the upper part of the distributions of worker and firm effects. In addition, I apply a Gelbach decomposition to provide an approximate quantification of the contribution of unobserved worker and firm effects as well as observable time-varying worker characteristics to the STEM premium. My results suggest that firm-specific wage components explain an increasing fraction of the growing STEM wage gap over time. This finding is in line with previous studies in that it highlights the rising importance of firm-specific rents in explaining the growing wage inequality (Card et al. 2013; Goldschmidt and Schmieder 2017).

The following three chapters are self-contained and can be read independently. Chapter 2 is joint work with Benjamin Bruns.

2. The Impact of Immigrants on Native Wages and Employment: An Analysis of Refugee Inflows in the Early 1990s

2.1. Introduction

The considerable rise in immigration following the Balkan conflicts and the fall of the Iron curtain in the late 1980s triggered an unprecedented inflow of refugee migrants to West Germany, and led to significant and lasting changes in the composition of its population: between 1985 and 1995, the stock of immigrants rose by 2.8 million individuals, equivalent to a 64% rise in the initial foreign population and implying a total population growth of 5%.[1] About two decades later, an even larger influx of refugees — sourced in the "Arab Spring" (Dustmann et al. 2016) — has put the topic of immigration back center-stage in the political discourse in Germany and all other refugee-receiving countries across Europe, with right-wing anti-immigration parties successively gaining important long term political mandates.[2] Despite the significance of the topic and the availability of a historical blueprint, there is only limited empirical evidence on one core question nourishing public uncertainty and the political debate: What are the labor market effects of such a refugee-driven supply shock on the resident native population? Building on detailed administrative data for the West German labor market, we provide the first comprehensive answer to this question by analyzing the short and long run effects of an unexpected refugee shock hitting the German economy in the early 1990s.

A large body of literature has investigated the effects of immigration, but researchers are still far from reaching consensus: for example, a synthesis of wage effects for the US (see Dustmann et al. 2016) shows a range of values reaching from strongly negative (Altonji and Card 1991; Borjas 2003; Aydemir and Borjas 2007; Borjas 2014, 2017) to zero and even positive (Card 2007; Card and Lewis 2007; Card 2009; Boustan et al. 2010; Peri and Yasenov 2017).[3] The considerable variation in empirical results not only permeates US

[1] We define refugees as all displaced individuals in a third country who reside in a refugee camp, who have been formally given refugee status, or who have been granted temporary forms of protection (Dustmann et al. 2016). For simplicity, we broadly consider as refugees all immigrants coming from a refugee sending country, though we acknowledge that this classification might be imprecise in some cases.

[2] Very recent examples include the rise of the AfD in Germany, FPÖ in Austria, Front National in France, and the PVV in Netherlands.

[3] See, e.g., Card and Peri (2016) for a summary of the ongoing dispute between George Borjas on the one hand, and David Card and Giovanni Peri on the other hand. Particular attention has been given to the mariel

studies, but also prevails in research focusing on European countries, and, most importantly for us, the German labor market. On the one hand, Bonin (2005), Haas et al. (2013), and Steinhardt (2011) report only modest (if any) wage effects, and Pischke and Velling (1997) find no detrimental effects on employment, though their analysis focuses on the 1985-1989 period, and thus misses the massive rise of inflows between 1990 and 1993.[4] On the other hand, Dustmann and Glitz (2015) [DG], considering a similar inflow as we do in this paper, and Prantl and Spitz-Oener (2014) who look at East German immigrants, discover significant wage losses in the nontradable and the competitive sector of the labor market. Meanwhile, Velling (1995), looking at the 1989-1993 immigration shock, and also Glitz (2012), considering the 1996-2001 inflow of ethnic Germans, document a negative effect on employment rates. One study that simultaneously reports negative wage and employment effects, and is at the same time the most relevant for us, is a recent paper by Dustmann, Schönberg, and Stuhler (2017) [DSS]. Their preferred estimates imply moderate wage and large employment losses of about 0.13 and 0.93%, suggesting nearly perfect displacement of natives at the local level. While offering compelling identification, however, their analysis is based on a relatively small border region in south-east Germany.

While all these papers make important contributions, the large heterogeneity in results, even when based on similar data and time periods, makes it difficult to discern a consensus or compare results across studies. The problem starts with the definition of immigrants and natives. Roughly between 1985 and 1995, three groups of immigrants entered the German labor market: foreign migrants (that we consider below), ethnic Germans, and East Germans.[5] Since all three groups differed, among other things, in their migration incentives, skills, and regional allocation, the empirical implications from studies considering these different groups are obviously not comparable. On top of that, some studies run into data issues identifying East and ethnic Germans as migrants (both held or received German citizenship upon arrival), raising concerns that any effect of immigration is driven by the allocation of these "German" inflows rather than changes in the resident native workforce. And even if studies use the same definition of immigrants and natives, they still rely on alternate measures of migration flows (skill-specific or overall), exploit different sources of variation (skill-cells, regions, or both), and include varying sets of control variables, implying that they identify conceptually different parameters that answer different questions

boatlift, first investigated in Card (1990). The original study finds no effects of marielitos on natives as does a recent analysis of Peri and Yasenov (2017) based on an improved identification strategy. This finding has been substantiated by Lewis (2004) and Bodvarsson et al. (2008) showing potential channels through which the immigrant shock might have been absorbed without affecting wages. In contrast, Borjas (2017) argues that marielitos did have a negative effect on wages, but only for high school dropouts.

[4] In addition, they use aggregate data for rather large regional units which, at a time when long distance commuting was more costly, might lead them to estimate a smaller employment response because cross-border job-to-job moves are not measured.

[5] Glitz (2012) analyses the inflow of ethnic Germans after 1995, exploiting a dispersal policy that allocated immigrants across districts. Prantl and Spitz-Oener (2014) investigate the inflow of East Germans using information on an individual's training occupation and residence in early childhood to identify a supply push to West Germany. D'Amuri et al. (2010) use the total inflow comprising of foreign immigrants, ethnic and East Germans.

(Dustmann et al. 2016).[6]

In this paper, we provide the first comprehensive assessment of the refugee-driven immigration shock between 1988 and 1993 on the resident *native* workforce in the West German labor market. In contrast to some earlier studies (e.g. Bonin 2005; Glitz 2012; Dustmann and Glitz 2015), we exclude East and ethnic Germans from our analysis, thus getting as close as possible to the impact of immigration on the resident native labor force. Our empirical design exploits spatial variation in immigrant inflows across commuting zones, enabling us to estimate the overall effect of immigration on various subgroups of the labor market, including skill and age groups, industries, and occupations. In doing so, we recast several wage and employment estimates from the literature in a general and consistent empirical framework. Unlike most existing empirical papers (also those based on longitudinal micro-data), we additionally trace out the dynamic effects of immigration, evaluate short run (5 years) and long run (10 years) effects, control for worker selection into employment, and evaluate the underlying adjustment mechanisms in our employment analysis. As indicated above, the inflow that we consider was of exceptional magnitude and dominated by refugees. However, while all refugee shocks share particular similarities, they also exhibit important differences that prevent direct conclusions about other refugee shocks such as today's. Most importantly, the refugee wave between 1988 and 1993 was presumably more substitutable to resident German workers as it encompassed many East European migrants who were often well educated and had basic knowledge of the German language. Despite this, we believe that a thorough understanding of the labor market effects of this particular immigration shock, and especially its dynamics, may help policymakers today to design more effective regulations in order to harvest the benefits of immigration.

Any study of the impact of immigration must deal with potential endogeneity of immigrant inflows. Since the immigration shock that we consider was largely composed of refugees — Yugoslavs fleeing the Balkan conflicts, Turks escaping violence against the Kurdish population, migrants leaving transition economies in the former Eastern Bloc states, and Kazakhs, Afghans, Iranians, and Lebanese driven out of their home country by ongoing conflicts — it is reasonable to assume that the timing and skill composition of the overall shock was largely exogenous.[7] Not so, however, the choice of the particular destination country, and, even more precarious for our case, the selection into a particular region within the destination country. For example, if these refugees, conditional on entering Germany, selected themselves into thriving regions with favorable employment prospects, simple correlations between local immigrant inflows and native wages or employment would be biased upward. To address this concern, many papers have exploited historical immigrant allocations to predict local immigration shocks that are uncorrelated with current demand

[6]For example, many of the previous studies consider skill-specific migration flows (rather than overall flows), and therefore identify the distributional effects of immigration on native outcomes. In contrast, the political and public debate is often more broadly concerned with the overall migration flows. In this paper, we focus on the latter.

[7]As noted in Borjas and Monras (2016), refugee-driven supply shocks are plausibly exogenous along various relevant dimensions such as skills, magnitude, and the economic condition in the destination country.

factors (Card 2001; Peri and Sparber 2009; Glitz 2012; Dustmann and Glitz 2015). However, in our setting, we find such an instrument to lack reasonable power of predicting the pattern of current inflows, and therefore devise an instrument exploiting a region's distance to the south and east German border. This instrument rests on two particular features of the German migration history: first, the guest worker period between 1955 and 1968 which generated a substantial south-north gradient in immigrant employment shares and led immigrants two decades later to settle where earlier immigrants already resided;[8] second, the total blockade of East-West migration since 1961, which induced exceptionally low shares of foreign workers from the Eastern Bloc states, and led later immigrants to trickle into the country from east to west, often staying close to their home country for commuting reasons. Taken together, by exploiting a refugee-driven immigrant shock, by estimating the overall impact on various important subgroups of the labor market, and by analyzing the dynamic effects over time, we are able to provide a comprehensive and consistent assessment of the impact of immigration on the resident native workforce for a leading European economy.

We begin our empirical analysis by estimating simple models relating the short run change in local native wages and employment to the associated overall inflow of immigrant employment between 1988 and 1993, instrumented using distance to border. Overall, our main result indicates that an inflow of foreign migrants has a negative short run effect on average native wages and employment: within five years, a one percentage point rise in local immigrant employment — around half of the average foreign employment inflow over that time period — reduces local wages and employment by about 0.68% and 1.13%, respectively. Taken at face value, the employment effect suggests an almost perfect short run displacement of native workers at the local level, where for every additional immigrant finding a job in a region, one native leaves or no longer enters employment in that region. These baseline results are robust to a variety of specification checks concerning the unit of observation, the choice of regions, the inclusion of further covariates, and alternative measures of the immigration shock and native outcomes.

It is important to bear in mind that our empirical estimates refer to a relatively short time frame (1988-1993), whereas many analyses of immigration consider decadal changes. In the short run, we expect negative responses to be more severe than in the longer term when firms might adjust their capital or production technology (Lewis 2011; DG), workers might continue to settle in other regions or specialize in higher skilled occupations (Peri and Sparber 2009), and future entrants might invest into more education (Hunt 2017). In line with this reasoning, we find positive wage and employment effects in the post-shock period, 1993-1998, that are sufficiently large to compensate for the entire wage and employment reduction in the 1988-1993 period.[9] In addition, we find that local employment reductions

[8]We acknowledge that the initial allocation of immigrants that we exploit is driven by demand considerations, since guest workers moved to (or were assigned to) areas requiring additional labor at the time. To the extent that this implies a positive correlation with wage and employment developments during our analysis period, this would generate an upward bias in our estimates and lead us to understate the negative effect. Our results do not point to such effects, and we provide evidence suggesting that our instrument is indeed exogenous.

[9]This dynamic adjustment process is consistent with a recent study by Ruist et al. (2017), who document

might be associated with native job-to-job mobility between regions, and we show that on average roughly two-thirds of the local employment response can be traced back to such cross-regional moves. Importantly, this implies that the economy-wide employment decline is substantially smaller than suggested by our local estimates, even in the short run.[10]

To provide a coherent picture of the native response to immigration and to better understand the sources of these effects, we then assess the impact of immigration on natives with different levels of education and age, and in different types of occupations and industries. We find that employment losses are more important among unskilled than skilled workers, and stronger for workers above 30 than for labor market entrants, while wage reductions are, somewhat surprisingly, more pronounced among skilled and middle-aged workers. Looking across occupations and industries, we find that wage effects are concentrated among workers in simple occupations and in nontradable sectors, whereas employment effects are concentrated in tradable sectors, consistent with earlier evidence reported in DG.

Finally, we decompose the employment effect into inflows and outflows, distinguishing between cross-regional employment moves (job-to-job) on the one hand, and transitions between employment and nonemployment on the other hand. Overall, we find that both inflows and outflows contribute to the net employment decline, with around two-thirds attributable to declining inflows, and one-third to rising outflows. If we further differentiate between job-to-job moves across regional borders and employment-nonemployment transitions, our key finding is that on average about two-thirds of the reduction in inflows sources in job-to-job transitions from other regions. This suggests that, as in DSS, firms react to an immigrant-induced labor supply shock by adjusting their hiring behavior, insulating incumbent workers from negative effects of immigration. While geographic flows are an important determinant of inflows, we find that outflows are primarily affected by nonemployment flows. In particular, we see a significant rise in these nonemployment flows among workers above 50, consistent with strong incentives prevailing for this group of workers to retire early.

In the next two sections, we provide some background on the German history of immigration and introduce the data. We then describe our empirical design in section 2.4, and discuss our identification strategy in section 2.5. Section 3.5 presents our results, and section 3.6 concludes our analysis.

that an overlap of (negative) short run and (positive) long run responses to immigration shocks might explain why immigration studies reach different conclusions regarding the wage impact of immigrants. Their argument focuses on the shift share type of instrument, which is also central to our identification. Note, however, that in our setting, an overlapping response bias is unlikely to be an important concern as the two decades preceding our analysis period have seen comparably small fluctuations in immigrant inflows.

[10]To the extent that immigrants induce natives to switch jobs, the newly created job matches could potentially benefit natives. For example, underneath Peri and Sparber (2009)'s idea of native specialization in communication-intensive or complex tasks is the notion of natives moving from one job to another.

2.2. Background and Macro Trends

Immigration to Germany: To provide context, Figure 2.1 plots net foreign migration flows between 1972 and 2002, broken out by three major regions of origin.[11] The first group includes immigrants from guest worker countries including the former Yugoslavia, Turkey, Greece, Italy, and Portugal; the second group consists of all major Eastern Bloc states such as Central and Eastern Europe, the former Soviet Union, Poland, and Romania; and the last group summarizes all remaining inflows, primarily composed of Asian source countries, including Kazakhstan, Afghanistan, Iran, and Lebanon. While the figure reveals sizable fluctuations in net migration throughout all years — caused by, e.g., the discontinuation of guest worker contracts in 1973, family reunification in the years to follow, and the economic recession of 1982 — the most striking observation emerges from the relatively short period between 1985 and 1995: within a decade, the stock of immigrants rose dramatically by 64% from 4.4 million to 7.2 million people, implying a net population growth of 5% relative to 1985.

The unprecedented surge of immigration between 1988 and 1993, in particular, was almost entirely composed of refugees from various flashpoints in Europe and Asia, among them, Yugoslavs fleeing the Balkan conflicts (27.2% of the total inflow), as well as Turks (21.9%), Kazakhs, Afghans, Iranians, and Lebanese, but also migrants leaving transition economies in the former Eastern Bloc states (Bauer et al. 2005).[12] This immigration shock (net inflows peaked at 600,000 immigrants in 1992) hit the German labor market and administration rather unexpected, and resulted in a gradual deterioration of the public opinion towards immigration which culminated in severe xenophobic riots and a profound reform of the asylum law. Indeed, the implementation of this law on July 1, 1993, explains the substantial reduction of inflows from this year onwards (Bundesamt für Migration und Flüchtlinge 2004). In sum, the sudden rise and the abrupt reduction between 1988 and 1993 may be seen as a large-scale natural experiment (D'Amuri et al. 2010; Borjas and Monras 2016) that allows studying the labor market effects on natives in an economic and public environment that was initially unprepared to deal with such large numbers of immigrant arrivals.

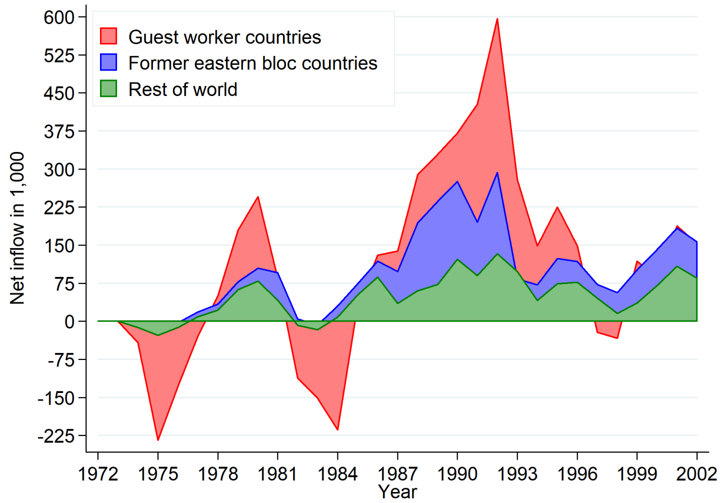
Immigrants and the Native Labor Market: An immediate concern arising from the previous discussion is whether these immigrants actually gained access to the labor market after arrival. Their refugee status, the lack of official certificates, delayed recognition of foreign degrees, or uncertainty about the permanence of stay generated severe impediments for refugee migrants to enter the job market. Also, legal working requirements were particularly restrictive until 1991, confining refugees' right to work throughout the asylum process (*Aufenthaltsgestattung*) and toleration status (*Duldung*).[13] As Figure 2.2 illustrates based on our

[11] Data are available at the German Federal Statistical Office.

[12] For this reason, we will use the terms immigrant and refugee migrants in our analysis interchangeably. However, we note that this is a slight abuse of language as the total immigration flow also comprises of a rather small fraction of migrants from non-refugee sending countries.

[13] For background information on the regulatory framework, see Appendix A.2.

Figure 2.1.: Total Immigrant Inflows by Region of Origin Between 1973 and 2002



Notes: Figure shows total net inflows (in thousands) from foreign countries to Germany between 1973 and 2002. We classify all nationalities into three major regions of origin (see main text). The figure illustrates the sharp rise of immigration between 1988 and 1993. Data source: German Federal Statistical Office.

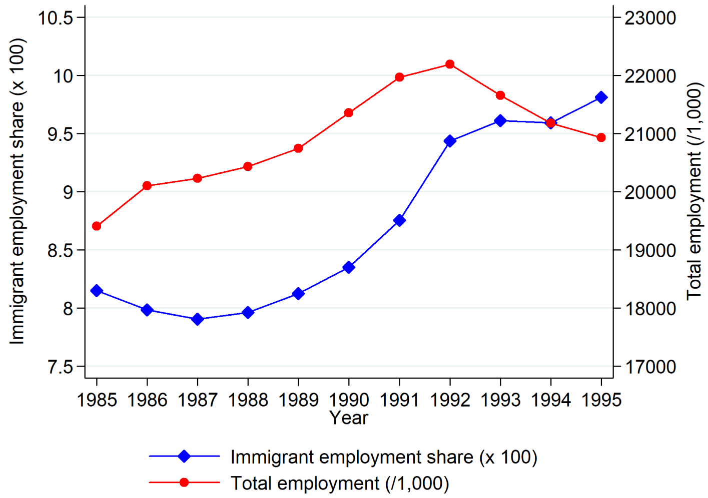
administrative data, the surge in overall population inflows indeed coincided with a parallel rise in immigrant employment rates by about 2 percentage points, equivalent to roughly 450 thousand additionally employed immigrants.[14] While less than the overall population growth (5%), this shock still constitutes a substantial immigrant-induced expansion of local labor supply and suggests that we capture the bulk of inflows most likely to affect resident native labor. However, it is conceivable that some immigrants found a job outside the social security system, e.g., through the system of bilateral labor treaties (*Werkvertragsarbeitnehmerabkommen*) first established in 1988 with Czechoslovakia, Hungary, Poland, Yugoslavia, and Austria in an attempt to recruit workers for the building industry. According to official figures, these agreements sparked a rise in the number of temporary labor migrants from about 15,000 workers in 1988 to 95,000 in 1992 (Bundesamt für Migration und Flüchtlinge 2006; Menz 2009). We would suspect that these labor migrants provided less competition for natives than immigrants employed subject to social security, however, to test whether this "undercounting" of the employment shock still affects our results, we show in section 2.6.4 that our results are robust to excluding the building sector from our analysis.

2.3. Data Set and Descriptive Overview

German Social Security Records: Our analysis is based on a representative 2% subsample of administrative records of all dependent employees subject to social security (SIAB 7510), provided by the IAB (*Institut für Arbeitsmarkt und Berufsforschung*). The data cover the

[14]For example, between 1988 and 1993, total West German employment (subject to social security) rose from about 20.4 million to 21.7 million workers, suggesting that total immigrant employment increased from about 1.63 (20.4 × 8.0%) to 2.08 (21.7 × 9.6%) million workers.

Figure 2.2.: Immigrant Employment Shares and Total Employment Counts Between 1985 and 1995



Notes: Figure shows the immigrant employment shares calculated from our analysis sample (see main text) and the evolution of total dependent employment of natives for the West German labor market between 1985 and 1995. Total native employment multiplied by 50 is reported net off East and ethnic German employment. Data source: SIAB 7510.

years 1975-2010 and integrate information on employment as well as periods of registered unemployment.[15] Two reasons make this data set particularly useful for our analysis. First, it allows us to construct accurate measures of employment not only for natives but also for immigrants. While the number of immigrants entering the social security system between 1988 and 1993 underestimates the total number of new arrivals (recall that many immigrants were not allowed to work), we suspect that our data capture well that part of the inflow which is most relevant for the development of native wages and employment.[16] Second, our data allow us to track individuals across space, employment states, and over time, enabling us to improve on many earlier studies by controlling for worker selection in wage regressions, and by casting light on the mechanisms underlying the employment effects.

Sample Restrictions and Variables: We restrict our analysis to all regularly employed and unemployed male and female workers aged 18-64 in 204 geographically disjoint commuting zones covering the entire West German labor market, excluding Berlin (Koller and Schwengler 2000).[17] From the resulting sample, which we refer to as "labor force", we draw two primary subsamples, one for our wage analysis and one for our employment analysis. For

[15]The data are representative for about 80% of the German workforce. Excluded are self-employed, civil servants, full-time students, and the military; see vom Berge et al. (2013b) for details.

[16]As commonly done in German data, we identify immigrants based on citizenship rather than country of birth (Bonin 2005; D'Amuri et al. 2010; Glitz and Wissmann 2017). We describe how we impute missing values in Appendix A.1. We also calculated the descriptive results below based on data from the German Socio-Economic Panel Study, which records the country of birth (rather than citizenship). We found similar trends, though as expected, the increase between 1988 and 1993 is somewhat more pronounced.

[17]We exclude workers in training and in marginal employment because wages of trainees are unlikely to reflect an individual's productivity and marginal employment is not consistently observed prior to 1999.

our wage analysis, we drop part-time workers since we only observe daily wages and the part-time status (no working hours), and include only workers observed in two consecutive periods in the same local labor market (see below).[18] In our employment analysis, we keep part-time employees, but weight them down by 1/2 or 2/3, depending on the particular part-time status (small vs. large). We distinguish between unskilled and skilled workers (based on the level of education) and between three age groups (18-29, 30-49, and 50-64). Individuals with at most a high school degree (*Abitur*) are considered unskilled, whereas individuals who completed an apprenticeship training or obtained a tertiary degree (e.g., Bachelor, Ph.D.) are considered skilled.[19] While our main reason for choosing a two-skill classification is to avoid sample size issues in small local labor markets, this grouping also facilitates comparability of our results with Anglo-Saxon countries, where many occupations that require apprenticeship training in Germany demand a college degree.

Identifying East and Ethnic German Inflows: An important issue for our analysis concerns the definition of immigrants and the resident native labor force. As indicated earlier, our analysis period has not only witnessed a dramatic rise in refugee migration, but also experienced substantial inflows of East and ethnic Germans — both recorded as Germans in our data set.[20] These coincident inflows of "Germans" might confound our analysis for two main reasons: first, due to their German citizenship, they potentially enter our left hand side variable, thus generating an upward or downward bias in our wage and employment analysis, depending on their wage development and whether their allocation is positively or negatively correlated with the inflow of refugee migrants;[21] and second, these migrants constitute a shock to the resident native labor force in and of themselves, so their exclusion might result in an omitted variable bias. To address these concerns, we draw on selection rules to identify East and ethnic Germans in our West German analysis sample. Specifically, following Glitz and Wissmann (2017) we define all individuals whose first employment spell indicates an East German location as East Germans, and exclude the complete employment biographies of these workers from our analysis.[22] Moreover, we identify ethnic Germans by exploiting administrative information on the receipt of registered integration programs such as language courses (Brücker and Jahn 2011).

Local Labor Market Trends: Table 2.1 summarizes our analysis sample for the years 1988 and 1993, calculated across employment weighted commuting zones. The next rows

[18] Wages are top-coded at the social security contribution ceiling. We impute censored wages following the approach in Glitz (2012); see Appendix A.1 for details.

[19] Due to data limitations in the education variable, we impose some minor corrections; see Appendix A.1.

[20] According to statistics from the Federal Office of Administration (*Bundesverwaltungsamt*) and the German Federal Statistical Office of Germany, about 1.65 million ethnic Germans and 1.45 million East Germans entered West Germany between 1988 and 1993.

[21] Note that, as explained below, our wage analysis is based on two-period regional stayers, suggesting that the influx of Germans with lower wage levels *per se* does not affect our estimates. However, if East and ethnic Germans featured smaller wage growth on average, our wage estimates would be downward biased.

[22] We note that since we only observe employment spells in East Germany after 1991, this method allows us to only partially identify East German migrants.

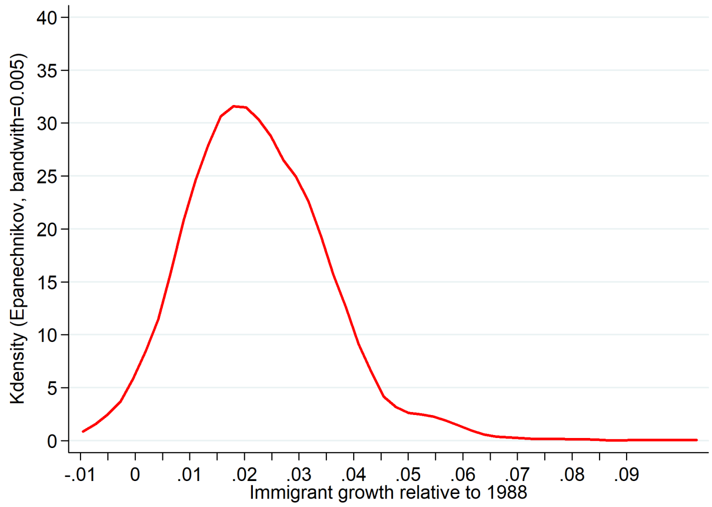
Table 2.1.: Summary Statistics of Local Labor Markets

	Year				Percent Change	
	1988		1993		1988-1993	
	Mean (1)	S.D. (2)	Mean (3)	S.D. (4)	Mean (5)	S.D. (6)
Immigrant Shares (×100)						
Labor force share	8.0	(3.9)	9.8	(4.2)	32.1	(49.6)
Employment share	8.0	(3.9)	9.6	(4.2)	31.1	(65.6)
Unskilled	23.1	(10.7)	30.5	(11.8)	50.4	(111.0)
Skilled	4.5	(2.4)	5.6	(2.7)	36.8	(58.0)
Age under 30	6.7	(3.3)	11.1	(4.9)	89.9	(106.8)
Age between 30 and 49	9.6	(4.5)	10.0	(4.3)	12.8	(69.8)
Age 50 and above	6.1	(3.6)	7.2	(3.6)	34.4	(65.7)
Age Distribution (×100); above 50 omitted						
Share of natives below 30	30.5	(3.7)	25.8	(3.0)	−15.3	(4.9)
Share of immigrants below 30	25.1	(6.7)	30.1	(5.7)	26.1	(42.2)
Share of natives b/w 30 and 49	47.6	(2.6)	50.8	(2.2)	6.8	(5.3)
Share of immigrants b/w 30 and 49	59.1	(6.6)	53.1	(5.2)	−9.5	(12.5)
Skill Distribution (×100); skilled omitted						
Unskilled share of natives	15.3	(15.3)	12.0	(2.4)	−21.5	(5.7)
Unskilled share of immigrants	53.8	(10.0)	50.9	(8.0)	−3.6	(19.6)
Occupational Distribution (×100)						
Share of natives in simple occupations	63.8	(4.8)	65.5	(4.5)	2.6	(2.4)
Share of immigrants in simple occupations	50.8	(10.4)	57.6	(9.8)	16.0	(26.0)
Employment/Labor force rate (×100)						
Average employment rate of natives	89.4	(2.7)	88.0	(2.0)	−1.5	(1.6)
Average employment rate of immigrants	88.6	(4.7)	85.7	(4.7)	−3.2	(6.1)
Wages (in 1995 Euros)						
Log average wage of natives (imputed)	4.33	(0.1)	4.41	(0.1)	8.1	(2.1)
Log average wage of immigrants (imputed)	4.21	(0.1)	4.22	(0.1)	0.4	(6.4)
Native-Immigrant wage gap	0.12	(0.1)	0.20	(0.1)	7.7	(6.2)
Local labor markets	204		204			

Notes: Table shows summary statistics of the baseline analysis sample, calculated across local labor markets (commuting zones), weighting each observation by total native employment. The sample is restricted to West Germany and excludes ethnic and East German migrants (see main text and Appendix for details). Wages are deflated to 1995. Data source: SIAB 7510.

show that local immigrant shares rose by more than 30%, with the largest increases among unskilled (50%) and workers under age 30 (90%), but also sizable gains among skilled and older workers. Comparisons of the age and skill structure between natives and immigrants show that the recent inflow of immigrants was disproportionately younger and lower educated than resident natives, offsetting the overall trend of aging and skill upgrading in the population. Similarly, we find that the share of immigrants working in simple occupations (defined below) rises considerably over time, though, surprisingly, starting from a lower level than natives to begin with. The bottom rows show the evolution of average native and immigrant wages (in logs) as well as the difference between the two. During our analysis period, native wages rose by about 8.1 log points, compared to only 0.4 log points for immigrants, implying that the wage gap increased by 7.7 log points (1.54 log points p.a.). As illustrated in Appendix

Figure 2.3.: Kernel Density Estimation of Changes in Immigrant Employment Shares Between 1988 and 1993



Notes: Figure shows kernel estimates of region level changes in immigrant employment shares between 1988 and 1993. Estimation is weighted by a region's total native employment in the base year, and uses an Epanechnikov kernel with bandwidth 0.005. Data source: SIAB 7510.

Table A.1, using a series of fixed effects models, these basic patterns also hold within detailed education-experience groups, and within the same regions, occupations, and industries.

The standard deviation of immigrant shares noted in Table 2.1 points to large variations across regional labor markets. To draw a more comprehensive picture, Figure 2.3 plots the density of region level changes in immigrant employment shares between 1988 and 1993, weighting each region by total native employment in the base year. Overall, the distribution is roughly centered around the mean, and reveals a somewhat longer right tail, with some regions experiencing an increase in immigrant employment of up to 10%.

In Table 2.2, we list the 1988 and 1993 immigrant shares for the 30 *largest* commuting zones, ranked by their total labor force (natives+immigrants) in 1988. The table illustrates substantial variation in immigrant shares and inflows over time. For instance, within the 5-year period, the share of immigrants rises by 2.9 percentage points in Heidelberg, Nürnberg, and Aachen, and by only 0.4 percentage points in Braunschweig. Another interesting feature coming out of this table is the broader geographic distribution of immigrant shares: of the 30 regions listed in the table, 14 exhibit two-digit immigrant shares by 1993, and these are all located in south (8) or middle (6) Germany. Computing the average growth in the immigrant labor force share (col. 4) for north, middle, and south regions listed in the table yields values of 1.2, 1.8, and 2.5 percentage points, respectively. The dominating role of, especially, southern Germany with respect to the rise in immigrant employment goes back to the early settlements of guest workers in the 1950s and 1960s, and it constitutes a core element of our identification strategy.

Table 2.2.: Distribution of Immigrant Labor Force Across Local Labor Markets

ID	Name of Region *usually largest single region*	Total LF in 1975 (1)	Immigrant Share 1988 (2)	Immigrant Share 1993 (3)	Difference 1988-1993 (4)
8	Hamburg	930,000	7.5	9.1	1.6
120	Stuttgart	929,450	16.4	18.5	2.1
159	München	914,150	13.9	16.8	2.8
92	Frankfurt/Main	852,150	14.0	16.4	2.4
45	Düsseldorf	606,650	10.6	12.3	1.7
57	Köln	587,900	11.6	13.5	1.9
17	Hannover	412,000	7.1	8.1	0.9
46	Duisburg	389,550	7.9	9.6	1.7
73	Dortmund	361,250	6.7	7.9	1.2
185	Nürnberg	359,650	9.1	12.0	2.9
63	Gelsenkirchen	335,400	7.5	9.0	1.4
47	Essen	278,750	6.5	8.0	1.5
42	Bremen	266,600	4.8	5.9	1.0
118	Saarbrücken	250,350	6.1	8.6	2.5
128	Karlsruhe	240,800	10.2	12.9	2.8
130	Mannheim	223,850	9.3	11.7	2.4
52	Wuppertal	220,950	10.9	12.8	1.9
67	Bielefeld	212,600	7.7	9.4	1.7
59	Bonn	208,450	8.3	9.7	1.5
6	Kiel	200,050	2.9	3.7	0.8
114	Ludwigshafen	188,300	7.9	9.2	1.3
64	Münster	187,500	4.1	5.8	1.6
200	Augsburg	185,100	9.5	11.7	2.1
129	Heidelberg	183,450	9.5	12.4	2.9
56	Aachen	163,650	9.0	11.9	2.9
9	Braunschweig	162,550	4.1	4.4	0.4
75	Lüdenscheid	153,300	11.1	12.4	1.3
72	Bochum	147,600	5.8	7.5	1.7
135	Freiburg	146,750	7.6	10.0	2.4
81	Kassel	146,600	6.2	7.1	0.9

Notes: Table shows the 30 largest local labor market regions in 1988. The labor force is calculated as the sum of employed and unemployed individuals and multiplied by 50. Columns 2 and 3 show the share of immigrants in the total labor force calculated based on the labor force data for the year indicated in the column heading. Entries in column 4 show the percentage point change between 1988 and 1993. Data source: SIAB 7510.

2.4. Empirical Framework

2.4.1. Set-up

In our main analysis, we estimate models of the change in wages and employment of natives in group i and region r on the total region-specific immigrant inflow between 1988 and 1993.[23] Formally:

$$\Delta \log w_{irt} = \alpha_i + \theta_{ir} T_r^{85-88} + \beta_{it} \Delta I_r^{88-93} + e_{ir} \tag{2.1}$$

[23] See DSS for a theoretical underpinning.

and

$$\Delta N_{irt} = \delta_i + \psi_{ir} T_r^{85-88} + \gamma_{it} \Delta I_r^{88-93} + u_{ir} \tag{2.2}$$

where

$$\Delta I_r^{88-93} = \begin{cases} 0, & \text{if } t \in \{86, 87, 88\} \\ \frac{I_{r,93} - I_{r,88}}{N_{r,88} + I_{r,88}}, & \text{if } t \in \{89, \ldots, 93\} \end{cases}$$

and

$$\Delta N_{irt} = \frac{N_{ir,t} - N_{ir,t-1}}{N_{ir,t-1}} \text{ and } \Delta \log w_{irt} = \log w_{ir,t} - \log w_{ir,t-1} \text{ for t} \in \{86 \ldots 93\}$$

We implement these models in first differences (losing the first year) and separately for each group i. Hence, in our baseline specification we eliminate region- and group-specific fixed effects, and allow the response of natives to differ across groups. We additionally allow for subgroup-specific growth rates in wages and employment, α_i and δ_i, and take out differential wage and employment trends for subgroup i in region r by including linear region-specific time trends for the pre-shock period, $\theta_{ir} T_r$ and $\psi_{ir} T_r$, where $T_r = 1$ for years 1986-1988.[24] To identify the region-specific trend separately from the immigration shock, we accordingly set the immigrant inflow in these years equal to zero — in line with our discussion in section 2.2.

We estimate equations (2.1) and (2.2) using a two-step instrumental variable procedure: in a first step, we regress the log wage change or an indicator for a particular employment transition in each year on a full set of age and education indicators interacted with a gender dummy; we then aggregate the residual of these regressions at the region-year level and regress region-level first differences on the immigrant inflow, instrumented using distance to border. Our models therefore identify the impact of immigrants conditional on the demographic structure in a regional labor market.

Our main interest concerns the parameters measuring the average subgroup-specific impact of an inflow of immigrants on native wages and employment in a region: β_i and γ_i. In our setting, these coefficients can be interpreted as the percent change in wages and employment of subgroup i in response to a one percentage point change in local immigrant employment between 1988 and 1993. They incorporate complementarities between skill and age groups as well as between capital and labor, and they accommodate the possibility of heterogeneous labor supply elasticities (or wage rigidities) for different demographic groups. Note that our immigration shock refers to a region as a whole — that is, we investigate how different native groups within a regional labor market are affected by an overall inflow of immigrants, irrespective of the specific composition of that inflow and the implied relative immigration shock for different native groups. We believe that this is a more policy relevant analysis,

[24]The coefficients θ_{ir} and ψ_{ir} can be interpreted as the average annual wage and employment growth for subgroup i in region r between 1986 and 1988. We arrive at equivalent parameter estimates when subtracting the corresponding average from our outcome variable and running regressions on the years 1989-1993. Standard errors in this case are somewhat smaller.

primarily because the demographic structure of immigrant inflows is often hard to measure (especially at the regional level), and devising appropriate regulations based on the particular composition of immigrants, while potentially superior in terms of effectiveness, seems difficult to achieve in practice.[25]

Following DSS, we measure the immigrant supply shock using the change in the number of *employed* immigrants as a fraction of total (native + immigrant) employment in the base period. This approach bears two issues relevant to the interpretation of our estimates. First, by relating the inflow to a fixed measure of employment in the base year, we avoid confounding changes in immigrant inflows with potentially correlated changes in native employment. As shown formally in Card and Peri (2016), the alternative specification based on changes in the share of employed immigrants might generate a downward bias in wage estimates, and is mechanically negatively correlated with changes in native employment as dependent variable.[26] Second, by focusing on employment inflows divided by total employment in the base period, a coefficient of -1 implies that for each additional immigrant employed, one worker who is already in the country leaves or no longer enters employment in a region. This worker is native (foreign) with probability equal to the share of natives (foreigners) in total employment — we summarize these probabilities in the top panel of Table 2.1. By focusing on the subgroup of immigrants who find a job, however, we might miss indirect effects on natives from immigrants who also arrived but did not find a job.[27] To see how the definition of the immigrant shock affects our results, we also used labor force and population measures to gauge the immigrant inflow. However, these alternative measures led to very similar conclusions, both qualitatively and quantitatively.

2.4.2. Implementation Issues

Controlling for Worker Selection: Since our data are longitudinal in nature, we can account for selection into employment in wage regressions even though our unit of analysis is the region. In particular, before performing the covariate-adjustment at the individual level (see above), we focus on only those workers who are employed full-time in the same region in two consecutive years.[28] In so doing, we eliminate the change in wages between two

[25] One reason is downgrading of immigrants, which refers to a systematic difference between the position of an immigrant in the labor market (e.g. measured by the position in the wage distribution) and the position of a native with the same observed education/experience level. Based on a simple imputation procedure described in Dustmann et al. (2016), we find substantial downgrading in our analysis period: the effective share of unskilled immigrants (who entered the German labor market within the last five years) is on average 25% higher than the share of observed unskilled immigrants.

[26] The key issue here is that changes in shares are downward biased if natives move into similar regions as do immigrants, e.g., due to positive demand shocks attracting both natives and immigrants, and implying higher wage and employment growth. Studies using this measure often draw a more negative picture of immigration shocks on native labor markets (Borjas 2003; Bonin 2005; Steinhardt 2011; Borjas 2014) than papers using a specification that avoids this bias (Card 2001, 2007, 2009; Peri and Sparber 2009).

[27] One possibility is that employers, faced with a larger labor supply, are able to use this as a threat to enforce lower wages for the extant workforce. One could also think of the reverse effect, where native employees increase their effort in the wake of immigrants waiting to 'take their jobs'.

[28] This procedure is similar to first-difference regressions estimated at the worker-level, with differences taken within worker-region spells.

periods that is generated by compositional adjustments in local employment (which might be caused by immigrants in the first place). If low wage workers are more strongly affected by immigrants than high wage workers — e.g., because immigrants are overrepresented in the low wage service sector —, and if their labor supply elasticity is relatively large — e.g., because they are more often employed under temporary contracts — not controlling for worker selection would lead to upward biased wage effects.

However, although restricting attention to stayers eliminates a composition bias in terms of wage *levels*, this approach may come at the cost of selection on wage *changes*. For example, if stayers are not only high wage but also high wage *growth* workers, our estimate of the impact of immigrants would be underestimated. We believe that this is less of an issue for two main reasons: first, comparisons of the variance of wage levels and wage changes show that the former is much larger than the latter. So, by eliminating the potential bias in wage levels, we likely account for the quantitatively more important bias. Second, we selected all (geographic) movers and stayers during the treatment years (1988-1993), and plotted their wage growth eight years earlier. Although the sample of movers is much smaller than the sample of stayers, we find remarkably similar wage growth distributions across all years.

Cumulative Coefficients: In our empirical implementation of equations (2.1) and (2.2), we decompose the overall impact of the 1988-1993 immigrant shock on natives, β_i and γ_i, into a series of annual effects, β_{it} and γ_{it}. We then calculate the cumulative effect over a particular (flexible) time period, say, 1988-1993, by adding up the parameters from the annual regressions, i.e., $\beta_i = \sum_{t=1989}^{1993} \beta_{it}$. The β_{it}'s and γ_{it}'s are interesting, as they help us to understand how local economies adjust to immigration shocks: technological adjustments (Lewis 2011; Peri 2012; DG), mobility of labor (Card and DiNardo 2000) and capital, occupational specialization (Peri and Sparber 2009), and also land and housing prices (Saiz 2003, 2007) are just a few of many potential margins of response. What they all have in common, though, is that their adjustment takes years to fully unfold, and our analysis of the dynamic adjustments over about a decade might provide evidence in favor of these mechanisms.

Two-Step Implementation of IV-Approach: Unlike conventional two-stage least squares (2SLS), we implement the first stage of our IV-models separately from the second stage as this enables us to adopt a different weighting scheme in each step of the estimation. In our first stage models, we want to weight by total native employment in 1988, first, because our immigrant shock refers to the region as a whole and is scaled to 1988, and second, because it accommodates the analysis of heterogeneous effects in response to the *same* shock. In our second stage models, however, the dependent variable refers to the subgroup-region-year, and hence, the appropriate weight should also refer to this cell. To account for the omitted first stage uncertainty in our "plug-in OLS" approach, we compute bootstrapped standard errors using a pairs bootstrap and 1,000 replications.[29]

[29] Another way of accounting for the first stage uncertainty would be to interpret the first stage as pre-estimation

2.5. Instrumental Variables

Immigrants are more likely to settle in regions that experience positive demand shocks over time, generating an upward bias in OLS estimates of both $\hat{\beta}_i$ and $\hat{\gamma}_i$. Since we estimate models of equation (2.1) and (2.2) in first differences, we eliminate time constant region- and subgroup-specific heterogeneity in wage and employment levels that might be correlated with immigrant inflows. While region-fixed effects take out many structural differences that are correlated with wage/employment levels (such as industry and occupation structure), subgroup-specific effects eliminate heterogeneity between age and skill groups. In addition, subgroup-region specific time trends control for differences in wage and employment *growth* prior to the immigrant shock. Still, there is space for omitted variable bias if shocks within regions and subgroups simultaneously affect immigrant inflows and native outcomes. To account for this possibility, we devise an instrumental variable strategy based on the (airline) distance between a local labor market and the southern (1) and eastern (2) German border.[30] This choice is guided by the specific composition of inflows indicated in section 2.2. We explain the details and summarize our results in the following.

Distance to Southern Border: The predictive power of distance to the southern German border dates back to the guest worker period during the 1950s/60s. Shortly after World War II, the German economy faced a shortage of labor caused by the preceding war period and draining inflows from East Germany. To fuel industrial production at home, politicians negotiated multiple recruitment agreements (*Anwerbeabkommen*) with several South European states, who themselves were facing high structural unemployment rates at the time. Hundreds of thousands of immigrants moved to Germany in the following years, crossing the southern border, and starting to settle in southern Germany (primarily Bavaria and Baden-Wuerttemberg) before starting to move north (North Rhine-Westphalia). These allocation choices, which were primarily demand-driven due to industrial melting pots in these areas, ultimately generated a south-north gradient in immigrant shares. While their settlement was meant to be transitory, many immigrants stayed after the last recruitment halt in 1973, beginning to reunify their families and thereby consolidating the temporary immigrant enclaves.[31] Since new immigrants tend to settle where earlier immigrants had moved to before, our distance-to-south instrument predicts the allocation of the large fraction of immigrants coming from former guest worker countries between 1988 and 1993.

Distance to Eastern Border: To target the settlement of another large group of immigrants between 1988 and 1993, those coming from former Eastern Bloc states (excluding ethnic

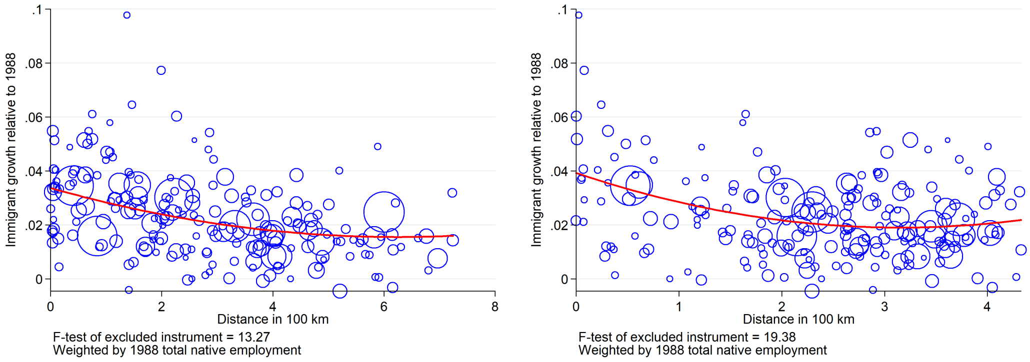
step in which we generate the instrument, and then implement a regular two-stage least squares regression using the generated immigrant inflow as instrument (Wooldridge 2010, ch. 6.1.2). We show in Table A.2 that both the "Generated IV" and the "plug-in OLS" approach yield similar results. In a slight abuse of terminology, we use the term 2SLS to refer to our "plug-in OLS" approach in what follows.

[30] Distance instruments have been widely used in the immigration literature, most often in the US context; see, e.g., Peri and Sparber (2009); Peri (2012); Smith (2012); Llull (2017).

[31] Already in 1965 the author Max Frisch epitomized the situation, saying: "We called for workers and people came".

Figure 2.4.: Spatial Distribution of Immigrant Inflows in West Germany Between 1988 and 1993

A. Distance to South German Border   B. Distance to East German Border



Notes: Figure plots immigrant employment growth rates for all 204 commuting zones between 1988 and 1993 against distance to border. Panel A shows distance to the southern border and panel B to the eastern border. The area of each circle is proportional to a region's employment share in 1988. Data source: SIAB 7510.

and East Germans), we use distance from eastern border as a second instrument. Prior to the fall of the inner German border, immigrants coming from the former Eastern Bloc accounted for only 0.3% of the total German population and about 3-4% of all immigrants. Following the collapse of the Soviet Union, however, immigrants from these regions came in large numbers (see section 2.2), and they began to trickle into the Federal Republic from east to west. Their settlement decision was not affected by prior compatriot settlements (as those were negligible), but instead reflected a trade-off between employment prospects on the one hand (inciting them to move to West Germany) and proximity to their home country on the other hand (inciting them not to move too far west). Travel costs were important for a large fraction of immigrants who worked during the week and returned to their families at weekends or migrated temporarily for several weeks or months (Moritz 2011). Based on these considerations, distance from today's eastern border is another useful predictor of immigrant settlements between 1988 and 1993.

First Stage Results: Figure 2.4 illustrates the association between changes in local immigrant employment shares in 1988-1993 and the airline distance (in 100 km) from the southern (panel A) and eastern (panel B) border. Each circle represents a local labor market, with its area being proportional to native employment in 1988. In line with our previous considerations, we find that regions closer to the border experienced higher immigrant inflows than regions further away. For example, the airline distance between Stuttgart and Hamburg, about 530 km, suggests that the change in immigrant employment between 1988 and 1993 was about 1.7 percentage points lower in Hamburg than in Stuttgart. A more detailed summary of our first stage relationships is provided in columns 1 to 4 of Table 2.3.[32] Our preferred specification, reported in column 1, includes distance to south and east separately. It explains

[32]Similar results emerge when we exclude the quadratic term or use a spline with 10 knots.

Table 2.3.: Summary of First Stage Regressions

	Foreign Immigrants					Ethnic Germans	East Germans	
	Distance to South and East Border	Distance to South	Distance to East	Average Distance	Shift Share IV Using '75 Density	Distance to South and East Border	Distance to East Border [SIAB]	Distance to East Border [MC]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance to South	−0.050	−0.058				0.032		
	(0.018)	(0.017)				(0.016)		
Distance to South sq.	0.004	0.005				−0.005		
	(0.003)	(0.003)				(0.002)		
Distance to East	−0.103		−0.128			0.074	0.011	−0.004
	(0.036)		(0.032)			(0.029)	(0.008)	(0.001)
Distance to East sq.	0.020		0.020			−0.014	−0.004	0.001
	(0.008)		(0.007)			(0.006)	(0.002)	(0.000)
Average Distance				−0.073				
				0.016				
Average Distance sq.				0.005				
				0.002				
Predicted %-growth 1988-1993					0.045			
					(0.025)			
R-squared (adjusted)	0.250	0.201	0.155	0.224	0.027	0.096	0.070	0.319
F-statistic (excl. Instr.)	16.43	13.27	19.38	30.47	3.34	2.90	12.26	13.82
Local labor markets	204	204	204	204	204	204	204	70

Notes: Table summarizes the first stage results. Columns 1 to 4 report the coefficients from models of the 1988-1993 change in immigrant employment on distance to border (divided by 1,000 km). Column 5 shows an alternative instrument using the 1975 distribution of immigrants by country of origin across local labor markets, interacted with the aggregate cumulative inflow from the same region of origin between 1988 and 1993. Column 6 uses the change in ethnic German employment instead of the change in foreign immigrant employment as dependent variable, and in columns 7 and 8, we use the change in East German employment. In column 7, we use data from the SIAB-sample, and in column 8, we use data from the German Microcensus as dependent variable. We use published data from Burchardi and Hassan (2013) to implement these regressions, and refer to their paper and Online Appendix for details. Data sources: SIAB 7510; German Microcensus 1991, 1993, 1995.

about 25% of the cross-regional variation in immigrant growth rates (adjusted R^2), and the coefficients are jointly significant (F-statistic=16.4). In the next two columns, we examine each distance instrument separately. As suggested by Figure 2.4, the negative relationship is rather strong in both cases, generating F-statistics of 13.3 and 19.4. In column 4, we combine the two distance measures into a single *average* distance from border. Compared to our preferred model, this yields a higher F-statistic (30.47), but a smaller adjusted R^2 due to the relative loss of information.[33]

Our distance-to-south instrument exploits a similar source of variation as the standard shift share instrument (though dating back to the 1950s/60s, and thus earlier than available data for regional immigrant densities), and it is natural to wonder how such a shift share instrument performs relative to our distance measure. We investigate this in column 5, finding only a weak association between predicted and actual immigrant employment growth

[33] We define the average distance as $\overline{d} = \left(d(south)^2 + d(east)^2\right)^{1/2}$, which yields the smallest values for regions in south-east Germany and is increasing in northern and western direction.

(F-statistic=3.43), which disqualifies this instrument for our analysis.[34]

Confounding Effects of East and Ethnic Germans: Between 1988 and 1993, two other large waves of immigrants entered the West German labor market: East Germans and ethnic Germans. Although we exclude these migrants from the outcome variables (focusing solely on resident native workers), we might still be concerned with a potential omitted variable bias (see section 2.3). On the one hand, if East Germans located in the same regions as refugee migrants, and if East Germans also put downward pressure on native outcomes, then our coefficient estimates would be more negative. On the other hand, if East Germans avoided regions with high foreign inflows, perhaps because they expected better employment opportunities in unaffected regions, our estimates would be biased towards zero. To probe into this issue, we formed a new set of first stage models using proxies for the regional inflows of ethnic Germans (as identified above) and East German immigrants as the dependent variables. To measure the inflow of East Germans, we use two approaches. First, we approximate the inflow of East Germans based on the SIAB sample, with the obvious shortcomings of being available only from 1992 onwards, and relying on a rather crude identification based on the location of the first spell (see above). Second, we draw on information from the German Microcensus, an annual survey of a 1% random sample of the German population. This survey asked individuals in 1991, 1993, and 1995, whether they had migrated from East Germany, and we thus use the change in the share of East German immigrants in West German regions as dependent variable.[35] We suspect that this measure is somewhat more reliable, though it suffers from the limitations of a smaller sample and potential measurement error. The relationship between these "German" inflows and our distance instrument is reported in columns 6 to 8 of Table 2.3. For ethnic Germans, we only find a relatively low correlation between inflows and distance to border, with an F-Statistic of less than 3. For East Germans, in contrast, we find a relatively strong correlation arising in both the SIAB and the Microcensus.[36] As this finding might cast doubt on our main results, we show in section 2.6.4 that our key conclusions hold up when we additionally exclude regions with unusually high inflows of natives in the critical years (potentially East or ethnic Germans that are not correctly identified), or regions within an 80km strip from the former inner German border (section 2.6.4).

[34] In Appendix A.5, we provide a detailed description of the construction of our shift share instrument. Moreover, we compare our approach with Dustmann and Glitz (2015) who provide a similar application of the instrument for West Germany during the same time frame but obtain a highly significant relation between the predicted and the actual change in local labor supply.

[35] Specifically, we draw on data prepared by Burchardi and Hassan (2013), and refer to their study for further details of the sample construction. They use a coarser definition of regional units and compute the difference in the share of East Germans in West Germany, rather than the change of East Germans in the West German population. Moreover, they refer to the total population rather than the number of employed individuals.

[36] Moreover, informal experimentation with German pension data suggests that this correlation breaks down if we focus on employment inflows rather than population inflows.

2.6. Results

2.6.1. Baseline Effects

Table 2.4 summarizes our baseline results of the impact of immigrants on local native wages and employment based on equations (2.1) and (2.2). Looking across the first row, reporting simple OLS effects, we first note that the growth in immigrant employment is uncorrelated with changes in native wages and employment. These simple correlations might be upward biased by positive demand shocks jointly attracting immigrants *and* generating higher wage and employment growth for natives. Instrumenting the change in immigrant employment with distance to border, we find considerably more negative effects. In particular, our coefficient estimates suggest that a one percentage point rise in local immigrant employment — about half of the average increase between 1988 and 1993 — reduced native wages and employment by 0.68 and 1.13%, respectively, though the employment effect is somewhat imprecisely estimated. To put the wage effect into perspective, note that over the same time period native real wages rose by about 1.3% per year (8.5% over 5 years; see Table 2.1), suggesting that the negative impact of immigrants did not result in real wage losses for natives, but rather counteracted what would otherwise have been an even larger wage growth.

To understand how these cumulative effects evolved over time, Figure 2.5 plots the sum of the 2SLS effects in each year relative to 1988, which we normalize to zero.[37] Two main conclusions emerge: first, our estimates for both wages and employment are close to zero and statistically insignificant before 1988 when immigrant inflows were low, suggesting that distance to border is uncorrelated with native wage and employment growth prior to the inflow of immigrants. This finding alleviates concerns that our instrumental variable strategy merely picks up persistent labor demand differentials between regions, e.g., due to different industry compositions, and thus serves as an indirect confirmation of the validity of our identification strategy (DSS). Second, coinciding with the rapid surge of immigration from then on, we observe a steady downward trend in wages until 1993, followed by a relatively flat development thereafter. Reassuringly, this pattern is just the reciprocal of the rise in immigrant employment shares over the same years (Figure 2.2). While the overall patterns are similar for employment, the effect starts somewhat delayed, is significant only in 1992, and tends to be compensated in the medium run (after 1993) by positive annual effects. Taken at face value, the point estimate for 1992 suggests a sizable displacement effect of about 1.02 employed natives per additional immigrant finding a job (-1.111×0.92).[38]

How can these estimates be reconciled with the positive or null effects often found in the literature? We argue that a key difference is *time*. In the short run, firms might be reluctant or unable to adjust their capital and production technology, workers might not have found a job

[37] This means that the difference between t and t+1 is equal to the coefficient estimate for t+1. For simplicity, we show confidence bands based on non-bootstrapped standard errors which are slightly smaller than bootstrapped standard errors.

[38] See Appendix A.3 for a derivation of the native displacement effect. DSS, who also focus on the short run, report estimates in a similar ballpark (0.9 natives per additional Czech immigrant employed).

Table 2.4.: Baseline Effects of Immigration on Native Wage and Employment Growth

	Wages			Employment		
	'88-'93	'93-'98	'88-'98	'88-'93	'93-'98	'88-'98
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Pooled sample						
OLS	−0.108	0.120	0.010	0.186	0.739	0.920
	(0.112)	(0.127)	(0.199)	(0.351)	(0.278)	(0.554)
2SLS	−0.677	0.527	−0.153	−1.125	1.504	0.377
	(0.281)	(0.187)	(0.393)	(0.718)	(0.474)	(1.096)
Panel B: Unskilled						
OLS	−0.089	−0.120	−0.212	−0.152	0.977	0.821
	(0.213)	(0.218)	(0.363)	(0.646)	(0.542)	(1.039)
2SLS	−0.695	−0.101	−0.725	−2.610	1.113	−1.513
	(0.459)	(0.321)	(0.607)	(1.166)	(0.789)	(1.575)
Panel C: Skilled						
OLS	−0.068	0.244	0.175	0.240	0.766	1.003
	(0.122)	(0.133)	(0.216)	(0.376)	(0.314)	(0.615)
2SLS	−0.581	0.843	0.245	−0.917	1.711	0.799
	(0.294)	(0.208)	(0.428)	(0.779)	(0.558)	(1.295)
Local labor markets	204	204	204	204	204	204

Notes: Table shows cumulative wage (columns 1 to 3) and employment (columns 4 to 6) effects from a series of models relating native annual wage and employment changes to the aggregate inflow of immigrants between 1988 and 1993. Two-stage least squares estimations are implemented in two steps as described in the main text. The first stage is weighted by initial native employment in 1988, and the second stage is weighted by native employment in the base year. Estimates are trend-adjusted using a region-specific linear trend based on years 1986 to 1988. Standard errors are calculated using a pairs bootstrap with 1,000 replications. Data source: SIAB 7510.

in another region or upgraded to higher skilled occupations yet, and future entrants might still be investing into more education (see above). All these adjustment processes bear the potential to turn a negative short run effect of immigration into a null or positive effect in the medium or long run (Ruist et al. 2017). To shed more light on this, we investigate in columns 2 to 3 and 5 to 6 how wages and employment evolve over the 5-year period following the immigration shock (1993-1998), and over the entire 10-year time frame (1988-1998). Indeed, the overall picture is now considerably more optimistic, with significantly positive effects for both outcomes in 1993-1998 compensating for the negative effects in the short run, and implying a null effect in the long run. However, while these patterns are consistent with standard economic theory (Borjas 2009) and empirical results in Monras (2015) and Edo (2017), they should be regarded suggestive since, as we expand the analysis period, potentially confounding shocks might debilitate a causal interpretation.[39]

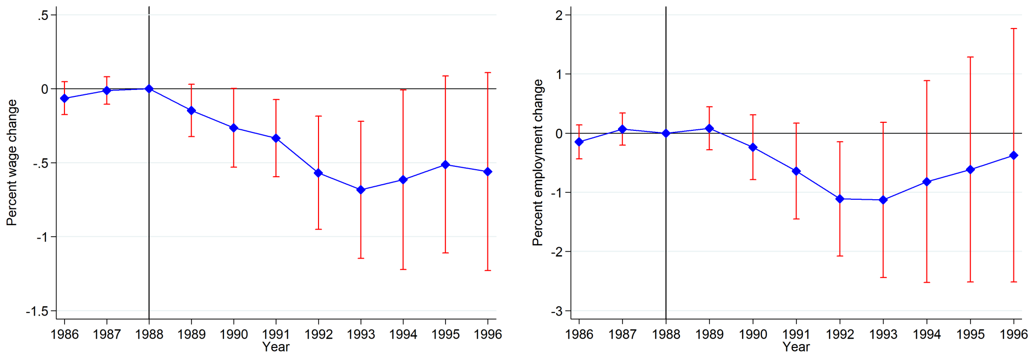
As indicated, these dynamic adjustments are consistent with various potential explanations. However, we believe that the magnitude of the short run effects calls for further explanations, and we investigate several possibilities below. We show that employment losses in one region

[39]A related concern of long run estimates in settings that use cross regional variation is a potential violation of the stable unit treatment value assumption (SUTVA). As we show below, we find substantial spillover effects induced by native regional mobility, meaning that as time passes, our counterfactual regions also experience an (indirect) impact of immigrants through native mobility. It is thus difficult to interpret the long run estimates as an overall causal effect.

Figure 2.5.: Cumulative Wage and Employment Effects of 1988-1993 Immigrant Inflow

A. Cumulative Wage Effects

B. Cumulative Employment Effects



Notes: Figure shows cumulative wage and employment effects for years 1986-1996. In each year, we plot the sum of coefficient estimates relative to 1988, that is, we sum backward and forward. 95% confidence bands are indicated in red. The vertical line represents the start of the immigration shock. Data source: SIAB 7510.

are to a large extent associated with employment gains in other regions, and demonstrate that employment declines are concentrated in particular subgroups, among them the elderly for whom an outflow from employment might be associated with early retirement, rather than job search. We also show that the net reduction in regional employment results from both a slowdown in inflows and an acceleration of outflows, meaning that unlike the typical notion of native displacement, part of the local employment loss is attributable to immigrants preventing native workers from finding a job in a region when they otherwise would have.

2.6.2. Skilled and Unskilled

The discussion in section 2.3 (Table 2.1) showed that immigrants were disproportionately unskilled, hence it is natural to wonder how different skill groups responded to the immigration shock. Indeed, panels B and C indicate some striking differences: for unskilled workers, we find no wage effects, but substantial employment effects of about -2.61%, which, on a per worker basis means that about 0.37 unskilled natives leave or no longer enter employment for each additional immigrant employed between 1988 and 1993.[40] This suggests that unskilled workers bear the burden of immigration predominantly through the employment margin: although they only account for about 15% of local native employment, they account for 35% (0.37/1.04) of its decline. In contrast, our estimates for skilled workers suggest that they respond primarily through wage losses.[41] Taken together, these findings point to a

[40]Using figures from Tables I (col. 1: native share = 0.975), II (col. 1: 0.276) and IV (col. 2: -1.371) from DSS, we also obtain a displacement of 0.37 unskilled per additional Czech immigrant employed. This comparison holds only approximately, because DSS report summary statistics including the unemployed (leading to a higher unskilled share), whereas our summary statistics refer to employed workers only, which should be used to calculate the native displacement effect.

[41]This corresponds to a native displacement of about 0.71 skilled workers. In principle, the sum of unskilled and skilled displaced workers should sum up to 1.04 (-1.125×0.92), the total effect in 1993. They do not add up exactly due to the different weighting used in our second stage analysis.

relatively larger short run labor supply elasticity (or stronger wage rigidities) for unskilled than skilled workers, meaning that part of what would otherwise have been a wage loss is compensated by a reduction in employment. These results underpin that a thorough evaluation of immigration effects must consider both wages *and* employment jointly. Specifically, if we had only examined wages (*assuming* inelastic labor supply), we would have found only skilled workers to be affected negatively by immigration, and our estimates suggest that this conclusion would have been severely misguided.[42]

As before, we also examined how the effects evolve over time (col. 2-3 and 5-6). With one exception (wages for unskilled), the post-migration period is marked by positive wage and employment effects that are highly significant among skilled workers, and tend to mitigate the short run contemporary impact of immigration.

2.6.3. Age Groups, Occupations, and Industries

We next narrow our attention to workers in different age groups (young, middle, and old), and in different types of occupations (simple and advanced) and industries (tradable and non-tradable), seeking to provide a coherent picture of the impact on the native labor market, and to better understand the proximate sources of these effects. We will focus on the short run effects, not only because the existing immigration literature has paid much less attention to this time horizon, but also because the short run dynamics constitute an essential ingredient for understanding how local labor markets adjust to immigration shocks.

Young, Middle, and Old Workers: Panel A of Table 2.5 shows 2SLS estimates of the impact of immigrants on young (under 30), middle (between 30 and 49), and old (above 50) workers. Since younger workers are both more mobile and on a steeper gradient of their age-earnings profile (making it easier for employers to enforce reduced wage growth), we might suspect that they react more strongly to immigrants than older workers with families settled and wage profiles plateauing. The entries in Table 2.5 generally confirm these considerations. Although we find no effects for workers below 30, looking at ages 30 to 49, we find that a one percentage point increase in local immigrant employment reduced native wage and employment growth by about 0.98% and 2.11%.[43] Old workers, in contrast, respond to the same inflow only on the employment margin (-1.93%), though as we illustrate below, much of this reduction arises through increased outflows into unemployment, which, at the time, was an attractive path to early retirement.[44]

Simple and Advanced Occupations: Since the immigrants that we consider were less educated on average than natives, and spoke the German language at lower levels of proficiency,

[42]Somewhat ironically, it is precisely this large employment decline in affected regions which may have shielded unskilled workers staying employed from incurring similar wage cuts as skilled.

[43]Note that the joint occurrence of negative wage *and* employment effects for the large group of middle-aged workers is consistent with the local supply of capital not being fully elastic (DSS).

[44]DSS report the largest employment effects for old, followed by young and middle aged workers. Our results show the largest response among middle aged workers.

Table 2.5.: Effects of Immigration on Native Wage and Employment Growth by Subgroups

	Wages (1)	Employment (2)
Pooled Sample	−0.677	−1.125
	(0.281)	(0.718)
Panel A: Age Groups		
Below 30	−0.650	0.892
	(0.471)	(0.947)
Between 30 and 49	−0.983	−2.109
	(0.305)	(0.777)
50 and above	−0.247	−1.927
	(0.352)	(0.957)
Panel B: Occupational Complexity		
Simple	−0.952	−1.169
	(0.316)	(0.810)
Advanced	−0.192	−0.919
	(0.303)	(0.887)
Panel C: Sectors		
Tradable	−0.506	−2.054
	(0.284)	(0.708)
Nontradable	−1.041	0.701
	(0.369)	(0.898)
Panel D: Gender		
Men	−0.651	−1.179
	(0.302)	(0.940)
Women	−0.713	−0.984
	(0.334)	(0.606)
Local labor markets	204	204

Notes: Table shows cumulative wage and employment effects from a series of models relating native annual wage and employment changes to the aggregate inflow of immigrants between 1988 and 1993, instrumented using distance to border. Two-stage least squares estimations are implemented in two steps, where the first stage is weighted by initial native employment in 1988, and the second stage is weighted by native employment in the base year. Estimates are trend-adjusted using a region-specific linear trend based on the 1986-1988 period. Standard errors are calculated using a pairs bootstrap with 1,000 replications. Data source: SIAB 7510.

we might expect the effect on natives to be stronger in low skill jobs (e.g., cleaning) than high skill jobs (e.g., planning, managing, or designing). To test this hypothesis, we slice the sample into simple and advanced occupations based on the task composition associated with each job. Using the 1985 wave of the BIBB/IAB Qualification and Career Survey (see Appendix A.1), we classify the following tasks as "advanced": designing, making plans, restoring, servicing and equipping machines (Prantl and Spitz-Oener 2014). We then define "job complexity" as the average share of advanced tasks in an occupation, and consider an occupation as advanced (simple) if the associated share is above the employment weighted median of the job complexity index. By design, simple occupations thus contain relatively high routine and manual task shares that can be easily performed by lesser skilled immigrants. As panel B of Table 2.5 shows, we find no effects in advanced jobs, but a large and significant wage depression of 0.95% in simple occupations — corroborating our expectation that the impact on natives is larger in jobs more likely to be performed by immigrants.

Tradable and Non-Tradable Industries: In a recent study for Germany, DG show that an inflow of immigrants has significant distributional effects on resident employment. Specifically, using variation between regions and skill groups, they find that an inflow of immigrants reduces the relative wage of workers in the non-tradable sector, but has no effect in the tradable sector.[45] In contrast to their empirical approach, we only exploit regional variation in immigration shocks, hence we can examine what type of native response in each sector generates the distributional effects. We investigate this in panel C of Table 2.5. A striking observation is that the wage impact in non-tradable industries is about twice as large as in tradable industries, whereas employment effects are entirely concentrated in the tradable sector. This means that the relative wage effect reported in DG is associated with an overall decline in average wages in the non-tradable sector of affected areas relative to unaffected ones, consistent with firms in the tradable sector responding to changes in labor supply primarily by means of technological adjustments (instead of wages). It is natural to wonder what provokes these markedly different response margins in the two sectors. One explanation might be that wages in the tradable sector are set at the industry level, implying that the local relative wage elasticity in response to immigration is low.[46] While beyond the scope of this paper, we believe that inquiring deeper into this heterogeneity, perhaps by incorporating the dimension of product and labor market regulation (Prantl and Spitz-Oener 2014), is a promising avenue for future research in this area.[47]

2.6.4. Robustness

In this section, we show that our main results are robust to a variety of robustness checks regarding the possibility of correlated shocks, the unit of observation, the selection of regions, the inclusion of further covariates, and alternative measures of the immigration shock and native outcomes. We moreover illustrate that worker selection into nonemployment generates an upward bias in standard cross-sectional wage estimates often applied in the immigration literature. Having established that our results hold up under these alternatives, we then turn to investigate in more detail the different response margins generating the overall employment response of natives.

Correlated Shocks: The causal interpretation of our IV estimates hinges on the identifying

[45] Using the terminology of Dustmann et al. (2016), DG's design is based on the mixture approach, which uses variation in immigration across regions and skills cells, thus identifying these distributional effects of immigration. We instead use the pure spatial approach, exploiting only variation in the immigration shock across regions.

[46] DG investigate this by looking at union coverage rates, concluding that industry level wage setting is probably not the main source.

[47] As shown in panel D, we also looked for heterogeneous effects across genders. Although a large literature shows that female labor supply is on average more elastic than male labor supply (see Evers et al. 2008, for a review), we do not find a larger employment effect for women than men.

assumption that distance to border is uncorrelated with other shocks affecting local native outcomes. The combination of region-specific linear trends and cleaned outcomes accounts for pre-existing structural differences in wage and employment growth as well as contemporary differences in the demographic structure possibly correlated with immigration and native outcomes. We also showed in Figure 2.5 that distance to border has no effect on native outcomes prior to 1988. However, it is conceivable that our instrument is correlated with shocks associated with German reunification, which only happened after 1988. For example, increased market access (Redding and Sturm 2008), the phasing out of subsidies to the former border region (*Zonenrandförderung*), or industrial relocation (Redding et al. 2011) might be functions of distance *and* correlate with native outcomes.[48] We address these concerns in two ways reported in columns 2 and 3 of Table 2.6. First, we exclude regions receiving border zone subsidies prior to 1994.[49] Second, we augment our baseline model with a Bartik instrument to control for coincident demand shocks (Bartik 1991). Specifically, we predict the 1988-1993 native wage (employment) growth in a region based on its industry structure in the base year and the industry-specific wage (employment) growth in all other regions. Reassuringly, these exercises yield very similar wage and employment effects as our baseline estimates, suggesting that our results are not driven by correlated shocks in the aftermath of reunification.

As noted above, we might still be concerned with confounding effects of East and ethnic German inflows. One approach would be to examine whether the results are robust to the exclusion of regions near the inner German border. Indeed, excluding all areas whose outer contour reaches into an 80 km strip from the former inner German border does not affect our estimates (column 4). Another approach would be to exclude regions with unusually high native inflows, assuming that these must be driven by East and ethnic Germans. We do so in column 5, again finding very similar effects as in the baseline model.

A final concern regards the possibility of understating the actual immigrant employment shock (and thus overestimating the immigration effect) since we only observe workers showing up in the social security system. Indeed, the period under consideration has seen a massive rise in labor migrants through the implementation of bilateral labor treaties (section 2.2), and these workers were generally not subject to social security contributions in the host country.[50] While it is difficult to fully account for this effect, we try to at least partially address this concern by excluding the building sector from the analysis, which has been the key employer of labor migrants. As shown in column 6, we find no evidence of an undercounting bias.

[48] It is *a prioi* unclear whether these shocks would lead to an upward or downward bias in our estimates. On the one hand, increased market access for border regions after reunification would imply an upward bias. On the other hand, the parallel phasing out of substantial subsidies for border regions until 1994 would suggest a downward bias.

[49] These are detailed in the Federal Law Gazette (*Bundesgesetzblatt*) 77, pp. 1217-1240 (1971). Estimations are based on the district-level.

[50] The number of labor migrants rose by 80,000 workers between 1988 and 1993, whereas the total number of social security employed immigrants rose by 450,000 workers. This suggests that we might overstate the impact of immigration by about one-fifth.

Table 2.6.: Wage and Employment Effects of Immigration Under Alternative Model Specifications

		Correlated Shocks					Unit of Observation		Selection of Regions		Additional Covariates			
	Base-line (1)	Exclude subsidized districts (2)	Baseline + Bartik demand IV (3)	Distance to border > 80 km (4)	High native inflows (5)	Exclude building sector (6)	District level (7)	Individ. level (8)	Highly exposed (9)	Large labor markets (10)	Unad-justed (11)	Col. 11 + demogr. controls (12)	Col. 12 + Bartik demand IV (13)	Long differ-ences (14)
Panel A: Wages														
All	−0.677	−0.680	−0.761	−0.681	−0.627	−0.639	−0.643	−0.757	−0.287	−0.839	−1.260	−0.956	−0.660	−0.638
	(0.281)	(0.325)	(0.259)	(0.278)	(0.256)	(0.297)	(0.247)	(0.241)	(0.456)	(0.337)	(0.375)	(0.595)	(0.476)	(0.275)
Unskilled	−0.695	−0.512	−0.301	−0.595	−0.655	−0.691	−0.723	−0.748	−1.011	−1.718	−1.092	−2.172	−0.989	1.177
	(0.459)	(0.539)	(0.411)	(0.506)	(0.511)	(0.450)	(0.412)	(0.473)	(0.831)	(1.550)	(0.461)	(1.224)	(0.932)	(0.622)
Skilled	−0.581	−0.610	−0.800	−0.605	−0.529	−0.517	−0.536	−0.687	−0.088	−0.781	−1.331	−0.793	−0.776	−0.914
	(0.294)	(0.335)	(0.267)	(0.302)	(0.253)	(0.316)	(0.249)	(0.244)	(0.423)	(0.353)	(0.372)	(0.587)	(0.497)	(0.296)
Local labor markets	204	272	204	144	204	204	325	204	100	94	204	204	204	204
Individuals							448,604							
Panel B: Employment														
All	−1.125	−1.459	−0.953	−1.793	−1.048	−1.224	−0.950	−0.398	−1.509	−2.302	−2.497	−0.919	−0.650	−1.617
	(0.718)	(1.032)	(0.538)	(0.691)	(0.716)	(0.740)	(0.794)	(0.301)	(0.896)	(0.548)	(0.762)	(1.462)	(0.904)	(0.626)
Unskilled	−2.610	−2.427	−2.742	−2.319	−2.466	−2.420	−2.366	−1.849	−1.723	−3.914	−3.554	−2.263	−1.998	−3.153
	(1.166)	(1.308)	(1.117)	(1.149)	(1.140)	(1.211)	(1.185)	(0.600)	(1.676)	(3.865)	(1.403)	(2.636)	(1.949)	(1.149)
Skilled	−0.917	−1.332	−0.631	−1.786	−0.850	−1.049	−0.739	−0.107	−1.485	−2.170	−2.530	−1.045	−0.273	−1.511
	(0.779)	(1.050)	(0.643)	(0.816)	(0.819)	(0.784)	(0.884)	(0.309)	(0.991)	(0.611)	(0.811)	(1.546)	(1.123)	(0.686)
Local labor markets	204	272	204	144	204	204	325	204	100	94	204	204	204	204
Individuals							530,282							

Notes: Table reports estimates of the wage and employment effect of immigration between 1988 and 1993 for different types of regions and under alternative model specifications. Column 1 reports the baseline results for reference. Columns 2 to 6 analyze sources of correlated shocks by imposing different restrictions on the sample. Columns 7 and 8 change the level of observation from commuting zones to the district level and to the individual level. In columns 9 and 10, we estimate the models for different types of regions. In columns 11 to 13, we use unadjusted native wages and employment (see main text for the adjustment procedure), and successively include additional control variables. Column 14 reports coefficients from models based on the long difference, i.e., without conditioning the wage analysis on workers being present in two consecutive periods. All models include a linear region-specific trend for years 1986-1988. Except for column 8, standard errors are bootstrapped using a pairs bootstrap with 1,000 replications. In column 8, standard errors are clustered at the commuting zone level. Data source: SIAB 7510.

Unit of Observation: Our main analysis is based on 204 commuting zones, representing aggregates of 325 districts. To test whether our results depend on the particular regional unit, we re-estimated our baseline wage and employment models at the district level rather than the commuting zone. The associated coefficients are reported in column 7, and they are very similar to our baseline estimates. Another specification check is to investigate the impact of immigrants directly on the worker level. To this end, we calculate first differences in wages and employment for each worker (and within regions in the case of wages) and regress the trend-adjusted change in each outcome on the instrumented immigrant inflow. We control for gender interacted with full sets of age and education dummies, and cluster standard errors at the regional level. As shown in column 8, point estimates and significance levels line up well with our baseline results.

Selection of Regions: One might be concerned that our results are driven by particular types of regions, e.g., the largest or smallest regions, or regions with particularly high immigrant exposure. To investigate this, columns 9 and 10 of Table 2.6 repeat our analysis for three different types of regions. First, we consider only regions with very high immigrant exposure, defined as an above median percent increase in immigrant employment between 1988 and 1993. Despite some variation in magnitude and significance in both outcomes, we find consistently negative signs and cannot detect systematic deviations from our baseline estimates. Second, we restrict the sample to large regions with an average labor force exceeding 50,000 individuals. As shown in column 10, both wage and employment effects are more pronounced in this subsample. One reason might be that large regions typically feature higher average wage and employment growth, which generates additional leeway for reductions in the associated growth rates due to immigration.

Additional Covariates: As explained above, we residualize native wages and employment in each year before calculating region level aggregates. However, since some native characteristics might themselves be endogenous — for example, facing an immigrant shock, native labor market entrants might decide to study one more year rather than compete with unskilled immigrants for jobs (Hunt 2017) — it may be preferable to condition on pre-shock characteristics instead. We analyze this in columns 11 to 13 of Table 2.6, starting from a specification with raw instead of cleaned outcomes and no controls except for a linear trend, and then augmenting these models with region level covariates averaged over the 1986-1988 period. The results in column 11 suggest somewhat larger wage and employment effects than our baseline results, indicating an overall average decline of 1.26 and 2.5%, respectively. In column 12, we add an array of region level covariates: the shares of middle and old workers, the fraction of females, the share of advanced occupations, the share of tradable industries, the overall employment level, and the unemployment rate. Overall, the results are now much closer to our baseline estimates both in terms of wages and employment, with the only notable deviation remaining for the small group of unskilled workers. Finally, in column 13, we also add a Bartik instrument (see above). This has two effects: on the one hand, it brings our wage estimates even closer to our baseline results (also for unskilled); on the other hand, the employment response shrinks to about 50% of our baseline coefficient, mainly driven by skilled workers. Overall, however, the patterns look very similar to our baseline estimates.

Worker Selection: Most studies of the effects of immigration rely on repeated cross-sections to estimate the response in native wages to an immigration shock (Card 2001; Glitz 2012). Only a few recent studies such as Bratsberg and Raaum (2012), Foged and Peri (2016), and DSS exploit longitudinal worker spell data to account for differential worker selection into nonemployment. If low wage workers are more likely to select into nonemployment, simple cross-sectional comparisons produce upward biased wage effects. To investigate this, we report in column 14 of Table 2.6 estimates based on regional wage changes calculated as the difference in mean wages between all workers in 1993 and all workers in 1988.[51] We find considerably larger coefficients for unskilled workers, with point estimates rising from an insignificant -0.695 to a significant 1.177, confirming the DSS findings and underpinning the importance of controlling for selection effects.[52]

Alternative Measures of Immigration Shocks: Our definition of the native employment response (dependent variable) and our measure of the immigration shock (independent variable) differ from specifications typically used in the immigration literature. Specifically, we standardize the change in local native employment by native *employment* in the base year, rather than dividing by the labor force or population (Altonji and Card 1991; Pischke and Velling 1997; Dustmann et al. 2005). In addition, our immigration shock is measured in terms of employment as opposed to, e.g., labor force or population inflows. To explore the sensitivity of our results against these alternatives, we collected data on native and immigrant populations at the district-level, enabling us to scale the employment change (ΔE) not only by employment (E, our baseline) but also by labor force (LF) and population (P). We also built two additional versions of our immigration shock variable, measuring the inflow either in terms of the labor force or in terms of the population.[53] We then regressed each native employment proxy (ΔE/E, ΔE/LF, ΔE/P) and the wage change on each of the three immigrant shock variables, instrumented using distance to border.

Table 2.7 reports the results, showing different employment measures from left to right, and different immigrant shock measures from top to bottom. Reading across columns thus tells us how a different scaling of the native employment response changes the estimated coefficients (conditional on how we measure the immigration shock), whereas reading across rows tells us how different ways of gauging the immigration shock affect our conclusions (conditional on how we measure the native employment response). For each combination of dependent and independent variable, we display cumulative coefficients (1988-93), standard

[51] In these models, we use a manual trend-adjustment, i.e., we subtract the average value of the outcome variable for each region over the years 1986-1988 from the dependent variable; see section 2.4.

[52] For employment, in contrast, net changes are relatively similar, with differences arising mainly through approximation errors and different weighting factors.

[53] To build the population series, we digitized Statistical Yearbooks (years 1985-1990) as well as multiple versions of the BBSR Laufende Raumbeobachtung (years 1986, 1989/90, 1992/93).

Table 2.7.: Comparison of Different Measures of Immigrant Inflows and Native Outcomes

		Employment			Wages
	Type of Standard Error (1)	%-change native employment o/ employment (baseline) (2)	%-change native employment o/ labor force (3)	%-change native employment o/ population (4)	%-change native wages (baseline) (5)
Panel A: Instrument: South and East Distance to Border					
%-change in immigrant		−1.125	−1.319	−0.368	−0.677
employment share	Pairs	(0.718)	(0.621)	(0.208)	(0.281)
	2SLS	(0.700)	(0.575)	(0.202)	(0.247)
First stage F-statistic		16.43	16.51	16.71	16.43
%-change in immigrant		−1.319	−1.247	−0.417	−0.879
labor force share	Pairs	(0.621)	(0.556)	(0.186)	(0.234)
	2SLS	(0.575)	(0.549)	(0.173)	(0.215)
First stage F-statistic		28.33	28.61	28.57	28.33
%-change in immigrant		−1.696	−1.623	−0.514	−1.424
population share	Pairs	(0.530)	(0.515)	(0.162)	(0.378)
	2SLS	(0.555)	(0.530)	(0.167)	(0.296)
First stage F-statistic		18.25	18.47	21.73	18.25
Local labor markets		204	204	204	204
Panel B: Instrument: Population Shift Share (Base Density 1961)					
%-change in immigrant		−2.708	−2.529	−0.816	−1.313
population share	Pairs	(0.542)	(0.506)	(0.158)	(0.289)
	2SLS	(0.768)	(0.754)	(0.261)	(0.456)
First stage F-statistic		32.56	31.22	30.24	32.56
Local labor markets		112	112	112	112
Weight (as of 1988)		native employment	native labor force	native population	native employment

Notes: Table shows cumulative native wage and employment outcomes for alternative measures of native employment (columns 1 to 3) and immigrant shocks (across rows) between 1988 and 1993. Columns 1 and 4 are based on the baseline definition of native employment and wage changes. Column 2 scales employment changes by the native labor force, and column 3 by native population. All models refer to the pooled regressions, combining skill and age groups. We weight the first stage by total native employment (columns 1 and 4), labor force (column 2), and population (column 3) in 1988, and the second stage by the corresponding values in the previous year. All models include a linear region-specific trend for years 1986-1988. The first row measures the immigrant shock by the percentage change in the immigrant employment share; the second row, by the percentage change in the immigrant labor force; and the third row by the percentage change in the immigrant population. Standard errors are bootstrapped using a pairs bootstrap with 1,000 replications. F-Statistics for instrument excludability are reported below standard errors. Data source: SIAB 7510.

errors, and F-Statistics for the first stage. Moving down column 1, we see that our baseline results (row 1; compare Table 2.4) hardly change if we measure the immigration shock in terms of labor force (row 2) or population (row 3) instead of employment. Moving across row 1, we observe a relatively stable effect for the labor force measure but a sizable drop by about two-thirds when measuring the employment change by the employment-to-population ratio (similar patterns emerge in rows 2 and 3). Hence, while the definition of our dependent variable might explain why we report larger employment effects than typically found in the literature, our particular definition of the immigrant inflow appears rather innocuous — if anything, our approach delivers conservative estimates. In the last column, we report the associated wage effects for each immigration proxy, again suggesting that our baseline estimates represent a lower bound.

As a final check, we collected population data for the earliest year available, 1961, to construct a standard shift share instrument based on historical immigrant population densities.[54] When we use this instrument instead of distance to border, we obtain the highest F-statistics, and also larger and more precisely estimated wage and employment coefficients, reinforcing our conclusion that the short run effects of immigration reported above tend to be conservative (see also Appendix A.5).

2.6.5. Margins of Adjustment: Inflows vs. Outflows and Geographic vs. Nonemployment Flows

Our results reveal substantial employment adjustments due to immigration for unskilled workers, middle and older age groups, and, more broadly, in the tradable sector. These findings raise three questions which we set out to answer in this section. First, are these effects generated by increased employment outflows, reduced inflows, or both? Second, are these flows emerging between regions (within employment), or rather between employment and nonemployment? Third, do the insignificant employment effects found for certain subgroups potentially conceal a rise in overall worker flows, with growing inflows and outflows compensating each other? To answer these questions, we decompose the net employment effects into outflows from and inflows into employment, distinguishing between two types of transitions: geographic transitions that lower employment in one region and increase employment in another; and nonemployment transitions that lower employment in one region without a corresponding gain in another region. We further decompose the nonemployment transitions into transitions from and to unemployment (if a person is observed as unemployed prior/post an employment spell) and transitions from and to nonemployment (if a person is not observed prior/post an employment spell).

The results for several subsamples are presented in Table 2.8. Columns 1 to 4 refer to inflows, columns 5 to 8 to outflows, and the last four columns to net flows, i.e., the difference between inflows and outflows. In each set of columns, we show, from left to right, geographic, nonemployment, and unemployment transitions, followed by the total effect in the last column — column 12 corresponds to the total employment effect reported in Tables 2.4 and 2.5.

Inflows and Outflows: The entries in columns 4 and 8 Table 2.8 show the total contributions of inflows and outflows, respectively. Overall, we find that the negative employment effects of natives are generated by a combination of reduced inflows into employment and higher outflows from employment. For example, using the average coefficients for the three groups

[54]The Stata file is available online at GESIS data archive, file name ZA2472. It provides, among others, census data for years 1961 and 1987. Bavaria and North Rhine-Westphalia are not included, which explains the reduced number of observations. We combine this file with population data from the German Federal Statistical Office for years 1985-2001.

Table 2.8.: Impact of Immigrants on Native Inflows/Outflows and Geographic/Nonemployment Flows

	Inflows				Outflows				Net Flows (= Inflows – Outflows)			
	Geo-graphic (1)	Nonem-ployment (2)	Unem-ployment (3)	Sum of Inflows (4)	Geo-graphic (5)	Nonem-ployment (6)	Unem-ployment (7)	Sum of Outflows (8)	Geo-graphic (9)	Nonem-ployment (10)	Unem-ployment (11)	Net Em-ployment (12)
Total employment flow	−0.67	−0.28	0.36	−0.59	0.05	−0.36	0.85	0.54	−0.72	0.09	−0.49	−1.13
	(0.43)	(0.41)	(0.25)	(0.64)	(0.41)	(0.32)	(0.40)	(0.53)	(0.39)	(0.53)	(0.37)	(0.71)
Panel A: Skill Groups												
Unskilled	−0.09	−1.04	−1.04	−2.17	0.11	−0.72	0.72	0.44	−0.20	−0.32	−1.77	−2.61
	(0.33)	(0.92)	(0.41)	(0.94)	(0.45)	(0.75)	(0.50)	(0.90)	(0.40)	(0.94)	(0.70)	(1.15)
Skilled	−0.79	−0.04	0.65	−0.23	0.09	−0.24	0.84	0.69	−0.88	0.20	−0.19	−0.92
	(0.47)	(0.46)	(0.27)	(0.82)	(0.38)	(0.33)	(0.42)	(0.52)	(0.44)	(0.61)	(0.35)	(0.80)
Panel B: Age Groups												
below 30	−2.30	1.16	1.70	0.56	−0.73	−0.93	1.67	−0.33	−1.57	2.09	0.03	0.89
	0.66	(0.75)	(0.54)	(1.16)	(0.55)	(0.54)	(0.59)	(0.88)	(0.65)	(0.74)	(0.44)	(0.95)
between 30 and 49	0.07	−1.25	−0.28	−1.43	0.54	0.26	−0.14	0.68	−0.46	−1.51	−0.14	−2.11
	(0.47)	(0.41)	(0.25)	(0.59)	(0.39)	(0.44)	(0.30)	(0.60)	(0.46)	(0.59)	(0.37)	(0.75)
50 and above	0.09	−0.45	−0.43	−0.76	0.16	−0.50	1.75	1.17	−0.07	0.05	−2.19	−1.93
	(0.38)	(0.35)	(0.31)	(0.52)	(0.32)	(0.66)	(0.67)	(0.85)	(0.30)	(0.73)	(0.72)	(0.96)
Panel C: Gender												
Men	−0.63	−0.38	0.68	−0.33	0.27	−0.63	1.21	0.85	−0.90	0.26	−0.53	−1.18
	0.54	(0.46)	(0.25)	(0.74)	(0.40)	(0.36)	(0.32)	(0.57)	(0.60)	(0.60)	(0.36)	(0.94)
Women	−0.75	−0.06	−0.10	−0.91	−0.29	0.02	0.34	0.07	−0.46	−0.08	−0.44	−0.98
	(0.36)	(0.51)	(0.23)	(0.74)	(0.37)	(0.50)	(0.49)	(0.69)	(0.28)	(0.64)	(0.43)	(0.67)
Local labor markets	204	204	204	204	204	204	204	204	204	204	204	204

Notes: Table shows estimates of the effect of changes in immigrant employment between 1988 and 1993 on native inflows and outflows. Columns 1 to 4 show inflow rates, columns 5-8 outflow rates, and columns 9 to 12 show net flows. Columns 1, 5, and 9 refer to geographic mobility which is identified from individual worker spells if a person is employed in two different regions in two consecutive years. Columns 2, 6, and 10 refer to nonemployment flows, defined as changes from unobserved into employment or from employment into unobserved. Columns 3, 7, and 11 refer to unemployment flows, defined as changes from observed unemployment into employment, and vice versa. Columns 4, 8, and 12 report the sum of all flow components. All models include a linear region-specific trend for years 1986-1988. Bootstrapped standard errors are reported in parentheses and calculated using a pairs bootstrap with 1,000 replications. Data source: SIAB 7510.

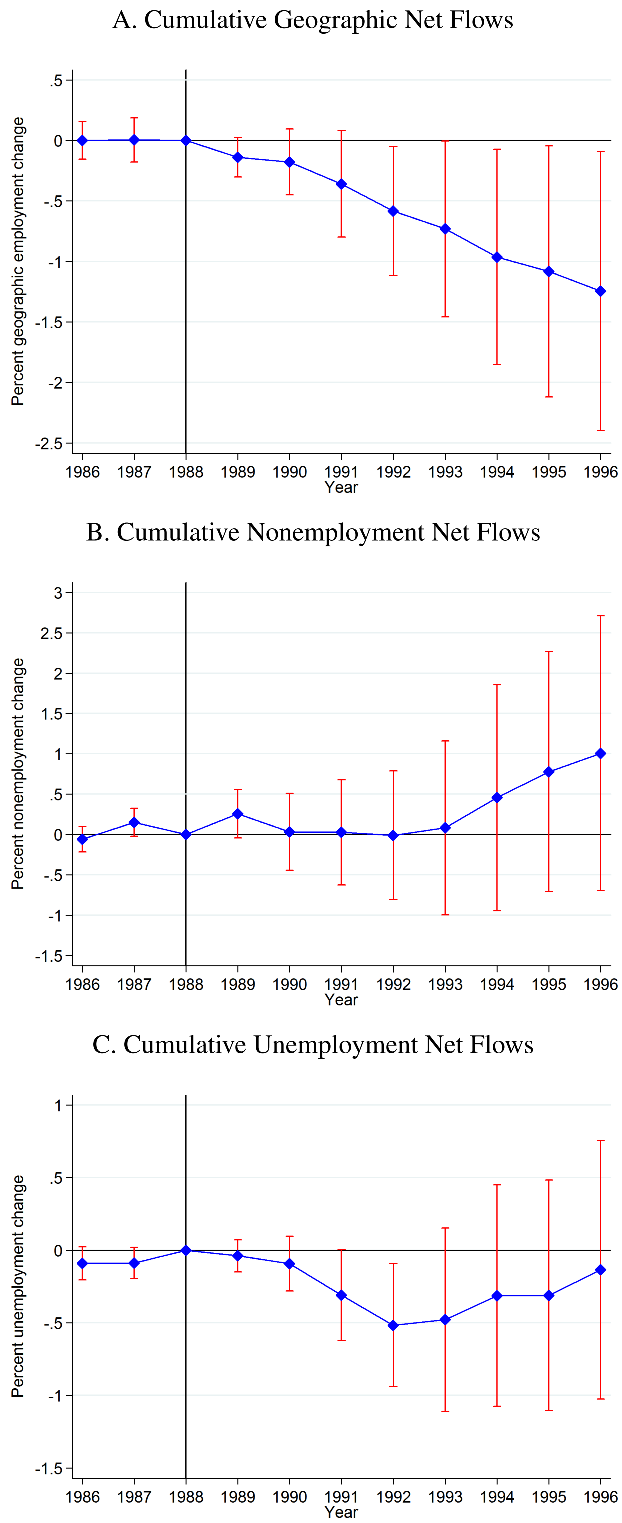
with significant employment declines (unskilled, ages 30-49, and 50+), we find that inflows account for about two-thirds (1.45%) and outflows for about one-third (0.76%) of the overall average employment decline of about 2.21% in these groups — the importance of inflows for the employment response is in line with DSS, but contrasts with evidence in Wozniak and Murray (2012) who find the main response margin of natives to be outflows. In sum, our findings suggest that employment losses from immigration are, at least in the short run, induced by employers adjusting their hiring rate or poaching behavior; that is, incumbent workers are protected from immigration effects at the expense of outsiders, possibly, because the latter feature a more elastic labor supply.

Geographic and Nonemployment Flows: In columns 9 to 11 of Table 2.8, we next partition the employment decline (column 12) into three terms gauging the contributions of geographic transitions (job-to-job), movements between employment and nonemployment, and movements between employment and unemployment. A striking conclusion from this exercise is the important role of geographic mobility in the pooled sample, suggesting that on average 64% (0.72/1.13) of the 1988-1993 employment decline in one region is compensated by employment gains in other regions. This means that, at the macro-level, the effect of immigration on native employment is considerably less negative than suggested by our region-level estimates. While the prominence of geographic mobility for the local native employment response is qualitatively in line with DSS, our estimate is about twice as large as theirs. One explanation might be that our immigrant shock was less clustered, meaning that natives had to move a shorter distance to avoid it. Moreover, our sample consists of many urban areas which, on average, feature higher labor mobility rates than the rural areas considered in DSS. A visual impression of the underlying adjustment processes is provided in Figure 2.6. While we find an immediate and sustained decline in geographic net flows (panel A), the contribution of nonemployment flows (panel B) is zero until 1993, and tends to increase thereafter. Unemployment flows (panel C), on the other hand, show an initial decline until 1992, and a return to zero in subsequent years. Overall, these results suggest that geographic mobility is a primary means of equilibrating local labor markets (Blanchard and Katz 1992; Cadena and Kovak 2016), with a sustained reduction in affected areas creating new job opportunities for nonemployed and unemployed workers in the longer run.

In panels A and B of Table 2.8, we break out the overall effect into different skill and age groups. For both skilled and young workers, geographic mobility exhibits sizable declines — among skilled, it virtually accounts for the entire reduction in local employment growth. In contrast, among unskilled workers and workers in older age groups, a reduction in nonemployment and unemployment flows dominates the overall employment effect.

To investigate the sources of these effects, we further decompose the contribution of each channel into inflows and outflows (columns 1 to 3 and 5 to 7). Considering first geographic flows, the overall picture is one of a decline in inflows rather than an increase in outflows, that is, instead of workers in affected regions seeking employment elsewhere, it is workers elsewhere no longer moving into affected regions. Not surprisingly, this is especially true for

Figure 2.6.: Decomposition of Cumulative Net Employment Effects Into Geographic and Nonemployment Flows



Notes: Figure shows a decomposition of the cumulative net employment flows for years 1986-1996 from Figure 2.5B into geographic, nonemployment, and unemployment flows. In each year, we plot the sum of coefficient estimates relative to 1988, that is, we sum backward and forward. 95% confidence bands are indicated in red. The vertical line represents the start of the immigration shock. Data source: SIAB 7510.

skilled and younger workers for whom geographic flows are of particular importance. This result contrasts with evidence for the US (Wozniak and Murray 2012), where immigrants increase outflows of high skilled natives, however, it is consistent with evidence for Germany (DSS), suggesting that geographic flows are primarily driven by the inflow margin. For nonemployment and unemployment transitions, we also find reduced inflows, especially among unskilled and middle-aged workers, but here outflows likewise play a role, most importantly in the youngest and oldest age groups. Looking at young workers in more detail, we note that the growing outflow into unemployment is somewhat compensated by a corresponding rise in inflows, implying an increase in gross native worker flows. One interpretation of this result is that immigration leads to a reshuffling of extant employment matches which would be consistent with a process of occupational specialization as suggested by Peri and Sparber (2009), and might constitute another means of alleviating wage losses.

Finally, turning to older workers, we find that the growing outflow into unemployment is the main source of employment adjustments. We suspect that this outflow effect is amplified by one particular feature of the social security system at the time, which rendered unemployment for male workers approaching retirement a very attractive and often used choice. Specifically, conditional on at least 52 weeks of registered unemployment, male workers were allowed to retire up to 5 years early. Though suggesting at most, the annual growth rate of men in West Germany exploiting this option rose from 0.5 percentage points per year in 1970-1990 to 2 percentage points in 1990-1995. In 1995-2000, i.e., after the immigration shock, this rate returned to 0.7 percentage points (German Pension Fund, 2017).

2.7. Conclusions

Research on the effects of immigration on native workers is abundant, yet a central concern, namely, whether immigrants reduce or raise native wages and employment remains a controversial topic: empirical evidence covers the whole range from negative effects, to zero and positive effects, even when analyses are based on the same time period, country, and data. Using detailed longitudinal administrative data, we provide the first comprehensive analysis of the short-run effects of a large and unexpected immigration shock between 1988 and 1993 on native wages and employment in the West German labor market.

Our analysis of local labor markets shows that immigrants tend to reduce native wages and employment by about 0.68% and 1.13% in the first five years after arrival, but tend to have no or even positive effects in the longer perspective. We find important heterogeneity across various groups of the labor market, which might be explained by different labor supply elasticities or wage rigidities. We also show that, while wage reductions are indeed borne by incumbent workers, i.e., those who are and stay employed in a given labor market, about two-thirds of the local employment losses are compensated by employment gains in other regions, meaning that those workers who move out of an affected region or those who no longer enter it often find employment in other regions. We show that the latter component is a key driver of the impact of cross-regional flows.

What do our findings imply for the current refugee debate? Latest since 2015, the topic of immigration has resumed center-stage in the political debate in virtually all receiving countries in Europe, with populist parties rapidly gaining voter approvals (Dustmann et al. 2016). At first glance, our results seem to spur those populist calls for more isolation, increased border control, and vetting of immigrants. But this is only half the story for two main reasons. First, the composition of source countries in the 1980/1990 refugee wave differed considerably from today's refugee inflow. Crucially, the inflow that we consider, comprising of many East Europeans and Yugoslavs (with guest worker background), was probably more educated and in a sense "culturally closer" than the composition of refugees arriving today. Consequently, the labor market impact of the current inflow would be expected to be less pronounced. Second, the specific conditions on the German labor market during our analysis period differed dramatically from today's situation. In the early 1990s, Germany was characterized by high and increasing stocks of unemployment, struggling with reunification, rigid labor market institutions, and a lack of international competitiveness. All these features prevented the German labor market from a speedy adjustment to the additional supply of labor and likely contributed to the temporary decline in native wages and employment. However, our results suggest that even in this difficult situation, the long term effects of immigration were, if anything, positive. Looking at Germany today, the picture has completely reversed: due to trade unions' continued accommodation of moderate wage growth and rising wage flexibility, and due to substantial reforms of the social security system, the German economy has risen its productivity (measured in unit labor costs) and by now belongs to one of the most productive economies in the world (Dustmann et al. 2014), with unemployment rates chasing one record low after the other. Against this backdrop, the German economy seems ready to leverage the opportunities associated with the inflow of immigrants in order to keep fueling the booming economy. To accomplish this, a fast processing of asylum applications and parallel integration into the labor market is imperative.

3. Effects of Relaxed Employment Protection on Labor Market Outcomes: Evidence from a 2004 German Reform

3.1. Introduction

Employment protection legislation constitutes a key feature of labor markets. Its major aim is to protect employees from arbitrary dismissals and increase job security. At the same time, it may impose rigidities on the ability of firms to adapt to changing economic conditions. Too stringent dismissal protection may hinder job creation and worker reallocation unless dismissal costs are passed on to workers through wages (Leonardi and Pica 2013; OECD 2013). Moreover, a growing regulatory gap between open-ended and temporary contracts may increase labor market segmentation by encouraging the excessive use of less-protected temporary employment (OECD 2014). Overall, the debate on the design of dismissal protection remains controversial both from a research and a policy perspective. Empirical evidence from the 2004 reform of the German employment protection legislation may provide valuable insights for theoretical work as well as for policymaking.

As the largest economy in the European Union and the fourth largest in the world (by GDP), Germany is a worthwhile example to study the interrelation between dismissal protection legislation and employment. Moreover, the latest reform of the German Protection Against Dismissal Act (PADA — *Kündigungsschutzgesetz* (KSchG)) in 2004 constitutes a suitable case for an empirical analysis of its causal effects on different labor market outcomes. Notably, the German government decided to change the minimum establishment size threshold determining coverage by the PADA from five to 10 employees as of January 1, 2004. The reform affected about 15 to 16% of all establishments (with at least one employee liable for social security payments) representing 7 to 8% of all employees liable for social security payments in Germany.[1]

In this paper, I exploit the temporal and cross-sectional variation in the PADA resulting from this 2004 reform as a quasi-experiment. Using a difference-in-differences (DiD) approach, I identify the causal effect of reduced dismissal protection on various labor market outcomes. To this end, I compare average outcomes of establishments subject to the policy change (treatment group) with establishments that exhibit similar establishment

[1] Own calculations based on data from the IAB Establishment Panel and the LIAB QM2 9310 following the approach of Rudolph (1996).

characteristics, yet, are not exposed to the change (control group) before and after the reform. By drawing on detailed administrative employer-employee panel data linked to establishment survey data (LIAB QM2 9310), I provide estimates on the impact of the change in the PADA in 2004 on establishment level worker flow, job flow and churning rates. Worker flows comprise yearly hires and separations whereas the job flow is defined as the yearly difference between the two. Churning, on the other hand, refers to worker flows in excess of job flows. In addition, I assess potential heterogeneous treatment effects by looking at effects on gender- and age-specific turnover rates as well as differential effects by unionization status and East-West divide of firms.[2] Finally, I examine other margins of adjustment that may mitigate potential effects on the above mentioned outcomes. Namely, I study effects on firm mean wages and the use of temporary employment relations. I take into consideration a three and a half year period after the reform until 2007, which allows me to capture potential short- and medium-term adjustments.

The present paper contributes to the strand of literature on the labor market effects of employment protection legislation. Since the seminal work of Lazear (1990), the impact of dismissal costs on labor market flows and employment is analyzed in a number of theoretical studies. Drawing on Lazear's result from 1990 that severance payments between the employer and the employee can be offset by an efficient labor contract, early literature uses partial equilibrium models with third party transfers to show that more stringent employment protection reduces layoffs in downturns, but also deters employers from hiring in upturns as firms take potential future dismissal costs into account. Thus, increased dismissal costs reduce worker flows while its impact on the level of overall employment remains ambiguous. These findings also hold in a number of studies using general equilibrium models (e.g., Mortensen and Pissarides 1999; Ljungqvist 2002). As for the PADA reform studied in this paper, economic theory thus predicts increased worker flows given an easing of dismissal protection, but does not provide a clear-cut prediction in terms of net employment effects. In addition, although the above mentioned studies do not explicitly discuss the effect on churning (defined as worker flows in excess of net employment), a decline in dismissal costs should tend to raise excess worker turnover.

A number of empirical studies exploit the quasi-experimental setting of changes in dismissal protection to examine the effect on worker turnover. Kugler and Pica (2008) analyze the impact of a labor market reform in Italy in 1990. They use an employer-employee panel and exploit the differential increase in dismissal protection for firms with fewer than 15 employees relative to firms with more than 15 employees. They find that the increase in dismissal costs reduces the individual probability of employment by 13 to 14% and for loss of employment by 14 to 15%. At the same time, year-to-year employment declines by 5 to 6% in smaller firms relative to larger firms. Martins (2009) studies the effects of a law introduced in Portugal in 1989 under which dismissal constraints were softened for all firms. However, firms with 20 or fewer employees were partially exempted from the new law such

[2] I use the term *worker turnover* in this paper to refer to hires, separations, job flows, and churning.

that they experienced an even greater reduction in dismissal costs relative to larger firms. Using longitudinal data from an annual employment survey covering firms and workers based in Portugal, he finds evidence for a small relative increase in the small firms' job flow rate driven by a moderate increase in their hiring rate that corresponds to 5% of their average hiring rate. Moreover, estimates on the churning rate are statistically insignificant. von Below and Thoursie (2010) investigate a reform of a size-contingent relaxation of seniority rules in Sweden in 2001 where firms with ten or fewer employees were allowed to exempt two workers from the prevalent 'last-in-first-out' principle. Using an employer-employee panel, they find that both the probabilities of hires and separations increase by 1.7 percentage points for small relative to large firms after the reform. Centeno and Novo (2012) explore a reform in Portugal in 2004 that increased the dismissal protection of open-ended contracts for firms with 11 to 20 workers. Among other outcomes, they estimate the impact of the reform on churning among workers with open-end and fixed-term contracts and find evidence for an increase among the latter. That is to say, churning of workers on fixed-term contracts in the treated firms increased by 1.3 percentage points, while no statistically significant effect is observed among workers with open-ended contracts.

Bauer et al. (2007) are the first to conduct a similar analysis for Germany by investigating previous changes in the minimum threshold of the PADA in 1996 and 1999. They use administrative data based on the Employment Statistic Register of West German establishments and study short-term changes in employment dynamics in a six months period after the 1996 reform and a three months period after the 1999 reform. Their results do not suggest a significant effect of changes in the stringency of dismissal protection on worker and job flows. Closest to this paper, Bauernschuster (2013) analyzes the effect of the latest adjustment of the threshold of the PADA in 2004 on the hiring behavior of firms. He uses survey data on establishments (IAB Establishment Panel) and estimates the impact of the change in the dismissal protection in the first one and a half years after the reform. He finds that the relaxed dismissal protection increases the hiring rate for small establishments relative to larger ones by 1.3 to 2.0 percentage points in 2004 and 2.0 to 2.1 percentage points in 2005.

My analysis differs from previous studies for Germany (e.g., Bauer et al. 2007; Bauernschuster 2013) on a number of key dimensions. First, I exploit the absence of any relevant changes to the PADA after 2004 and examine an extended period of three and a half years as opposed to the short-term view of at most one and a half years. Second, I provide a more comprehensive analysis by assessing the heterogeneity in treatment effects for different groups of workers and firms. Third, I consider a potential impact of the reform along other margins of adjustment, in particular, wages and the use of temporary employment. Lastly, I put particular emphasis on the sample selection criteria and discuss different methods in the light of the literature on firm size-contingent labor market reforms.

My estimates from the difference-in-differences approach do not provide robust evidence for a causal impact of the 2004 PADA reform on overall hiring, separation, job flow and churning rates of treated establishments. This is line with the Bauer et al. (2007) analysis on previous threshold changes but to some extent inconsistent with the positive effect on the hiring rate found by Bauernschuster (2013). In a further analysis, I show that differences to the later study can be explained by different assignment methods to the treatment and control groups. In addition, I find some evidence of increases in the hiring and job flow rates of women in response to the relaxed dismissal protection, but no effect on age group-specific turnover rates. Finally, I do not find any support for an adjustment along the wage margin and changes in the use of temporary employment relations. While there are as yet no comparable analyses for Germany, Leonardi and Pica (2013) find a negative relationship between dismissal costs and wages in Italy exploiting the Italian 1990 reform mentioned above. As for temporary work, Boockmann and Hagen (2001) and Fritsch and Schank (2005) conduct very similar analyses on the effect of the previous PADA reform in 1996 on the use of fixed-term employment in Germany, but do not provide a consistent conclusion since only the former find a significant negative effect of a reduction in dismissal protection on the demand for fixed-term employment. In contrast, there is strong evidence that stricter dismissal protection in the United States boosted the use of temporary services employment between the beginning of the 1970s and mid 1990s (Autor 2003).

The remainder of this paper is structured as follows: In the next section, I provide background information on the institutional setting of the PADA reform. In section 3.3, I explain the identification strategy. I then describe the data, the sample selection rules, and the summary statistics and assess the validity of the stable unit treatment value assumption (SUTVA) in section 4.2.1. Next, I present and discuss the estimation results in section 3.5. Finally, I conclude in section 3.6.

3.2. Institutional Background

Since 1951, the German employment protection legislation is regulated by the Civil Code (*Bürgerliches Gesetzbuch* (BGB))) and the Protection Against Dismissal Act (PADA — *Kündigungsschutzgesetz* (KSchG)). Its aim is to protect employees from arbitrary dismissals by their employer. In principle, the PADA applies to all employees of an establishment unless a person is employed on a fixed-term contract.[3] Given an employer is not exempted from the PADA and the duration of employment exceeds the probationary period of six months, a dismissal with notice is only effective if it is socially justified on either personal grounds, grounds of conduct, or operational grounds (KSchG §1 (2)). Prior to any dismissal, the employer has to give notice to its works council (if in place) under the terms of the Works Constitution Act (*Betriebsverfassungsgesetz* (BetrVG) §102). In case of a dismissal due to operational requirements, the employer has to carry out a social selection among his employees taking into consideration job tenure, age, maintenance obligation, and severe disability (KSchG §1 (3)). In addition, any dismissed employee may appeal to the labor

[3] Although the Law on Part-Time Work and Temporary Employment Contracts (*Teilzeit- und Befristungsgesetz* (TzBfG)) does allow for individual or collective agreements which grant the right of regular termination to employees on fixed-term contracts (TzBfG §15 (3)), this is in practice the exception rather than the rule.

court and contest the termination of her contract (KSchG §4). If the court decides in favor of the employee, the employer commonly has to pay a severance payment as a reinstatement of the employee is not feasible in most cases. Ultimately, studies show that dismissals under the PADA often produce legal effects and are considered to be costly (e.g., Jahn and Schnabel 2003; Pfarr et al. 2004; Jahn 2005).

Some employers are exempted from the PADA by legislation. In particular, the PADA comprises an exception provision for small establishments such that dismissals in establishments with less than a minimum number of employees do not have to comply with the PADA but only need to fulfill some general statutory dismissal rules. As part of the *Agenda 2010* reform package, the minimum threshold for the applicability of the PADA was raised from five to 10 full-time equivalent weighted (FTE) employees as of January 1, 2004 for any employee hired after December 31, 2003, while incumbent workers stayed protected as long as their initial number did not fall below the former threshold of five FTE employees. Prior to this reform, the threshold had been adjusted twice; first, it was raised from five to 10 employees in 1996, and then reduced from 10 to five employees in 1999. Since the latest reform in 2004, no further adjustments to the PADA have taken place.[4]

The reform of the PADA is a result of negotiations in the conciliation committee as of December 16, 2003 and was approved on December 19, 2003, less than two weeks before it became effective. Prior to that, the German government proposed in the government policy statement on March 14, 2003 and lastly reiterated in an information brochure published on November 14, 2003 to facilitate the hiring of fixed-term contract employees in establishments with less than five workers. Eventually, the short-dated announcement and introduction of the threshold reform are advantageous for my setting in that the analysis of the 2004 PADA reform is unlikely to be distorted by anticipation effects.

However, it is worth mentioning that besides the PADA reform, the Agenda 2010 entailed further modifications of labor market policies that were gradually implemented between 2003 and 2005, known as the four *Hartz* reforms. The active labor market policies of Hartz I deregulated temporary employment (i.e., fixed-term contract (FTC) and temporary agency (TA) employment) in 2003.[5] Hartz II comprised changes in the regulation of freelance work and marginal employment. Hartz III regulated the restructuring of the Federal Employment Office. Hartz IV included revisions of the social and unemployment assistance. In addition,

[4]The PADA reform in 2004 comprised some further minor modifications that are not contingent on establishment size: The social selection process was simplified, the period for filing a suit was standardized to three weeks, and severance payment on the waiver taking legal action was introduced.

[5]With respect to FTC employment, Hartz I implied a lowering of the age threshold for unlimited use of fixed-term contracts without valid reason from 58 to 52. For employees below this age, the maximum duration of fixed-term contracts without valid reason remained at two years. In terms of TA employment, the maximum period of assignment was raised from 12 to 24 months in 2002 and eventually completely abolished in 2003. Moreover, the rehiring and synchronization ban was suspended such that TA workers could be repeatedly hired by a particular agency and labor contracts could be synchronized with the duration of a specific assignment. Lastly, with an interim arrangement until 2004, the principal of 'equal pay' and 'equal treatment' was introduced. Except for the introduction of an upper limit of five years for fixed-term contracts without valid reason for workers above the age of 52 as of May 1, 2007, there have been no further changes to the regulation of FTC and TA employment in the observation periods of this study (i.e., until June 30, 2007).

the German Trade and Crafts Code was reformed as of January 1, 2004 by abolishing the master craftsman requirement for 53 out of 94 regulated crafts occupations (Rostam-Afschar 2014). However, it is important to note, that contrary to the PADA reform, none of these additional reforms were specific to the establishment size.

3.3. Identification Strategy

The aim of my paper is to identify the impact of the change in the PADA on different labor market outcomes. To this end, I exploit the 2004 PADA reform as a quasi-experiment and apply a difference-in-differences (DiD) approach by comparing outcomes of establishments subject to a change in the PADA (treatment group) with establishments not exposed to this change (control group) before and after the policy reform (Meyer 1995). More formally, this double difference can be expressed by the equation:

$$\rho = \{E[Y_{it}|D_i = 1] - E[Y_{it}|D_i = 0]\} - \{E[Y_{it'}|D_i = 1] - E[Y_{it'}|D_i = 0]\},$$

where Y_{it} and $Y_{it'}$ denote the observable outcome of observation i in period t and t', t is a time period after and t' a time period before the policy reform. D_i is a dummy variable indicating whether observation i belongs to the treatment group ($D_i = 1$) or the control group ($D_i = 0$). In the present analysis an observation i refers to an establishment (or firm).[6] The key identifying assumption is a common time trend, meaning that in the absence of treatment the average outcomes of both treatment and control firms would have evolved in the same way. Formally, this can be expressed as:

$$E[Y_{it}^0 - Y_{it'}^0|D_i = 1] = E[Y_{it}^0 - Y_{it'}^0|D_i = 0],$$

where Y_{it}^0 and $Y_{it'}^0$ denote the potential outcomes of firm i in period t and t' in the absence of the treatment. In other words, the common trend assumption justifies the replacement of the counterfactual (unobserved) non-treatment difference in average outcomes of the treated by the observed non-treatment difference of the (observed) non-treated. Assuming that the common time trend assumption holds, ρ identifies the average causal effect of the treatment on the treated firms. This assumption, however, is inherently not testable and must thus be defended by economic reasoning. Moreover, note that up until now, I have used a time constant definition of the treatment identifier (D_i). In what follows, I will add the bracketed subscript (t) to the identifier which indicates that the treatment assignment may also vary over time, depending on the underlying assignment method to the treatment and control group ($D_{i(t)}$).[7]

[6] Although multiple establishments may belong to the same firm (or company), I use the terms establishment and firm interchangeably throughout this paper.

[7] I note that a time-varying treatment assignment necessitates that the above expressions hold accordingly for D_{it}. I elaborate on different assignment methods to the treatment and control group at the end of this section and in section 3.4.2.

Under the assumption of an additive causal effect ρ, the approach can be generalized to the linear regression equation:

$$Y_{it} = \alpha_i + \lambda D_{i(t)} + \rho(D_{i(t)} \times Post_t) + \delta_t + \varepsilon_{it}, \tag{3.1}$$

where α_i accounts for firm fixed effects and thus allows for time constant confounding observables and unobservables, $D_{i(t)}$ is a dummy variable that takes the value 1 for treated firms and 0 for control firms and, as described above, may or may vary over time. $Post_t$ is a dummy variable that takes the value 1 in the years after 2003 and 0 otherwise, δ_t captures yearly effects that are assumed to be common to all firms, and ε_{it} is an idiosyncratic error term. The parameter of interest is ρ and reflects the differential effect on the outcome variable Y_{it} due to the policy reform.

I opt to estimate equation (3.1) with the fixed effects (FE) estimator for the sample periods 2001 to 2007.[8] To define 2003 as the baseline period and at the same time provide descriptive evidence for the common time trend assumption, I estimate a version of equation (3.1) that includes $\mu(D_{i(t)} \times Pre_t)$ where Pre_t is a dummy variable that takes the value 1 in the years before 2003 and 0 otherwise. Eventually, μ captures a pooled pre-treatment difference between the treatment and control group for the years 2001 and 2002. Put differently, a statistically insignificant estimate for μ suggests that the treatment and control group do not follow a different time trend in the pre-treatment periods, which I consider as support for the common time trend assumption.

To capture time trends of the treatment effect, I additionally estimate a modified version of equation (3.1) whereby the interaction terms $D_{i(t)} \times Pre_t$ and $D_{i(t)} \times Post_t$ are replaced by a series of annual leads and lags of the reform:

$$Y_{it} = \alpha_i + \lambda D_{i(t)} + \sum_{\substack{\tau=2001 \\ (\tau \neq 2003)}}^{2007} \rho_\tau (D_{i(t)} \times \delta_t^\tau) + \delta_t + \varepsilon_{it}, \tag{3.2}$$

where τ denotes years, each δ_t^τ represents a dummy variable that takes the value 1 in year $t = \tau$ and 0 otherwise, and all other variables are defined as above. The parameters ρ_τ capture the time trend of the treatment by providing estimates of the annual treatment effect. Thereby, I regard statistically insignificant estimates of the leads (ρ_τ for $\tau < 2003$) as support for the common time trend assumption.

In contrast to other studies on firm size-contingent reforms of dismissal protection (e.g., Bauer et al. 2007; Centeno and Novo 2012), I omit X_{it} in equations (3.1) and (3.2). That is, I do not control for observable time-varying firm-level characteristics in my main specification. Recall that the firm fixed effects already take into account time constant differences between firms. If I want to include further variables, I have to assume that these variables are not influenced by the treatment. Given that I can only consider variables that change over time

[8] Note that FE estimates given by equation (3.1) and ordinary least squares (OLS) estimates given by an analogous equation that includes α instead of α_i are identical if the sample is a balanced panel and the treatment status is time constant.

and that are also measured after the treatment, there is a good chance that these variables are *bad controls* and violate the assumption of exogeneity (Angrist and Pischke 2009; Lechner 2010). On the other hand, one could make the counterargument that the exclusion of time-varying variables that are not influenced by the reform but still correlated with both the treatment status and the outcome variable might lead to an omitted variable bias and/or decrease the precision of the estimates. Reassuringly, I obtain very similar results that lead to the same conclusions when I include a set of time-varying controls (see the sensitivity check in section 3.5.5).[9]

Due to the panel data structure, serial correlation in the error term of an establishment may be an issue when conducting statistical inference. Though, by using a FE estimator, I implicitly take into account any serial correlation that is captured by the unobserved time-invariant firm effect. Moreover, I cluster standard errors at the establishment level to further allow for unrestricted correlation between observations of each establishment.

A consistent estimation of the causal effect ρ (and ρ_τ, respectively) based on equation (3.1) and (3.2) relies on a series of identifying assumptions. In addition to the common time trend assumption mentioned above (for which I provide evidence along with the discussion of the main results in section 3.5), I have to assume that the threshold change has no effect on control firms (i.e., stable unit treatment value assumption, SUTVA). Most notably, firms that are just above the new threshold may alter their hiring and separation decisions in response to the reform. Accordingly, I show support for the absence of threshold effects in section 3.4.5. Next, while there is hardly any scope for anticipation effects due to the short-dated introduction of the reform (see discussion in section 3.2), the analyses may suffer from a bias due to selection into (or out of) treatment after the reform. To circumvent this problem, some studies define a time-invariant treatment status based on firm size in the pre-reform periods only (e.g., Martins 2009; Bauernschuster 2013), while others allow for a time-varying treatment status but argue that a FE estimator should, for the most part, obviate issues of endogeneity as it allows for selection into treatment based on time-invariant unobservable characteristics that also influence the outcome variable.(e.g., Centeno and Novo 2012).[10] Eventually, I apply both approaches by using different methods to assign establishments to the treatment and control group (see section 3.4.2 for an in-depth discussion of the various assignment methods). Finally, I have to assume that the other reforms discussed in section 3.2 did not affect the treatment and control firms in systematically different ways.

[9]The set of explanatory variables consists of worker characteristics averaged at the firm level. See Table 3.3 for a list.

[10]Technically, the absence of self-selection into (or out of) treatment requires that $E[D_{it}\varepsilon_{it}|\alpha_i] = 0, \forall t$.

3.4. Data

3.4.1. Data Source

The analysis uses the Cross-sectional Model 2 of the Linked-Employer-Employee Data 1993-2010 (LIAB QM2 9310) from the German Institute of Employment Research (IAB).[11] The data set is a representative sample of German establishments and contains both survey data on establishments and administrative data on individuals. For the years under consideration, the annual sample size amounts to roughly 16,000 establishments representing approximately 1% of the universe of German establishments. The data on individuals cover employees that are liable for social security payments and work for one of the establishments.[12] Given the administrative nature of the individual-level information, the data set is considered to be highly reliable. The data are particularly suitable for the analysis, as the individual-level data can be aggregated at the establishment-level using a unique identifier and allow for tracing employees over time to determine establishment-level worker flows. Moreover, the data provide sufficient information to calculate the establishment size in line with the legislation and thus to determine coverage by the PADA for each establishment.

3.4.2. Treatment Assignment

The threshold of the PADA applies to establishments (not companies with several establishments in several municipalities), which is also the unit of measurement in the data. Panel A in Table 3.1 summarizes the procedure to determine the full-time equivalent weighted (FTE) firm size defined by the PADA in column 1 and the availability of the associated information in the data in column 2. Panel B lists the relevance of workers by their employment status.

In principle, all regular employees including marginal employees, employees with fixed-term contracts and employees hired within the last six months should be counted.[13] Part-time employees should be weighted depending on contractual weekly working hours. According to the legislation (KSchG §23), employees working up to 20 hours per week should be weighted by 0.5, employees working more than 20 and up to 30 hours per week should be weighted by 0.75, and employees working more than 30 hours should be weighted with 1.0. Although the data set provides information on part-time employees, it only records whether a person works up to 18 hours per week (small part-time) or more than 18 hours per week (large part-time). I weight the former by 0.5 and the latter by 0.75. Accordingly, part-time employees working more than 18 and up to 20 hours per week as well as workers identified as part-time employees but working more than 30 hours per week are not weighted exactly according to the PADA. Owners and executive staff not subject to directives, family

[11] See Heining et al. (2013) for a detailed description of the data.

[12] Employees liable for social security payments are all white-collar and blue-collar workers including apprentices and, since 1999, also marginal employees and unpaid family workers. Civil servants, self-employed and regular students are not recorded.

[13] Against a common misconception, the PADA applies to marginal employees without any restrictions.

Table 3.1.: Determination of FTE Establishment Size

	PADA (1)	LIAB QM2 (2)
Panel A: Full-time equivalent weight		
0.5 (small part-time)	[0, 20]	[0, 18]
0.75 (large part-time)	(20, 30]	(18, full-time)
1.0 (full-time)	(30, ∞)	full-time
Panel B: Employment status		
Regular employees liable for social security payments	Included	Included
Marginal employees	Included	Included
Fixed-term contract workers	Included	Included
Workers employed for less than 6 months	Included	Included
Owners and executive staff (not subject to directives)	Excluded	Excluded (if identified)
Family members without working contract	Excluded	Excluded
Vocational trainees (incl. apprentices)	Excluded	Excluded
Employees with an inactive work relationship	Excluded (if replaced)	Excluded (always)
Temporary agency worker (in user establishment)	Excluded	Excluded

Notes: Panel A lists the full-time equivalent weights for different intervals of workings hours per week. Column 1 indicates the intervals defined by the PADA. Column 2 shows the intervals observed and used in the data. Panel B lists different types of employment status and their respective consideration in the PADA and the data.

members without a labor contract and vocational trainees should not be counted and are consequently excluded. Employees with an inactive work relationships (e.g., maternity leave) should be excluded in case of replacement. As the data do not record inactive employees and replacements cannot be identified, I assume that inactive employees are replaced in all cases.[14]

I restrict the sample by excluding establishments in the shipping and aircraft transport industry since other legislation applies to these sectors (KSchG §24). In addition, I remove establishments in the highly subsidized agricultural and mining sectors as well as non-profit firms and private households. Moreover, I abstract from establishment entries and exits and only keep firms that are always present during the sample periods 2000 to 2007.[15,16]

For the analysis, I compare average outcomes of establishments subject to the policy change (treatment group) with establishments that exhibit similar establishment characteristics, yet, are not exposed to the change (control group) before and after the reform. The binary treatment variable $D_{i(t)}$ identifies each group and depending on the assignment method may or may not vary over time. The assignment of firms to the treatment and control group is determined by the FTE firm size range and the selection of assignment periods.

[14]Only recently, German Federal Labor Court (*Bundesarbeitsgericht* (BAG)) has decided that TA workers should also be counted if they regularly work for the user establishment (BAG, judgment of January 24, 2013, 2 AZR 140/12). However, in the periods under consideration, the common perception was that TA employees should not be added to the number of employees of the user establishment. In line with this argument, I do not consider TA employment in the determination of the establishment size.

[15]In order to conduct the analysis for the sample periods 2001 to 2007, firms already need to be present in the data in 2000 since worker turnover rates in period t are based on information in periods $t-1$ and t.

[16]For further details on the sample processing, see Appendix B.1.1.

The FTE size range of the treatment group is defined by the size interval $(5, 10]$. This is in line with the reform, which should only affect firms in this size category. In addition, the size interval $(10, 20]$ constitutes the FTE size range for the control group. The firm size restriction for the control group follows Bauernschuster (2013) and seems appropriate as it selects firms that should be very similar to the treated firms except for their difference in size and, at the same time, samples a number of firms that is comparable to the number of firms in the treatment group. I test the sensitivity of the results to different choices for the size limits of the control group in section 3.5.5.

The selection of the assignment periods relies on the debate on regression fallacy. Since the study by Davis et al. (1996), it has been a recurring question in the literature on *small firm job creation* as to how to avoid regression fallacy.[17] However, more recently, there seems to be a consensus that an assignment of firms to size categories based on the *average size* or *base-year size* method should be preferred (Neumark et al. 2011; Haltiwanger et al. 2013; de Wit and de Kok 2014).[18] Further taking into account discussions in previous studies on the effects of size-contingent dismissal reforms, I consider an assignment procedure to the treatment and control group based on the FTE establishment size in a two-year window in the 'before' periods.[19] Similar to the *average size* method, this method assigns firms to a size category based on the firm size in the periods $t-1$ and t. Yet, it differs in that it restricts the sample to establishments that remain in the same size category for two years rather than relying on averages. Eventually, this should mitigate the potential bias from regression fallacy and, in addition, avoid the problem of an ambiguous allocation of worker turnover to firms that enter or exit the treatment or control group during the two-year assignment window.[20] As I consider this assignment method as the most natural approach, I focus on the sample based on this method in the discussion of the summary statistics.

To make my results comparable to previous studies, I apply three further assignment methods that rely on methods used in previous studies (see Table 3.2 for an overview). In contrast to the first method, the second method uses a four-year window in the 'before' periods to mitigate a regression-to-the-mean bias if transitory employment fluctuations last

[17]Following the argument of Davis et al. (1996) and Neumark et al. (2011), the regression fallacy may occur if firms are assigned to a firm size category based on employment in a given year rather than on the firm's long run size. For the case of two size categories (i.e., small and large firms), Neumark et al. (2011) argue that firms assigned to the small size category based on a single year are more likely to have experienced a transitory decrease in employment. Correspondingly, these firms are more likely to return to their long run size revealing a positive job flow rate falsely attributed to the small size category. Eventually, small firms may appear to outperform large firms in terms of job flow rates only because of regression to the mean. The bias in the job flow rate may result from an upward bias in the hiring or a downward bias in the separation rate or both. The reverse argument can be made for large firms. Neumark et al. (2011) further argue that under the assumption that transitory employment changes are not highly correlated, assignment to firm size categories based on multiple years should mitigate a potential bias from the regression fallacy. Note, however, that contrary to the case of two size categories, the direction of the bias for the treatment and control group in the present study is undetermined since the phenomena described above may occur at the upper and lower limit of the size ranges.

[18]The *average size* method assigns firms to a size category based on the simple average of the firm size in $t-1$ and t. The *base-year size* method assigns firms to a size category based on the firm size in $t-1$.

[19]See Appendix Table B.1 for an overview of the literature and the different methods used for the treatment assignment in other studies.

[20]Since the *base-year size* method suffers from the latter issue, I disregard this method.

Table 3.2.: Assignment Methods to Treatment Group and Control Group

	2-Years in 'Before' Periods (1)	4-Years in 'Before' Periods (2)	Always the same (3)	Adjacent Periods (4)
Assignment periods	2002 and 2003	2000 to 2003	2000 to 2007	$t-1$ and t
Treatment units		- FTE firm size $\in$ (5,10] -		
Control units		- FTE firm size $\in$ (10,20] -		
Time-varying treatment status		- No -		Yes
Balanced panel		- Yes -		No

Notes: Table summarizes the four assignment methods to the treatment and control group used in the present study. For further explanations on each assignment methods see section 3.4.2.

up to four years. Eventually, both methods fix the treatment status prior to the reform which is supposed to prevent biases from selection into (out of) treatment. However, they allow firms to exceed the defined firm size limits of the treatment and control group in the periods beyond the assignment window which may distort the actual treatment effect.[21] Therefore, the third method denoted as 'always the same' further restricts the sample to establishments that remain in their size category over the sample periods 2000 to 2007. Finally, the fourth method denoted as 'adjacent period' assigns establishments to the treatment and control group on a rolling basis based on the firm size in the base year ($t-1$) and the current year (t), thus, excluding year-to-year *movers*. Consequently, this method allows the treatment status to vary over time and identification therefore relies on the assumption that there are no unobserved variables that vary over time and influence both the treatment status D_{it} and the outcome variable. In what follows, I denote the four assignment methods by (1) to (4).

3.4.3. Outcome Variables

To examine changes in hires, separations, job flows, and churning, I define worker turnover variables as point-in-time comparisons. FTE_Hires_{it} denotes the number of employees working at establishment i in period t but not $t-1$ weighted in full-time equivalent units according to the PADA. Likewise, $FTE\_Separations_{it}$ denotes the FTE number of employees working at establishment i in period $t-1$ but not t. Since t refers to June 30 in each year, short-term working relationships that begin and end within 12 months (or vice versa) and do not cover June 30 are not taken into account. Due to data limitations, the focus is on all separations irrespective of whether the contract is terminated by the employer or the employee. I use conventional flow rates by dividing the year-to-year flows of establishment i in period t by the average total number of FTE employees between t and $t-1$, denoted FTE_{it} and FTE_{it-1} (e.g., Davis and Haltiwanger 1999; Martins 2009; Centeno and Novo 2012).[22] Consequently, I define the hiring and separation rate as:

[21] Appendix Figure B.1 shows that by 2007 up to 35% of firms are exceeding the size limits if the treatment and control group.

[22] My results are robust to an alternative rate measure that only uses FTE_{it-1} as the denominator.

$$HR_{it} = \frac{FTE\_Hires_{it}}{(FTE_{it} + FTE_{it-1})/2}$$

and

$$SR_{it} = \frac{FTE\_Separations_{it}}{(FTE_{it} + FTE_{it-1})/2}.$$

In addition, I obtain job flows (or net employment changes) as the difference between hires and separations and define the job flow rate as $JFR_{it} = HR_{it} - SR_{it}$. Lastly, following Burgess et al. (2000), I define churning which measures the turnover in excess of the net employment change as $CR_{it} = HR_{it} + SR_{it} - |JFR_{it}|$.

Since the legislation became effective on January 1, 2004, the worker turnover rates for period $t = 2004$ (covering hires and separations between June 30, 2003 and June 30, 2004) are only subject to the policy change for the last six months. To examine this period in more detail, I conduct a supplementary analysis that distinguishes between hires in the first half (from July to December in period $t-1$) and the second half (from January to June in period t) of the 12-months observation period (see section 3.5.2). Due to data limitations, this additional analysis cannot be performed for separations, job flows, and churning. To test for potential heterogeneity of the treatment effects, I further define within-firm gender- and age-specific worker turnover rates as described in section 3.5.3. Finally, I define firm-level mean wages and shares of temporary workers in order to examine other margins of adjustment as described in section 3.5.4.

3.4.4. Summary Statistics

Table 3.3 summarizes the descriptive statistics of the treatment and control group for the sample based on the assignment method (1) in the baseline period 2003. The sample consists of 587 establishments, of which 289 belong to the treatment group. Columns 1 to 4 depict the means and standard deviations of selected firm-level characteristics. Column 5 presents the differences in the means between the two groups whereby asterisks indicate the significance level of t-tests for mean equality. Hiring and separation rates fluctuate around 11 to 12%. This is in line with Bellmann et al. (2017) who find similar rates ranging from 10 to 13% using the same data for the years 1993 to 2014. More importantly, firms in the treatment and control group are very similar in terms of their worker turnover rates. The differences in the rates are uniformly less than 1% and statistically not different from each other.

With regard to the other observable firm-level characteristics, the treatment and control group are also very alike. Although treated firms are slightly overrepresented in wholesale and retail trade and underrepresented in manufacturing and real estate, differences in the industry distribution are not statistically significant at a reasonable level. The same holds with respect to the geographic distribution where there is a minor overrepresentation of

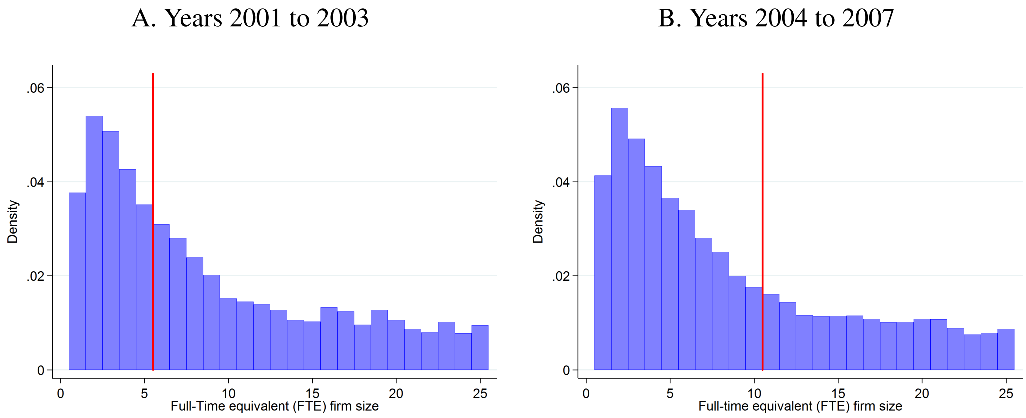
Table 3.3.: Summary Statistics of Establishment Characteristics in 2003 for Sample Based on Assignment Method *2-Years in 'Before' Periods*

	Treatment Group		Control Group		
	Mean	S.D.	Mean	S.D.	Difference
	(1)	(2)	(3)	(4)	(5)
Treatment identifier					
FTE establishment size	7.444	(1.321)	14.848	(2.648)	-7.404^{***}
Outcome variables					
Hiring rate	0.111	(0.123)	0.116	(0.104)	−0.004
Separation rate	0.125	(0.129)	0.122	(0.104)	0.003
Job flow rate	−0.014	(0.133)	−0.006	(0.111)	−0.007
Churning rate	0.145	(0.194)	0.154	(0.157)	−0.009
Industry distribution					
Manufacturing	0.298	(0.458)	0.319	(0.467)	−0.021
Construction	0.187	(0.390)	0.181	(0.386)	0.006
Wholesale and retail trade	0.235	(0.425)	0.205	(0.404)	0.031
Real estate	0.104	(0.306)	0.117	(0.322)	−0.014
Others	0.176	(0.382)	0.178	(0.383)	−0.001
Geographic distribution					
North	0.087	(0.282)	0.111	(0.314)	−0.024
East	0.367	(0.483)	0.396	(0.490)	−0.029
Berlin region	0.163	(0.370)	0.148	(0.355)	0.015
South	0.208	(0.406)	0.161	(0.368)	0.047
West	0.176	(0.382)	0.185	(0.389)	−0.008
Average worker characteristics					
Avg. share of women	0.457	(0.313)	0.412	(0.301)	0.045^{*}
Avg. share of blue-collar worker	0.101	(0.184)	0.130	(0.199)	-0.028^{*}
Avg. share of part-time worker	0.199	(0.215)	0.187	(0.208)	0.012
Avg. share of apprentices	0.066	(0.099)	0.065	(0.091)	0.001
Mean age	42.971	(5.327)	42.973	(4.492)	−0.002
Mean age squared	1876.129	(466.369)	1867.639	(387.340)	8.490
Firms	289		298		

Notes: Table shows the summary statistics of the establishment sample that is based on assignment method *2-Years in 'Before' Periods* for the baseline year 2003. Treatment group: More than 5 and up to 10 full-time equivalent weighted employees in 2002 to 2003. Control group: More than 10 and up to 20 full-time equivalent weighted employees in 2002 to 2003. *Others* includes the industries *electricity, gas and water supply*, *hotels and restaurants*, *transport, storage and communication*, *financial intermediation*, *education*, *health and social work*, and *other community, social and personal service activities*. *North* includes the federal states Bremen, Hamburg, Lower Saxony, and Schleswig-Holstein. *East* includes Mecklenburg Western Pomerania, Saxony, Saxony-Anhalt, and Thuringia. *Berlin region* includes Berlin and Brandenburg. *South* includes Baden-Wuerttemberg, Bavaria, and Hesse and *West* includes North Rhine-Westphalia, Rhineland Palatinate, and Saarland. Due to reasons of data protection, a further industrial and geographic breakdown is not feasible. Asterisks denote significance of t-test for mean equality between treatment and control group. Significance levels: * 10%, ** 5%, and *** 1%. Data source: LIAB QM2.

treated firms in the Berlin region and an underrepresentation in the North and East, but differences are again statistically insignificant. Next, there are some noteworthy differences in the firm's average worker characteristics. Namely, the average share of women is 4.5 percentage points higher in the treated firms and the average share of blue-collar workers is 2.8 percentage points lower, yet, both differences are significant only at a 10% level. Lastly, Appendix Table B.2 summarizes the differences in means and respective tests for mean

Figure 3.1.: Firm Size Distribution Before and After Reform



Notes: Figure shows histograms of full-time equivalent weighted (FTE) establishment size, i.e., FTE workers per establishment, in the before (panel A) and after (panel B) periods of the 2004 PADA reform. Vertical lines indicate the minimum threshold determining coverage by the PADA before (panel A) and after (panel B) 2004. To the right dismissal protection is less strict. The sample consists of establishments that are always present in the data between 2000 and 2007. Data source: LIAB QM2.

equality for all four assignment methods used in this study. The first column recapitulates column 5 in Table 3.3. Columns 2 to 4 correspond to the other three assignment methods described in section 3.4.2. Reassuringly, firms in the treatment and control group are very similar in terms of their observable firm-level characteristics in all four cases.

3.4.5. Threshold Effects

One threat to the validity of the analysis lies in the violation of SUTVA due to the presence of spillover effects. In the present setting, the threshold change might indirectly influence the behavior of firms in the control group, that is firms above the new threshold. For instance, these firms may strategically reduce their size below the new threshold such that remaining workers lose their protection from the PADA and costs for further adjustments diminish. To provide some descriptive evidence for the absence of threshold effects, I will look at the firm size distribution before and after the reform and, in addition, apply a more formal test for threshold effects following an approach by Bauer et al. (2007) and Schivardi and Torrini (2008).

The histograms in Figure 3.1 show the distribution of firms by FTE firm size before (panel A) and after (panel B) the PADA reform for firms with sizes ranging between 1 to 25 FTE employees. The sample is based on firms that are always present during the sample periods 2000 to 2007. Before the reform, the threshold was at five FTE employees (left panel) and after the reform, it is at ten FTE employees (right panel). As expected, small firms constitute the vast majority in the sample and the density decreases with size. More importantly, in both cases, there are no noticeable discontinuities in the distribution of firms near the thresholds. Thus, the figure suggests that firms do not behave strategically by clustering around the thresholds. As a more formal test for threshold effects, I follow Bauer et al. (2007) and

Table 3.4.: Estimation Results for Probability of Downsizing After 2004 PADA Reform

	OLS (1)	Probit (2)	Logit (3)
$\mathbb{1}\left[FTE_{it-1} \in (10,11]\right]$	−0.011	−0.011	−0.011
	(0.045)	(0.044)	(0.044)
$\mathbb{1}\left[FTE_{it-1} \in (11,12]\right]$	0.034	0.033	0.033
	(0.046)	(0.045)	(0.045)
$\mathbb{1}\left[FTE_{it-1} \in (12,13]\right]$	0.003	0.004	0.003
	(0.051)	(0.050)	(0.050)
Firms	763	763	763
Observations	3,073	3,073	3,073

Notes: Results from OLS, Probit, and Logit estimates of empirical model (3.3) for the sample periods 2004 to 2007, with a dummy variable that takes the value 1 if an establishment downsizes between periods $t-1$ and t and 0 otherwise as the outcome and establishment-year as the unit of observation. $\mathbb{1}[...]$ refers to estimates of the threshold effect (λ_k for $k = 1,2,3$) for the respective indicator function. For Probit and Logit, table reports average partial effects. The sample consists of firms in the size range of five to 20 full-time equivalent weighted employees that are always present in the data between 2000 and 2007. Year dummies included. Standard errors are clustered at the establishment level. Significance levels: * 10%, ** 5%, and *** 1%. Data source: LIAB QM2 9310.

Schivardi and Torrini (2008) and estimate a model of the form:

$$Y_{it}^{down} = \alpha + \sum_{j=1}^{4} \beta_j FTE_{it-1}^{j} + \sum_{k=1}^{K} \lambda_k \mathbb{1}\left[FTE_{it-1} \in (9+k, 10+k]\right] + \delta_t + \varepsilon_{it}, \tag{3.3}$$

where Y_{it}^{down} is a dummy variable that takes the value 1 if a firm downsizes between periods $t-1$ and t, i.e., $\Delta FTE_{it-1,t} < 0$, and 0 otherwise. FTE_{it-1}^{j} is the j^{th} polynomial of the FTE firm size in period $t-1$, $\mathbb{1}\left[FTE_{it-1} \in (9+k, 10+k]\right]$ is a set of K dummy variables defined by indicator functions that take the value 1 if the FTE firm size in period $t-1$ lies in the interval $(9+k, 10+k]$ for integers $k = 1, \ldots, K$, and 0 otherwise. δ_t captures yearly effects that are assumed to be common to all firms and ε_{it} is an idiosyncratic error term. The model's underlying assumption is that there is a smooth relation between the probability of downsizing and the fourth degree polynomial in FTE firm size. Moreover, the model requires firms just above the new threshold to behave exactly like firms further away from the threshold in absence of threshold constraints (Bauer et al. 2007). Given these assumptions hold, the parameter of interest λ_k for $k = 1, ...K$ should capture potential threshold effects. That is to say, if firms just above the new threshold indeed strategically reduce their firm size, λ_k should be statistically significantly larger than zero, in particular for small k.

I estimate equation (3.3) by OLS, Probit, and Logit for the sample periods after the reform (2004 to 2007). Following Schivardi and Torrini (2008), I use $K = 3$, that is I test for threshold effects up to the FTE firm size of 13. The sample consists of firms that are always present during the sample periods 2000 to 2007 and employ more than five and up to 20 FTE employees. Since the outcome variable is binary and the model is not saturated, the use of

OLS may introduce problems. In particular, predicted probabilities of this linear probability model (LPM) are unbounded (Angrist and Pischke 2009). Reassuringly, I obtain very similar results when looking at the average partial effects from Probit and Logit models. Table 3.4 reports the results. Column 1 corresponds to OLS, column 2 to Probit, and column 3 to Logit estimates. For all three methods, none of the estimates is statistically significant. I consider this as evidence in favor of the view that there are no threshold effects for firms just above the new threshold that may threaten my identification strategy.[23]

3.5. Results

To study the causal effect of the PADA reform on different labor market outcomes, I start by looking at estimates for the overall hiring, separation, job flow, and churning rate. Thereby, I discuss my results against the backdrop of findings in previous studies and attempt to shed light on potential contractions. Next, I explore potential heterogeneity in treatment effects by analyzing the effect of the reform on gender- and age-specific worker turnover rates as well as on rates by union status and East-West divide of firms. Finally, I examine other margins of adjustment by looking at effects on firm mean wages and the use of temporary employment.

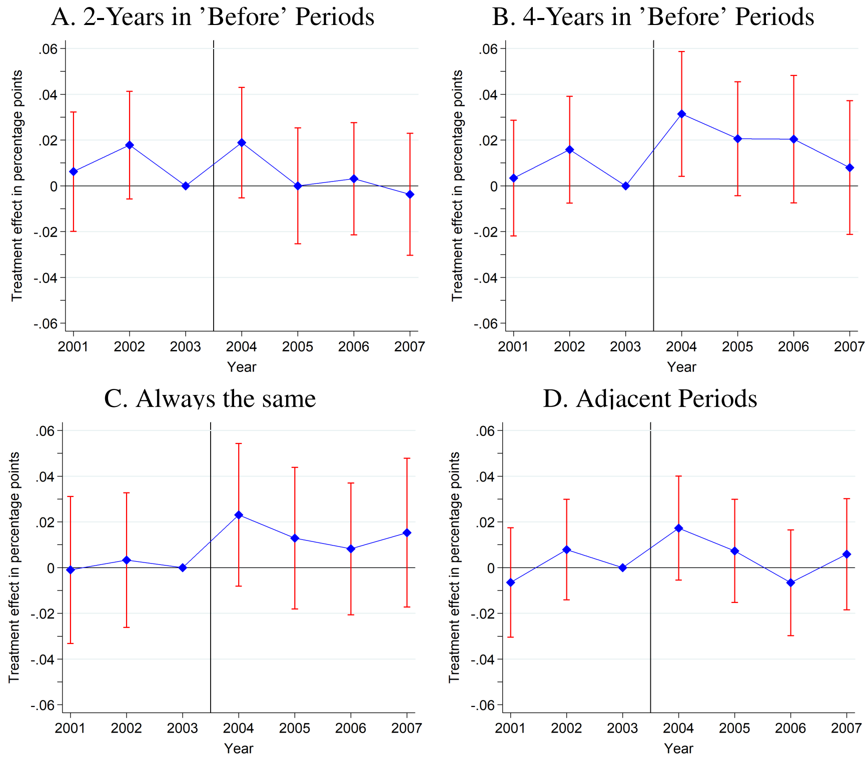
3.5.1. Worker Turnover

Figure 3.2 shows yearly DiD estimates of the hiring rate from model (3.2) two periods before and four periods after the PADA reform.[24] The vertical bands correspond to 95% confidence intervals. Each of the four panels coincides with one of the assignment methods described in section 3.4.2. Estimates in the pre-treatment periods are statistically insignificant in all cases which I regard as support for the common time trend assumption underlying the identification strategy. Moreover, point estimates for the after periods tend to be positive. At first glance, this is in line with the theoretical predictions that hires should increase in response to softened dismissal protection. However, except for the 2004 coefficient from assignment method

[23]To test for the robustness of the results, I restrict the sample to firms with up to 15 FTE employees and extend the sample to firms with up to 25 FTE employees, include more or less indicator functions (i.e., $K \in 2,4,5$), estimate the model separately for each unit interval by including in each run only one indicator function for $k = 1,...5$, and include additional controls (i.e., industry and federal state dummies and/or average share of blue-collar workers, average share of part-time workers, average share of apprentices, average share of women, average age of employees and its square). Furthermore, I exploit the time variation in the threshold and allow the coefficients of the indicator functions to differ before and after the reform by augmenting model (3.3) with interaction terms between the indicator functions and a dummy variable that takes the value 1 in the periods after the reform and 0 otherwise. Formally, I add $\sum_{k=1}^{K} \gamma_k \{\mathbb{1}\,[FTE_{it-1} \in (9+k, 10+k]] \times Post_t\}$ to equation (3.3). I re-estimate the augmented model for the sample periods 2001 to 2007. In the model, λ_k close to zero and γ_k statistically significantly larger than zero for some k would suggest a threshold effect after the reform. However, none of the tests provides evidence for a robust significant threshold effect. Finally, I conduct an analogous analysis for the growth probability of firms just below the new threshold using the following model: $Y_{it}^{growth} = \alpha + \sum_{j=1}^{4} \beta_j FTE_{it-1}^{j} + \sum_{k=1}^{K} \lambda_k \mathbb{1}\,[FTE_{it-1} \in (10-k, 11-k]] + \delta_t + \varepsilon_{it}$, where Y_{it}^{growth} is a dummy variable that takes the value 1 if a FTE firm size increases between periods $t-1$ and t, i.e., $\Delta FTE_{it-1,t} > 0$, and 0 otherwise. Once more, I am not able to identify any robust significant threshold effects for firms just below the new threshold. All results are available upon request.

[24]The yearly DiD estimates correspond to the interaction terms in model (3.2).

Figure 3.2.: Dynamic Difference-in-Differences Results: Hiring Rate



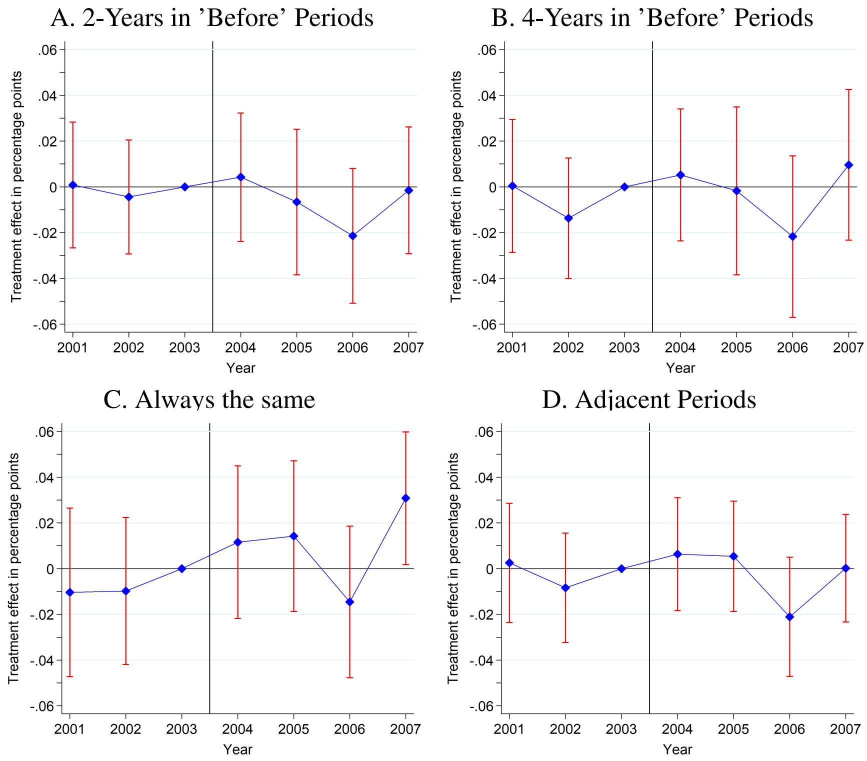
Notes: Figure shows coefficients and their 95% confidence interval of yearly difference-in-differences estimates (ρ_τ for $\tau = 2001, ..., 2007$, $\tau \neq 2003$) as given by empirical model (3.2) for the sample periods 2001 to 2007, with the hiring rate as the outcome and establishment-year as the unit of observation. The year 2003 is the baseline period. The vertical line represents the timing of the PADA reform. Each panel presents separate estimates for one of the four assignment methods described in section 3.4.2. Standard errors are clustered at the establishment level. Data source: LIAB QM2 9310.

(2) (see panel B), none of the coefficients is statistically significant at the 5% level. Panel A in Table 3.5 on page 61 summarizes the results of the DiD estimates by displaying the total (DiD 2004-07) and pre-treatment (DiD 2001-02) effect.[25] The statistically insignificant pre-treatment DiD estimates can again be regarded as support for the common time trend. Furthermore, as the evolution of the yearly effects already suggested, I do not find convincing evidence for a positive effect of the PADA reform on firm hiring rates. Only the total effect for assignment method (2) of 2.0 percentage points is statistically significant at a 10% level. The results are in conformity with Bauer et al. (2007) who also do not find robust evidence for a causal relation between similar threshold reforms in 1996 and 1999 and hiring rates. Yet, my results are inconsistent with findings of Bauernschuster (2013) who concludes that the 2004 PADA reform had a positive effect on firms' hiring in the years 2004 and 2005. I explore this contradiction in more detail in section 3.5.2.

Figures 3.3, 3.4, and 3.5 display dynamic patterns of the treatment effect on the separation, job flow, and churning rates. Panel B, C, and D in Table 3.5 further summarize the total

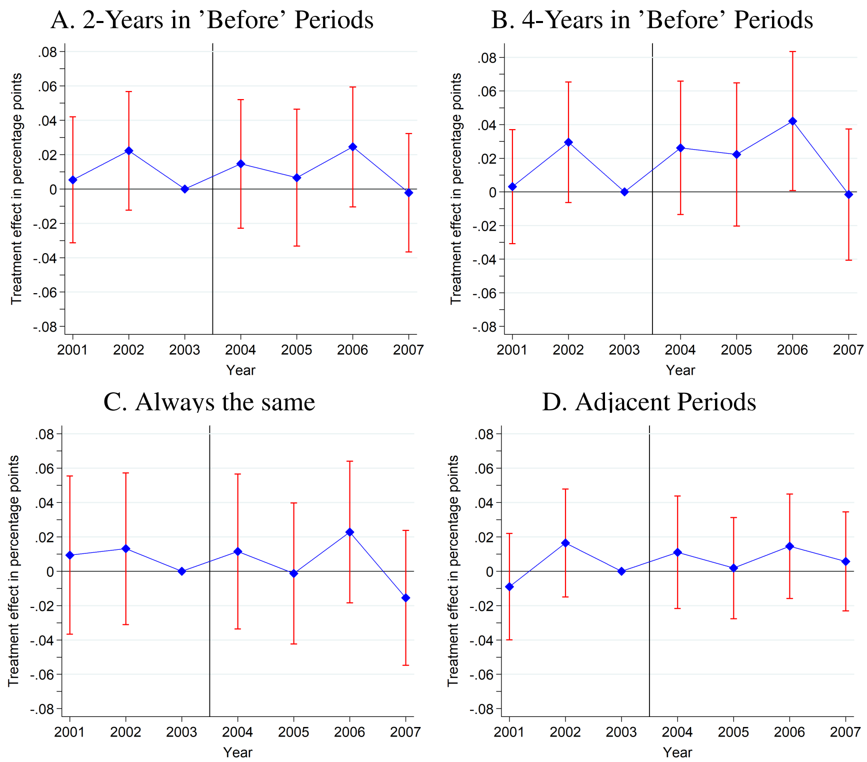
[25] The DiD estimates for total effect corresponds to the interaction term in model (3.1) and is technically an average of the annual effects of the post-treatment periods. Respectively, the DiD estimate for the pre-treatment period is technically an average of the yearly estimates for 2001 and 2002.

Figure 3.3.: Dynamic Difference-in-Differences Results: Separation Rate



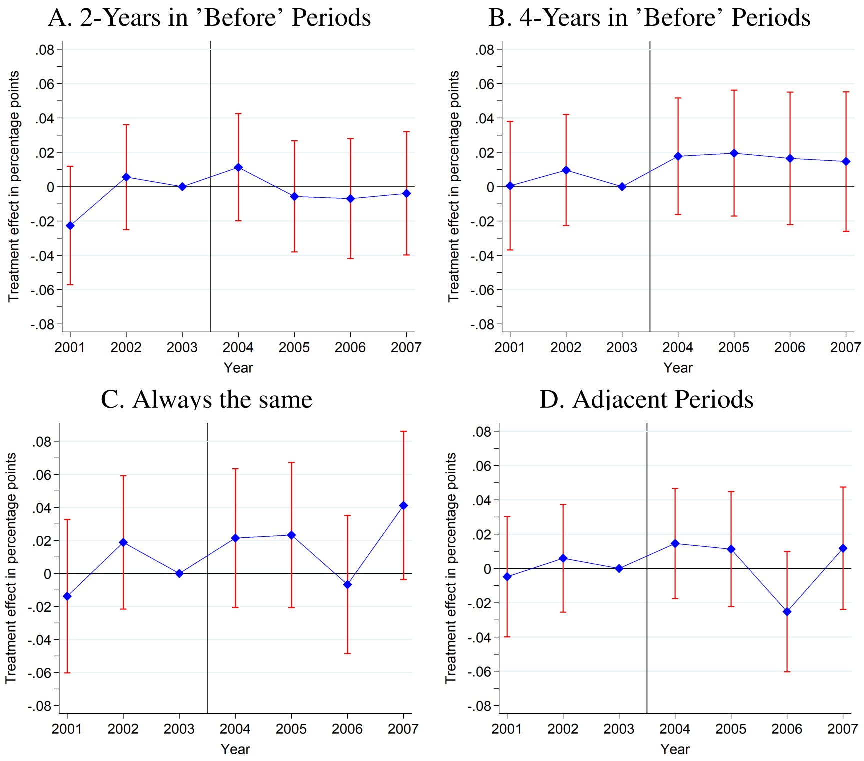
Notes: See notes to Figure 3.2. The outcome is the separation rate. Data source: LIAB QM2 9310.

Figure 3.4.: Dynamic Difference-in-Differences Results: Job Flow Rate



Notes: See notes to Figure 3.2. The outcome is the job flow rate. Data source: LIAB QM2 9310.

Figure 3.5.: Dynamic Difference-in-Differences Results: Churning Rate



Notes: See notes to Figure 3.2. The outcome is the churning rate. Data source: LIAB QM2 9310.

and pre-treatment effect for these outcome variables. Reassuringly, for none of assignment methods and outcome variables do I obtain statistically significant estimates in periods before the reform which reinforces the common time trend assumption for these outcome variables. Based on economic theory, I would expect the separation rate to increase in response to reduced dismissal costs at some point. However, turning to the post-reform periods, the effect on the separation rate is uniformly statistically insignificant. In the short run, this is to some extent not surprising since the relaxed dismissal protection only applies to new hires in the treated firms. Consequently, dismissal costs are not instantaneously reduced and I would only expect longer run effects. However, also in the long term only the 2007 coefficient for the assignment method (3) is statistically significant (see panel C in Figure 3.3) which I do not consider as robust evidence for an increase in the separation rate in later periods. In summary, estimates for the total effect on the separation rate are mostly negative, in absolute terms not larger than 1.0 percentage point, and in all cases statistically insignificant (see Table 3.5). Again, the results are consistent with Bauer et al. (2007) who also do not observe any significant short-run effects on separation rates in 1996 and 1999.

As the hiring rate exceeds the separation rate in almost all post-treatment periods, the predominantly positive estimates for the job flow rate do not come at a surprise. Yet, the coefficients remain statistically insignificant and thus cannot be interpreted as support for a positive net employment effect of the reform (see Figure 3.4 and panel C in Table 3.5). Similarly, Bauer et al. (2007) do not find any evidence for a significant relationship between

Table 3.5.: Difference-in-Differences Results: Overall Turnover Rates

		2-Years in 'Before' Periods (1)	4-Years in 'Before' Periods (2)	Always the same (3)	Adjacent Periods (4)
Panel A: Hiring rate	DiD 2004-07	0.005	0.020*	0.015	0.007
		(0.010)	(0.010)	(0.013)	(0.010)
	DiD 2001-02	0.012	0.010	0.001	0.002
		(0.010)	(0.010)	(0.013)	(0.010)
Panel B: Separation rate	DiD 2004-07	−0.006	−0.002	0.011	−0.001
		(0.011)	(0.013)	(0.013)	(0.010)
	DiD 2001-02	−0.002	−0.007	−0.010	−0.003
		(0.011)	(0.012)	(0.015)	(0.011)
Panel C: Job flow rate	DiD 2004-07	0.011	0.022	0.004	0.009
		(0.013)	(0.015)	(0.017)	(0.013)
	DiD 2001-02	0.014	0.016	0.011	0.005
		(0.014)	(0.015)	(0.020)	(0.014)
Panel D: Churning rate	DiD 2004-07	−0.001	0.017	0.020	0.004
		(0.014)	(0.015)	(0.017)	(0.014)
	DiD 2001-02	−0.009	0.005	0.003	0.001
		(0.014)	(0.015)	(0.019)	(0.015)
Firms		587	422	247	957
Observations		4109	2954	1729	4304

Notes: Table shows coefficients of difference-in-differences (DiD) estimates (ρ) as given by empirical model (3.1) for the sample periods 2001 to 2007, with the indicated variable as the outcome and establishment-year as the unit of observation. The years after DiD indicate the pooled periods under consideration. The year 2003 is the baseline period. Each column presents separate estimates for one of the four assignment methods described in section 3.4.2. Standard errors are clustered at the establishment level. Significance levels: * 10%, ** 5%, and *** 1%. Data source: LIAB QM2 9310.

relaxed dismissal protection and job flows for previous threshold reforms. Moreover, the absence of an effect is not contradictory to economic theory which is also inconclusive in terms of the relationship between changes in dismissal costs and net employment.

Finally, while theory suggests an increase in churning in response to a reduction in dismissal costs, the DiD estimates for the churning rate do not underpin this prediction. Although estimates for the total effect are mostly positive and go in absolute terms up to 2.0 percentage points, the coefficients are statistically not significant at a reasonable level (see Figure 3.5 and panel D in Table 3.5).

Summing up, I do not find evidence that the reform of the PADA in 2004 had a causal effect on overall worker turnover rates of treated firms.

3.5.2. Consolidation with Previous Findings

To consolidate my statistically insignificant estimates on the hiring rate with the positive effect on this outcome detected by Bauernschuster (2013), I apply the four assignment methods described in section 3.4.2 to the IAB Establishment Panel (IAB EP) after I preprocessed the data following Bauernschuster (2013).[26] Thereby, I extend Bauernschuster (2013)'s analysis for the years 2004 and 2005 by the sample periods 2006 and 2007. Since the IAB EP refers to hires within the first six months of each year, I conduct a comparable analysis based on the LIAB QM2 that distinguishes between hires in the first (June to December in year $t-1$) and the second (January to June in year t) half of the twelve month observation period. More precisely, following the definition of flow rates in section 3.4.3, I compute half-year hiring rates for each period by replacing annual hires with six months hires in the numerator. This is possible since I know the exact entry date of each worker in a firm. Eventually, I estimate the effect on the half-year hiring rates (January to June) based on models (3.1) and (3.2) for the sample periods 2001 to 2007 for both the IAB EP and LIAB QM2. To obtain separate estimates for a short- and medium-term effect that can be compared to results in Bauernschuster (2013), I re-estimate equation (3.1) once more whereby I substitute $\rho_S(D_{i(t)} \times Short_t) + \rho_M(D_{i(t)} \times Medium_t)$ for $\rho_1(D_{i(t)} \times Post_t)$. $Short_t$ is a dummy variable that takes the value 1 in the years 2004 and 2005 and 0 otherwise. Likewise, $Medium_t$ is a dummy variable that takes the value 1 in the years 2006 and 2007 and 0 otherwise.

The left side of Table 3.6 shows the extension of Bauernschuster (2013)'s estimates based on the IAB EP. Column 2 corresponds to the assignment method used in his main analysis (*Table 1, p. 301*). The results are comforting in that I obtain a statistically significant short-term effect (DiD 2004-05) of 1.5 percentage points, while his original estimates for the years 2004 and 2005 range between 1.3 and 2.1 percentage points.[27] In contrast, estimates from assignment methods (1), (3), and (4) for the short-term effect are smaller in absolute terms and statistically insignificant. The picture is similar for the total effect (DiD 2004-07) where only assignment method (2) leads to a statistically significant DiD coefficient. Taken together, the results based on the IAB EP already cast some doubt on Bauernschuster (2013)'s finding of a causal short-term effect on the hiring rate.

The right side of Table 3.6 depicts the analogous estimates based on the LIAB QM2. Interestingly, looking at column 6 which represents assignment method (2), the coefficient of the total effect from the LIAB QM2 of 1.5 percentage points (significant at the 10% level) is very close to the respective estimate from the IAB EP of 1.6 percentage points (significant at the 5% level), despite the fact that the former is only driven by a significant short-term effect while the latter is driven by both a statistically significant short- and medium term effect.[28] Even more important, results for the total effect from the other assignment methods

[26] I thank Stefan Bauernschuster for making available the statistical programs for the data processing of the IAB EP.

[27] The slightly lower significance in my estimates could potentially be explained by the smaller sample as the extension of the sample periods require firms from the initial sample to also be present in the periods 2006 and 2007.

[28] There are a number of reasons that may explain differences in the estimates from the two data sources. To name a few, the LIAB QM2 constitutes administrative data whereas the IAB EP is obtained from surveys, the hires in the LIAB QM2 are determined as point-in-time comparisons whereas hires in the IAB EP refer to all hires, and the LIAB QM2 allows for a more precise full-time equivalent weighting scheme as opposed to global part-time weights used for the IAB EP.

Table 3.6.: Difference-in-Differences Results: IAB EP vs. LIAB QM2

		IAB EP				LIAB QM2			
		2-Years in 'Before' Periods	4-Years in 'Before' Periods	Always the same	Adjacent Periods	2-Years in 'Before' Periods	4-Years in 'Before' Periods	Always the same	Adjacent Periods
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Six month hiring rate (Jan. to June)	DiD 2004-05	0.013	0.015*	0.008	−0.007	0.014*	0.023***	0.017	0.016**
		(0.010)	(0.008)	(0.009)	(0.007)	(0.008)	(0.008)	(0.011)	(0.008)
	DiD 2006-07	0.002	0.017**	0.005	−0.005	0.001	0.008	0.007	0.005
		(0.009)	(0.008)	(0.010)	(0.007)	(0.008)	(0.009)	(0.011)	(0.008)
	DiD 2004-07	0.007	0.016**	0.006	−0.006	0.008	0.015*	0.001	0.011
		(0.009)	(0.007)	(0.009)	(0.007)	(0.007)	(0.008)	(0.010)	(0.007)
	DiD 2001-02	0.007	0.008	0.006	0.001	0.011	0.010	0.004	0.004
		(0.010)	(0.007)	(0.010)	(0.008)	(0.008)	(0.008)	(0.011)	(0.008)
Firms		558	383	349	1184	587	422	247	957
Observations		3906	2681	2443	4304	4109	2954	1729	4304

Notes: Table shows coefficients of difference-in-differences (DiD) estimates (ρ) as given by empirical model (3.1) for the sample periods 2001 to 2007, with the indicated variable as the outcome and establishment-year as the unit of observation. The years after DiD indicate the pooled periods under consideration. The year 2003 is the baseline period. Each column presents separate estimates for one of the four assignment methods described in section 3.4.2. Columns 1 to 4 refer to estimates for the six month hiring rate as the outcome based on the IAB EP and columns 5 to 8 to estimates for the analogous outcome based on the LIAB QM2. Standard errors are clustered at the establishment level. Significance levels: * 10%, ** 5%, and *** 1%. See text for additional details. Data sources: LIAB QM2 9310 and IAB Establishment Panel.

in columns 5, 7, and 8 are again statistically insignificant, although the short-term effect remains statistically significant in method (1) and (4).[29]

To sum up, this supplementary analysis highlights the dependence of previous findings from Bauernschuster (2013) on the assignment methods to the treatment and control group. As discussed in section 3.4.2, there is no obvious justification for prioritizing one assignment method over the other and therefore the findings based on both data sources do not allow one to draw a clear-cut conclusion on the causal effect of the relaxed dismissal protection on firm hiring rates. If anything, the findings suggest a short-term effect on the six months hiring rate which does not persist in the medium-term.

3.5.3. Heterogeneity

Although I do not find an effect on the firm overall worker turnover rates, the PADA reform might have differential impacts on subgroups of workers or firms. To this end, I first exploit worker-level information on gender and age and construct an array of subgroup-specific turnover rates for which I re-estimate model (3.1). In analogy to the overall rates, the men's (women's) hiring rate is determined by dividing the FTE hires of men (women) in period t by the average of the FTE firm size in periods $t-1$ and t. Age-specific rates are constructed in the same way based on two age groups, i.e., young workers (aged < 35 years) and older workers (aged ≥ 35 years).[30] As in Centeno and Novo (2014), I use the cut-off age at 35 so that the group of young workers predominantly comprises employees that are still establishing themselves in the labor market and have a reasonable likelihood to pursue further education. In addition, I assess differential effects by union status and the East-West divide between firms. To this end, I split the establishment samples by the respective observable firm-level characteristic and re-estimate model (3.1) for each subsample.

From a theoretical point of view, differential effects of the PADA reform on worker subgroups could be explained by differences in labor supply elasticities (Bertola et al. 2007; Centeno and Novo 2014). The labor supply of women and young workers is generally more elastic relative to prime-aged men given that women are more likely to decide between home production and market work and young workers between market work and education (Blundell and MaCurdy 1999; Bertola et al. 2007; Evers et al. 2008).[31] Therefore, a less

[29] In unreported estimations, I also look at the dynamic pattern of the effects. Since the reform became effective in the beginning of 2004 and the outcome flow variables of the LIAB QM2 are point-in-time comparisons of the June 30 of consecutive years, the first treatment period 2004 is only subject to the reform in the last six month (January to June 2004). Hence, I would expect an effect on the hiring rate only in the second half of this observation period. Reassuringly, estimates reveal that positive and statistically significant effects on the hiring rate in 2004 for the different assignment methods are indeed driven by the second half of the observation period. Coefficients range between 2.0 and 2.7 percentage points and are significance at least at 10% levels. In contrast, estimates for the first six months (July to December 2003) vary between -0.1 and 0.5 percentage points and are uniformly statistically insignificant.

[30] By construction, the sums of subgroup-specific hiring, separation, and job flow rates for men/women and young/older workers are equal to the respective overall flow rate. Consequently, the sums of the subgroup-specific total DiD coefficients are equal to the respective total DiD coefficients from section 3.5.1.

[31] For example, Prifti and Vuri (2013) find that a strengthening of the dismissal protection in Italy in 1990 had a positive and sizable causal effect on women's fertility decisions.

stringent dismissal protection should have more pronounced employment effects on women and younger workers as employment rather than wage is the more important margin of adjustment (relative to men) (Bertola et al. 2007). Consequently, worker turnover rates of women and younger workers are more likely to be affected by the reform than rates of men and older workers.

Gender: Table 3.7 summarizes the results for the gender-specific turnover rates. The left side shows the results for the men's rates, the right side the analogous estimates for the women's rates. DiD estimates for the hiring, separation, and job flow rates in the pre-treatment periods are statistically insignificant which supports the common time trend assumption for these subgroup-specific outcome variables. In contrast, the women's churning rate exhibits differential trends in the pre-reform periods for two of the four assignment methods. Consequently, I focus on the gender-specific hiring, separation, and job flow rates and disregard the results for the churning rate. Looking at the post-reform periods, I find evidence for a positive effect of the PADA reform on the women's hiring rate (see panel A in Table 3.7). Depending on the assignment method, the rate increased by 1.3 to 2.1 percentage points with estimates being statistically significant at 5% and 1% levels. In comparison, estimates for the men's hiring rate are in absolute terms not larger than 0.8 percentage points and uniformly statistically insignificant. The significant increase of the women's hiring rate of 1.3 to 2.1 percentage points is sizeable. Relative to the women's mean rate of 4.3% in the baseline year 2003, it is a 30 to 48% increase. Taking into account that the estimates for the gender-specific separation rates are very small, and for both men and women statistically insignificant (see panel B in Table 3.7), the DiD coefficients for the women's job flow rates are, as expected, positive (see panel C in Table 3.7). Moreover, given all assignment methods yield positive estimates that range between 1.5 and 2.3 percentage points of which three out of four are statistically significant at the 5% level, the results further suggest a positive causal effect of the PADA reform on the women's job flow rate.

Age groups: Contrary to the gender-specific findings which are consistent with the initial hypothesis on labor supply elasticities, the results do not support the prediction of a differential effect for younger workers. As panel A to D in Table 3.8 show, the age-specific estimates are almost exclusively statistically insignificant.

Union status: To test for heterogeneity across the union status of firms, I divide the sample into two groups, one with firms that are covered by either a firm-level or an industry-wide union agreement and one with firms without any union coverage. As union membership is generally associated with more rigid wages, I expect worker turnover rates of firms in the group within unionized firms to be more susceptible to a reduction in dismissal protection as these firms are more likely to adjust along the employment margin. Panel A in Table 3.9 depicts the results on the hiring rate by firm unionization status. The total effect ranges from 1.7 to 3.1 percentage points, but is only statistically significant in two cases. I do not regard this as convincing evidence for a positive effect on the hiring rate of unionized firms.

Table 3.7.: Difference-in-Differences Results: Turnover Rates by Gender

		Men				Women			
		2-Years in 'Before' Periods	4-Years in 'Before' Periods	Always the same	Adjacent Periods	2-Years in 'Before' Periods	4-Years in 'Before' Periods	Always the same	Adjacent Periods
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Hiring rate	DiD 2004-07	−0.008	0.002	−0.006	−0.006	0.013**	0.018***	0.021***	0.014**
		(0.008)	(0.008)	(0.010)	(0.008)	(0.005)	(0.006)	(0.008)	(0.006)
	DiD 2001-02	0.007	0.003	−0.008	−0.004	0.005	0.007	0.010	0.006
		(0.008)	(0.008)	(0.010)	(0.008)	(0.007)	(0.007)	(0.009)	(0.006)
Panel B: Separation rate	DiD 2004-07	0.000	0.003	0.005	0.000	−0.006	−0.005	0.005	−0.002
		(0.008)	(0.010)	(0.010)	(0.008)	(0.007)	(0.008)	(0.008)	(0.006)
	DiD 2001-02	0.000	−0.007	−0.014	−0.004	−0.001	0.000	0.004	0.001
		(0.008)	(0.009)	(0.011)	(0.008)	(0.007)	(0.008)	(0.010)	(0.007)
Panel C: Job flow rate	DiD 2004-07	−0.008	−0.001	−0.012	−0.007	0.019**	0.023**	0.016	0.015**
		(0.011)	(0.012)	(0.014)	(0.011)	(0.008)	(0.009)	(0.011)	(0.008)
	DiD 2001-02	0.007	0.010	0.006	0.000	0.007	0.007	0.005	0.005
		(0.011)	(0.012)	(0.015)	(0.011)	(0.009)	(0.010)	(0.013)	(0.009)
Panel D: Churning rate	DiD 2004-07	−0.001	0.006	0.009	0.003	0.011	0.022**	0.029***	0.012
		(0.010)	(0.011)	(0.012)	(0.010)	(0.008)	(0.008)	(0.010)	(0.008)
	DiD 2001-02	−0.006	−0.008	−0.013	−0.009	0.006	0.019*	0.026**	0.014
		(0.010)	(0.010)	(0.012)	(0.010)	(0.010)	(0.010)	(0.013)	(0.010)
Firms		587	422	247	957	587	422	247	957
Observations		4109	2954	1729	4304	4109	2954	1729	4304

Notes: Table shows coefficients of difference-in-differences (DiD) estimates (ρ) as given by empirical model (3.1) for the sample periods 2001 to 2007, with the indicated variable as the outcome and establishment-year as the unit of observation. The years after DiD indicate the pooled periods under consideration. The year 2003 is the baseline period. Each column presents separate estimates for one of the four assignment methods described in section 3.4.2. Columns 1 to 4 refer to estimates for the gender-specific worker turnover rates of men and columns 5 to 8 to estimates for the gender-specific worker turnover rates of women. Standard errors are clustered at the establishment level. Significance levels: * 10%, ** 5%, and *** 1%. Data source: LIAB QM2 9310.

Table 3.8.: Difference-in-Differences Results: Turnover Rates by Age Groups

		Young (workers aged $<$ 35)				Older (workers aged $\geq$ 35)			
		2-Years in 'Before' Periods (1)	4-Years in 'Before' Periods (2)	Always the same (3)	Adjacent Periods (4)	2-Years in 'Before' Periods (5)	4-Years in 'Before' Periods (6)	Always the same (7)	Adjacent Periods (8)
Panel A: Hiring rate	DiD 2004-07	0.004	0.009	0.012	0.003	0.000	0.011	0.002	0.004
		(0.006)	(0.007)	(0.008)	(0.006)	(0.008)	(0.008)	(0.009)	(0.007)
	DiD 2001-02	0.004	0.005	0.006	0.002	0.008	0.005	−0.005	−0.001
		(0.006)	(0.006)	(0.008)	(0.006)	(0.008)	(0.009)	(0.011)	(0.008)
Panel B: Separation rate	DiD 2004-07	−0.001	0.007	0.013	0.003	−0.005	−0.010	−0.002	−0.005
		(0.006)	(0.007)	(0.008)	(0.006)	(0.009)	(0.010)	(0.011)	(0.008)
	DiD 2001-02	−0.003	−0.001	−0.005	−0.002	0.001	−0.005	−0.005	−0.001
		(0.007)	(0.008)	(0.010)	(0.007)	(0.009)	(0.009)	(0.012)	(0.009)
Panel C: Job flow rate	DiD 2004-07	0.006	0.002	0.000	0.000	0.005	0.020*	0.005	0.009
		(0.008)	(0.009)	(0.011)	(0.008)	(0.011)	(0.012)	(0.014)	(0.010)
	DiD 2001-02	0.007	0.006	0.012	0.004	0.007	0.010	0.000	0.001
		(0.008)	(0.010)	(0.013)	(0.009)	(0.011)	(0.012)	(0.015)	(0.011)
Panel D: Churning rate	DiD 2004-07	−0.001	0.009	0.018**	0.001	−0.007	0.000	0.003	−0.002
		(0.007)	(0.008)	(0.009)	(0.008)	(0.010)	(0.010)	(0.013)	(0.010)
	DiD 2001-02	−0.012	−0.006	−0.001	−0.006	−0.002	0.000	−0.007	−0.004
		(0.008)	(0.008)	(0.009)	(0.008)	(0.011)	(0.011)	(0.013)	(0.011)
Firms		587	422	247	957	587	422	247	957
Observations		4109	2954	1729	4304	4109	2954	1729	4304

Notes: Table shows coefficients of difference-in-differences (DiD) estimates (ρ) as given by empirical model (3.1) for the sample periods 2001 to 2007, with the indicated variable as the outcome and establishment-year as the unit of observation. The years after DiD indicate the pooled periods under consideration. The year 2003 is the baseline period. Each column presents separate estimates for one of the four assignment methods described in section 3.4.2. Columns 1 to 4 refer to estimates for the age-specific worker turnover rates of young workers (aged $<$ 35) and columns 5 to 8 to estimates for the age-specific worker turnover rates of older workers (aged $\geq$ 35). Standard errors are clustered at the establishment level. Significance levels: * 10%, ** 5%, and *** 1%. Data source: LIAB QM2 9310.

Table 3.9.: Difference-in-Differences Results: Turnover Rates by Union Status of Establishments

		Union agreement				No union agreement			
		2-Years in 'Before' Periods	4-Years in 'Before' Periods	Always the same	Adjacent Periods	2-Years in 'Before' Periods	4-Years in 'Before' Periods	Always the same	Adjacent Periods
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Hiring rate	DiD 2004-07	0.025**	0.031**	0.027	0.017	−0.012	0.010	0.005	−0.002
		(0.013)	(0.014)	(0.018)	(0.013)	(0.014)	(0.015)	(0.018)	(0.014)
	DiD 2001-02	0.021	0.015	0.023	0.015	0.005	0.005	−0.017	−0.010
		(0.014)	(0.014)	(0.018)	(0.013)	(0.015)	(0.015)	(0.019)	(0.015)
Panel B: Separation rate	DiD 2004-07	0.002	0.008	0.014	0.005	−0.014	−0.012	0.007	−0.008
		(0.016)	(0.019)	(0.019)	(0.014)	(0.015)	(0.017)	(0.019)	(0.014)
	DiD 2001-02	0.001	−0.002	−0.011	−0.007	−0.005	−0.011	−0.010	0.000
		(0.016)	(0.018)	(0.022)	(0.016)	(0.015)	(0.017)	(0.021)	(0.016)
Panel C: Job flow rate	DiD 2004-07	0.023	0.022	0.013	0.013	0.001	0.022	−0.002	0.006
		(0.018)	(0.019)	(0.024)	(0.017)	(0.018)	(0.022)	(0.025)	(0.018)
	DiD 2001-02	0.019	0.017	0.034	0.023	0.009	0.016	−0.007	−0.010
		(0.020)	(0.022)	(0.029)	(0.020)	(0.020)	(0.021)	(0.027)	(0.019)
Panel D: Churning rate	DiD 2004-07	0.009	0.033	0.043*	0.012	−0.010	0.003	0.000	−0.002
		(0.019)	(0.021)	(0.026)	(0.019)	(0.019)	(0.020)	(0.024)	(0.020)
	DiD 2001-02	−0.004	0.007	0.016	−0.005	−0.012	0.004	−0.008	0.006
		(0.018)	(0.021)	(0.028)	(0.019)	(0.021)	(0.021)	(0.025)	(0.021)
Firms		263	199	118	429	324	223	129	528
Observations		1841	1393	826	1944	2268	1561	903	2360

Notes: Table shows coefficients of difference-in-differences (DiD) estimates (ρ) as given by empirical model (3.1) for the sample periods 2001 to 2007, with the indicated variable as the outcome and establishment-year as the unit of observation. The years after DiD indicate the pooled periods under consideration. The year 2003 is the baseline period. Each column presents separate estimates for one of the four assignment methods described in section 3.4.2. Columns 1 to 4 refer to estimates for unionized establishments and columns 5 to 8 to estimates for non-unionized establishments. An establishment is considered as unionized if it is covered by either a firm-level or an industry-wide union agreement. Standard errors are clustered at the establishment level. Significance levels: * 10%, ** 5%, and *** 1%. Data source: LIAB QM2 9310.

Table 3.10.: Difference-in-Differences Results: Turnover Rates by East-West Divide of Establishments

		West Germany				East Germany			
		2-Years in 'Before' Periods	4-Years in 'Before' Periods	Always the same	Adjacent Periods	2-Years in 'Before' Periods	4-Years in 'Before' Periods	Always the same	Adjacent Periods
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Hiring rate	DiD 2004-07	−0.002	0.030**	0.022	0.007	0.013	0.012	0.010	0.008
		(0.014)	(0.014)	(0.019)	(0.013)	(0.014)	(0.015)	(0.017)	(0.014)
	DiD 2001-02	0.012	0.006	−0.012	−0.014	0.012	0.013	0.011	0.018
		(0.015)	(0.014)	(0.019)	(0.013)	(0.014)	(0.015)	(0.019)	(0.015)
Panel B: Separation rate	DiD 2004-07	−0.005	0.001	0.027*	0.007	−0.007	−0.002	−0.003	−0.011
		(0.015)	(0.017)	(0.016)	(0.014)	(0.016)	(0.018)	(0.020)	(0.015)
	DiD 2001-02	−0.002	−0.017	−0.024	−0.015	−0.002	0.004	0.002	0.008
		(0.015)	(0.017)	(0.020)	(0.015)	(0.016)	(0.018)	(0.022)	(0.016)
Panel C: Job flow rate	DiD 2004-07	0.003	0.029	−0.006	0.000	0.019	0.014	0.013	0.019
		(0.018)	(0.020)	(0.023)	(0.018)	(0.019)	(0.021)	(0.025)	(0.018)
	DiD 2001-02	0.014	0.023	0.013	0.001	0.013	0.009	0.009	0.009
		(0.020)	(0.021)	(0.028)	(0.019)	(0.020)	(0.022)	(0.028)	(0.020)
Panel D: Churning rate	DiD 2004-07	−0.002	0.021	0.026	0.015	0.000	0.014	0.014	−0.008
		(0.018)	(0.020)	(0.024)	(0.019)	(0.020)	(0.022)	(0.025)	(0.021)
	DiD 2001-02	−0.024	−0.013	−0.032	−0.017	0.008	0.023	0.031	0.020
		(0.020)	(0.020)	(0.023)	(0.020)	(0.021)	(0.022)	(0.028)	(0.021)
Firms		315	215	115	538	272	207	132	419
Observations		2205	1505	805	2342	1904	1449	924	1961

Notes: Table shows coefficients of difference-in-differences (DiD) estimates (ρ) as given by empirical model (3.1) for the sample periods 2001 to 2007, with the indicated variable as the outcome and establishment-year as the unit of observation. The years after DiD indicate the pooled periods under consideration. The year 2003 is the baseline period. Each column presents separate estimates for one of the four assignment methods described in section 3.4.2. Columns 1 to 4 refer to estimates for West German establishments and columns 5 to 8 to estimates for East German establishments. Standard errors are clustered at the establishment level. Significance levels: * 10%, ** 5%, and *** 1%. Data source: LIAB QM2 9310.

Unreported sensitivity checks (in analogy to the sensitivity tests conducted in section 3.5.5) further support this conclusion in that the positive effect on the hiring rate of unionized firms becomes in most of the tests statistically insignificant. Panel B, C, and D of Table 3.9 further show that estimates for both subgroups on the separation, job flow, and churning rate are predominantly statistically insignificant and thus do not provide evidence for a differential effect by firm unionization status.

East-West divide: Finally, I analyze differences in the effects on firms located in former West Germany versus firms located in former East Germany.[32] Although the PADA reform was introduced country-wide, persistent structural differences in the economies of the two former halves of Germany may still yield differential reform effects. However, as Table 3.10 shows, the DiD coefficients for both groups are again almost exclusively statistically insignificant and thus there is no evidence that firms in either part of Germany responded differently to the PADA reform.

3.5.4. Other Margins of Adjustment

Firms might possibly have adjusted to the reduced dismissal costs along other margins. If firms take into account firing costs in the hiring decisions, a reduction in dismissal costs might be passed on to the workers which would lead to a raise in wages (Lazear 1990; Leonardi and Pica 2013). In contrast, placing reliance upon the insider-outsider theory, a fall in dismissal protection might just as well reduce wages due to a loss in the bargaining power of incumbent workers (Lindbeck and Snower 2001; Martins 2009).

To shed light on potential wage effects, I examine the impact on firm mean wages. Since I do not have information on hours worked, I limit my attention to full-time workers. I determine log mean daily wages by $Log\_wage_{it} = \log\left(\frac{1}{FT_{it}}\sum_{e=1}^{FT_{it}} wage_{eit}\right)$ where $wage_{eit}$ is the wage of the full-time employee e in firm i in period t and FT_{it} is the number of full-time workers in the respective firm and period.[33] Panel A in Table 3.11 reports the DiD results from an re-estimation of model (3.1) using Log_wage_{it} as the outcome variable. Although statistically insignificant coefficients for the pre-treatment periods sustain the common time trend assumption for this outcome, the results for the post-reform periods do not corroborate an adjustment along the wage dimension. Estimates are in absolute terms not larger than 1.0 percentage point and uniformly statistically insignificant.[34]

Alternatively, firms might employ workers on a temporary basis to evade costly firing restrictions. That is, theory predicts that a larger protection gap between open-ended and temporary contracts entails an increased substitution of temporary for permanent workers

[32] West Germany comprises the federal states Baden-Wuerttemberg, Bavaria, Bremen, Hamburg, Hesse, Lower Saxony, North Rhine-Westphalia, Rhineland Palatinate, and Schleswig-Holstein. Former East Germany includes the federal states Berlin, Brandenburg, Mecklenburg Western Pomerania, Saxony, Saxony-Anhalt, and Thuringia.

[33] I impute right-censored wages following Dustmann et al. (2009). For details on the imputation method see Appendix B.1.1.

[34] The results do qualitatively not change when I use non-imputed wages. Results are available upon request.

Table 3.11.: Difference-in-Differences Results: Other Margins of Adjustment

		2-Years in 'Before' Periods (1)	4-Years in 'Before' Periods (2)	Always the same (3)	Adjacent Periods (4)
Panel A: Log mean wages	DiD 2004-07	−0.004	−0.005	−0.009	−0.010
		(0.008)	(0.010)	(0.011)	(0.009)
	DiD 2001-02	0.010	0.011	0.015	0.008
		(0.008)	(0.010)	(0.014)	(0.009)
Firms		575	412	243	956
Observations		4025	2884	1701	4287
Panel B: Share of FTC workers	DiD 2004-07	−0.003	−0.006	−0.014*	−0.003
		(0.007)	(0.008)	(0.008)	(0.006)
	DiD 2001-02	−0.005	−0.010	−0.016**	−0.003
		(0.006)	(0.006)	(0.008)	(0.006)
Firms		381	269	167	536
Observations		2667	1883	1169	2605
Panel C: Share of TA workers	DiD 2004-07	−0.001	−0.001	−0.004	−0.001
		(0.003)	(0.003)	(0.003)	(0.003)
	DiD 2002	−0.005	−0.004	−0.005	−0.001
		(0.003)	(0.003)	(0.004)	(0.003)
Firms		380	268	168	523
Observations		2280	1608	1008	2233

Notes: Table shows coefficients of difference-in-differences (DiD) estimates (ρ) as given by empirical model (3.1) for the sample periods 2001 to 2007 (panel A and B) and 2002 to 2007 (panel C), with the indicated variable as the outcome and establishment-year as the unit of observation. The years after DiD indicate the pooled periods under consideration. The year 2003 is the baseline period. Each column presents separate estimates for one of the four assignment methods described in section 3.4.2. Outcome variables for estimates in panel B and C are based on the additional information from the IAB EP. Standard errors are clustered at the establishment level. Significance levels: * 10%, ** 5%, and *** 1%. Data sources: LIAB QM2 9310 and IAB Establishment Panel.

(Boeri 2011; Cahuc et al. 2016).[35] In terms of the PADA reform under study, the less stringent dismissal protection for some firms reduces the respective protection gap and should cut the use of temporary employment relations in treated firms.

To test for this hypothesis, I examine the extent of the use of workers on fixed-term contracts (FTC) or workers from temporary agencies (TA). Both types of employment allow firms to bypass coverage by the general rules of the PADA. I determine the share of FTC workers by $Share\_FTC_{it} = \frac{(\text{Number of FTC workers})_{it}}{E_{it}}$ where the numerator is the number of (unweighted) FTC workers in firm i in period t and E_{it} is the total number of (unweighted) workers in the respective firm and period. The share of TA workers is constructed the

[35]For example, Autor (2003) finds that 20% of the growth of temporary services employment in the Unites States between 1973 and 1995 results from a stricter dismissal protection. As for Germany, empirical evidence on the effects of changes in dismissal protection on the use of temporary employment exploiting previous threshold changes is limited and contradictory. Boockmann and Hagen (2001) find some indication that the raise of the PADA threshold in 1996 lowered the probability of using fixed-term contracts and had no effect on temporary agency work in establishments subject to less stringent dismissal protection. However, using the same survey data (IAB EP), Fritsch and Schank (2005) do not confirm the result on fixed-term employment. Neither in 1996 nor in 1999 do they find a significant effect of threshold changes in the PADA on the use and the share of fixed-term contracts.

same way using the number of TA instead of FTC workers in the numerator. Once more, I re-estimate model (3.1) separately for the two outcome variables.[36]

Panel B in Table 3.11 shows the results for the share of FTC workers. The total effects are negative but close to zero, and statistically insignificant in three out of four cases. Only for assignment method (3) do I find a statistically significant negative effect of 1.4 percentage points. However, it is also for this assignment method where the pre-reform period is also statistically significant which casts doubt on the common time trend assumption for this outcome. Overall, there is no clear evidence that the relaxed dismissal protection reduced the use of FTC workers in treated firms. Panel C of Table 3.11 reports the estimates for the share of TA workers. Initially, the pre-reform coefficient passes the test for common time trend for this outcome variable.[37] However, although the total effect is negative in all cases, it is never statistically significant and thus, there is no support for a significant reduction in the use of TA workers. To sum up, the results do not support the theoretical prediction that a tightening of the protection gap reduces the use of temporary employment.

3.5.5. Sensitivity Analyses

So far, the results presented do not reveal statistically significant effects of the PADA reform on the overall worker turnover rates within firms. However, the analysis of the heterogeneity of treatment effects suggests that the reform had an effect on subgroups of workers. Namely, I find evidence for a positive effect on the women's hiring and job flow rates. In this final section of the paper, I assess the robustness of these findings in more detail.

Controlling for Firm Characteristics: As discussed in section (3.3), the main results are based on regressions unconditional on time-varying confounding factors. As a first sensitivity check, I re-estimate model (3.1) whereby I control for observable time-varying firm-level characteristics. As controls, I include the average share of blue-collar workers, part-time workers, apprentices, and women as well as the average age of employees and its square. If these variables are not influenced by the reform but still correlated with both the treatment status and the outcome variable, their inclusion might mitigate an omitted variable bias (and/or increase the precision of the estimation). However, the results in Table 3.12 do not suggest that my results suffer from this bias. Conditioning on additional variables only has a negligible impact on the DiD estimates both in terms of the size of the estimates as well as the significance levels of the coefficients.

Varying Upper Firm Size Limits: The upper limit of the control group is supposed to enhance the credibility of the common time trend assumption. A tighter limit may increase

[36] As the individual-level data of the LIAB QM2 do not provide information on the contract type, I use survey information on the number of FTC and TA workers from the IAB EP. For details on the matching of the two data sources see Appendix B.1.1. In particular, I note that in order to maintain consistency of the data I follow the procedure described in Alda (2005) which reduces sample sizes substantially.

[37] The pre-treatment analysis for the share of TA workers is only based on the period 2002, since the survey did not asked firms for the number of TA workers in 2001.

Table 3.12.: Sensitivity Check: Controlling for Firm Characteristics

		2-Years in 'Before' Periods (1)	4-Years in 'Before' Periods (2)	Always the same (3)	Adjacent Periods (4)
Panel A: Hiring rate	DiD 2004-07	0.004	0.020*	0.015	0.007
		(0.010)	(0.010)	(0.013)	(0.010)
	DiD 2001-02	0.012	0.009	0.002	0.002
		(0.010)	(0.010)	(0.013)	(0.010)
Panel B: Separation rate	DiD 2004-07	−0.007	−0.002	0.012	−0.002
		(0.011)	(0.013)	(0.013)	(0.010)
	DiD 2001-02	−0.003	−0.009	−0.010	−0.004
		(0.011)	(0.012)	(0.015)	(0.011)
Panel C: Job flow rate	DiD 2004-07	0.011	0.022	0.005	0.009
		(0.013)	(0.015)	(0.017)	(0.013)
	DiD 2001-02	0.015	0.018	0.013	0.005
		(0.014)	(0.015)	(0.020)	(0.014)
Panel D: Churning rate	DiD 2004-07	−0.002	0.017	0.020	0.003
		(0.014)	(0.015)	(0.017)	(0.014)
	DiD 2001-02	−0.009	0.003	0.002	0.001
		(0.014)	(0.015)	(0.019)	(0.015)
Panel E: Hiring rate, women	DiD 2004-07	0.013**	0.018***	0.021***	0.013**
		(0.005)	(0.006)	(0.008)	(0.006)
	DiD 2001-02	0.004	0.005	0.009	0.005
		(0.007)	(0.007)	(0.009)	(0.006)
Panel F: Job flow rate, women	DiD 2004-07	0.019**	0.023**	0.016	0.016**
		(0.008)	(0.009)	(0.011)	(0.008)
	DiD 2001-02	0.007	0.007	0.006	0.006
		(0.009)	(0.010)	(0.013)	(0.009)
Firms		587	422	247	957
Observations		4109	2954	1729	4303

Notes: Table shows coefficients of difference-in-differences (DiD) estimates (ρ) as given by empirical model (3.1) for the sample periods 2001 to 2007, with the indicated variable as the outcome and establishment-year as the unit of observation. The years after DiD indicate the pooled periods under consideration. The year 2003 is the baseline period. Each column presents separate estimates for one of the four assignment methods described in section 3.4.2. In difference to the main analysis in section 3.5, difference-in-differences estimates in this table are based on an empirical model that additionally includes time-varying establishment-level characteristics (X_{it}). X_{it} contains average share of blue-collar workers, average share of part-time workers, average share of apprentices, average share of women, average age of employees and its square. Standard errors are clustered at the establishment level. Significance levels: * 10%, ** 5%, and *** 1%. Data source: LIAB QM2 9310.

the plausibility of this assumption. At the same time, it confines the sample size of the control group. For the main analysis, I defined the upper limit of the control group's size range at 20 FTE employees. To test whether my results are sensitive towards the choice of the upper limit, I replicate the analysis using a more restrictive limit of 15 FTE employees and a more relaxed limit of 25 FTE employees. Panel A to D in Table 3.13 show the extended results for the overall worker turnover rates. For each assignment method, the center column resembles the main results presented in section 3.5.1 and the left (right) column report results for the lower (higher) upper limit. The results emphasize that the insignificance of the overall

Table 3.13.: Sensitivity Check: Varying Upper Firm Size Limits

		2-Years in 'Before' Periods			4-Years in 'Before' Periods			Always the same			Adjacent Periods		
	Max. size for C:	15	20	25	15	20	25	15	20	25	15	20	25
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Hiring rate	DiD 2004-07	0.016	0.005	0.009	0.035**	0.020*	0.027***	0.010	0.015	0.017	0.017	0.007	0.010
		(0.011)	(0.010)	(0.009)	(0.014)	(0.010)	(0.009)	(0.017)	(0.013)	(0.011)	(0.009)	(0.010)	(0.009)
	DiD2001-02	0.016	0.012	0.015	0.022	0.010	0.015	0.010	0.001	0.001	0.013	0.002	0.005
		(0.012)	(0.010)	(0.010)	(0.014)	(0.010)	(0.009)	(0.021)	(0.013)	(0.012)	(0.012)	(0.010)	(0.009)
Panel B: Separation rate	DiD 2004-07	−0.001	−0.006	0.003	0.005	−0.002	0.007	0.023	0.011	0.019	0.000	−0.001	0.006
		(0.012)	(0.011)	(0.010)	(0.016)	(0.013)	(0.011)	(0.017)	(0.013)	(0.012)	(0.010)	(0.010)	(0.010)
	DiD2001-02	−0.012	−0.002	0.004	0.000	−0.007	0.000	0.021	−0.010	0.001	0.004	−0.003	0.002
		(0.013)	(0.011)	(0.011)	(0.015)	(0.012)	(0.012)	(0.020)	(0.015)	(0.014)	(0.013)	(0.011)	(0.011)
Panel C: Job flow rate	DiD 2004-07	0.017	0.011	0.006	0.030	0.022	0.020	−0.013	0.004	−0.002	0.017	0.009	0.004
		(0.014)	(0.013)	(0.012)	(0.019)	(0.015)	(0.013)	(0.023)	(0.017)	(0.015)	(0.012)	(0.013)	(0.012)
	DiD2001-02	0.028*	0.014	0.011	0.022	0.016	0.015	−0.011	0.011	0.000	0.010	0.005	0.002
		(0.016)	(0.014)	(0.014)	(0.019)	(0.015)	(0.014)	(0.026)	(0.020)	(0.018)	(0.015)	(0.014)	(0.013)
Panel D: Churning rate	DiD 2004-07	0.015	−0.001	0.004	0.037*	0.017	0.028**	0.023	0.020	0.029*	0.011	0.004	0.010
		(0.017)	(0.014)	(0.013)	(0.019)	(0.015)	(0.014)	(0.022)	(0.017)	(0.015)	(0.013)	(0.014)	(0.013)
	DiD2001-02	−0.011	−0.009	−0.008	0.000	0.005	0.013	0.019	0.003	0.005	0.010	0.001	0.007
		(0.018)	(0.014)	(0.013)	(0.020)	(0.015)	(0.014)	(0.028)	(0.014)	(0.017)	(0.018)	(0.015)	(0.014)
Panel E: Hiring rate, women	DiD 2004-07	0.015**	0.013**	0.014***	0.024***	0.018***	0.021***	0.023**	0.021***	0.022***	0.015**	0.014**	0.015***
		(0.006)	(0.005)	(0.005)	(0.008)	(0.006)	(0.006)	(0.010)	(0.008)	(0.007)	(0.005)	(0.006)	(0.005)
	DiD2001-02	0.008	0.005	0.007	0.011	0.007	0.010	0.022*	0.010	0.010	0.007	0.006	0.008
		(0.008)	(0.007)	(0.006)	(0.009)	(0.007)	(0.006)	(0.013)	(0.006)	(0.009)	(0.008)	(0.006)	(0.006)
Panel F: Job flow rate women	DiD 2004-07	0.021**	0.019**	0.016**	0.021*	0.023**	0.022**	0.005	0.016	0.012	0.019**	0.015**	0.013*
		(0.009)	(0.008)	(0.007)	(0.012)	(0.009)	(0.008)	(0.015)	(0.011)	(0.010)	(0.007)	(0.008)	(0.007)
	DiD2001-02	0.014	0.007	0.006	0.006	0.007	0.007	0.002	0.005	0.002	0.006	0.005	0.005
		(0.010)	(0.009)	(0.008)	(0.012)	(0.010)	(0.009)	(0.018)	(0.009)	(0.012)	(0.010)	(0.009)	(0.008)
Firms		419	587	724	268	422	554	153	247	363	769	957	1082
Observations		2933	4109	5068	1876	2954	3878	1071	1729	2541	3180	4304	5233

Notes: Table shows coefficients of difference-in-differences (DiD) estimates (ρ) as given by empirical model (3.1) for the sample periods 2001 to 2007, with the indicated variable as the outcome and establishment-year as the unit of observation. The years after DiD indicate the pooled periods under consideration. The year 2003 is the baseline period. Each column presents separate estimates for one of the four assignment methods described in section 3.4.2. Columns 2, 5, 8, and 11 resemble the main estimates from section 3.5 for a sample based on an upper size limit for the control group of 20 full-time equivalent weighted employees. Respectively, columns 1, 4, 7, and 10 show difference-in-differences estimates for a sample based on an upper size limit of 15 and columns 3, 6, 9, and 12 for a sample based on an upper size limit of 25. Significance levels: * 10%, ** 5%, and *** 1%. Data source: LIAB QM2 9310.

effects is for the most part not sensitive to the choice of the control group's upper limit. Only the churning rate becomes significantly positive in three cases. However, given that majority of estimates for this outcome is still statistically insignificant, I do not consider this as evidence for a positive causal effect. As for the women's hiring rate, Panel E in Table 3.13 demonstrates that the effect is robust to different size limits. Notably, all estimates on the women's hiring rate are statistically significant at least at the 5% level. Moreover, the size of the coefficients is fairly stable ranging from 1.3 to 2.4 percentage points. Panel F in Table 3.13 further shows that the estimates on the women's job flow rate are also insensitive to different upper limits. The coefficients of the assignment methods (1), (2), and (4) remain statistically significant at least at the 10% level while estimates for assignment method (3) are still statistically insignificant.

Tightening Firm Size Intervals: While the LIAB QM2 allows for a relatively accurate computation of the FTE firm size that determines coverage by the PADA, I cannot preclude measurement errors. For example, the part-time weighting scheme defined by the PADA is not perfectly replicated by the data and I have to assume that workers in inactive work relationships are always replaced by new (temporary) hires (see discussion in section 3.4.2). As this may pose a problem in particular for establishments near the old and new thresholds, I exclude firms close to two thresholds during the assignment periods by tightening the size interval of the treatment group to $[6,9]$ and size interval of the control group to $[11,20]$ FTE employees. Table 3.14 shows that the main results from sections 3.5.1 and 3.5.3 are robust to this alternative sample selection criteria, and therefore the previous conclusions remain valid.

Excluding Firms Clustered at Thresholds: In section 3.4.5, I already assessed potential threshold effects and did not find evidence that firms just above (or below) the new threshold adjusted their firm size strategically. To provide further evidence that my findings are not distorted by threshold effects, I remove firms from the sample that are clustered around the size thresholds. To do so, I exclude firms from the treatment group with a FTE firm size below six in at least one of the before periods (given the old threshold at five FTE employees) and I further drop firms from the treatment and control group with a FTE firm size larger than nine and smaller than 11 in at least one of the after periods (given the new threshold at 10 FTE employees). Panels A to D in Table 3.15 summarize the results for the overall worker turnover rates and confirm the absence of a robust effect. Panel E and D further depict the estimates for the hiring and job flow rate of women. While the increased effect on the hiring rate of women is strongly confirmed, the size of the effect on women's job flow rates for the assignment method (4) further decreases and becomes statistically insignificant. Nevertheless, taking into account the array of sensitivity checks, I maintain my main conclusion and interpret the estimates on both the hiring and job flow rates of women as positive causal effects.

Table 3.14.: Sensitivity Check: Tightening Firm Size Intervals

		2-Years in 'Before' Periods (1)	4-Years in 'Before' Periods (2)	Always the same (3)	Adjacent Periods (4)
Panel A: Hiring rate	DiD 2004-07	0.012	0.018	0.019	0.015
		(0.011)	(0.014)	(0.017)	(0.011)
	DiD 2001-02	0.012	−0.003	0.002	−0.002
		(0.011)	(0.013)	(0.018)	(0.011)
Panel B: Separation rate	DiD 2004-07	0.000	0.013	0.022	0.009
		(0.012)	(0.014)	(0.016)	(0.011)
	DiD 2001-02	0.000	−0.003	−0.006	−0.005
		(0.012)	(0.014)	(0.019)	(0.012)
Panel C: Job flow rate	DiD 2004-07	0.011	0.004	−0.003	0.006
		(0.013)	(0.017)	(0.020)	(0.013)
	DiD 2001-02	0.012	0.000	0.008	0.003
		(0.015)	(0.017)	(0.023)	(0.015)
Panel D: Churning rate	DiD 2004-07	−0.004	0.025	0.027	0.019
		(0.016)	(0.018)	(0.023)	(0.017)
	DiD 2001-02	−0.013	0.000	0.004	−0.001
		(0.016)	(0.018)	(0.024)	(0.017)
Panel E: Hiring rate, women	DiD 2004-07	0.019***	0.019**	0.022**	0.022***
		(0.006)	(0.008)	(0.011)	(0.007)
	DiD 2001-02	0.010	0.005	0.007	0.007
		(0.008)	(0.009)	(0.013)	(0.007)
Panel F: Job flow rate, women	DiD 2004-07	0.021**	0.019*	0.017	0.020**
		(0.008)	(0.011)	(0.014)	(0.008)
	DiD 2001-02	0.009	0.003	0.007	0.007
		(0.010)	(0.012)	(0.017)	(0.010)
Firms		461	307	158	875
Observations		3227	2149	1106	3361

Notes: Table shows coefficients of difference-in-differences (DiD) estimates (ρ) as given by empirical model (3.1) for the sample periods 2001 to 2007, with the indicated variable as the outcome and establishment-year as the unit of observation. The years after DiD indicate the pooled periods under consideration. The year 2003 is the baseline period. Each column presents separate estimates for one of the four assignment methods described in section 3.4.2. In difference to the main analysis in section 3.5), difference-in-differences estimates in this table are based on a sample with the size interval of the treatment group tightened to $[6,9]$ full-time equivalent weighted employees and the size interval of the control group tightened to $[11,20]$ full-time equivalent weighted employees. Significance levels: * 10%, ** 5%, and *** 1%. Data source: LIAB QM2 9310.

Table 3.15.: Sensitivity Check: Excluding Firms Clustered at Thresholds

		2-Years in 'Before' Periods (1)	4-Years in 'Before' Periods (2)	Always the same (3)	Adjacent Periods (4)
Panel A: Hiring rate	DiD 2004-07	−0.010	0.004	0.015	0.001
		(0.011)	(0.013)	(0.016)	(0.010)
	DiD 2001-02	0.000	−0.008	−0.002	−0.007
		(0.012)	(0.013)	(0.016)	(0.010)
Panel B: Separation rate	DiD 2004-07	0.010	0.012	0.017	0.013
		(0.012)	(0.015)	(0.015)	(0.010)
	DiD 2001-02	−0.003	−0.005	−0.003	0.002
		(0.012)	(0.014)	(0.017)	(0.011)
Panel C: Job flow rate	DiD 2004-07	−0.019	−0.008	−0.002	−0.012
		(0.015)	(0.017)	(0.019)	(0.013)
	DiD 2001-02	0.003	−0.003	0.001	−0.009
		(0.017)	(0.018)	(0.022)	(0.014)
Panel D: Churning rate	DiD 2004-07	−0.010	0.007	0.018	0.009
		(0.016)	(0.018)	(0.022)	(0.015)
	DiD 2001-02	−0.027	−0.003	0.002	−0.001
		(0.016)	(0.018)	(0.023)	(0.015)
Panel E: Hiring rate, women	DiD 2004-07	0.015**	0.019**	0.021**	0.018***
		(0.006)	(0.008)	(0.010)	(0.006)
	DiD 2001-02	0.009	0.005	0.007	0.006
		(0.008)	(0.009)	(0.012)	(0.006)
Panel F: Job flow rate, women	DiD 2004-07	0.019**	0.022**	0.017	0.011
		(0.009)	(0.011)	(0.013)	(0.008)
	DiD 2001-02	0.014	0.006	0.003	0.003
		(0.011)	(0.012)	(0.015)	(0.009)
Firms		407	283	178	923
Observations		2849	1981	1246	3843

Notes: Table shows coefficients of difference-in-differences (DiD) estimates (ρ) as given by empirical model (3.1) for the sample periods 2001 to 2007, with the indicated variable as the outcome and establishment-year as the unit of observation. The years after DiD indicate the pooled periods under consideration. The year 2003 is the baseline period. Each column presents separate estimates for one of the four assignment methods described in section 3.4.2. In difference to the main analysis in section 3.5), difference-in-differences estimates in this table are based on a sample that excludes establishments with a full-time equivalent weighted firm size below six in at least one of the pre-reform periods or establishments with a full-time equivalent weighted firm size larger than nine and smaller than 11 in at least one of the after-reform periods. Significance levels: * 10%, ** 5%, and *** 1%. Data source: LIAB QM2 9310.

3.6. Conclusions

In this paper, I analyze the impact of a change in the German PADA in 2004 on different labor market outcomes in small establishments. I use detailed administrative employer-employee panel data linked to establishment survey data (LIAB QM2 9310) to estimate the causal effect of the change in the PADA on hiring, separation, job flow and churning rates, as well as on wages and the use of temporary employment relations. The identification strategy is based on a difference-in-differences approach exploiting a temporal and cross-sectional variation in the PADA.

I find no robust evidence for a causal impact of the 2004 PADA reform on the overall worker turnover rates of firms. Thereby, I show that a positive effect on the hiring rate, that has previously been detected by Bauernschuster (2013), is highly sensitive to the assignment method to the treatment and control group. Moreover, my findings are in line with Bauer et al. (2007). They study the impact of similar PADA reforms in 1996 and 1999 on worker turnover and also do not find statistically significant effects. From a policy point of view, the absence of causal evidence for a positive effect on the overall job flow rate is of particular interest because advocates for a raise of the minimum threshold determining coverage by the PADA often justify their policy recommendation by positive employment effects.

I further assess potential heterogeneous treatment effects and find some evidence of increases in the hiring and job flow rates of women in response to the relaxed dismissal protection which could be explained by higher labor supply elasticities of women. This introduces an important gender aspect to the evaluation of reforms of the dismissal protection that has not been addressed in the literature so far.

Lastly, I examine other margins of adjustment that may offset the effects on worker turnover. Contrary to findings in other countries (e.g., Centeno and Novo 2012; Leonardi and Pica 2013), I neither find evidence that the reduced dismissal costs impacted wages nor do I find evidence that firms reduced the use of temporary employment.

There are a number of potential reasons for the lack of a sizable effect of the PADA reform. To name a few, the reduction in dismissal costs may not have been of a magnitude such that it had a significant and persistent effect on worker turnover. For example, incumbent workers remained protected by the PADA and thus only the dismissal costs of new hires were directly affected. Besides, some establishments may not have been aware of the change in the threshold or generally misjudged coverage by the PADA. Survey evidence shows that a considerable share of establishments falsely assumed coverage by the PADA prior to the reform (Pfarr et al. 2003). Finally, firms may have adjusted along the extensive margin by firm entries and exits (Kugler and Pica 2008), a phenomenon that could be addressed with data that allow for identifying events of firm entry and exit.

4. The Role of STEM Occupations in the German Labor Market

4.1. Introduction

Wage inequality in West Germany has increased dramatically from the mid-1990s onwards. This development followed a moderate growth in wage dispersion during the preceding two decades. While much of the rise in wage inequality has been driven by declines in wages at the bottom of the wage distribution, wage gains in the upper part of the wage distribution also contributed substantially to this development (Dustmann et al. 2009, 2014).[1] During the same period, non-routine cognitive skills, in particular in combination with technical expertise and scientific capabilities, have increasingly been in demand (Spitz-Oener 2006; Autor 2014). One occupational group that is confined to the top of the wage distribution and, in addition, is considered to be particularly in demand due to its provision of technical skills, is the group of workers in Science, Technology, Engineering, and Math (STEM) occupations. This suggests that STEM workers might have been an important factor accounting for part of the rise in wage inequality. To date, however, there is no systematic evidence on the role of STEM occupations for changes in the German wage structure.

One strand of literature on wage inequality in West Germany has focused on supply and demand factors including the supply of skilled workers and technological changes (e.g., Antonczyk et al. 2009; Dustmann et al. 2009; Glitz and Wissmann 2017).[2] These studies investigate different drivers underlying changes in the German wage structure, with a particular emphasis on skill-specific differences. Notably, while Antonczyk et al. (2009) perform a Blinder-Oaxaca decomposition into separate effects of worker characteristics and task assignments, Dustmann et al. (2009) and Glitz and Wissmann (2017) study the rise in skill premiums within a CES production function framework under the assumption of a competitive labor market.[3] Another, more recent strand of literature uses models with additive worker and establishment fixed effects to highlight the contribution of establishment-specific wage differentials to the growth in wage inequality in West Germany. Card et al.

[1]Similar increases in wage inequality have been observed in many other industrialized countries, attracting continuing attention from both researchers and policy makers (Acemoglu and Autor 2011; OECD 2015).

[2]Other prominent explanations are changes in institutions such as the decline in unionization, globalization and changes in the social skills (Dustmann et al. 2009; Dauth et al. 2016; Deming 2017).

[3]The CES framework was initially introduced by Katz and Murphy (1992) using data for the U.S. labor market. For further studies focusing on the U.S., see, e.g., Bound and Johnson (1992); Juhn et al. (1993); Card and Lemieux (2001); Goldin and Katz (2007); Acemoglu and Autor (2011).

(2013) estimate a model for four subintervals of similar length separately for men and women using West German data between 1985 and 2009. They find that wage inequality for both genders increased due to a combination of a rising dispersion in worker and establishment effects as well as increasing assortativeness in the assignment of workers to establishments. Goldschmidt and Schmieder (2017) provide an application of the model to assess wage changes of a selected occupational group. To this end, they estimate the same model (again separately for men and women) based the same data for the entire period 1975 to 2009 and show that the outsourcing of food, cleaning, security and logistics workers accounts for about 7 to 9% of the wage dispersion between 1985 and 2008 with equal parts due to increased dispersion of the establishment effect and increased assortativeness of low wage workers to low wage establishments.[4]

In this paper, I use detailed administrative data on West German employees to study trends in STEM employment and wages and explore potential drivers underlying an increase in wage differentials between STEM and non-STEM workers — which I call the *STEM premium*. My main contribution is a deeper analysis of the evolution of the STEM premium using two empirical approaches. First, I analyze wage differentials between STEM and non-STEM workers within the scope of a competitive labor market using a CES framework. Second, I investigate the STEM premium using estimates from a model with additive worker and firm fixed effects.[5] Notably, the model is a departure from the competitive labor market approach and allows for firm-specific wage rents which turn out to be an important component of wages in other studies (e.g., Card et al. 2013).[6] STEM workers are particularly interesting because, as I will show, they are located in the upper part of the wage distribution where a substantial part of the wage inequality occurs. Moreover, by virtue of the German dual apprenticeship system, about half of these highly paid STEM workers have received vocational training without a college or university degree and are therefore *only* considered medium-skilled in the German context (see section 4.3). Consequently, an analysis along broad skill categories may mask extensive heterogeneity that is of importance to explain trends in wages.[7]

To document the role of STEM occupations, I begin with a broad overview of labor market trends of STEM workers between 1980 and 2010. Despite a decrease in the share of STEM workers among the highly-skilled, the overall share of STEM workers in total employment

[4]For a review of the literature on firm effects that includes studies for other countries see Card et al. (2017).

[5]In what follows, I use the term firm to refer to an establishment. Strictly speaking, a firm may consist of multiple establishments and the data refer to the latter.

[6]The empirical model used in Card et al. (2013) was initially introduced without a theoretical economic underpinning. However, Card et al. (2017) have developed a microeconomic foundation and show that the empirical findings can readily be matched with a frictional labor market model.

[7]While there is a growing number of studies that focus on STEM occupations, they primarily target other aspects. Hanson and Slaughter (2013), Kerr and Kerr (2013), Peri et al. (2014), Kerr et al. (2015), Hanson and Slaughter (2016) and Jaimovich and Sin (2017) assess the impact of the immigration of STEM workers in the U.S. labor market. Grave and Goerlitz (2012) and Black et al. (2015) look at the relationship between wages and education in STEM subjects. Card and Payne (2017) and Kahn and Ginther (2017) analyze a gender gap in STEM jobs. Moreover, numerous policy papers discuss a potential shortage of skilled workers (*Fachkräftemangel/-engpass*) in Germany (e.g., Bundesministerium für Arbeit und Soziales 2011; Bundesagentur für Arbeit 2016; Anger et al. 2017) and the U.S. (e.g., Rothwell 2013; Holzer 2015).

increased from 11% to 16% for men and from 3% to 7% for women within three decades.[8] Moreover, I find that both STEM and non-STEM wages increased between 1980 and 2010, however, wage growth was greater in STEM than in non-STEM jobs, leading to a significant increase in the wage differential between the two occupational groups for both genders during this period. Notably, even when I take into account compositional differences by controlling for skill-age profiles of individuals, I find a sizable increase in the wage differential between STEM and non-STEM workers by 10 percentage points (from 20% to 30%) for men and a moderate increase of 3 percentage points (from 24% to 27%) for women. With respect to the time pattern, I show that the STEM premium for men grew in the early 1980s by 4 percentage points, plateaued between 1985 and 1995, and from then on continuously grew by another 6 percentage points until 2010. In contrast, the women's premium decreased between 1980 and 1995 by 3 percentage points and from then on steadily increased over the next 15 years by 7 percentage points. Importantly, the timing of these trends coincides with the rise in overall wage inequality, suggesting that the rise of STEM occupations contributed to the accelerated increase in the German wage inequality since the mid-1990s.

To further assess the supply and demand factors that underlie the evolution of the STEM premium, I introduce a CES production function framework which allows for imperfect substitutability between STEM and non-STEM workers. The framework closely follows previous work by Katz and Murphy (1992) and Goldin and Katz (2007) who use the same approach to analyze wage differentials between low-skilled and high-skilled workers. My model estimates for a combined sample of men and women for the entire period 1980 to 2010 indicate a negative relationship between the STEM premiums and relative STEM/non-STEM supplies by year, each purged of a linear time trend. This suggests that STEM and non-STEM workers are gross substitutes. Moreover, it identifies — within the boundaries of the model — the deceleration of detrended supplies of STEM workers as a potential driver for the growth in the annual STEM premium since the mid-1990s. Overall, the evolution of the STEM premiums can be characterized by an elasticity of substitution between the STEM and non-STEM labor inputs of 1.70. Interestingly, this estimate is of a similar magnitude as estimates by Glitz and Wissmann (2017) of the elasticity of substitution between college and non-college labor in West Germany between 1980 and 2008.

While the CES framework provides a coherent model to relate the rise in wage premiums to supply and demand factors in a competitive labor market, the more recent literature uses models with additive fixed effects that allow for firm-specific wage differentials to study changes in the wage structure. Accordingly, I use estimates of unobserved worker and firm fixed effects from Card et al. (2013) to assess factors that underlie changes in the composition of the STEM premium over time. The distributions of the worker and firm effects demonstrate that male and female STEM workers are positively selected in terms of their estimated worker effects and, in addition, distinctly allocated at the upper part of the distribution of firm effects. However, while the selectivity in terms of the worker effects as well as the firm effects of

[8] For the definition of STEM jobs used in the present study see section 4.2.1.

women remains fairly stable, there is a pronounced right shift of the distribution of firm effects for male STEM workers over time. An application of Gelbach's decomposition method to the unconditional STEM premium in different subintervals confirms this observation. To this end, I estimate the contribution of unobserved worker and firm effects as well as observable time-varying worker characteristics to the STEM premium in two subintervals (1985 to 1991 and 2002 to 2009) and compare the change in the contribution over time. Taking my results at face value, I find that the men's fraction of the premium that is explained by firm effects increases from 12% to 22%, while the respective fraction for women increases only from 21% to 24%. This shows that firm-specific wage components have become more important in explaining the STEM wage gap over time.

The remainder of the paper is organized as follows. In the next section, I describe the data set used in the analyses. In section 4.3, I document labor market trends in STEM employment and wages and their relation to wage inequality. I then analyze the evolution of the STEM premium on the basis of a CES production function with imperfect substitutability between STEM and non-STEM labor inputs in a competitive environment in section 4.4. Next, I assess the importance of worker and firm effects for the wage structure of STEM jobs on the basis of externally provided estimates from a wage model with additive fixed effects in section 4.5. Finally, section 4.6 concludes.

4.2. Data

4.2.1. Data Source

I use the Sample of Integrated Labor Market Biographies Regional File 1975-2010 (SIAB-R 7510), which is a 2% random sample drawn from the Integrated Employment Biographies (IEB) of the German Institute for Employment Research (IAB).[9] The IEB contain administrative data on the universe of employees subject to social security (including marginal employees from 1999 onwards), benefit recipients, and job seekers.[10] The data source for workers in employment is the Employee History (BeH) which results from the integrated notification procedure for health, pension and unemployment insurance and is therefore highly accurate.

The individual data for workers in employment are recorded in job spells. Each job spell contains information on the beginning and end date, daily wage (subject to right censoring at the upper earnings limit for statutory pension insurance), full-time or part-time status, employment type (e.g., regular employee, apprentice, marginal employee), gender, birth date, education, occupation as well as industry code and geographic location of the employing firm.[11] In addition, I use supplementary data on estimated worker and firm fixed effects by

[9] See vom Berge et al. (2013a) for a detailed description of the data.

[10] Self-employed and civil servants are not included.

[11] The occupation variable contains 120 occupational categories, which are aggregates of 330 occupations from the German *KldB 1988* classification. The industry variable contains 14 industry sectors, which are aggregates of

Card et al. (2013).[12] Notably, the IAB provides estimates of the so-called *CHK effects* for the 2% sample of the SIAB-R in a separate data file. Due to data protection regulations, only the 5%-percentile positions of (weighted) firm fixed effects in the overall distribution of firm effects are available.

I focus on full-time workers from West Germany (including Berlin) between the age of 20 and 60 who are not marginally employed or in inactive work relationships (e.g., maternity leave). I correct the education variable following the imputation procedure 1 (*IP1*) by Fitzenberger et al. (2006) and aggregate educational attainment into three skill groups: Low-skilled are workers without vocational training or a university degree. Medium-skilled are workers with vocational training. High-skilled are workers who completed a degree from a college of applied sciences (*Fachhochschule*), university or more (e.g., Ph.D.).[13]

To obtain the main job spell by worker and year, I first collapse all spells at the same employer in a given year into a single worker-firm-year record. Next, I select the worker-year record with the highest total earnings during a calendar year. I calculate the average daily wage for the selected job spell by dividing the total earnings by the duration of the spell. For a worker who is observed in multiple job spells with the same employer in the same year, I follow Card et al. (2013) and assign the highest education, occupation, region and industry category to that worker-firm-year observation. From the remaining observations, I exclude individuals who earn on average less than 10 Euro per day (in 1995 Euros) or who are undergoing training. Moreover, since my analysis predominantly rests upon information on occupation and education, I remove workers with missing information for these variables. The imputed education variable is missing for 1.1% of the job spells and the occupation variable for around 0.5%. Finally, I exclude employees working in firms in agriculture and mining (1.8%). Between 1996 and 1998, the occupation code 102 (for *doctors and pharmacists*) is very rare compared to the neighboring years. The reasons for this are unknown (for details, see vom Berge et al. (2013a), p.31). To account for this problem, I impute the number of workers and wages for this occupation at varying cell levels (e.g., gender or gender-skill cells) using a simple linear interpolation method in some figures and tables.[14]

I deflate all wages in Euros using the Consumer Price Index of the German Central Bank, with 1995 as the base year. The wage measure only records bonus payments from 1984 onwards. I correct for this structural break following the approach of Fitzenberger (1999)

the 3-digit code of the German *WZ93* classification.

[12]Selecting the full-time workers' main job spells in each year from the full sample of the IEB, Card et al. (2013) estimate wage models with additive worker and firm fixed effects for four subintervals (1985-1991, 1990-1996, 1996-2002, 2002-2009), so called *CHK effects*, following the framework developed in Abowd et al. (1999). For a further description of the estimation method and data, see section 4.5.

[13]The classification of educational attainment into skill groups follows, e.g., Antonczyk et al. (2009). The classification deviates from, e.g., Dustmann et al. (2009) and Glitz and Wissmann (2017) who consider individuals with missing values as low-skilled and workers with a high school degree (*Abitur*) as medium-skilled. However, I consider my classification to be more suitable for the present study, because employment in STEM jobs regularly requires either apprenticeship training or a tertiary education.

[14]Whenever a figure or table is based on data using this imputation method, it is indicated in the notes.

and Dustmann et al. (2009).[15] I impute wages above the right censoring limit according to Gartner (2005). Specifically, I run separate Tobit regressions for each year and gender of right censored log wages on indicators of three skill groups, eight age groups, and all possible interactions. This imputation approach is by now common practice for this data and has been extensively evaluated (e.g., Dustmann et al. 2009; Card et al. 2013; Glitz and Wissmann 2017). I test the sensitivity of the imputation method in Appendix C.1.1. Finally, I exclude the years 1975 to 1979 due to an unusually high share of right censored wages among high-skilled workers (e.g., Dustmann et al. 2009; Glitz and Wissmann 2017).[16]

My definition of STEM occupations is guided by the classification used in U.S. literature (e.g., Langdon et al. 2011; Black et al. 2015; Hanson and Slaughter 2016; Card and Payne 2017). Most importantly, I define the occupational group of STEM workers by including both STEM jobs and STEM-related jobs (i.e., medical workers). The Appendix Table C.1 lists the 16 out of 120 SIAB-R occupations classified as STEM, grouped into five broad categories (i.e., engineers, computer scientists, technicians, math/ physics/ chemistry/ economics, and medical workers).[17] For the most part of this study, the assignment to the group of STEM and non-STEM workers is based on a worker's current job title, and may therefore change over time. However, due to empirical limitations, a time constant assignment to the two occupational groups based on the mode of a worker's job titles is used in section 4.5 (see discussion in section 4.5.2).

4.2.2. Summary Statistics

Table 4.1 displays the summary statistics of the pooled sample for non-STEM and STEM workers. Columns 1 to 4 report means (odd columns) and standard deviations (even columns) whereas column 5 shows the difference in means between the two occupational groups. Overall the sample consists of 10,977,380 worker-year observations, of which 1,194,952 (10.9%) belong to workers in STEM jobs. With a difference of 51 log points, the average wage of STEM workers is much higher than that of non-STEM workers. Moreover, STEM workers exhibit a higher level of education. While only 2% of STEM workers have no vocational training or university degree, 53% have completed a vocational training and 45% have obtained a college or university degree. In contrast, 14% of non-STEM workers are considered as low-skilled and 79% as medium-skilled, resulting in a 6% share of high-skilled

[15] For a detailed description of the approach, see the online appendix to Dustmann et al. (2009).

[16] Overall, my sample selection criteria closely follow the data processing by Card et al. (2013). However, contrary to them, I distinguish between three instead of five education groups based on the imputed educational attainment. Moreover, since the imputation method for wages by Card et al. (2013) controls for variables that are not available in the SIAB-R, I opt to impute wages as described above. Finally, to better suit my purposes, I impose some further minor sample restrictions, namely the exclusion of the agriculture or mining sector and the requirement for non-missing values in imputed educational attainment and occupation.

[17] Due to the aggregation of occupations in the SIAB-R, employment in *humanities* — which is evidently not regarded as STEM — is comprised in the occupation code 113 (for *economics, social scientists, statisticians, humanities, and other natural scientists*). However, using frequency counts from the SIAB, the share of *humanities* among STEM employment is on average only 1 to 2% between 1980 and 2010. Moreover, the main results in the present study are robust to the exclusion of the occupation code 113.

Table 4.1.: Summary Statistics, Pooled Sample 1980 to 2010

	Non-STEM		STEM		
	Mean (1)	S.D. (2)	Mean (3)	S.D. (4)	Diff. (5)
Log daily wage	4.20	(0.00)	4.70	(0.42)	0.51
Skill groups					
Low-skilled	0.14	(0.00)	0.02	(0.27)	−0.12
Medium-skilled	0.79	(0.00)	0.53	(0.15)	−0.26
High-skilled	0.06	(0.00)	0.45	(0.50)	0.38
Female	0.37	(0.00)	0.15	(0.50)	−0.21
Age	38.37	(0.00)	40.28	(10.24)	1.91
Urban area ($\geq$ 100.000 empl.)	0.60	(0.00)	0.68	(0.47)	0.08
Industry sectors					
Production of rubber and plastic products	0.03	(0.00)	0.02	(0.22)	−0.01
Chemical industry	0.02	(0.00)	0.05	(0.36)	0.03
Mechanical engineering	0.10	(0.00)	0.15	(0.39)	0.05
Automotive, electrical engineering	0.08	(0.00)	0.19	(0.21)	0.10
Consumer goods	0.10	(0.00)	0.05	(0.03)	−0.05
Hospitality industry	0.03	(0.00)	0.00	(0.21)	−0.03
Construction	0.09	(0.00)	0.04	(0.27)	−0.05
Wholesale, retail trade and other services	0.15	(0.00)	0.08	(0.13)	−0.07
Transport and communication	0.06	(0.00)	0.02	(0.42)	−0.04
Financial intermediation and real estate	0.13	(0.00)	0.22	(0.20)	0.10
Public, personal and household services	0.05	(0.00)	0.04	(0.28)	−0.01
Education, social and healthcare facilities	0.10	(0.00)	0.08	(0.23)	−0.01
Public administration, social security	0.06	(0.00)	0.05	(0.03)	0.00
Unknown/missing	0.00	(0.00)	0.00	(0.00)	0.00
Worker-year observations	9,782,428		1,194,952		

Notes: Table shows summary statistics of the baseline sample pooled over the years 1980 and 2010. Sample includes full-time workers in West Germany (including Berlin) age 20-60 who are not marginally employed, in inactive work relationship or undergoing training. For each worker, the job spell with the highest total earnings during a calendar year is selected. Real wages are based on average daily earnings, deflated by the Consumer Price Index to 1995 levels, and imputed if they exceed the right censoring limit (see text for additional details). Urban areas are local labor markets with an average size of more than 100,000 employees between 1980 and 2010, based on data from the German Federal Statistical Office. Low-skilled are workers without vocational training or university degree. Medium-skilled are workers with vocational training. High-skilled are workers who completed a degree from a college or university degree or more. Data source: SIAB-R 7510.

non-STEM workers.

The next rows demonstrate further demographic differences between the two occupational groups: STEM workers are more male dominated (+21%) and on average 1.9 years older. In addition, STEM employment is more prevalent in urban areas (defined as regions with more than 100,000 employees) and is particularly present in the industry sectors *mechanical engineering* and *automotive and electrical engineering* (34%) as well as *financial intermediation, land and housing, and the real estate* (22%). While the large shares in the engineering sectors do not come at a surprise, the considerable share in the finance and housing-related industry is to some extent unexpected. However, a closer examination shows that employment in this industry sector is dominated by computer scientists (32.1%) as well as technical jobs related to land and housing occupations (49.9%).

While the summary statistics refer to a combined sample of men and women, I will from now on focus on each gender separately.[18] I do this for three reasons: Firstly, men and women differ considerably in terms of their employment history. In particular during the first half of my analysis period, there were persistent societal gender differences and much lower labor force participation rates among women than among men. Consequently, a combined analysis may mask important differences in the evolution of gender-specific employment and wages. And indeed, as I will show in the next sections, there are salient differences between men and women, especially in terms of the time pattern of changes in wage structures. Secondly, gender-specific analyses are in line with previous studies on wage inequality in Germany that also look at men and women separately (e.g., Dustmann et al. 2009; Card et al. 2013; Goldschmidt and Schmieder 2017). Lastly, Card et al. (2013) estimated worker and firm fixed effects separately for men and women and the data only indicate the 5%-percentile positions of the firm effects in the overall *gender-specific* distribution of firm effects. A comparison of worker and firm effects across both genders requires a normalization on the basis of the full matched employee-employer data which are not available to me (Card et al. 2016).

4.3. Labor Market Trends

In this section, I establish some stylized facts on the evolution of STEM occupations in West Germany over a period of three decades. I start by looking at overall trends in wage inequality between 1980 and 2010 for men and women. Next, I discuss trends in employment and wages of STEM occupations over the same period. Thereby, I put particular emphasis on the evolution of wage differentials between STEM and non-STEM workers as well as its relation to the time pattern of wage inequality. My findings will provide a motivation for the further empirical analyses presented in sections 4.4 and 4.5.

Panels A and B in Figure 4.1 display the indexed wage growth (in 1995 Euros) of the 15th, 50th, and 85th wage percentile between 1980 and 2010 by gender.[19] As pointed out in previous studies on the West German wage structure (e.g., Steiner and Wagner 1998; Kohn 2006; Dustmann et al. 2009; Antonczyk et al. 2010; Dustmann et al. 2014; Glitz and Wissmann 2017), wage inequality has increased substantially within the last three decades. Notably, wage inequality rose moderately in the 1980s and the early 1990s (for men more than woman), but dramatically accelerated from the mid-1990s onwards for both genders.

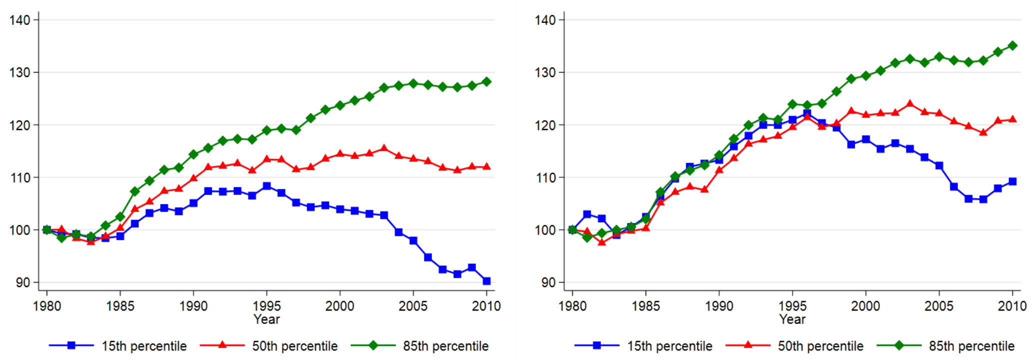
More precisely, the 15th and 85th percentile of men diverged slowly until 1995, with a growth of less than 10% for the 15th percentile and an increase of almost 20% for the 85th percentile. The picture looks very different from then onwards. Most importantly, wages diverged due to an almost 20 percentage points decline of the 15th percentile (below its

[18] I make an exception in section 4.4 where I study the relationship between supply and demand factors using a combined sample of men and women.

[19] Following Dustmann et al. (2009), I focus on the 15th and 85th percentile as the lower and upper tails to limit the dependence on the wage imputation.

Figure 4.1.: Indexed Wage Growth of the 15th, 50th, and 85th Percentiles

A. Men B. Women



Notes: Figure shows the indexed log real wage growth of the 15th, 50th, and 85th percentiles of the wage distribution. Data source: SIAB-R 7510.

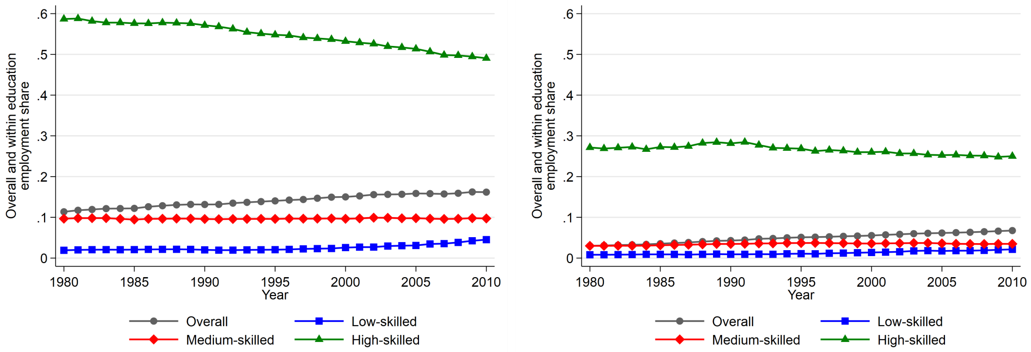
initial level in 1980) coupled with a 10 percentage points increase of the 85th percentile. Overall, the rise in the difference between the men's 85th and 15th percentile was about 10 percentage points between 1980 and 1995 and almost 30 percentage points between 1995 and 2010. Given that the 50th percentile increased by almost 15% until the mid-1990s and remained fairly constant thereafter, wage inequality for men rose in both the top and the bottom part of the wage distribution.

The pattern for women looks somewhat different. Until 1995, wages in all percentiles jointly grew by about 20%, meaning that wage inequality changed very little. However after 1995, women's wage inequality increased as well, with falling wages in the 15th percentile (yet, still above the 1980 level) and rising wages for the 85th percentile. Accordingly, also the rise in the difference between women's 85th and 15th percentile was much larger in the last 15 years of the observation period. Specifically, while the difference grew by less than 5 percentage points between 1980 and 1995, it increased by more than 20 percentage points between 1995 and 2010. Further taking into account the development of the 50th percentile, women's wage inequality rose as well at both the lower and upper ends of the wage distribution.

Next, looking at employment trends in STEM jobs, Figure 4.2 shows the employment shares of STEM workers in total employment (plotted with dots) as well as the within skill group shares of low-skilled (plotted with squares), medium-skilled (plotted with diamonds) and high-skilled (plotted with triangles) male and female workers. Overall, the men's share of STEM employment monotonically increased from 11.4% in 1980 to 16.2% in 2010. Despite a much lower share in the initial year of only 3.0%, the time pattern for women looks very similar with a continuous increase in the STEM share up to 6.8% in 2010. Somewhat surprisingly, however, the share of STEM jobs within the highly-skilled group actually decreased between 1980 and 2010 for both men and women, while the share of STEM workers among the group of medium-skilled remained almost constant, and the share within the group of low-skilled increased slightly (though it was still at a very low level by 2010).

Figure 4.2.: Evolution of Shares of STEM Workers in Total Employment and Within Skill Groups

A. Men B. Women



Notes: Figure shows the shares of STEM workers in total employment and within skill groups. Employment of workers with occupation code 102 (for *doctors and pharmacists*) is imputed at the level of gender-skill-year cells between 1996 and 1998. See section 4.2.1 for additional details. Data source: SIAB-R 7510.

Yet, the rise in the overall share of STEM employment can be explained by the substantial educational upgrading, with overall high-skilled shares growing from 6.4% to 17.5% for men and 2.7% to 15.7% for women coupled with a large share of STEM workers in this skill group.

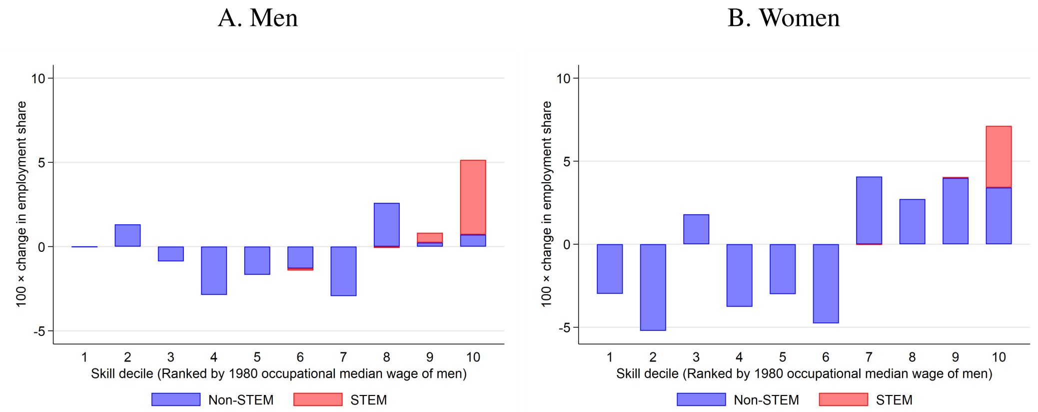
Using a standard shift share decomposition, Appendix Table C.2 further reveals that the rise in STEM employment primarily reflects increased intensity of STEM jobs within rather than between industries for men and women.[20] For men, this is the case in the first two decades, while in the last decade the growth between industries became more relevant. In contrast, for women the majority of the employment shifts came from within industry growth in STEM jobs in all three decades.

Panels A and B in Figure 4.3 further illustrate the overall change in employment shares by skill deciles between 1980 and 2010 and highlight the contribution of STEM employment (red colored bars).[21] It is apparent that employment growth in the top deciles is to a large extent

[20] Following Acemoglu and Autor (2011), I use a shift share decomposition of the form $\Delta E_{ot} = \sum_{i=1}^{13} \Delta E_{it} \lambda_{oi} + \sum_{o=1}^{120} \Delta \lambda_{oit} E_i$, where ΔE_{ot} is the change in the share of employment in occupation o over the time interval t, the first term on the RHS is the change in occupation o's share of employment due to changes in the industrial composition, the second term on the RHS is the change in the occupation o's employment share due to within-industry shifts. Moreover, ΔE_{it} is the change in the industry i's employment share in the time interval t, λ_{oit} is occupation o's mean share of industry i employment over the time interval, i.e., $\lambda_{oit} = (\lambda_{oit_1} - \lambda_{oit_0})/2$. $\Delta\lambda_{oit}$ is the change in occupation o's share of industry i employment in the time interval t, and E_i is the mean employment share of industry i over the time interval, i.e., $E_i = (E_{it_1} + E_{it_0})/2$. The decomposition is conducted for 120 occupations and 13 industry groups separately for each decade as well as the entire observation period.

[21] As it is common practice in the literature, I use each occupation's position in the wage distribution as a proxy for the skill percentile rank (e.g., Dustmann et al. 2009; Acemoglu and Autor 2011; Autor and Dorn 2013; Dauth 2014). To avoid picking up effects of a gender pay gap, I follow Dauth (2014) and maintain the same percentile ranks for STEM occupations between men and women by using for both genders the 1980 median wages of men as a proxy. I use Stata code made available online by David Autor and David Dorn (see Autor and Dorn (2013) for further details). However, instead of plotting smoothed changes in employment by skill percentiles based on a kernel-weighted local polynomial regression, I sum up the actual change in employment over each decile. This allows for a better illustration of the actual contribution of STEM jobs along the skill deciles.

Figure 4.3.: Observed Changes in Employment by Skill Deciles



Notes: Figure shows the percentage change in the employment share by skill deciles between 1980 and 2010. The 120 occupations are ranked by the median wages of men in 1980 and then grouped into 10 equally sized groups. Data source: SIAB-R 7510.

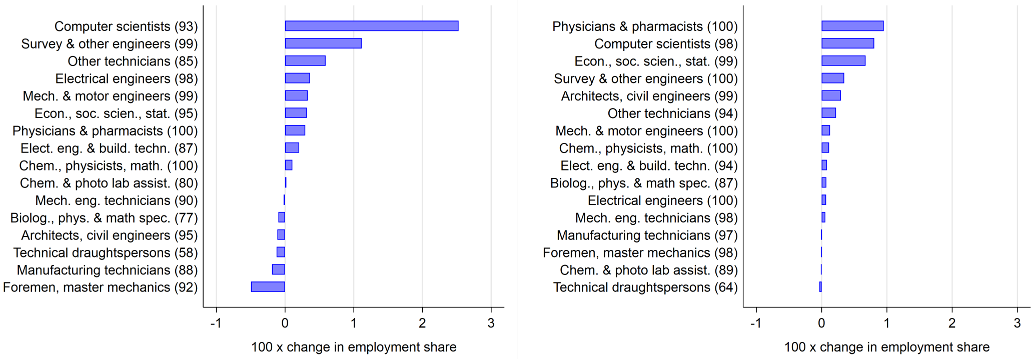
driven by growth of STEM employment. Panel A in Figure 4.3 underscores the employment polarization for men since the 1980s which has been detected in previous studies (e.g., Dustmann et al. 2009). Moreover, Panel B shows that women experienced educational upskilling with employment growth in the upper skill deciles and employment declines in the lower deciles. Interestingly, in contrast to Black et al. (2015), STEM occupations in Germany are mostly confined to the top deciles and therefore, growth in STEM employment did not counteract but rather catalyzed a trend in employment polarization.

Next, Panels A and B in Figure 4.4 illustrate the change in relative employment shares in STEM occupations between 1980 and 2010 together with the skill percentile rank of each occupation (in brackets). The increases in relative employment shares in the majority of STEM jobs highlight changes in the occupational distribution. The overall change in employment shares of STEM occupations adds up to 4.8 percentage points for men and 3.7 percentage points for women and corresponds to the observed changes in employment in STEM jobs from Figure 4.3. As expected, computer scientists experienced the largest increase in the relative employment share, with an increase of 2.5 percentage points for men and 0.8 percentage points for women. More importantly, STEM jobs are highly clustered in the top skill percentiles: 10 (men) and 16 (women) out of 16 STEM jobs are located in the top skill decile and none of the STEM occupations is ranked below the 58th skill percentile rank for men and the 64th skill percentile rank for women.

Turning to wages, Panels A and B in Figure 4.5 show the evolution of indexed mean log wages for STEM (plotted with squares) and non-STEM (plotted with triangles) workers for men and women. While both occupational groups profited from wage gains, wage growth was larger in STEM than in non-STEM jobs for both genders. Moreover, the figures reveal that male STEM workers already experienced larger wage gains in the 1980s (relative to non-STEM workers), while female STEM workers only received higher wages from 1995 onwards. By 2010, total wage growth in STEM jobs was approximately 14 percentage points

Figure 4.4.: Change in Relative Employment for STEM Occupations Between 1980 and 2010

A. Men B. Women



Notes: Each row presents 100 times the change in employment share between 1980 and 2010 for the indicated STEM occupation. Values in brackets indicate the skill percentile rank of each occupation based on the median wages of men in 1980. Data source: SIAB-R 7510.

for men and 9 percentage points for women larger than in non-STEM jobs.

In order to shed more light on the relationship between STEM and non-STEM wages, I next examine the evolution of the annual mean differences between log real wages in STEM and non-STEM jobs — which I call the *STEM premium* — between 1980 and 2010 in detail. I differentiate between an *unadjusted* and *adjusted* STEM premium. The former corresponds to yearly differences in the mean of wages of STEM and non-STEM workers, the latter further takes into account differences in the workers' skill-age profiles between the two occupational groups. To obtain estimates on yearly premiums together with standard errors, I estimate the following mincer-type equation by ordinary least squares (OLS) separately for each year t:

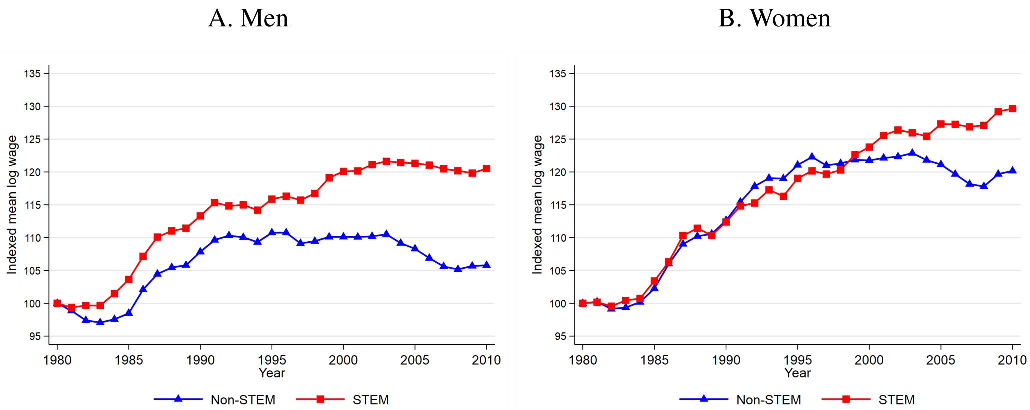
$$w_{it} = \alpha_t + x_{it}'\beta_t + s_{it}\gamma_t + \varepsilon_{it}, \tag{4.1}$$

where w_{it} is the log daily real wage of an individual i in year t, α_t is an annual constant, x_{it} is a vector of individual-level controls (i.e., linear, quadratic and cubic terms in age fully interacted with skill groups), s_{it} is a dummy variable that takes the value 1 if the individual i is employed in a STEM job in year t and 0 otherwise, and ε_{it} is the error term.[22] Accordingly, γ_t for $t = 1980, ..., 2010$ captures the annual wage differential between STEM and non-STEM workers. However, I note that the estimates of the regression only provide descriptive evidence, that is the STEM dummies capture correlations and do not allow for a causal interpretation.

For the unadjusted STEM premium, I only include a constant and the STEM dummy. Thus, the γ_i's reflect the mean difference in log wages between the two occupational groups. However, since STEM and non-STEM workers differ substantially in terms of their skill-age

[22]Note that the model assumes equal returns to the x_{it}'s for both STEM and non-STEM workers.

Figure 4.5.: Evolution of Indexed Mean Log Wages by Occupational Groups



Notes: Figure shows the indexed mean log wages of non-STEM and STEM workers. Wages of workers with occupation code 102 (for *doctors and pharmacists*) are imputed at the level of non-STEM/STEM-year cells between 1996 and 1998. See section 4.2.1 for additional details. Data source: SIAB-R 7510.

composition (see Table 4.1), differences in unadjusted mean wages could potentially reflect wage premiums for better educated workers. Moreover, if the share of medium-skilled and/ or high-skilled workers in the each occupational group evolves differently over time (as suggested by Figure 4.2), changes in the unadjusted STEM premium may again only be a reflection of an educational premium. Consequently, I estimate an adjusted STEM premium by controlling for the skill-age profiles of individuals such that the estimates of γ_i mirror annual STEM premiums conditional on the skill and age distributions.

Panel A and B in Figure 4.6 visualize the unadjusted (plotted with squares) and adjusted (plotted with triangles) STEM premiums by displaying the estimates for the coefficients on the STEM dummy ($\hat{\gamma}_t$) from 1980 to 2010.[23] Overall, the unadjusted STEM premium increased by 14 percentage points from 37% to 51% for men and by 9 percentage points from 36% to 45% for women between 1980 and 2010. By construction, this is equivalent to the differences in wage growth displayed in Figure 4.5. In contrast, the adjusted STEM premium rose by 10 percentage points from 20% to 30% for men and by 3 percentage points from 24% to 27% for women.[24] With respect to the time pattern, the figures reveal that both the unadjusted and adjusted STEM premium for men grew in the early 1980s, plateaued between 1985 and 1995, and from then on continuously increased until 2010. For women, the time pattern looks somewhat different with the premium decreasing between 1980 and 1995 and from then on steadily increasing until 2010 to levels above the initial level in 1980. Altogether, the results highlight that only part of the increasing STEM premium can be explained by skill-age profiles.[25]

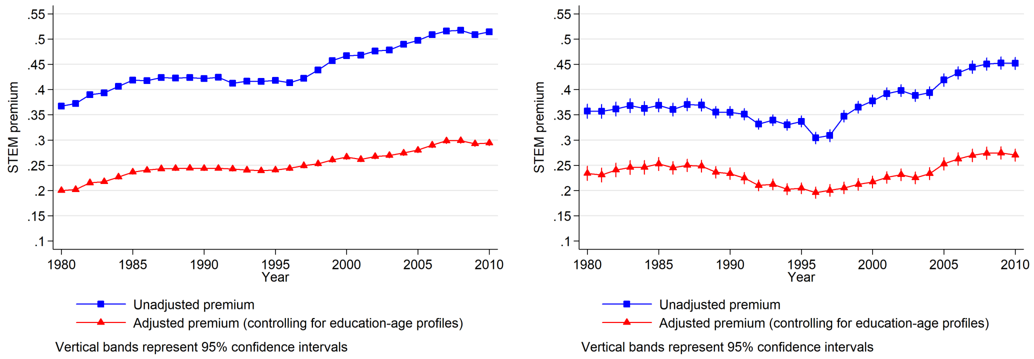
[23]The vertical bands represent the 95% confidence interval. Due to the precision of the estimation, the bands are not visible for men.

[24]Hanson and Slaughter (2013) estimate a composition-adjusted STEM premium for workers in the U.S. (combined for men and women) and show that the wage differential rose from 15% in 1967 to 22% in 2011. However, in contrast to my results, the STEM premium in the U.S. only grew in the 1970s and persisted with minor fluctuations at a level of 22% until 2011.

[25]Unreported results show that the time pattern of the STEM premiums look the same when I additionally

Figure 4.6.: Evolution of Unadjusted and Adjusted Mean Differences in Log Real Wages Between STEM and non-STEM Occupations

A. Men B. Women



Notes: Figure shows annual estimates of the coefficient on the STEM dummy s_{it} as well as the 95% confidence interval from OLS estimates of model (4.1). The squares refer to a model without additional controls. The triangles refer to a model that controls for linear, quadratic and cubic terms in age fully interacted with skill groups. Data source: SIAB-R 7510.

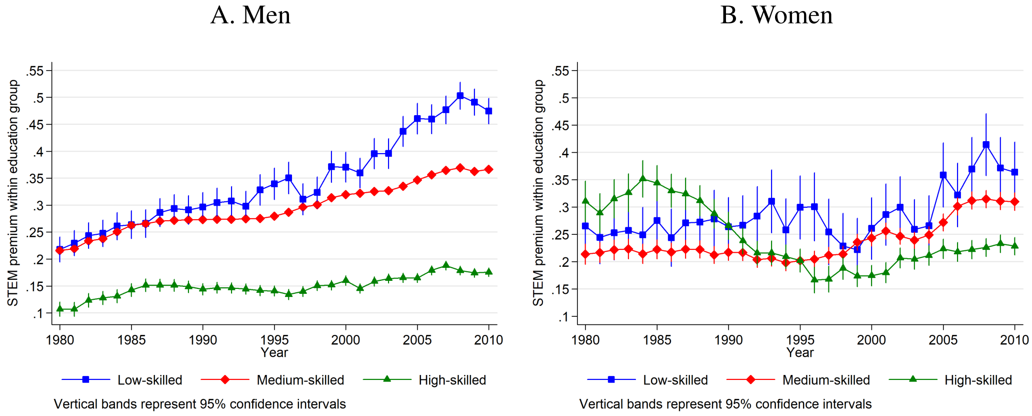
To further disentangle an education premium from the STEM premium, I re-estimate model (4.1) separately for each skill group controlling only for age profiles. Panel A and B in Figure 4.7 show the skill-specific STEM premiums for low-skilled (plotted with squares), medium-skilled (plotted with diamonds), and high-skilled (plotted with triangles) workers. For men, the STEM premium grew for all three skill groups, although low-skilled and medium-skilled workers benefited from higher levels in the STEM premium to begin with and, in addition, moved along a steeper growth curve. In contrast, growth in the skill-specific STEM premium for women only picked up in the mid-1990s. Before then, the evolution of the premium for low-skilled and medium-skilled women was flat and the STEM premium for high-skilled workers fell sharply between 1985 and 1995. Eventually, even by 2010, the premium for female high-skilled workers was still 9 percentage points below the highest level in 1985.[26]

Taken together, two conclusions emerge. First, skill-age profiles can only explain part of the rise in the STEM premium over time for both men and women which suggests that the STEM premium is not just a reflection of a growing education premium or observed compositional changes. Second, and most important, comparing the time pattern of Panel A and B in Figure 4.1 with the evolution of the STEM premium, we can see that increases

control for 13 industry and 183 local labor market fixed effects, although their inclusion reduces the level of the premium by approximately 4 to 5 percentage points for men and 4 to 7 percentage points for women. Moreover, I note that I estimated the unadjusted and adjusted STEM premium without imputing the number of *doctors and pharmacists* (occupation code 102) in the years 1996 to 1998 (see discussion in section 4.2.1). Most likely, this causes the drop in the unadjusted STEM premium for women in the years 1995 and 1996, but seems to have less of an impact once I control for the skill-age profiles.

[26]Note that medium- and high-skilled workers comprise 98% of STEM employment (see Table 4.1) since STEM occupations typically require some higher education. Nevertheless, about 2% of STEM workers are recorded without vocational training or university degree. This group is to a large extent composed of computer scientists (24 to 27%) and other technicians (14 to 17%).

Figure 4.7.: Evolution of Adjusted Mean Differences in Log Real Wages Between STEM and non-STEM Occupations Within Skill Groups



Notes: Figure shows annual estimates of the coefficient on the STEM dummy s_{it} as well as the 95% confidence interval from OLS estimates of model (4.1) controlling for linear, quadratic and cubic terms in age. The model is estimated separately for each skill group. Data source: SIAB-R 7510.

in wage inequality coincide with increases in the STEM premiums, in particular for men. For example, if I regress the difference between the 85th and 15th wage percentile on the adjusted STEM premium, I obtain highly significant coefficients for men (4.09 with a t-test of 20.83) and to a lesser extent also for women (2.12 with a t-test of 2.88). In addition, taking into account that STEM occupations are confined to the top of the wage distribution (see Figure 4.4) and employment and wages of STEM workers increased in both relative and absolute terms (see Figure 4.2 and 4.5), the coincident timing suggests that STEM jobs account for a notable fraction of the accelerated increase in the German wage inequality since mid-1990s, a point not addressed in the previous literature.

This is not to say that other coincident labor market developments had not contributed to the rising wage inequality over the same time period. Notably, Dustmann et al. (2014) present evidence that the more competitive market structure due to the fall of the Iron Curtain, coupled with the fiscal burden of German reunification, led to increasing deviations from industry-wide wage agreements since the mid-1990s. Ultimately, this has resulted in lower wages for many workers and contributed particularly to the decline in wages at the bottom of the wage distribution.[27]

Against the backdrop of rising STEM premiums, I apply two empirical approaches to assess potential drivers that underlie this evolution in the next two sections. First, I use a CES production function in a competitive labor market environment to relate changes in the STEM premium to supply and demand factors. Second, I use estimates from a model with additive worker and firm fixed effects from Card et al. (2013) and compare their distribution between STEM and non-STEM workers. Lastly, I apply Gelbach (2016)'s decomposition approach to quantify the contribution of unobservable worker and firm effects as well as observable

[27] For future research it might be interesting to further disentangle the role of institutional wage agreements for the wage-setting process of STEM workers.

time-varying worker characteristics to the STEM premium in different subintervals on the basis of an auxiliary wage model.

4.4. Supply and Demand Factors

4.4.1. Empirical Approach

To analyze the evolution of the STEM premium through the lens of supply and demand factors, I use a CES production function framework which allows for imperfect substitutability between STEM and non-STEM workers. The approach closely follows previous work by, e.g., Katz and Murphy (1992) and Goldin and Katz (2007) who apply this framework using low- and high-skilled workers as the two labor input factors.[28] Formally, I assume that aggregate output Q_t in each year t is generated by a CES production function depending on STEM (L_{S_t}) and non-STEM (L_{N_t}) labor supplies[29]:

$$Q_t = [\alpha(a_t L_{S_t})^{\rho} + (1-\alpha)(b_t L_{N_t})^{\rho}]^{\frac{1}{\rho}}, \tag{4.2}$$

where α is a technology parameter indexing the share of work allocated to STEM labor (i.e., extensive margin), a_t and b_t represent the STEM and non-STEM labor augmenting technological change (i.e., intensive margin), and ρ determines the aggregate elasticity of substitution between STEM and non-STEM labor, where $\sigma = 1/(1-\rho) \in (0,\infty)$. Increases in (a_t/b_t) reflect STEM-biased technological progress. If the two labor inputs are gross substitutes (i.e., $\sigma \geq 1$), a STEM-biased technological change will increase the STEM wage premium.

Under perfect competition, firms choose a level of each type of labor input such that marginal costs equal the marginal product. Consequently, I can relate the partial derivatives of Q_t with respect to STEM and non-STEM labor supplies to the relative wages by the following equation:

$$\ln\left(\frac{w_{S_t}}{w_{N_t}}\right) = \ln\left(\frac{\alpha}{1-\alpha}\right) + \frac{\sigma-1}{\sigma}\ln\left(\frac{a_t}{b_t}\right) - \frac{1}{\sigma}\ln\left(\frac{L_{S_t}}{L_{N_t}}\right). \tag{4.3}$$

Finally, under the assumption that there is a log linear increase in the demand for STEM workers over time coming from technology, the relationship between relative STEM/non-STEM wages and supplies in each year t can be expressed by the following linear regression

[28] Various versions of this modeling approach have been used to study the development and underlying drivers of skill premiums. Further studies focusing on the U.S. include Bound and Johnson (1992); Juhn et al. (1993); Card and Lemieux (2001); Acemoglu and Autor (2011); for studies focusing on West Germany see, e.g., Dustmann et al. (2009); Glitz and Wissmann (2017).

[29] Following the common interpretation in the literature on skill premiums, the production function represents a one-good market that depends on two types of workers: STEM and non-STEM. Alternatively, the model may represent an economy where the consumer's utility function is defined over two goods each produced by only one type of worker. Finally, the model may represent a mixture of the two former interpretations where different goods are produced in different sectors, and STEM and non-STEM workers are employed in both (Acemoglu and Autor 2011).

equation[30]:

$$\ln\left(\frac{w_{S_t}}{w_{N_t}}\right) = \gamma_0 + \gamma_1 t + \beta \ln\left(\frac{L_{S_t}}{L_{N_t}}\right) + \varepsilon_t. \tag{4.4}$$

I estimate equation (4.4) by OLS. That is, I regress the log of relative wages on a constant, a time dummy, and the log of relative supplies. The aggregate elasticity of substitution σ (which is equivalent to $-1/\beta$) is the parameter of interest and determines the relationship between changes in relative STEM/non-STEM supplies and STEM premiums.

I measure labor supplies L_{S_t} and L_{N_t} in efficiency units by determining productivity adjusted full-time equivalents of STEM and non-STEM workers. This is necessary because the model assumes that workers are perfect substitutes within each group of labor inputs (Glitz and Wissmann 2017). Moreover, I use composition constant wages to obtain the pure price for STEM and non-STEM workers net of any compositional differences.[31]

4.4.2. Results

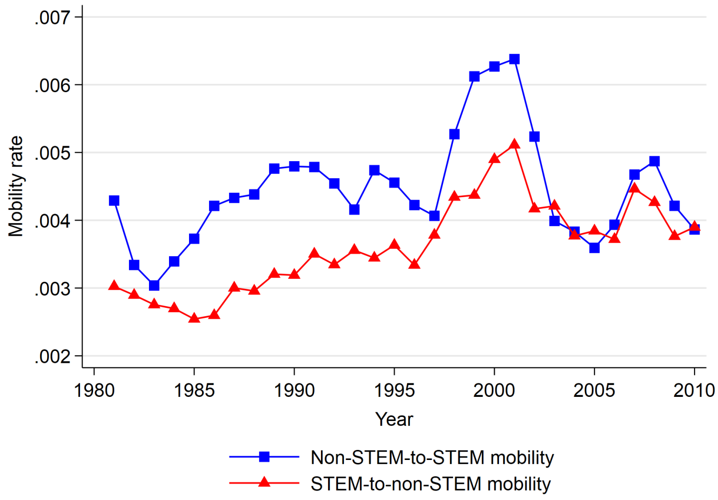
Before I turn to the estimation results of model (4.4), I want to briefly discuss the identification issues of the model. As Glitz and Wissmann (2017) point out, the identification of the elasticity of substitution σ relies on labor supplies to be predetermined. In the present case, this means that STEM and non-STEM labor supplies may not be correlated with any unobservables that also determine the STEM premium. Moreover, there may not be a contemporaneous correlation between the premiums and labor supplies. As opposed to the usual specification of the model along the skill dimension, the identification may be more problematic in my setting. In particular, I define STEM worker by the current job title, and thus occupational group changes allow for short-term adjustments in relative supplies. In contrast, the skill-specific labor supply is considered to be less elastic since education is viewed as a long-term investment into human capital (Glitz and Wissmann 2017). However, as Figure 4.8 shows, the mobility into and out of STEM jobs — defined as the number of movers between STEM and non-STEM jobs between two consecutive years divided by the total number of workers employed in year $t-1$ and t — is very low. The annual non-STEM-to-STEM mobility rates (plotted with squares) fluctuate between 0.0030 and 0.0064 resulting in an average rate of 0.0044 in the years 1980 to 2010. Likewise, the STEM-to-non-STEM mobility rates (plotted with triangles) fluctuate between 0.0025 and 0.0051 resulting in an average rate of 0.0036 during the same period. In other words, 0.44% of workers move into STEM and 0.36% out of STEM jobs between two consecutive years.[32] Consequently,

[30]Formally, the trend in the STEM-biased technological change is of the form: $\ln\left(\frac{\alpha}{1-\alpha}\right) + \frac{\sigma-1}{\sigma}\ln\left(\frac{a_t}{b_t}\right) = \gamma_0 + \gamma_1 t$.

[31]For details on the determination of labor supplies and composition constant wages, see Appendices C.1.2 and C.1.3.

[32]The average mobility rate for switches between skill groups during the same observation period is of a similar magnitude (i.e., 0.006). Note, however, that due to the imputation method for the education variable, skill changes can only be one-directional.

Figure 4.8.: Evolution of Mobility Rates Between non-STEM and STEM Occupations



Notes: Figure shows the annual mobility rates between the occupational group of non-STEM and STEM workers, defined as the number of movers from non-STEM to STEM jobs (plotted with squares) and movers from STEM to non-STEM jobs (plotted with triangles) divided by the total number of workers employed in two subsequent periods. Data source: SIAB-R 7510.

there is only limited scope for movements between occupational groups that would violate the identification of the model. In addition, I provide some evidence for the validity of the model's identification by excluding workers in their 20s. I do so because young workers exhibit much higher mobility rates between STEM and non-STEM occupations (1.2% for workers aged ≤ 31 vs. 0.4% for workers aged > 30). Accordingly, there is more scope for contemporaneously adjustments of this age group to wage differentials. Finally, if anything, I expect estimates of β to be upwardly biased due to a simultaneous reaction of labor supplies to the STEM premiums; that is, I consider the estimated elasticity of substitution $\hat{\sigma}$ defined as $-1/\hat{\beta}$ as an upper bound.[33]

Table 4.2 shows the OLS estimates for empirical model (4.4) for the combined sample of men and women.[34] Row 1 refers to the estimate on the relative labor supplies of STEM to non-STEM workers, $\hat{\beta} = -1/\hat{\sigma}$. Row 2 captures the log linear trend in the STEM-biased technological change. Column 1 shows the results for the baseline sample while columns 2 to 6 provide further sensitivity checks. The statistically significant estimate of -0.588 in column 1 suggests that the evolution of the STEM premium can be characterized by an elasticity of substitution between the two labor inputs of $\hat{\sigma} = 1.70$, meaning that the two occupational groups are gross substitutes. In addition, the positive estimate on the time trend in row 2 points to a STEM-biased technological change that increased the relative demand for STEM workers. Interestingly, Glitz and Wissmann (2017) find a similar elasticity of

[33]Glitz and Wissmann (2017) make the same argument with respect to the relationship between relative wages and supplies by skill groups.

[34]Except for column 4, all estimates in Table 4.2 are based on a sample that uses imputed employment and wages of occupation code 102 (for *doctors and pharmacists*) at the level of skill-age-gender-year cells between 1996 and 1998.

Table 4.2.: Estimation Results for CES Regression Model

	Baseline (1)	Non-Imputed (2)	Full Hetero. (3)	Med. workers excl. (4)	Full & Part-Time (5)	Aged ≥ 31 (6)
STEM/non-STEM	-0.588^{***}	-0.489^{**}	-0.429^{***}	-0.412^{**}	-0.450^{***}	-0.244^{***}
relative supply	(0.196)	(0.208)	(0.184)	(0.158)	(0.132)	(0.084)
Time trend	0.012^{***}	0.011^{***}	0.010^{***}	0.009^{***}	0.008^{***}	0.005^{***}
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)
Constant	-0.860^{*}	-0.744	-0.536^{**}	-0.504^{*}	-0.555	-0.032
	(0.439)	(0.468)	(0.412)	(0.363)	(0.295)	(0.171)
Observations	31	31	31	31	31	31
R-squared	0.739	0.833	0.853	0.815	0.736	0.669

Notes: Table shows results from OLS estimates of empirical model (4.4) for the pooled years 1980 to 2010. Employment and wages of workers with occupation code 102 (for *doctors and pharmacists*) are imputed at the level of skill-age-gender-year cells between 1996 and 1998. Estimates in column 1 are based on all full-time workers without trainees and one observation by worker-year (baseline sample). Estimate in column 2 are based on the baseline sample but use right censored (non-imputed) wages. Estimates in column 3 are based on the baseline sample but use imputed wages following the 'normal, full heteroscedasticity' imputation method (see Appendix C.1.1 for additional details). Estimates in column 4 are based on the baseline sample but exclude workers with the occupation code 102 (for *doctors and pharmacists*). Estimates in column 5 are based on all full- and part-time workers including trainees and multiple observations by worker-year weighted by days worked. Estimates in column 6 are based on the baseline sample but exclude workers aged ≤ 30. Significance levels: * 10%, ** 5%, and *** 1%. Data source: SIAB-R 7510.

substitution between college and non-college labor of 1.6 in West Germany between 1980 and 2008.[35] In contrast, Dustmann et al. (2009) find a somewhat larger estimate of 4.0 for the elasticity of substitution between high/medium-skilled to low-skilled men in West Germany between 1975 and 2004.

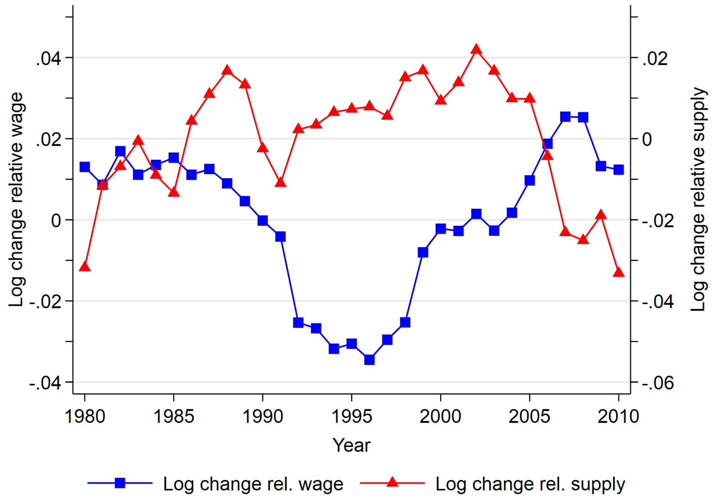
To visualize the relationship between relative wages and supplies, Figure 4.9 show the evolution of the STEM premium (plotted with squares) and relative STEM/non-STEM supplies (plotted with triangles) by year, each purged of the linear time trend. The negative relationship suggests that — within the boundaries of the model — decelerating (detrended) supplies of STEM workers were a driver for the growth in the STEM premium, in particular since the mid-1990s.

Columns 2 to 5 in Table 4.2 show that the main results are robust to a variety of sensitivity checks regarding the imputation of wages and the sample selection. Results in columns 2 and 3 are based on the same sample, but the estimation uses right censored (non-imputed) wages and wages based on the 'normal, full heteroscedasticity' method by Dustmann et al. (2009), respectively, in the determination of efficiency units and composition constant wages.[36] In column 4, I estimate the model for a sample that excludes the occupation code 102 (for *doctors and pharmacists*) to show that the results do not depend upon the imputation of

[35] Note that Glitz and Wissmann (2017) use a nested CES framework which further allows for imperfect substitutability between young and old workers within skill groups.

[36] See Appendix C.1.1 for details on the alternative imputation method.

Figure 4.9.: Detrended Changes in STEM/non-STEM Relative Supplies and Relative Wages



Notes: Figure shows residuals from separate OLS regressions of the STEM/non-STEM relative wages and relative supplies on a constant and a linear time trend. Wages are adjusted holding the skill-, age-, and gender composition constant. Supplies are measured in efficiency units. Employment and wages of workers with occupation code 102 (for *doctors and pharmacists*) are imputed at the level of skill-age-gender-year cells between 1996 and 1998. See Appendix C.1.2 and C.1.3 for additional details. Data source: SIAB-R 7510.

employment and wages for this occupation code.[37] Results in column 5 are based on a sample that closely follows Glitz and Wissmann (2017). Notably, the sample includes part-time employment weighted by 1/2 and 2/3 (9.5% of total days worked), workers undergoing training (3.1% of total days worked) and multiple job spells by the same worker during a year (7.6% of total days worked) to determine supply measures and, in addition, weights job spells and wages by the spell-length measured in days worked per year. This enlarged sample could be considered as a more precise measure of supplies and wages. However, as columns 2 to 5 show, my main estimates (in column 1) are robust to these alternative specifications. Overall, the estimates for the negative inverse of the elasticity of substitution range between -0.412 and -0.489 and are always statistically significant.

Finally, to provide a test for the validity of the model's identification, I exclude workers aged ≤ 30 from the sample and re-estimate the model once more (see discussion above). As shown in column 6, I again obtain a statistically significant estimate of β which is, however, significantly higher than the estimates in columns 2 to 5. Nevertheless, the estimate is of reasonable size implying an elasticity of substitution of 4.10. Taken together, the framework provides a simple yet intuitive interpretation of how the demand and supply of STEM and non-STEM workers shaped the distribution of relative wages between 1980 and 2010.

[37] As described in section 4.2.1, there is an unusual decline in the number of workers with the occupation code 102 (for *doctors and pharmacists*) between 1996 and 1998 compared to the neighboring years.

4.5. Worker and Firm Effects

While the CES framework provides a coherent model to relate the rise in STEM premiums to supply and demand factors under the assumption of imperfect substitutability between different labor inputs and a competitive labor market, the more recent literature takes into account firm-specific wage differentials that are generally not incorporated in the CES models (e.g., Card et al. 2013; Macis and Schivardi 2016; Song et al. 2016; Goldschmidt and Schmieder 2017).[38] Using models with additive fixed effects, these studies decompose wages into worker- and firm-specific components to explain developments in the wage structure over time. In what follows, I use the same framework and I tie changes in the distributional pattern of worker and firm effects to the evolution of the STEM premium. In section 4.5.1, I summarize the empirical approach of Card et al. (2013) to estimate worker and firm fixed effects and discuss potential caveats for the use of these effects in the present study. Moreover, I outline an application of Gelbach (2016)'s approach to decompose the contribution of unobservable worker and firm effects as well as observable time-varying worker characteristics to the STEM premium. In section 4.5.2, I discuss the results.

4.5.1. Empirical Approach

Worker and Firm Fixed Effects: To investigate the role of worker and firm fixed effects for the evolution of STEM wages, I use supplementary data provided by the IAB. The data contain estimates of unobserved worker and firm effects for male and female full-time workers by Card et al. (2013). Based on a well-established framework developed by Abowd et al. (1999), Card et al. (2013) estimate the following model with additive fixed effects for the West German labor market separately for men and women and four subintervals[39]:

$$w_{it} = \alpha_i + \psi_{J(i,t)} + x'_{it}\beta + r_{it}, \tag{4.5}$$

where w_{it} is the log daily real wage of worker i in year t, α_i is a worker fixed effect interpreted as a combination of skills and other (unobserved) factors that are rewarded equally across firms, $\psi_{J(i,t)}$ captures a firm fixed effect that constitutes a proportional pay premium (or discount) that is common to all workers at firm j, and β captures (observable) worker characteristics that constitute the time-varying component of an individual's earnings power.[40] Together, α_i and x_{it} constitute worker i's fully portable earnings capacity. Abowd et al. (1999) show that under the assumption of conditional random worker mobility across firms, firm and worker effects can be separately identified and estimated without bias using

[38] See Card et al. (2017) for a microeconomic foundation of the modeling framework on the grounds of frictional labor markets.

[39] See Card et al. (2015) for a detailed description of the model and its estimation.

[40] x_{it} includes an unrestricted set of year dummies as well as quadratic and cubic terms in age fully interacted with five education groups.

OLS.[41] Card et al. (2013) estimate the model for West Germany based on the full population of the IEB for the subintervals 1985 to 1991, 1990 to 1996, 1996 to 2002, and 2002 to 2009. Estimates of the worker and firm fixed effects are provided for the 2% subsample used in the present study. However, due to data protection regulation, the (weighted) firm effects are only available as 5%-percentile positions in the overall distribution of (weighted) firm effects while the worker effects are provided as both the 5%-percentile positions in the overall distribution of workers effects as well as the exact estimate of the worker effect.[42]

In section 4.5.2, I use the supplementary data to assess the role of worker and firm fixed effects for STEM and non-STEM workers in different subintervals.[43] However, I acknowledge that my approach raises some conceptual issues given the data that are available to me. Most importantly, model (4.5) does not control for the occupational group and estimated worker and/or firm effects absorb a potential *STEM effect*. Accordingly, a relatively high firm effect may describe two (complementary) phenomena: On the one hand, a large firm effect could represent a high-paying firm which corresponds to the usual interpretation in the literature. On the other hand, given STEM effects are positive, a relatively high firm effect might just as well be a reflection of a relatively higher share of STEM workers in a given firm. Moreover, also part of the worker effect might be contaminated by a STEM effect. Taken together, the discussion of the results in section 4.5.2 must be seen against the backdrop of these caveats. In particular, while I interpret the worker and firm effects as *true* effects representing low/high wage workers and low/high-paying firms, I recognize that further research is needed to distinguish the *true* worker and firm effects from potential *STEM effects*. In the following, I outline potential analyses that could be conducted in the future on the basis of sufficiently large linked-employer-employee datasets.

To begin with, one could augment model (4.5) with a time-varying STEM dummy (s_{it}) — defined by worker i's job title in year t — to partial out a market-wide STEM effect. In this case, mobility has to be exogenous conditional on worker effects, firm effects, the covariate index and, in addition, the STEM component. Alternatively, one could use a time constant assignment of workers to the occupational group of STEM and non-STEM, e.g., defined by the mode of an individual's job titles. In this case, model (4.5) is correctly specified, though the STEM effect is absorbed by the worker fixed effect. While this still poses a problem in terms of the interpretation of the worker and firm effects, it allows quantifying

[41] Formally, identification requires that $E[r_{it}|x_{it}, \alpha_i, \psi_{J(i,t)}] = 0$, where the error component in Card et al. (2013)'s version of the AKM model r_{it} comprises a match-specific wage component ($\eta_{iJ(i,t)}$), a unit root drift component (ζ_{it}), and a transitory error (ε_{it}). As Card et al. (2013) highlight, the most controversial assumption is that the residual is uncorrelated with the entire sequence of firm identifiers in a worker's employment history which precludes mobility based on the match-specific wage component. Card et al. (2013) provide several tests probing the exogenous mobility assumption and conclude that it appears approximately satisfied in the West German labor market. In particular, they show that the match-specific wage component is small, stable over time, and uncorrelated with the direction of worker's mobility between firms.

[42] The firm weights are constructed as the average number of worker-year observation in a firm divided by the number of years the firm is active. This is necessary to adjust for a left-skewness in the distribution of firm as large firms (with usually higher firm effects) are overrepresented in the SIAB-R.

[43] My analytical approach is inspired by Dauth et al. (2016) who estimate model (4.5) for reunified Germany separately for four five-year subintervals and conduct a descriptive analysis of the role of worker and firm effects for an *urban wage premium*.

the contribution of worker and firm fixed effects to the raw STEM premium in the spirit of Cardoso et al. (2016). They apply a Gelbach decomposition to quantify the proportion of the Portuguese gender pay gap due to a covariate index and worker, firm and occupation fixed effects. Importantly, in their approach the variable of interest is a gender dummy which is obviously not varying over time. In a similar fashion, estimates from model (4.5) could be used in a Gelbach decomposition on the time constant STEM dummy. Unfortunately, this approach is not feasible with the available data as it requires the exact estimates of all components of the model. However, as described below, I still follow Cardoso et al. (2016)'s approach by approximating Gelbach's decomposition for a time constant STEM dummy on the basis of an auxiliary wage model (see discussion below). Finally, a time constant definition of STEM workers would allow for applying the approach of Card et al. (2016) by combining a worker-firm fixed effect model with an Oaxaca-style decomposition to the STEM wage gap. That is, one could estimate model (4.5) separately for STEM and non-STEM workers and assess differences in the firm-specific wage component of the two occupational groups for the largest connected set of firms that employ both types of workers. Subsequently, an Oaxaca-style decomposition would allow for distinguishing between the explained part of the difference in the firm-specific pay premium (e.g., sorting effect) and an unexplained within firm component (e.g., efficiency wages or bargaining power).

Gelbach Decomposition: To approximately quantify the contribution of worker effects, firm effects and observed time-varying worker characteristics to the unadjusted STEM premium over time, I apply Gelbach (2016)'s decomposition approach to an auxiliary wage model in the first and last subinterval. In short, Gelbach's decomposition allows for an unequivocal partition of the fraction of a wage gap of interest due to different (groups of) variables between a basic model and a full model through the formula of an omitted variable bias formula (OVB) (see Appendix C.2 for a detailed derivation of Gelbach's decomposition formula). To apply the approach on the basis of model (4.5), I define a time constant STEM dummy which is 1 if the majority of a worker's job titles in a subinterval is considered as STEM and 0 otherwise.[44] This allows me to use the application of the Gelbach decomposition that is outlined in Cardoso et al. (2016) (see discussion above). In particular, I specify the full model as in equation (4.5) and define the associated basic model as follows:

$$w_{it} = \gamma_B s_i + r_{it}, \tag{4.6}$$

where w_{it} is the log daily real wage and s_i indicates worker i's STEM status in each subinterval (defined by the mode). Respectively, $\hat{\gamma}_B$ is an estimate of the unconditional STEM wage gap.

Even though a time constant STEM dummy in model (4.5) cannot be separately identified due to the inclusion of worker fixed effects, it is nevertheless possible to identify the

[44] I argue that this is a reasonable assumption given the very low mobility between STEM and non-STEM jobs (see Figure 4.8). Notably, using a worker's mode of his job tiles in each subinterval as opposed to the current job title changes the STEM dummy for 3.3% and 3.2% (first and last subinterval) of the worker-year observations for men and 1.1% and 1.3% (first and last subinterval) for women.

contribution of the worker effects, the firm effects and the covariate index to the STEM gap based on the following equation (Cardoso et al. 2016):

$$\hat{\gamma}_B = \hat{\delta}_\alpha + \hat{\delta}_\psi + \hat{\delta}_\beta, \tag{4.7}$$

where $\hat{\gamma}_B$ refers to the unadjusted STEM premium, $\hat{\delta}_\alpha$ is the contribution of the worker effects, $\hat{\delta}_\psi$ is the contribution of firm effects, and $\hat{\delta}_\beta$ is the contribution of the time-varying covariate index. Accordingly, the relative contribution of each wage component to the unconditional STEM premium is determined by the ratios $\hat{\delta}_\alpha / \hat{\gamma}_B$, $\hat{\delta}_\psi / \hat{\gamma}_B$, and $\hat{\delta}_\beta / \hat{\gamma}_B$. Note that if the allocation of workers to STEM and non-STEM jobs as well as to firms is random, $\hat{\gamma}_B$ would be zero. Thus, $\hat{\gamma}_B$ can be interpreted as the log point premium in wages that occurs for STEM workers due to the allocation of these workers to specific firms with specific worker effects and observable time-varying workers characteristics.

In the present study, I face the practical issue that I only have the 5%-percentile positions of firm effects (as opposed to the exact estimate) and, in addition, I do not have Card et al. (2013)'s exact estimate of the covariate index. As a workaround, I apply Gelbach's approach to an auxiliary wage model, acknowledging that this only provides an approximate quantification. To this end, I base the decomposition on an auxiliary wage model that controls for worker fixed effects, a series of dummy variables to control for workers' 5%-percentile positions of the firm fixed effects (based on CHK estimates), and a time-varying covariate index:

$$w_{it} = \alpha_i + \sum_{q=2}^{20} \tilde{\psi}_q F^q_{J(i,t)} + x'_{it}\beta + r_{it}, \tag{4.8}$$

where w_{it} is the log daily real wage, α_i is a worker fixed effect, each $F^q_{J(i,t)}$ for $q = 2, ..., 20$ represents a dummy variable that is 1 if an individual i's weighted CHK firm fixed effect falls into the qth 5%-percentile of the weighted CHK firm effects and 0 otherwise (with the 1st 5%-percentile as the reference category), and x_{it} is a covariate index.[45] As in model (4.5), a time constant STEM dummy is absorbed by the worker effect. Given that model (4.8) replicates the estimates of model (4.5) sufficiently well, an application of Gelbach's decomposition to model (4.8) allows for an approximate quantification of the contribution of worker effects ($\hat{\delta}_\alpha$), firm effects ($\hat{\delta}_{\tilde{\psi}}$) and observed worker characteristics ($\hat{\delta}_\beta$).[46]

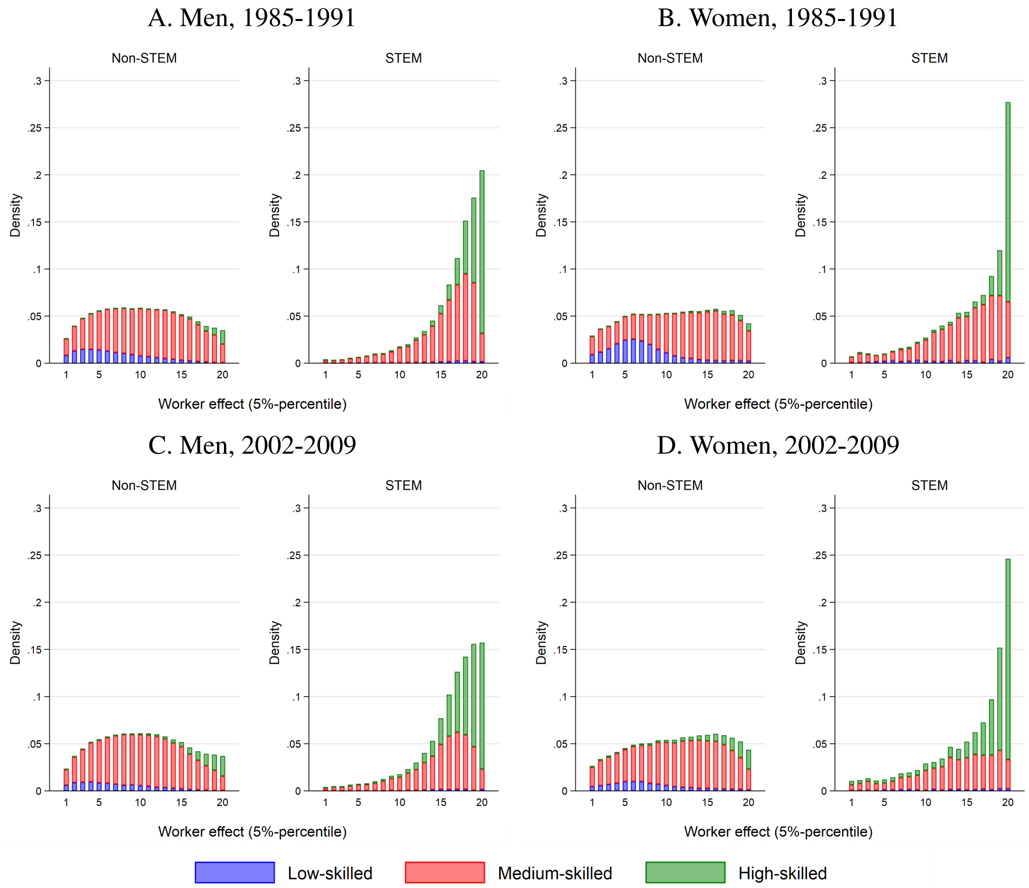
4.5.2. Results

Worker and Firm Fixed Effects: To begin with, Figure 4.10 illustrates the distribution of 5%-percentile positions of worker effects in the first (1985 to 1991, panels A and B) and

[45] x_{it} includes an unrestricted set of year dummies as well as quadratic and cubic terms in age fully interacted with the skill groups.

[46] Note that the contribution of the firm effects in the auxiliary wage model is denoted by $\hat{\delta}_{\tilde{\psi}}$ (instead of $\hat{\delta}_\psi$) to emphasize that it is based on the series of firm dummy covariates as given by model (4.8) as opposed to the exact estimates of firm effects as given by the model (4.5). For further details see Appendix C.2.

Figure 4.10.: Distribution of CHK Worker Effects by Occupational Groups



Notes: Figure shows densities of worker effects by 5%-percentiles separately for non-STEM and STEM workers in the first and last subinterval. The colors indicate the share of each skill group within each 5%-percentile. The histograms are based on all worker-year observations in a given subinterval. Data sources: SIAB-R 7510 and supplementary IAB data on CHK effects.

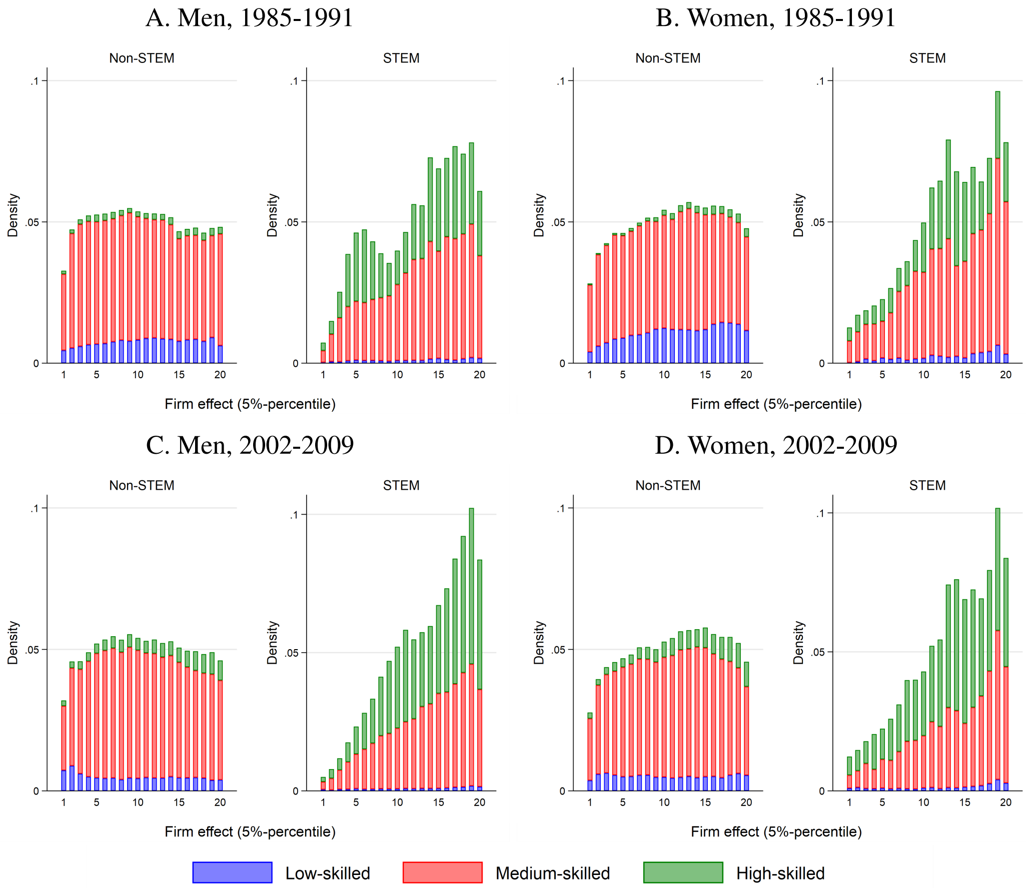
last (2002 to 2009, panels C and D) subinterval.[47] In each panel, the left histogram refers to non-STEM workers and the right histogram to STEM workers. The share of workers by skill group within each 5%-percentile is indicated by the bar color (i.e., low-skilled in blue, medium-skilled in red, high-skilled in green).

For both men and women, the distributions of STEM worker effects are clearly left-skewed.[48] In other words, the figures suggest that STEM workers are positively selected

[47] The histograms are based on all worker-year observations in a given subinterval, thus allowing for multiple observations of the same worker. This can be seen as an implicit weighting that accounts for the attachment of each worker to the labor market. Note, however, that the histograms look very similar in samples that include only one observation by worker-firm or one observation by worker (selected by the highest wage) in a given subinterval.

[48] Note that the densities of worker effects in the lowest 5%-percentiles are too low (see Appendix Figure C.1 for histograms of the combined sample of non-STEM and STEM workers). One potential reasons could be that my selection rules differ slightly from Card et al. (2013)'s sample that underlies the estimation of the fixed effects (see section 4.2.1). Furthermore, the selection of a 2% subsample in the SIAB-R may yield some deviations from a uniform distribution of worker effects. However, since the distributions of worker effects of the two occupational groups are sufficiently different, I do not see this short-coming as too worrisome with respect to the conclusions I draw.

Figure 4.11.: Distribution of Weighted CHK Firm Effects by Occupational Groups



Notes: Figure shows densities of weighted firm effects by 5%-percentiles separately for non-STEM and STEM workers in the first and last subinterval. The colors indicate the share of each skill group within each 5%-percentile. The histograms are based on all worker-year observations in a given subinterval. Data sources: SIAB-R 7510 and supplementary IAB data on CHK effects.

in terms of their time constant unobserved worker characteristics.[49] Moreover, for STEM workers, there is a weak reduction in the density at the highest 5%-percentile leading to a slightly more fat-tailed distribution over time. With respect to the skill group distribution within each 5%-percentile, the histograms show a monotonous increase in the relative share of high-skilled workers by 5%-percentiles for both non-STEM and STEM workers. Furthermore, the figures reveal that the distribution of worker effects for STEM workers is left-skewed for both medium-skilled and high-skilled, while for non-STEM workers this is only the case for the high-skilled.[50]

Next, turning to the distribution of firm effects, panels A and C of Figure 4.11 reveal a similar left-skewness for the firm effects of male STEM workers.[51] In addition, there is an

[49] With reference to the discussion in section 4.5.1, I acknowledge that the worker effect does not necessarily reflect the *true* worker effect, but could also (in parts) be a reflection of an unobserved *STEM effect*.

[50] Note that the shape of the distribution of worker effects for each skill group can be deduced by comparing the bars of each color separately.

[51] The histograms are based on the 5%-percentile position of the weighted firm effects (see discussion above). However, note that the densities of firm effects in the lowest and highest 5%-percentiles are too low (see Appendix

obvious trend for an increasing allocation of male STEM workers into high-paying firms over time.[52] While panel A shows a bimodal distribution where a considerable share of STEM workers is clustered around the 4th, 5th, and 6th 5%-percentile during the first subinterval (in addition to a large share of STEM workers located above the 15th 5%-percentile), panel C unveils an almost monotonous increase in the density of firm effects from the lower to the upper 5%-percentiles. Interestingly, the lower peak among STEM workers in the first subinterval (4th, 5th, and 6th 5%-percentile) seems to be driven by a bimodal distribution of firm effects among the high-skilled and is comprised to a large extent of STEM workers employed in education, social and healthcare facilities and public administration (on average 62%).

Next focusing on women, panels B and D in Figure 4.11 reveal that female STEM workers are as well clustered in high-paying firms. As for men, the distribution for female STEM workers in the first subinterval is bimodal, though the lower peak is less distinct, located around the 10th, 11th, and 12th 5%-percentile, and driven by bimodal distributions for both medium-skilled and high-skilled STEM workers. Moreover, the distribution of firm effects remains bimodal in the last subinterval and there is no clear evidence for an intensified allocation of women into high wage firms over time. Lastly, the lower peak (10th, 11th, and 12th 5%-percentile) consists in both subintervals on average to more than 50% of female STEM workers employed in education, social and healthcare facilities and public administration.

The results can be lined up well with findings of Goldschmidt and Schmieder (2017) on the effect of outsourcing on wages. They provide evidence for a substantial growth in domestic outsourcing of workers in food, cleaning, security and logistic (FCSL) since the early 1990s. Moreover, by using estimates based on model (4.5) for all West German full-time workers for the entire time period 1979 to 2009 separately for men and women, they show that an outsourcing event reduces the average firm effect of FCSL workers by about 10 percentage points for men and 7 percentage points for women, which translates into wage losses of similar size. In order to relate their analysis to the present study, panel A to D in Appendix Figure C.3 illustrate the distributions of firm effects of male and female FCSL workers in the first and last subinterval using a similar definition of FCSL jobs.[53] The

Figure C.2 for histograms of the combined sample of non-STEM and STEM workers). In addition to the reasons mentioned in footnote 51, the non-representativity of the SIAB-R with respect to the distribution of firms coupled with the weighting scheme for firm effects may constitute an additional caveat. However, since the distributions of firm effects of the two occupational groups are again sufficiently different, I do not see this short-coming as too worrisome with respect to the conclusions I draw.

[52]With reference to the discussion in section 4.5.1, the term *high-paying firm* is used in a slight abuse of terminology as the estimated firm effect does not necessarily reflect the *true* firm effect, e.g. firm-specific pay premium, but could also (in parts) be a reflection of positive STEM effects coupled with a relatively high share of STEM workers in a given firm.

[53]I identify FCLS workers by the occupation codes as described in Goldschmidt and Schmieder (2017). Note, however, that their list is based on the 330 occupations of the *KldB 1988* classification while the SIAB-R only differentiates between 120 occupations. Consequently, the identification of FCSL workers suffers from some measurement error in that four non-FCSL occupations are part of the FCSL definition used in the present study (see Appendix Table C.3 for details). Reassuringly, unreported figures for samples that exclude the affected SIAB-R occupation categories look very similar. Moreover, note that the SIAB-R does not allow to further

histograms are clearly right-skewed, and, more importantly, there is a shift in the densities to the left between the first and the last subinterval. This is consistent with Goldschmidt and Schmieder (2017)'s finding that FCSL workers are increasingly outsourced to firms with lower firm-specific wage premiums. Ultimately, FCSL workers could be regarded as a counterpart to STEM workers: While the former are outsourced to low wage firms, the latter are increasingly allocated at high wage firms.

It is important to emphasize that in the present setting, there are several channels that may lead to changes in the distribution of firm effects. Between different subintervals, a worker's firm effect can either change if the worker moves to a firm with a different firm effect or if the firm effect itself changes while the worker remains at the same firm (or both). This is different from Goldschmidt and Schmieder (2017) who estimate one firm effect for each firm based on data for the entire observation period. In consequence, they directly link outsourcing activities of FCLS jobs to moves between two firms with different firm effects and disregard any distributional changes in firm effects. While beyond the scope of this paper, an analysis of the importance of within firm versus across firm changes in STEM workers' firm effects may be a promising avenue for future research in this area.

To assess the degree of assortative matching — that is, the assignment of high wage workers to high wage firms — over time, Table 4.3 displays the rank correlations between each individual's 5%-percentile position of the worker effect and the 5%-percentile position of the weighted firm effect by subinterval. Panel A refers to a sample that includes all worker-year observations, Panel B and C refer to samples that only include one observation by worker-firm or one observation by worker, respectively. The table documents a distinct rise in the assortative matching of workers to firms for men (column 1) which is driven by both non-STEM and STEM workers (column 2 and 3). That is, the rank correlations continuously increase by each subinterval for both occupational groups (and sample selection criteria). Further taking into account the distributional changes shown above, this suggests that STEM workers moved to the upper part of the joint distribution of fixed effects while non-STEM workers moved to the lower end. Columns 4 to 6 show a similar pattern for women, though, the increases in the rank correlations are much smaller. Notably, women exhibit a somewhat higher assortativeness to begin with, but less growth of assortativeness over time, which confirms the visual impression that the women's distribution of firm effects is (compared to men) more left-skewed in the first subinterval and that there is no distinct increase in the allocation of women into high-paying firms over time.

Gelbach Decomposition: The increasing assortativeness of STEM workers may be an important driver for the rising STEM premium over time. To further elaborate on this, I apply Gelbach's decomposition approach to the auxiliary wage model (4.8) in the first and last subinterval. This provides an approximate quantification of the contribution of worker

identify outsourcing activities of FCSL workers which Goldschmidt and Schmieder (2017) define as either on-site outsourcing using worker flows or FCSL workers in business service firms. While the former method requires linked-employer-employee data, the latter approach is not feasible due to the broader industry classification in the SIAB-R.

Table 4.3.: Rank Correlations of 5%-Percentile Position of CHK Worker and Weighted CHK Firm Effects by Occupational Groups

	Men			Women		
	All (1)	Non-STEM (2)	STEM (3)	All (4)	Non-STEM (5)	STEM (6)
Panel A: All worker-year observations						
1985-1991	0.056	0.005	0.058	0.103	0.092	0.061
N	1,587,636	1,378,606	209,030	757,302	725,403	31,899
1990-1996	0.113	0.058	0.111	0.148	0.140	0.091
N	1,675,918	1,443,909	232,009	826,997	786,611	40,386
1996-2002	0.203	0.148	0.159	0.178	0.169	0.085
N	1,577,214	1,340,443	236,771	767,060	724,944	42,116
2002-2009	0.300	0.251	0.206	0.209	0.199	0.096
N	1,702,954	1,428,268	274,686	817,475	763,992	53,483
Panel B: One observation by worker-firm						
1985-1991	0.110	0.066	0.078	0.110	0.099	0.066
N	477,790	423,219	54,571	263,242	252,371	10,871
1990-1996	0.182	0.132	0.152	0.156	0.149	0.087
N	510,342	448,058	62,284	284,099	270,483	13,616
1996-2002	0.281	0.231	0.191	0.201	0.192	0.082
N	507,842	436,970	70,872	273,763	258,506	15,257
2002-2009	0.381	0.334	0.245	0.234	0.222	0.110
N	497,479	426,039	71,440	269,572	252,370	17,202
Panel C: One observation by worker						
1985-1991	0.040	−0.004	0.035	0.089	0.080	0.031
N	312,126	272,488	39,638	180,746	173,009	7,737
1990-1996	0.105	0.059	0.092	0.132	0.126	0.056
N	329,460	285,754	43,706	192,618	183,136	9,482
1996-2002	0.188	0.140	0.116	0.163	0.154	0.048
N	313,185	267,738	45,447	183,498	173,380	10,118
2002-2009	0.282	0.237	0.177	0.190	0.178	0.083
N	301,273	255,321	45,952	178,490	167,142	11,348

Notes: Table shows correlation coefficients between each individual's 5%-percentile position of worker and firm effect by occupational groups and subintervals. Estimates in Panel A are based on all worker-year observations in a given subinterval. Estimates in Panel B are based on one observation by worker-firm in each subinterval. Estimates in Panel C are based on one observation by worker in each subinterval. Data sources: SIAB-R 7510 and supplementary IAB data on CHK effects.

and firm effects as well as observable worker characteristics to the STEM premium and additionally allows to assess changes in the contribution of each component over time.

Before presenting the results, I show some evidence that my auxiliary wage model replicates (at least) the worker effects of Card et al. (2013) sufficiently well. Therefore, I regress estimated CHK worker effects — provided in the supplementary IAB data — on estimated worker effects as given by model (4.8) in each subinterval, expecting highly significant coefficients that are close to one in case of a good approximation. Appendix Table C.4 shows the OLS results for the first (columns 1 and 3) and last (columns 2 and 4) subinterval by gender. Reassuringly, I obtain coefficients that are indeed close to one (i.e., ranging between 0.94 and 1.02) and highly significant (with t-tests well over 1,000).

Table 4.4.: Gelbach Decomposition of Unadjusted Mean Difference in Log Real Wages Between STEM and non-STEM Occupations

	Men			Women		
	1985-1991 (1)	2002-2009 (2)	Δ Total (3)	1985-1991 (4)	2002-2009 (5)	Δ Total (6)
Unadjusted STEM premium ($\hat{\gamma}_B$)	0.41	0.50	0.08	0.35	0.41	0.06
Component attributable to ...						
Worker effects ($\hat{\delta}_\alpha$)	0.27	0.35	0.09	0.26	0.27	0.01
	(0.001)	(0.001)		(0.002)	(0.002)	
	64.9	*71.2*		*73.2*	*65.6*	
Firm effects ($\hat{\delta}_{\bar{\psi}}$)	0.05	0.11	0.06	0.07	0.10	0.03
	(0.000)	(0.000)		(0.001)	(0.001)	
	12.0	*21.8*		*20.8*	*24.4*	
Covariate index ($\hat{\delta}_\beta$)	0.10	0.04	−0.06	0.02	0.04	0.02
	(0.000)	(0.000)		(0.001)	(0.001)	
	23.0	*7.1*		*6.0*	*10.0*	
Number of worker-year observations	1,587,636	1,702,954		757,302	817,475	
Number of workers	312,126	301,273		180,746	178,490	

Notes: Table shows estimates from a Gelbach decomposition for model (4.8). Column 1 to 3 refer to the subinterval 1985 to 1991 and columns 4 to 6 to the subinterval 2002 to 2009. The contribution of the worker effects ($\hat{\delta}_\alpha$) is obtained by regressing the predicted worker effects on the STEM dummy. The contribution of the weighted firm effects ($\hat{\delta}_{\bar{\psi}}$) is obtained by summing over the series of firm dummy covariates for each worker, and regressing the compound firm index on the STEM dummy. The contribution of the covariate index ($\hat{\delta}_\beta$) is obtained by summing over all covariates for each worker, and regressing the compound covariate index on the STEM dummy (Gelbach 2016). See text for additional details. Entries in parentheses are robust standard errors for the components indicated in the row headings, retrieved from Gelbach (2016) Stata module *b1x2*. Entries in italic are percentage shares of the unadjusted STEM premium attributable to the components indicated in the row headings. All estimates are based on a time constant definition of the STEM dummy. Differences between the first and last subinterval do not necessarily add up to total change because of rounding. Data sources: SIAB-R 7510 and supplementary IAB data on CHK effects.

Moreover, the series of coefficient estimates that control for the 5%-percentile positions of weighted firm effects ($\hat{\bar{\psi}}_q$ for $q = 2,...,20$) is monotonically increasing from the lowest to the highest 5%-percentile in both subintervals and for both genders, which is consistent with the underlying meaning of the firm effects.

Turning to the results of the Gelbach decomposition, Table 4.4 shows the estimation results for men (left side) and women (right side) in the first and the last subinterval (columns 1, 2, 4, and 5) as well as the total change between the two intervals (columns 3 and 6). The top row refers to the estimates of the unadjusted STEM premiums.[54] The next rows display the approximate contribution of the worker effects, the firm effects, and the covariate index to the unadjusted STEM premium. The numbers in italics indicate the share of the contribution of each component to the total unadjusted premium.

[54]The unadjusted STEM premiums displayed in the table correspond to the employment-weighted averages of the annual premiums shown in panel A and B of Figure 4.6. Minor differences may emerge due to the time constant definition of the STEM dummy in the Gelbach approach. For instance, the unadjusted STEM premiums for the years 1985 to 1991 are 41.3% for men and 35.0% for women (values shown in Table 4.4). The counterparts on the basis of a time-varying STEM dummy are 41.9% for men and 35.7% for women (employment-weighted averages of estimates for the years 1985 to 1991 shown in Figure 4.6).

The unadjusted STEM premium for men is 41 log points throughout the first subinterval (1985 to 1991) and rises to 50 log points in the last subinterval (2002 to 2009). This implies a sizable increase of the raw wage gap of 8 log points (column 2 minus column 1 rounded to two decimal places). Column 1 further shows that the worker effect explains about two thirds of the premium in the first subinterval, while the firm effects account for 12% and covariate index for 23%.[55] Comparing these figures with the decomposition results for the last subinterval, the results show that the fraction explained by the firm effects increases considerably by about 10 percentage points. At the same time, the fraction explained by the worker effects increases only by about 6 percentage points, while the fraction explained by the covariate index decreases by the respective 16 percentage points. The mounting importance of the firm effects is also reflected by column 3, which displays the contribution of each component to the total change of the unadjusted STEM premium. Notably, the ratio between the *change* attributed to the firm effects to the *change* attributed to the worker effects of about 0.66 ($= 0.06/0.09$) is disproportionately high compared to the ratio of the *fraction* explained by the firm effects to the *fraction* explained by the worker effects, which is only 0.18 ($= 12.0/64.9$) in the first subinterval and increases to 0.31 ($= 21.8/71.2$) in the last subinterval.

Looking at women, columns 4 to 6 in Table 4.4 reveal a rise of the unadjusted STEM premium of 6 log points leading to a raw differential of 41 log points in the last subinterval. In contrast to men, the fraction of the wage gap explained by the worker and firm effects are substantially greater in the first subinterval. While the covariate index explains only 6% of the wage gap, the worker effects explain almost three quarters and the firm effects another 21%. Contrasting these results with estimates for the last subinterval in column 5, the results shown in the table reveal that the fraction explained by the firm effects increases for women as well, yet only by about 4 percentage points. Moreover, contrary to men, the fraction explained by the covariate index actually increases by 4 percentage points, while the fraction explained by the worker effects decreases by the corresponding 8 percentage points. Overall, the results for women suggest that also for them the firm-specific wage component has become more relevant, though to a lesser extent. Ultimately, the fraction of the unadjusted STEM premium that is explained by the firm effects for men and women converge to about one quarter in the last subinterval.

Taken together, the results of the Gelbach decomposition line up well with the visual impressions from above. For one thing, the increase in the fraction of the men's unadjusted STEM premium that is explained by firm effects reflects the right shift of the distribution of firm effects between the first and last subinterval and suggests that male STEM workers are increasingly allocating into high-paying firms over time. Moreover, the substantially higher contribution of the firm effects to the women's STEM premium in the first subinterval (relative to men) coupled with a smaller increase in the fraction of the female STEM premium

[55] Using Gelbach's approach to decompose the gender wage gap in Portugal, Cardoso et al. (2016) also find that the worker effect is the most relevant component, that is it accounts for the largest fraction of the wage differential between men and women (58%).

that is explained by firm effects reflect the more pronounced left-skewness of the firm effect distribution of female STEM workers in the first subinterval together with a less distinct change in the pattern of the firm effect distribution.

4.6. Conclusions

The rising wage inequality in the West Germany over the past 30 years has been and still is a heavily discussed topic in the economic literature (e.g., Dustmann et al. 2009; Card et al. 2013; Dustmann et al. 2014; Glitz and Wissmann 2017). At the same time, the last decade has been characterized by increasing interest in STEM occupations by both policy makers and researchers (see, among others, Black et al. 2015; Hanson and Slaughter 2016; Card and Payne 2017). In this paper, I bring these two topics together by documenting the evolution of STEM occupations and its role for the increase in wage inequality and, in addition, explore potential drivers underlying the growth of the wage differential between STEM and non-STEM workers — which I call the STEM premium.

Drawing on detailed administrative data for the West German labor market, I document an increase in STEM employment and wages in both absolute and relative terms for men and women. Moreover, the coinciding time pattern between the STEM premium (adjusted for skill-age profiles) and wage inequality suggests that STEM jobs contributed to the accelerated increase in the West German wage inequality since the mid-1990s.

Using a CES production function framework in a competitive market environment, I further show that the rise in the STEM premium can be explained by supply and demand factors under a STEM-biased technological change. This is interesting because it offers a refined perspective on the often discussed shortage of skilled workers, in particular in the field of STEM occupations. That is, within the boundaries of the framework, the results confirm a relative shortage of STEM workers, in particular since the mid-1990s.

Finally, using estimates from a model with additive worker and firm fixed effects, I show that both male and female STEM workers are clustered at the upper part of the distributions of the worker and firm effects. Moreover, there is a pronounced right shift of the distribution of firm effects for male STEM workers over time. An application of Gelbach's decomposition method further provides an approximate quantification of the contribution of unobserved worker and firm effects as well as observable time-varying worker characteristics to the STEM premium. Taking my results at face value, I find that the men's fraction of the STEM premium that is explained by firm effects increases by 10 percentage points, while the respective fraction for women increases by 3 percentage points. Overall, my findings are in line with previous studies in that they emphasize the rising importance of firm-specific rents in explaining the increase in wage inequality (Card et al. 2013; Goldschmidt and Schmieder 2017).

Technological changes will most likely further spur the demand for STEM workers given their technical expertise and scientific capabilities. Further studies on the role of STEM workers may provide useful guidance to gauge future consequences of this development and should therefore be a high priority on the research agenda. In particular, the increasing availability of sufficiently large linked-employer-employee datasets will allow more detailed investigations of sorting and bargaining aspects of STEM workers in relation to firms.

A. Appendix to Chapter 2: The Impact of Immigrants on Native Wages and Employment: An Analysis of Refugee Inflows in the Early 1990s

A.1. Sample Processing

A.1.1. Sample of Integrated Employment Biographies (SIAB 7510) 1975-2010

Our analysis is based on the Sample of Integrated Labour Market Biographies (SIAB 7510) 1975-2010 from the German Institute of Employment Research (IAB). For a detailed description, see vom Berge et al. (2013b) The sample provides individual level administrative data for a 2% sample of employees liable to social security contributions. From the initial sample, we delete workers in training ($stib = 0$), workers from home ($stib = 7$), employees in partial retirement ($erwstat = 103$), interns and student trainees ($erwstat = 105, 106$), and individuals with missing employment information. Marginally employed ($erwstat = 109, 209$) are not considered since they are not consistently observed prior to 1999. For each individual, we delete parallel employment spells ($level2 \neq 0$), and restrict the sample to worker spells covering June 30 each year. We consider the remaining sample as the labor force which is composed of employed persons ($erwstat \geq 101$), with part-time employees weighted by 1/2 ($stib = 8$) or 2/3 ($stib = 9$), and unemployed ($erwstat \leq 5$) persons.

Since the data do not record the place-of-birth, we follow Bonin (2005), D'Amuri et al. (2010), and Glitz and Wissmann (2017) using citizenship information to identify natives and immigrants. We impute missing or inconsistent values for nationality following the procedure of Drews et al. (2007). After filling gaps, we impute a constant citizenship at the person level assigning the modal value to all years; in case of ties, we assign German nationality. We regard individuals whose first employment spell is in East Germany as East German migrants. We note that since we only observe employment spells in East Germany after 1991, this method allows us to only partially identify East German migrants. For the identification of ethnic Germans (then called Aussiedler) who received German citizenship upon arrival), we follow Brücker and Jahn (2011), exploiting information on the receipt of any type of subsidy exclusively offered to ethnic Germans to support their integration (e.g., language courses; $lart = 1010, 1016, 1018, 1036, 1037, 1041, 1045, 1058$).

We impute missing or unknown values for education with an individual's information in the previous spell, if available, and impose that individuals cannot downgrade education. We group education levels into two skill groups: Unskilled are individuals with at most a high school degree (Abitur; $bild = 1, 3$), including individuals with missing information after the correction (2.9% of observations). This choice is guided by comparing average wages across education groups between 1985 and 1995. Skilled are individuals who completed an apprenticeship training or obtained a tertiary degree (e.g., Bachelor, Ph.D.; $bild = 2, 4, 5, 6$). We further impute missing or unknown regional information for employed and unemployed with the most recent information of the previous or next spell, if available.

For our wage analysis, we only keep full-time workers ($stib \neq 8, 9$) and deflate wages by the German Consumer Price Index (CPI), with 1995 as the base year. We consider wages that are above the annual social security contribution limits provided by the IAB (rounded to the nearest lower integer) as right censored. We then impute censored wages following the approach in Glitz (2012), which fits a series of annual Tobit models of the log wage on a dummy for immigrants, seven education groups (including missing and no education as separate categories), gender, and the commuting zone. We then replace each censored wage observation by an uncensored prediction based on the estimated parameters and a random draw from the associated truncated normal distribution. This imputation approach is by now common practice for this data and has been extensively evaluated (e.g., Dustmann et al. 2009; Glitz and Wissmann 2017).

A.1.2. Other Data Sources

For our instrument, we determine the shortest airline distance between the border of each local labor market and the eastern and southern border of West Germany using geospatial data from the Federal Office for Cartography and Geodesy.

To determine the share of immigrants in the population at the regional level in 1961, we use data from the 1961 Census provided by the Genesis Data Archive. The Stata file is available online at GESIS data archive, file name ZA2472. Bavaria and North Rhine-Westphalia are not included.

In addition, we use district level population data from the German Federal Statistical Office for years 1985-2001. For years 1985-1989, we converted scanned versions of the Statistical Yearbooks for Germany (available online) into machine-readable data sets. Since our analysis refers to June 30 each year, while the Statistical Yearbooks are dated to January 1, we use the arithmetic mean between two consecutive years as the population measure in our analysis.

We obtain task information on the occupational level from the Qualification and Career Survey (QCS) conducted by the German Federal Institute for Vocational Training (Bundesinstitut für Berufsbildung, BIBB). We use the 1985 wave and include all workers between 18 and 64 who work between 30 to 90 hours. We distinguish between simple and advanced occupations based on the task composition associated with each job. As in Prantl and Spitz-Oener (2014), we classify the following tasks as "advance": designing, making plans, restoring, servicing machines and equipping machines, and define "job complexity" as the average share of advanced tasks in an occupation.[1] We consider an occupation as advanced (simple) if the associated share is above the employment weighted median of the job complexity index.

A.2. Regulatory Framework

During our analysis period (1988-1993), numerous amendments to the laws regulating the access of asylum seekers to the West German labor market took place. In general, German law granted unrestricted access to the labor market to any asylum seeker conditional on a favorable asylum decision from the Federal Office for Migration and Refugees (*Bundesamt für Migration und Flüchtlinge*).[2] However, prior to an asylum decision, access to the labor market was restricted by waiting periods. With the introduction of the new Asylum law on January 15, 1987, asylum seekers were not allowed to work for a period of five years unless they came from former Eastern Bloc countries in which case the waiting period was limited to 12 months (*Gesetz zur Änderung asylverfahrensrechtlicher, arbeitserlaubnisrechtlicher und ausländerrechtlicher Vorschriften*) (Münch 1992; Thränhardt 2015) It was not until 1990/1991, when the waiting period for all asylum seekers was first harmonized to 12 months on December 21, 1990 and eventually banned on June 21, 1991 (*Neunte Verordnung zur Änderung der Arbeitserlaubnisverordnung*). On April 1, 1993, a revision of the asylum procedure reintroduced a waiting period for asylum seekers while residing in reception centers. The residence of asylum seekers in these centers was mandatory and lasted from a minimum of six weeks to a maximum of three months. Only three months later, on July 1, 1993, a fundamental renovation of the asylum law became effective, which introduced the principle of safe countries of origin and guidelines on third countries (which facilitated a repatriation of refugees to the EU border countries). These renovations led to a drastic reduction of immigration inflows to West Germany (Münch 1992).

A.3. Displacement Effect

In this section, we show how to interpret the employment coefficient from our main regression in different subpopulations. We focus on two aspects that impact on the magnitude of the coefficient: first, the share of the subpopulation in total employment; and second, the share of immigrants.

[1]The occupation variable contains 120 occupational categories, which are aggregates of 330 occupations from the German *Klassifikation der Berufe 1988* classification used in the SIAB data.

[2]Before 2005, this institution was called Federal Office for the Recognition of Foreign Refugees (*Bundesamt für die Anerkennung ausländischer Flüchtlinge*).

Let us denote by $N_{ir,t}$ the number of employed natives in group i, region r, and year t. The total number of employed natives is given by $N_{r,t} = \sum_i N_{ir,t}$ and the total number of immigrants is given by $I_{r,t}$. A simplified version of our regression reads as follows:

$$\frac{\Delta N_{ir,t}}{N_{ir,t-1}} = \delta_i \frac{\Delta I_{rt}}{N_{r,t-1} + I_{r,t-1}} \tag{A.1}$$

The left hand side represents the percentage change in employment of native skill group i between $t-1$ and t, and the right hand side represents the percentage inflow of all immigrants in total employment over the same time period. In our empirical analysis, we estimate equation (A.1) for various i, holding the right hand side constant. Rearranging a little bit yields the following:

$$\Delta N_{ir,t} = \delta_i \Delta I_{rt} \frac{N_{ir,t-1}}{N_{r,t-1} + I_{r,t-1}} \tag{A.2}$$

This expression represents the impact of immigrants on a per worker basis. For example, for $\delta_i = -1$, we have that, as one immigrant enters employment in a region ($\Delta I_{rt} = 1$) between $t-1$ and t, the number of native workers in group i who are displaced (or nor longer enter employment) is given by the $t-1$ share of that group in total employment (natives+immigrants). Put differently, if the share of native unskilled in total employment is about 15%, then a coefficient of -1 implies that 0.15 native unskilled workers are displaced. Conversely, if more than 0.15 unskilled are displaced, the coefficient is more negative. Note that in the case of the pooled sample, the share is just equal to the employment share of natives.

To illustrate, consider the following numerical example. Let $N_{r,t-1} = 90$, $I_{r,t-1} = 10$, $N_{Lr,t-1} = 18$ and $N_{Hr,t-1} = 72$. These figures imply an unskilled share among natives of about 20%. Substituting these numbers into equation (A.2) gives:

$$\Delta N_{ir,t} = \delta_i \Delta I_{r,t} \frac{18}{72 + 18 + 10} = \delta_i \Delta I_{r,t} \frac{18}{100} \tag{A.3}$$

Now, suppose that for every 10 immigrants finding employment in region r, 5 unskilled natives are displaced (into another region or into nonemployment), i.e. $\Delta I_{r,t} = 10$ and $\Delta N_{ir,t} = -5$. Then:

$$-5 = \delta_i \times 10 \times \frac{18}{100} \Leftrightarrow \delta_i = -2.78 \tag{A.4}$$

As indicated above, this numerical example shows that if certain groups exhibit employment losses larger than their employment share, this leads to coefficients larger than -1 in absolute values.

Now, consider the second issue, the share of immigrants. Using the equations from above, it is obvious that a larger number of immigrants in the initial period also increases the magnitude of the displacement coefficient. To illustrate, assume that $I_{r,t-1} = 0$, i.e., there are no immigrants in the base period. Then

$$\Delta N_{ir,t} = \delta_i \Delta I_{r,t} \frac{18}{72+18} = \delta_i \Delta I_{r,t} \frac{18}{90} \quad \text{(A.5)}$$

The same shock of $\Delta I_{rt} = 10$ and the same displacement of $\Delta N_{ir,t} = -5$ then implies:

$$-5 = \delta_i \times 10 \times \frac{18}{90} \Leftrightarrow \delta_i = -2.5 \quad \text{(A.6)}$$

which is exactly 90% (the employment share of natives) the size of the coefficient from an estimation that includes immigrants in the denominator.

A.4. Modeling Framework: An Equilibrium Model with Heterogeneous Labor Supply and Wage Rigidities

This appendix provides the basic modeling set-up along with all necessary calculations to derive the estimation equations relating native wage and employment responses to an immigrant-induced labor supply shock. While the basic model has been featured in many migration studies, the extensions we consider here — in particular, heterogeneous labor supply — were first introduced by Dustmann et al. (2017). In the following, we stick to their notation, but provide all additional steps required to arrive at the final equations. Throughout the following derivations, we focus on a single representative local labor market, and omit an area-subscript. Each local labor market is considered small, in that wage and employment adjustments in other areas do not affect the equilibrium outcome in the labor market under consideration (and vice versa). While an approximation, it is likely to be consistent with our empirical specification, where a region corresponds to the commuting zone.

A.4.1. Production

Assume that output Q is produced according to a Cobb-Douglas production function:

$$Q = AK^{\alpha}L^{1-\alpha} \quad \text{(A.7)}$$

where K denotes capital and L a CES aggregator of unskilled and skilled labor, g={U,S}

$$L = \left[\theta_U L_U^{\beta} + \theta_S L_S^{\beta}\right]^{\frac{1}{\beta}} \quad \text{(A.8)}$$

with $\theta_U + \theta_S = 1$, and $\sigma = \frac{1}{1-\beta}$ denoting the elasticity of substitution between the two skill groups ($\beta \leq 1$). We assume that natives (L_g^N) and immigrants (L_g^I) are perfect substitutes within each skill group, i.e., $L_g = L_g^N + L_g^I$. In our empirical specification, we do not attempt to assign immigrants to skill groups, so that the question whether immigrants are perfect or imperfect substitutes for natives will be part of the parameter that we estimate (in the model, it would show up as another nest within each skill group).[3]

A.4.2. Factor Demands

We begin by deriving the labor demand function. With firms being price takers on the product, labor, and capital markets (we normalize the price of the output good to unity), the optimal choice of labor and capital requires that marginal costs equal marginal products. For labor L_g, we obtain:

$$\begin{aligned} \frac{\partial Q}{\partial L_g} &= A(1-\alpha)L^{-\alpha}K^{\alpha}\frac{1}{\beta}\left[\theta_U L_U^{\beta}+\theta_S L_S^{\beta}\right]^{\frac{1-\beta}{\beta}}\theta_g\beta L_g^{\beta-1} \text{ for g=U, S} \\ &= A(1-\alpha)L^{-\alpha}K^{\alpha}L^{1-\beta}\theta_g L_g^{\beta-1} \stackrel{!}{=} w_g \\ \Rightarrow \log w_g &= \log(A(1-\alpha))+\alpha(\log K-\log L)+(\beta-1)(\log L_g-\log L)+\log\theta_g \end{aligned} \tag{A.9}$$

and for capital K, we get:

$$\begin{aligned} \frac{\partial Q}{\partial K} &= AL^{1-\alpha}\alpha K^{\alpha-1} \stackrel{!}{=} r \\ \Rightarrow \log r &= \log(\alpha A)+(\alpha-1)(\log K-\log L) \end{aligned} \tag{A.10}$$

Suppose that the local supply of capital depends on the local rental rate of capital, r, and the rental rates of capital in all other regions, **r'**, where **r'** is a vector. That is

$$K = h(r, \mathbf{r'}), \tag{A.11}$$

which implies an (own-)elasticity of capital supply

$$\frac{\partial K}{\partial r} = \frac{\partial h(r,\mathbf{r'})}{\partial r}\frac{r}{h(r,\mathbf{r'})} = \frac{1}{\lambda}. \tag{A.12}$$

Here, λ denotes the inverse elasticity, i.e., the percentage change of the rental rate r for a 1% change in the local capital stock.

In the next steps, we will totally differentiate the FOC's to derive the equilibrium response in terms of changes. We begin with the demand for capital, and then move on to the demand for labor. First, rewrite the capital supply elasticity in terms of logarithms:

$$\frac{\partial K}{\partial r}\frac{r}{K} = \frac{1}{\lambda} \Leftrightarrow \frac{\frac{\partial K}{K}}{\frac{\partial r}{r}}\frac{r}{K} = \frac{1}{\lambda} \Leftrightarrow \frac{d\log K}{d\log r} = \frac{1}{\lambda} \Leftrightarrow d\log r = \lambda d\log K \tag{A.13}$$

[3] It would make a difference, if we were to give the parameters of the model a structural interpretation, since then the assumption of perfect substitutability might bias the estimates of, e.g., the elasticity of substitution between skill groups.

Next, totally differentiate the FOC of capital demand:

$$\begin{aligned}
\log r &= \log(\alpha A) + (\alpha - 1)(\log K - \log L) \\
\Rightarrow d\log r &= (\alpha - 1)(d\log K - d\log L) \\
\Leftrightarrow \lambda d\log K &= (\alpha - 1)(d\log K - d\log L) \\
\Leftrightarrow d\log K(\lambda + 1 - \alpha) &= (1-\alpha)\, d\log L \\
\Leftrightarrow d\log K &= -\frac{\alpha - 1}{1-\alpha+\lambda} d\log L
\end{aligned} \tag{A.14}$$

The third line follows from substitution of (A.13).

A.4.3. Equilibrium

We start by deriving a firm's change in the demand for *native* workers (net of immigrant workers). In what follows, we assume (without loss of generality) that there are no immigrants in the baseline period. First, we note that total skill-specific employment is given by $L_g = L_g^N + L_g^I$ and total employment is given by $L = L_U + L_S$. In the baseline period, we have $L_g = L_g^N$ for g=U, S.

$$\begin{aligned}
dL_g = dL_g^N + dL_g^I \Leftrightarrow \frac{dL_g}{L_g} = \frac{dL_g^N}{L_g} + \frac{dL_g^I}{L_g} \Leftrightarrow \frac{dL_g}{L_g} &= \frac{dL_g^N}{L_g^N} + \frac{dL_g^I}{L_g^N} \\
\Leftrightarrow d\log L_g &= \frac{dL_g^N}{L_g^N} + \frac{\pi_g^I}{\pi_g^N} \underbrace{\frac{dL_g^I}{L^N}}_{=dI}
\end{aligned} \tag{A.15}$$

Here, we used the fact that we can express the total skill supply as $L_g = \pi_g^N L^N$ and $dL_g^I = \pi_g^I dL^I$ with $\pi_g^N = \frac{L_g^N}{L_U^N + L_S^N}$ and $\pi_g^N = \frac{L_g^I}{L_U^I + L_S^I}$, i.e., as fractions of the total supply of each nationality.

We next differentiate the CES aggregator of unskilled and skilled labor:

$$\begin{aligned}
L(L_U, L_S) &= \left[\theta_U L_U^\beta + \theta_S L_S^\beta\right]^{\frac{1}{\beta}} \\
\Rightarrow dL &= \frac{\partial L(L_U, L_S)}{\partial L_U} dL_U + \frac{\partial L(L_U, L_S)}{\partial L_S} dL_S \\
&= \frac{1}{\beta}\left[\theta_U L_U^\beta + \theta_S L_S^\beta\right]^{\frac{1-\beta}{\beta}} \theta_U \beta L_U^{\beta-1} dL_U + \frac{1}{\beta}\left[\theta_U L_U^\beta + \theta_S L_S^\beta\right]^{\frac{1-\beta}{\beta}} \theta_S \beta L_S^{\beta-1} dL_S \\
&= L^{1-\beta}\theta_U L_U^{\beta-1} dL_U + L^{1-\beta}\theta_S L_S^{\beta-1} dL_S \\
\Leftrightarrow \frac{dL}{L} &= L^{-\beta}\theta_U L_U^{\beta-1} dL_U + L^{-\beta}\theta_S L_S^{\beta-1} dL_S \\
&= \underbrace{\frac{\theta_U L_U^\beta}{\theta_U L_U^\beta + \theta_S L_S^\beta}}_{=s_U} \frac{dL_U}{L_U} + \underbrace{\frac{\theta_S L_S^\beta}{\theta_U L_U^\beta + \theta_S L_S^\beta}}_{=s_S} \frac{dL_S}{L_S} \\
\Leftrightarrow d\log L &= s_U d\log L_U + s_S d\log L_S
\end{aligned} \tag{A.16}$$

Note that $s_U + s_S = 1$, which we will use below. The last row expresses the percentage change in total labor inputs as an efficiency-weighted average of the percentage changes of each labor type. We can now substitute $d\log L_g = \frac{dL_g^N}{L_g^N} + \frac{\pi_g^I}{\pi_g^N} dI$ for g={U, S}, and simplify:

$$
\begin{aligned}
d\log L &= s_U d\log L_U^N + s_U \frac{\pi_U^I}{\pi_U^N} dI + s_S d\log L_S^N + s_S \frac{pi_S^I}{\pi_S^N} dI \\
\Leftrightarrow d\log L &= \underbrace{\left(s_U \frac{\pi_U^I}{\pi_U^N} + s_S \frac{\pi_S^I}{\pi_S^N} \right)}_{=\Pi} dI + s_U d\log L_U^N + s_S d\log L_S^N \qquad \text{(A.17)}
\end{aligned}
$$

Next, we turn to totally differentiate the labor demand function, given by equation (A.9):

$$
\begin{aligned}
d\log w_g &= \alpha \left(d\log K - d\log L \right) + (\beta - 1)\left(d\log L_g - d\log L \right) \\
&= \alpha \left(-\frac{\alpha - 1}{1 - \alpha + \lambda} d\log L - d\log L \right) + (\beta - 1)\left(d\log L_g - d\log L \right) \\
&= \underbrace{\alpha \left(-\frac{\alpha - 1}{1 - \alpha + \lambda} - 1 \right)}_{\frac{1-\alpha-1+\alpha-\lambda}{1-\alpha+\lambda} = -\frac{\lambda}{1-\alpha+\lambda}} d\log L + (\beta - 1)\left(d\log L_g - d\log L \right) \\
&= \underbrace{-\frac{\alpha\lambda}{1 - \alpha + \lambda}}_{=\varphi} d\log L + (\beta - 1)\left(d\log L_g - d\log L \right) \qquad \text{(A.18)}
\end{aligned}
$$

In the second row, we substitute for $d\log K$ the expression derived in equation (A.14). Now, we plug in (A.15) and (A.17), and solve for $d\log L_g^N$.

$$
\begin{aligned}
d\log w_g =& \varphi \left[\Pi dI + s_g d\log L_g^N + s_{g'} d\log L_{g'}^N \right] \\
&+ (\beta - 1) \left(\left[\frac{\pi_g^I}{\pi_g^N} dI + d\log L_g^N \right] - \left[\Pi dI + s_U d\log L_g^N + s_{g'} d\log L_{g'}^N \right] \right) \\
&= \varphi \Pi dI - (\beta - 1) \Pi dI + (\beta - 1) \frac{\pi_g^I}{\pi_g^N} dI \\
&+ \varphi s_{g'} d\log L_{g'}^N - (\beta - 1) s_{g'} d\log L_{g'}^N \\
&+ \varphi s_g d\log L_g^N + (\beta - 1) d\log L_g^N - (\beta - 1) S_g d\log L_g^N \qquad \text{(A.19)}
\end{aligned}
$$

where the last three rows each contain all terms related to one of the three key variables. This can be simplified to yield:

$$
\begin{aligned}
d\log L_g^N \left[\varphi s_g + (\beta - 1)(1 - s_g) \right] =& d\log w_g - \left((\varphi - (\beta - 1)) \Pi + (\beta - 1) \frac{\pi_g^I}{\pi_g^N} \right) dI \\
&- (\varphi - (\beta - 1)) s_{g'} d\log L_{g'}
\end{aligned}
$$

$$
\begin{aligned}
&= \frac{1}{\varphi s_g + (\beta-1)(1-s_g)} d\log w_g \\
&- \frac{(\varphi-(\beta-1))\Pi + (\beta-1)\frac{\pi_g^I}{\pi_g^N}}{\varphi s_g + (\beta-1)(1-s_g)} dI \\
&- \frac{(\varphi-(\beta-1))s_{g'}}{\varphi s_g + (\beta-1)(1-s_g)} d\log L_{g'}
\end{aligned} \tag{A.20}
$$

This is equation A.7 from the Online Appendix of DSS. It is a function that depends on the own wage, the immigrant shock, and the labor supply of the other skill group, which itself depends on its own wage, the immigrant shock, and the labor supply of this skill group. To solve this means to derive an equation that depends only on wages (or employment) and the immigrant shock. Therefore, we now replace g by U and g' by S, and insert the expression for S into the corresponding expression for U:

$$
\begin{aligned}
d\log L_U^N =& \frac{1}{\varphi s_U + (\beta-1)\underbrace{(1-s_U)}_{s_S}} d\log w_U - \frac{(\varphi-(\beta-1))\Pi + (\beta-1)\frac{\pi_U^I}{\pi_U^N}}{\varphi s_U + (\beta-1)\underbrace{(1-s_U)}_{=s_S}} dI \\
& - \frac{(\varphi-(\beta-1))s_S}{\varphi s_U + (\beta-1)\underbrace{(1-s_U)}_{=s_S}} \left[\frac{1}{\varphi s_S + (\beta-1)\underbrace{(1-s_S)}_{=s_U}} d\log w_S \right. \\
& \left. - \frac{(\varphi-(\beta-1))\Pi + (\beta-1)\frac{\pi_S^I}{\pi_S^N}}{\varphi s_S + (\beta-1)\underbrace{(1-s_S)}_{=s_U}} dI - \frac{(\varphi-(\beta-1))s_S}{\varphi s_U + (\beta-1)\underbrace{(1-s_U)}_{=s_S}} d\log L_U^N \right]
\end{aligned} \tag{A.21}
$$

With all $1-s_g$ replaced by $s_{g'}$, we next collect all $d\log L_U^N$ terms on the LHS:

$$
\begin{aligned}
& d\log L_U^N - \frac{\varphi s_S - (\beta-1)s_S}{\varphi s_U + (\beta-1)s_S} \frac{\varphi s_U - (\beta-1)s_U}{\varphi s_S + (\beta-1)s_U} d\log L_U^N \\
=& \frac{1}{\varphi s_U + (\beta-1)s_S} d\log w_U - \frac{\varphi s_S - (\beta-1)s_S}{\varphi s_U + (\beta-1)s_S} \frac{1}{\varphi s_S + (\beta-1)s_U} d\log w_S \\
+& \frac{\varphi s_S - (\beta-1)s_S}{\varphi s_U + (\beta-1)s_S} \frac{(\varphi-(\beta-1))\Pi + (\beta-1)\frac{\pi_S^I}{\pi_S^N}}{\varphi s_S + (\beta-1)s_U} dI \\
-& \frac{(\varphi s_S-(\beta-1))\Pi + (\beta-1)\frac{\pi_U^I}{\pi_U^N}}{\varphi s_U + (\beta-1)s_S} dI
\end{aligned} \tag{A.22}
$$

Now, multiply by $\varphi s_U + (\beta-1)\,s_S$ and simplify a little bit:

$$
\begin{aligned}
&d\log L_U^N\left(\varphi s_U+(\beta-1)\,s_S-\frac{(\varphi s_S-(\beta-1)\,s_S)\,(\varphi s_U-(\beta-1)\,s_U)}{\varphi s_S+(\beta-1)\,s_U}\right)\\
&\qquad = d\log w_U-\frac{\varphi s_S-(\beta-1)\,s_S}{\varphi s_S+(\beta-1)\,s_U}d\log w_S\\
&\qquad +\frac{\varphi s_S-(\beta-1)\,s_S}{\varphi s_S+(\beta-1)\,s_U}\left[(\varphi-(\beta-1))\,\Pi+(\beta-1)\,\frac{\pi_S^I}{\pi_S^N}\right]dI\\
&\qquad -\left[(\varphi s_S-(\beta-1))\,\Pi+(\beta-1)\,\frac{\pi_U^I}{\pi_U^N}\right]dI\\
&\Leftrightarrow d\log L_U^N\left((\varphi s_U+(\beta-1)\,s_S)\,(\varphi s_S+(\beta-1)\,s_U)-(\varphi s_S-(\beta-1)\,s_S)\,(\varphi s_U-(\beta-1)\,s_U)\right)\\
&\qquad =(\varphi s_S+(\beta-1)\,s_U)\,d\log w_U-(\varphi s_S-(\beta-1)\,s_S)\,d\log w_S\\
&\qquad +(\varphi s_S-(\beta-1)\,s_S)\left[(\varphi-(\beta-1))\,\Pi+(\beta-1)\,\frac{\pi_S^I}{\pi_S^N}\right]dI\\
&\qquad -(\varphi s_S+(\beta-1)\,s_U)\left[(\varphi-(\beta-1))\,\Pi+(\beta-1)\,\frac{\pi_U^I}{\pi_U^N}\right]dI
\end{aligned}
\tag{A.23}
$$

To simplify this expression, we have to work on the coefficient pre-multiplying $d\log L_U^N$ and dI. We begin with dI, i.e., the last two rows:

$$
\begin{aligned}
&[(\varphi s_S-(\beta-1)\,s_S)\,(\varphi-(\beta-1))\,\Pi-(\varphi s_S+(\beta-1)\,s_U)\,(\varphi-(\beta-1))\,\Pi]\\
&+\left[(\varphi s_S-(\beta-1)\,s_S)\,(\beta-1)\,\frac{\pi_S^I}{\pi_S^N}-\left(\varphi s_S-(\beta-1)\,\underbrace{s_U}_{1-s_S}\right)(\beta-1)\,\frac{\pi_U^I}{\pi_U^N}\right]dI\\
&=\left[(\varphi-(\beta-1))\,\Pi\left(\underbrace{\varphi s_S-\varphi s_S}_{=0}-(\beta-1)\,s_S-(\beta-1)\,s_U\right)\right]\\
&+\left[(\varphi s_S-(\beta-1)\,s_S)\,(\beta-1)\,\frac{\pi_S^I}{\pi_S^N}-(\varphi s_S-(\beta-1)\,s_S+(\beta-1))\,(\beta-1)\,\frac{\pi_U^I}{\pi_U^N}\right]dI\\
&=\left[-(\varphi-(\beta-1))\,\Pi\,(\beta-1)\left(\underbrace{s_S+s_U}_{=1}\right)\right]\\
&+\left[(\varphi s_S-(\beta-1)\,s_S)\,(\beta-1)\,\frac{\pi_S^I}{\pi_S^N}-(\varphi s_S-(\beta-1)\,s_S\,(\beta-1))\,\frac{\pi_U^I}{\pi_U^N}-(\beta-1)^2\,\frac{\pi_U^I}{\pi_U^N}\right]dI\\
&=[-(\varphi-(\beta-1))\,\Pi\,(\beta-1)]\\
&+\left[(\varphi s_S-(\beta-1)\,s_S)\,(\beta-1)\left(\frac{\pi_S^I}{\pi_S^N}-\frac{\pi_U^I}{\pi_U^N}\right)-(\beta-1)^2\,\frac{\pi_U^I}{\pi_U^N}\right]dI\\
&=\left[-(\varphi-(\beta-1))\left(s_U\frac{\pi_U^I}{\pi_U^N}+s_S\frac{\pi_S^I}{\pi_S^N}\right)(\beta-1)\right]\\
&+\left[(\varphi s_S-(\beta-1)\,s_S)\,(\beta-1)\left(\frac{\pi_S^I}{\pi_S^N}-\frac{\pi_U^I}{\pi_U^N}\right)-(\beta-1)^2\,\frac{\pi_U^I}{\pi_U^N}\right]dI
\end{aligned}
$$

$$
\begin{aligned}
&=\Bigg[-\varphi\left(\beta-1\right)\left(s_U\frac{\pi_U^I}{\pi_U^N}+s_S\frac{\pi_S^I}{\pi_S^N}\right)+\left(\beta-1\right)^2\left(s_U\frac{\pi_U^I}{\pi_U^N}+s_S\frac{\pi_S^I}{\pi_S^N}\right)-\left(\beta-1\right)^2\frac{\pi_U^I}{\pi_U^N}\\
&\quad+\left(\varphi-\left(\beta-1\right)\right)s_S\left(\beta-1\right)\left(\frac{\pi_S^I}{\pi_S^N}-\frac{\pi_U^I}{\pi_U^N}\right)\Bigg]dI\\
&=\Bigg[-\varphi\left(\beta-1\right)\left(s_U\frac{\pi_U^I}{\pi_U^N}+s_S\frac{\pi_S^I}{\pi_S^N}\right)+\left(\varphi\left(\beta-1\right)\right)\left(s_S\frac{\pi_S^I}{\pi_S^N}-s_S\frac{\pi_U^I}{\pi_U^N}\right)\\
&\quad-\left(\beta-1\right)^2\left(s_S\frac{\pi_S^I}{\pi_S^N}-s_S\frac{\pi_U^I}{\pi_U^N}\right)+\left(\beta-1\right)^2\left(s_U\frac{\pi_U^I}{\pi_U^N}+s_S\frac{\pi_S^I}{\pi_S^N}-\frac{\pi_U^I}{\pi_U^N}\right)\Bigg]dI\\
&=\Bigg[-\varphi\left(\beta-1\right)\underbrace{\left(s_U\frac{\pi_U^I}{\pi_U^N}+s_S\frac{\pi_S^I}{\pi_S^N}-s_S\frac{\pi_S^I}{\pi_S^N}+s_S\frac{\pi_U^I}{\pi_U^N}\right)}_{=(s_U+s_S)\frac{\pi_U^I}{\pi_U^N}=\frac{\pi_U^I}{\pi_U^N}}\\
&\quad+\left(\beta-1\right)^2\left(s_U\frac{\pi_U^I}{\pi_U^N}+s_S\frac{\pi_S^I}{\pi_S^N}-\frac{\pi_U^I}{\pi_U^N}-s_S\frac{\pi_S^I}{\pi_S^N}+s_S\frac{\pi_U^I}{\pi_U^N}\right)\Bigg]dI\\
&=\Bigg[-\varphi\left(\beta-1\right)\frac{\pi_U^I}{\pi_U^N}+\left(\beta-1\right)^2\underbrace{\left(\frac{\pi_U^I}{\pi_U^N}-\frac{\pi_U^I}{\pi_U^N}\right)}_{=0}\Bigg]dI\\
&=-\varphi\left(\beta-1\right)\frac{\pi_U^I}{\pi_U^N}dI
\end{aligned}
\tag{A.24}
$$

Now, let us consider the coefficient pre-multiplying the LHS:

$$
\begin{aligned}
&\left(\left(\varphi s_U+\left(\beta-1\right)s_S\right)\left(\varphi s_S+\left(\beta-1\right)s_U\right)-\left(\varphi s_S-\left(\beta-1\right)s_S\right)\left(\varphi s_U-\left(\beta-1\right)s_U\right)\right)\\
=&\varphi^2 s_U s_S+\varphi s_U\left(\beta-1\right)s_U+\left(\beta-1\right)s_S\varphi s_S+\left(\beta-1\right)^2 s_S s_U\\
&-\varphi^2 s_U s_S+\varphi s_S\left(\beta-1\right)s_U+\left(\beta-1\right)s_S\varphi s_U-\left(\beta-1\right)^2 s_S s_U\\
=&\varphi\left(\beta-1\right)s_U^2 s_U+\varphi\left(\beta-1\right)s_S^2+2\varphi\left(\beta-1\right)s_S s_U\\
=&\varphi\left(\beta-1\right)\left(s_U+s_S\right)^2\\
=&\varphi\left(\beta-1\right)
\end{aligned}
\tag{A.25}
$$

Inserting expression (A.24) and (A.25) into equation (A.23) and rearranging terms gives:

$$
\begin{aligned}
&\varphi\left(\beta-1\right)d\log L_U^N\\
&\quad=\left(\varphi s_S+\left(\beta-1\right)s_U\right)d\log w_U-\left(\varphi s_S-\left(\beta-1\right)s_S\right)d\log w_S-\varphi\left(\beta-1\right)\frac{\pi_U^I}{\pi_U^N}dI\\
\Leftrightarrow\quad &d\log L_U^N=\frac{\left(\varphi s_S+\left(\beta-1\right)s_U\right)}{\varphi\left(\beta-1\right)}d\log w_U-\frac{\left(\varphi s_S-\left(\beta-1\right)s_S\right)}{\varphi\left(\beta-1\right)}d\log w_S-\frac{\pi_U^I}{\pi_U^N}dI
\end{aligned}
\tag{A.26}
$$

Finally, to arrive at key equation (2) of the main text of DSS, we only need to divide by dI. The final expression gives the response of native labor to an immigrant-induced change in labor supply as a function of the own wage, the other skill group's wage, and the ratio of skill intensities of immigrants and natives. For $\beta < 1$, the first term on the RHS is unambiguously negative, i.e., employment and wages of a given skill group move in opposite directions, whereas the second term might be positive or negative, depending on the slope of the aggregate demand curve, φ. This slope depends on the capital share in output, α and the degree of capital mobility, λ. The last term shows that a positive immigrant shock will reduce native employment, and this reduction is more negative, the more unskilled immigrants are relative to natives.

A.5. Shift Share Instruments for the German Labor Market

The shift share instrument is widely used in the spatial correlation literature on immigration to deal with the endogeneity of the regional settlement of newly arriving immigrants. In what follows, we describe the implementation of two versions of the shift share instrument, both motivated by the idea that newly arriving immigrants tend to settle in regions in which other immigrants already settled earlier, and that the settlement decision of earlier immigrants is uncorrelated with current demand shocks. While the simple version of the instrument only considers the pattern of the overall immigrant settlement (see, e.g., Altonji and Card 1991), the more complex version further takes into account the source countries in the construction of the instrument (see, e.g., Card, 2001, 2007, 2009; Glitz, 2012; Smith, 2012; Peri and Sparber, 2009; Dustmann and Glitz, 2015).

In the simple version of the shift share instrument, we predict the immigrant inflow into a local labor market based on the foreign population density in some initial period to instrument the actual region level changes in immigrant population shares. Formally,

$$\Delta\tilde{I}^{Pop}_{r,88-93} = \gamma_{r,t_0}\frac{I^{Pop}_{93} - I^{Pop}_{88}}{(N^{Pop}_{r,88} + I^{Pop}_{r,88})} \tag{A.27}$$

where $\gamma_{r,t_0} = I^{Pop}_{r,t_0}/I^{Pop}_{t_0}$ denotes the share of foreigners in the population that resides in region r in some initial period t_0, $I^{Pop}_{93} - I^{Pop}_{88}$ is the nationwide net inflow of immigrants between 1988 and 1993, and $Pop_{r,88}$ is the population in region r in 1988. The initial year t_0 in our example is 1961, the earliest year for which population data for natives and immigrants are available. We use census information from the GESIS data archive (available online under the file name ZA2472) and combine these data with population data from the German Federal Statistical Office for years 1985-2001, which we reassembled from Statistical Yearbooks and published tables (both available online). To obtain first stage results, we regress the changes in local immigrant employment shares in 1988-1993 on the instrument $\Delta\tilde{I}^{Pop}_{r,88-93}$. The results in Table 2.7 (see last rows) suggest that the historical immigrant settlement pattern is indeed a strong predictor for settlement of immigrants between 1988 and 1993, with F-statistics of 30.24 to 32.56 (depending on the measure for the outcome variable). However, we do not use this instrument in our main analysis because it is only available for 112 out of 204 local labor markets.

A more sophisticated version of the shift share instrument relies, in principle, on a similar idea, but instead of using the overall share of past immigrant settlements, it uses information on the source country of arriving immigrants to account for more detailed ties between specific ethnic enclaves. Formally, the instrument is constructed by interacting past *country-specific* immigrant densities across regions with nationwide *country-specific* net inflows of immigrants,

$$\Delta \tilde{I}^{Emp}_{r,88-93} = \sum_{c} \lambda_{c,r,t_0} \frac{I^{Pop}_{c,93} - I^{Pop}_{c,88}}{(N^{Emp}_{r,88} + I^{Emp}_{r,88})} \tag{A.28}$$

where $\lambda_{c,r,t_0} = I^{Emp}_{c,r,t_0} / I^{Emp}_{c,t_0}$ denotes the share of all immigrants from source country c that work in region r in some base year, and $I^{Pop}_{c,93} - I^{Pop}_{c,88}$ is the nationwide net inflow of country c immigrants between 1988 and 1993.[4] The denominator scales the predicted net change in levels by total employment (natives+immigrants) in region r in year 1988. We choose 1975 as the initial year because it is the earliest year available in our administrative records, and because it comes as close as possible to the settlement structure underlying our distance instrument (although the latter dates back one more decade). The results are summarized in Table A.3. Somewhat surprisingly, column 1 (which corresponds to column 5 in Table 2.3) shows a low correlation (F-statistic=3.34) between predicted and actual immigrant employment growth, which disqualifies this instrument for our analysis. The entries in column 2 demonstrate that the performance of the supply push instrument deteriorates further if we (in analogy to DG) extend the observation period backward and forward to 1985-1995.

The relatively poor performance of the shift-share instrument seems to contradict the evidence in a recent study of DG. They use the shift-share instrument to predict changes in the local skill-specific labor supply, reporting F-statistics well above 20 in their analysis of all skill groups. There is, however, an important conceptual differences between the present analysis and the study of DG. Notably, we do not use skill-specific inflows of immigrants (relying on variation in immigration across regions and skill cells) but instead base our analysis on the overall immigrant inflow. In the following, we show that when we estimate a first stage specification exploiting skill-specific variation in employment growth, we obtain very similar results as DG, yet these appear to be driven by native rather than immigrant employment changes.

We begin by defining the change in skill-specific employment between 1985 and 1995 as the percentage change of the total (native+immigrant) employment of skill group i in region r between 1985 and 1995.[5] In concordance with the endogenous variable of the first stage model, we define the shift-share instrument to predict the *skill-specific* inflow of immigrants by interacting the predicted inflow in a region with a nation-wide average skill distribution

[4] We use five nationality groups defined as (1) Poland, the Former Soviet Union, Romania, and Central and Eastern Europe, (2) Turkey, (3) Italy, (4) Former Yugoslavia, Greece, and Portugal, and (5) Western Europe plus rest of the world.

[5] Note that, as in DG, we include ethnic Germans and East Germans in this analysis.

of immigrants who arrived between 1985 and 1995. Formally,

$$\Delta \tilde{I}^{Emp}_{i,r,85-95} = \frac{\sum_c \lambda_{c,r,t_0} \theta_{c,i} (I^{Pop}_{c,95} - I^{Pop}_{c,85})}{(N^{Emp}_{r,85} + I^{Emp}_{r,85})} \tag{A.29}$$

The three components in the numerator reflect, for each source country c, the initial regional distribution of immigrant employment in 1975 ($\lambda_{c,r,t_0} = I^{Emp}_{c,r,t_0} / I^{Emp}_{c,t_0}$), the average skill distribution between 1985 and 1995, and the total net inflow between 1985 and 1995. The denominator scales the predicted inflow by skill-specific employment in the base year (1985). We use published data from DG to obtain the skill-specific regional immigrants net flows by source country (numerator).[6]

Column 3 in Table A.3 shows the first stage relation between the relative change in skill-specific total employment (natives+immigrants) and the predicted skill-specific inflows of immigrants (conditional on a full set of region and skill group fixed effects). Although our slope parameter is somewhat larger and the F-statistic somewhat smaller compared to DG's results (slope: 0.448 compared to 0.297 (DG); F-statistic: 16.26 compared to 26.0 (DG)), we arrive at substantially similar conclusions: the instrument works well in this setting. However, once we decompose the change in total employment into the change in natives and immigrant employment, and regress each component (both divided by total employment) separately on the instrument, we find that the predicted immigrant growth between 1985 and 1995 is highly correlated with native employment changes but virtually uncorrelated with changes in immigrant employment (see columns 4 and 5 in Table A.3).[7] This seems to contradict the original enclave-based idea of the supply push instrument, which should lead to a positive correlation between the percentage change in *immigrant* employment and the predicted immigrant growth.

In the following, we suggest a possible explanation for why the complex shift share instrument performs worse than a distance-based measure in the German context: first, the initial settlements, especially of former guest workers, were highly concentrated in a relatively small number of regions, leading to exceptionally high immigrant employment rates in some areas, but still low shares in other, geographically close regions; second, immigrants from former Eastern Bloc states were virtually non-existent introducing substantial randomness in the assignment of later flows. For example, based on 1975 data, we find that there are areas of high concentration in Baden-Wuerttemberg and North Rhine-Westphalia as well as the wider

[6] DG obtain immigrant net flows by source country from the German Federal Statistical Office and the skill distribution from the German Microcensus using information on the year of immigration and the current education level. Note that DG's data distinguished between 15 nationality groups.

[7] Note that we focus on the percentage change in immigrant employment rather than the total labor force. Moreover, we merge medium- and high-skilled workers in the analysis, which stands in contrast to the analysis in DG. However, we repeated the same exercise for the labor force subdivided into three skill groups and obtained very similar results: while estimates using the percentage change in the total labor force are even closer to DG's results, the F-statistic is close to zero when we use the percentage change in the immigrant labor force. Also note that DG use weights for total labor force (including immigrants) in the *tradable* sector, whereas we present results based on total employment in all sectors, but exclude immigrants. But again, in unreported results, we found that using the alternative weighting scheme does not have a relevant impact on the results and we arrive at the same conclusions.

area of Munich and some dispersed areas further north (like Hamburg). In contrast, the north and eastern border of Bavaria reveals rather low concentrations of immigrants. Applying the mechanics of the shift share instrument, we can calculate the counterfactual immigrant density that would be observed if future immigrant settlements were determined only by the initial density in 1975 (see Figure A.3). The shift share instrument — using shares in 1975 — overpredicts changes in immigrant shares between 1985 and 1995 in areas that were initially high immigrant regions. That is, later immigrants did *not* move proportionately into areas that were initially high immigrant regions. This points to some spillover effects, possibly because the former guest workers who were granted the right to stay, required further and cheaper housing space as they started to reunify their families in later decades.[8] In addition, the shift share IV performs particularly bad in many southern regions that either share a border with high immigrant regions as of 1975 or are located further east. This illustrates the inability of the shift share prediction to adequately predict the settlement of Eastern Bloc migrants.

[8] Anecdotal evidence suggests that housing space was scarce in the hot spots of 1975. As immigrants reunified their families over the subsequent decade, they may have been forced to move to neighboring regions.

A.6. Appendix Tables

Table A.1.: Fixed Effects Regressions (Worker Level)

	Native-Immigrant Wage Gap		Annualized
	1985	1995	Change
	(1)	(2)	(3)
Baseline	0.122	0.193	0.70
	(0.002)	(0.002)	
Individuals	315,492	332,605	
Baseline + region fixed effects	0.155	0.226	0.71
	(0.002)	(0.002)	
Individuals	315,492	332,605	
Baseline + region and occupation fixed effects	0.048	0.095	0.47
	(0.002)	(0.002)	
Individuals	315,338	332,308	
Baseline + region and occupation-industry fixed effects	0.044	0.079	0.35
	(0.002)	(0.002)	
Individuals	315,229	332,217	

Notes: Table shows coefficients on a dummy for German nationality from regressions of the log wage (imputed) on a gender dummy and a quadratic polynomial of experience (baseline) plus fixed effects as indicated in the row heading. We fit the models on cross-sections indicated in the column heading. Column 3 calculates the annual percentage increase by dividing the log point difference of columns 1 and 2 by 10. Data source: SIAB 7510.

Table A.2.: Comparison of Different Estimation Methods

	Wages		Employment	
	Plug-in OLS (1)	Generated Instr. Var. (2)	Plug-in OLS (3)	Generated Instr. Var. (4)
Panel A: All skill groups				
2nd stage coefficient	−0.677	−0.677	−1.125	−1.125
- Robust	(0.247)	(0.285)	(0.700)	(0.745)
- No Bootstrap	–	(0.246)	–	(0.698)
- Bootstrap	(0.281)	(0.272)	(0.718)	(0.697)
Panel B: Unskilled				
2nd stage coefficient	−0.695	−0.658	−2.610	−2.593
- Robust	(0.505)	(0.496)	(1.123)	(1.224)
- No Bootstrap	–	(0.478)	–	(1.116)
- Bootstrap	(0.459)	(0.429)	(1.166)	(1.160)
Panel C: Skilled				
2nd stage coefficient	−0.581	−0.586	−0.917	−0.917
- Robust	(0.244)	(0.274)	(0.808)	(0.819)
- No Bootstrap	–	(0.246)	–	(0.807)
- Bootstrap	(0.294)	(0.295)	(0.779)	(0.795)
Local labor markets	204	204	204	204

Notes: Table shows the baseline results reported in Table 5 under two different estimation methods and for different computations of standard errors. Columns 1 and 3 refer to our main approach which replicates the DSS estimation, whereas columns 2 and 4 report the Generated Instrumental Variable (GIV) approach. All models include a linear region-specific trend for years 1986-1988. We report up to three sets of standard errors: robust standard errors for both approaches, non-bootstrapped standard errors for the GIV approach, and bootstrapped standard errors again for both approaches. Data source: SIAB 7510.

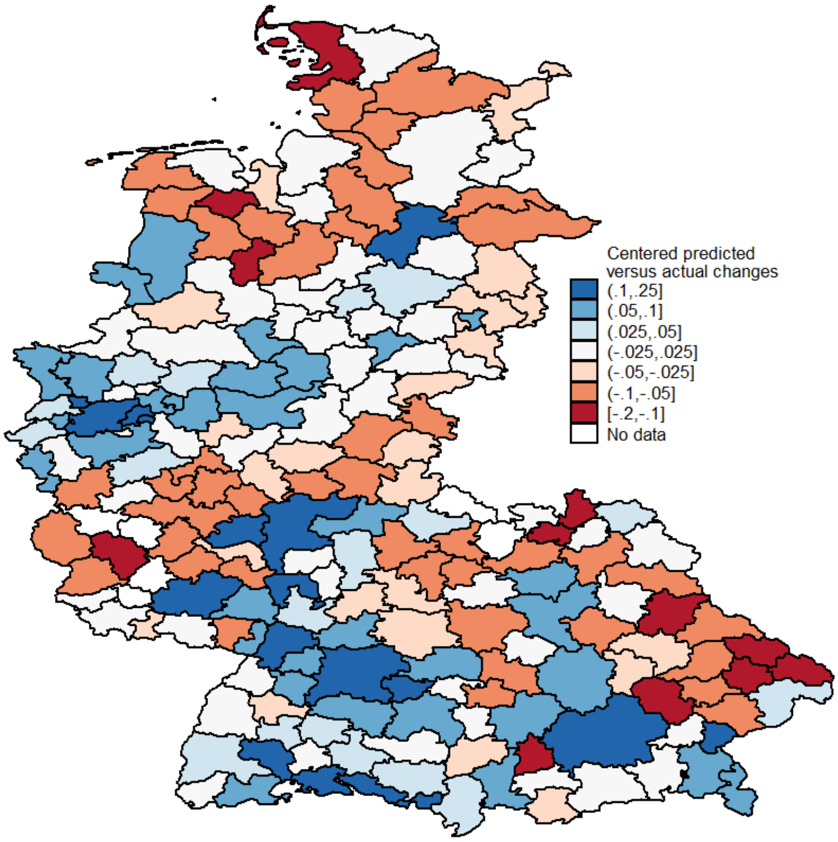
Table A.3.: Overview of Shift Share Instruments

	Overall Inflow		Skill-Specific Inflow		
	Immigrant Employment 1988-1993 (col. 5, Tab. 2.3)	Immigrant Employment 1985-1995	Total Employment 1985-1995 (comp. DG)	Native Employment 1985-1995	Immigrant Employment 1985-1995
	(1)	(2)	(3)	(4)	(5)
Predicted inflow using 1975 regional distrib. of immigrants	0.045 (0.025)	0.009 (0.022)	0.448 (0.111)	0.529 (0.125)	−0.081 (0.032)
R-squared (adjusted)	0.027	0.00	0.93	0.92	−0.06
F-statistic (excl. Instrument)	3.34	0.17	16.27	18.02	6.26
Local labor markets	204	204	204	204	204
Observations	204	204	408	408	408

Notes: Table summarizes the relationship between the predicted immigrant inflow and actual changes in immigrant employment or labor force shares. Column 1 replicates the results of column 5, Table 2.3. Column 2 adjusts the observation period to 1985-1995. In columns 3-5, we use as instrument the predicted skill-specific inflow from DG, showing that the correlation between an immigrant-induced change in relative skill supplies and the local employment is primarily driven by changes in the relative supplies of natives and not immigrants. Data source: SIAB 7510.

A.7. Appendix Figures

Figure A.1.: Predicted versus Observed Changes in Immigrant Employment Shares Between 1985 and 1995



Notes: Figure shows regional differences between the predicted and observed changes in the immigrant employment share between 1985 and 1995. The difference is centered around zero by subtracting from each region the average difference of all regions. The predicted changes are based on a shift share instrument following DG (see Appendix A.5 for details). Data sources: SIAB 7510 and DG.

B. Appendix to Chapter 3: Effects of Relaxed Employment Protection on Labor Market Outcomes: Evidence from a 2004 German Reform

B.1. Sample Processing

B.1.1. Cross-Sectional Model 2 of the Linked-Employer-Employee Data 1993-2010 (LIAB QM2 9310)

The Cross-sectional Model 2 of the Linked-Employer-Employee Data 1993-2010 (LIAB QM2 9310) from the German Institute of Employment Research (IAB) combines survey data on establishments from the annual waves of the IAB Establishment Panel (IAB EP) with administrative data on individuals drawn from the Integrated Employment Biographies (IEB) (for details see Heining et al. 2013). The data was accessed via on-site use at the Research Data Center (FDZ) and subsequently via remote data access.

The IAB EP (for details see Fischer et al. 2008) is a stratified sample of German establishments with at least one employee liable for social security payments as of June 30 in the year prior to the survey. For the years under consideration, the annual sample size amounts to roughly 16,000 establishments representing approximately 1% of the universe of German establishments. The data on individuals are drawn from the IEB and entail administrative data from the Employee History (BeH) which covers all employees liable for social security payments. Since the data basis of the BeH is the integrated notification procedure for health, pension and unemployment insurance, it is considered to be highly reliable. The individual-level data are supplemented with basic establishment information (e.g., 3-digit industry code) from the Establishment History Panel (BHP) (for details see Gruhl et al. 2012).

The LIAB QM2 merges the data from these various sources using a unique establishment identifier. It is constructed according to the following procedure: First, all establishments from the IAB EP with a valid interview in the respective year are selected. Subsequent, for each year information on all individuals that are employed at one of these establishments at the cut-off date June 30 are drawn from the IEB. Although not all surveyed establishments can be linked to individual-level data from the IEB, the yearly coverage rate of 89 to 98% is fairly high and maintains the representativity of the sample for the universe of German establishments.

I process the LIAB QM2 9310 as follows: I keep individuals who are regular employees liable for social security payments ($erwstat = 101$), employees in partial retirement ($erwstat = 103$), student trainees ($erwstat = 106$), or marginally employed ($erwstat = 109, 209$). Individuals in vocational training ($stib = 0$) or owner and executive staff ($beruf = 751$ and $erwstat = 101, 102$ and $stib \neq 0, 1, 7$) are only considered in the denominator of the worker turnover rates. Full-time equivalent weights are 0.5 for part-time employees not eligible for unemployment benefits ($stib = 8$), student trainees, and marginal employees, 0.75 for part-time employees eligible for unemployment benefits ($stib = 9$), and 1 otherwise. All wages are converted in Euros and deflated by the Consumer Price Index, with 2000 as the base year. I impute missing or unknown values for education following Fitzenberger et al. (2006) and aggregate education levels to three groups: *low* for individuals without vocational training or missing information, *medium* for individuals with a vocational qualification, and *high* for individuals with a university degree or more. Since I do not have information on the hours of work, I limit the attention to individuals who are working full-time ($stib < 8$) in the analysis of firms' log mean daily wages. I impute right-censored wages following Dustmann et al. (2009). For each year and gender, I estimate separate Tobit models that control for all possible interactions between the three imputed education levels and eight age categories. All individual-level data are aggregated using a unique establishment identifier (*idnum*). I exclude establishments in the shipping and aircraft transport industry, agricultural and mining sectors as well as non-profit firms and private households. Moreover, I abstract from establishment entries and exits and only keep firms that are always present during the sample periods 2000 to 2007. For further sample restrictions in each of the four assignment methods to the treatment and control group see section 3.4.2 and Table 3.2.

To obtain firm-level shares for workers on fixed-term contracts (FTC) or temporary agency (TA) worker, I merge the aggregated data with establishment-level data from the IAB EP. Since there are multiple sources for inconsistent establishment matches (e.g., a new establishment identifier is issued whenever a plant changes ownership only in the IEB), I follow the procedure described in Alda (2005) and drop establishments with substantial differences in the establishment size according to the two data sources. Therefore, I define firm size-contingent limits for the allowed difference in the number of employees according to the data sources. For establishments with up to five employees, the limit is 40%, for establishments with five to 19 employees 30%, and for establishments with 20 to 100 employees 20%. If the limit is exceeded in one of the sample periods under consideration, the establishment is removed from the sample. Depending on the assignment method, the procedure reduces the sample by 32 to 39%. The reduction in sample size appears reasonable given Alda (2005) finds annual rates exceeding the tolerance levels of up to 30%. Furthermore, I drop establishments with a share of FTC or TA employment larger than one from the sample used for the analysis of the use of temporary employment. The share of FTC employment can exceed one only due to misreporting while the share of TA employment may also exceed one if the number of TA workers in a given firm exceeds its total number of employees (which does not include TA workers).

B.2. Appendix Tables

Table B.1.: Overview of Treatment Assignment Methods Used in Other Studies

3-years in 'before' periods:	Martins (2009)
	Bellmann et al. (2014)
4-years in 'before' periods:	Bauernschuster (2013)
Always the same:	Kugler and Pica (2008)
Period-by-period:	Bauer et al. (2007)
	Centeno and Novo (2012)
	von Below and Thoursie (2010)

Notes: Table lists different assignment methods to the treatment and control group and studies that applied the respective assignment method. The method *3-Years (4-Years) in 'Before' Periods* assigns firms to the treatment and control group that remain in a specified firm size interval for three (four) periods before the treatment. The method *Always the same* assigns firms to the treatment and control group that remain in a specified firm size interval for all observation periods before and after the treatment. The method *Period-by-Period* assigns firms to the treatment and control group based on the firm size in each period.

Table B.2.: Differences in Means Between Treatment and Control Group by Assignment Method

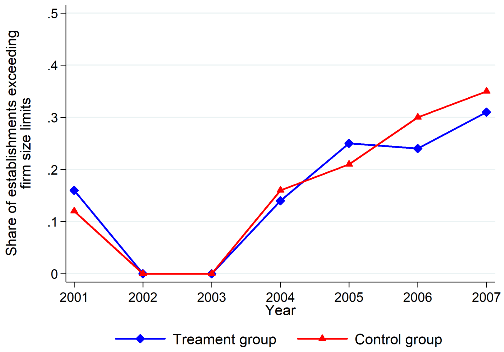
	2-Years in 'Before' Periods (1)	4-Years in 'Before' Periods (2)	Always the same (3)	Adjacent Periods (4)
Treatment identifier				
FTE establishment size	-7.404^{***}	-7.388^{***}	-7.195^{***}	-7.404^{***}
Outcome variables				
Hiring rate	−0.004	−0.014	−0.006	−0.005
Separation rate	0.003	0.001	−0.009	0.002
Job flow rate	−0.007	−0.015	0.003	−0.007
Churning rate	−0.009	−0.018	−0.016	−0.009
Industry distribution				
Manufacturing	−0.021	−0.019	−0.057	−0.022
Construction	0.006	0.005	−0.021	0.005
Wholesale and retail trade	0.031	0.048	0.030	0.030
Real estate	−0.014	−0.020	0.011	−0.011
Others	−0.001	−0.014	0.037	−0.002
Geographic distribution				
North	−0.024	−0.039	−0.030	−0.025
East	−0.029	−0.055	−0.032	−0.027
Berlin region	0.015	0.008	0.003	0.014
South	0.047	0.050	0.037	0.046
West	−0.008	0.036	0.022	−0.009
Average worker characteristics				
Avg. share of women	0.045^{*}	0.056^{*}	0.068^{*}	0.046^{*}
Avg. share of blue-collar worker	-0.028^{*}	−0.024	−0.031	-0.029^{*}
Avg. share of part-time worker	0.012	0.027	0.033	0.012
Avg. share of apprentices	0.001	0.002	0.002	0.001
Mean age	−0.002	−0.158	−0.616	−0.003
Mean age squared	8.490	−0.851	−42.205	8.330
Firms	587	422	247	588

Notes: Table shows the mean differences in the indicated variables between the treatment and control group. Each column refers to one of the four assignment methods described in section 3.4.2. For details on the industry and geographic categories see notes to Table 3.3. Asterisks denote significance of t-test for mean equality between treatment and control group. Significance levels: * 10%, ** 5%, and *** 1%. Data source: LIAB QM2.

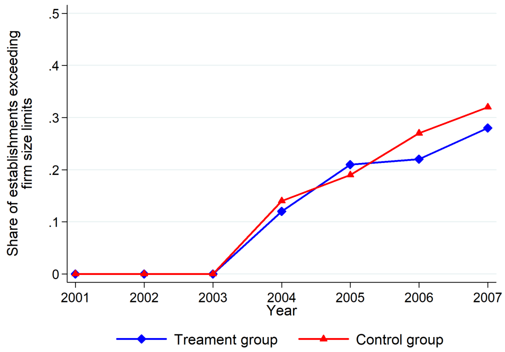
B.3. Appendix Figures

Figure B.1.: Shares of Establishments Exceeding Firm Size Intervals of Treatment and Control Group

A. 2-Years in 'Before' Period



B. 4-Years in 'Before' Period



Notes: Figure shows the share of establishments that exceed the firm size intervals of five to 10 full-time equivalent weighted employees (treatment group) and 10 to 20 full-time equivalent weighted employees (control group) in the periods beyond the assignment periods. In panel A, the assignment periods are 2002 and 2003. In panel B, the assignment periods are 2000 to 2003. Data source: LIAB QM2.

C. Appendix to Chapter 4: The Role of STEM Occupations in the German Labor Market

C.1. Sample Processing

C.1.1. Imputation of Censored Wages

A limitation of the data is that wages are censored at the upper earnings limit for statutory pension insurance. I consider wages that were two Euros below the maximum wage observed in each year as right censored. Between 1980 and 2010, censoring affects on average 11.2% of men (36.2% among STEM jobs) and 2.4% of women (12.8% among STEM jobs). I impute right censored wages following the approach by Gartner (2005). Specifically, I run separate Tobit regressions for each year and gender of right censored log wages on indicators for three skill groups, eight age groups, and all possible interactions.[1] Imputed wages are the sum of predicted wages and a random draw from a truncated normal distribution where the upper earnings limit provides the lower truncation limit and the two moments of the distribution are the predicted mean and variance of the corresponding Tobit regression. Dustmann et al. (2009) refer to this approach as the 'normal, no heteroscedasticity' method since it restricts the variance to be the same across skill and age groups.

I examine the sensitivity of my imputation approach using an alternative method. Notably, I impute censored log wages by the 'normal, full heteroscedasticity' method that allows for a separate variance for each skill and age cell by estimating separate Tobit regressions for each year, gender, skill and age group (Dustmann et al. 2009). The Appendix Figure C.4 compares the evolution of the unadjusted and adjusted mean difference in log real wages between STEM and non-STEM jobs, for censored wages, the imputation method of the main results (i.e., 'normal, no heteroscedasticity'), and the alternative imputation method (i.e., 'normal, full heteroscedasticity'). Reassuringly, despite a minor shift in overall levels, the time pattern for the mean differences in log wages between STEM and non-STEM is very similar.

[1] The eight age groups are defined as: 20-25; 26-30; 31-35; 36-40; 41-45; 46-50; 51-55; 56-60.

C.1.2. Efficiency Labor Supplies

I determine labor supplies in efficiency units in analogy to Glitz and Wissmann (2017). To this end, I differentiate between STEM and non-STEM workers and calculate the efficiency weighted number of full-time workers in each group and year. The efficiency weights are calculated in a two-step procedure: First, I normalize wages by dividing the mean wage in each STEM/non-STEM-skill-age-gender-year cell by a baseline wage which I define as the mean wage of male medium-skilled STEM worker in the age group 41-45.[2] Second, I average the normalized wage for each STEM/non-STEM-skill-age-gender-year cell over all years to obtain time constant efficiency weights for each cell. Formally, the supplies of STEM and non-STEM workers L_w where $w \in \{N, S\}$ (S: STEM and N: non-STEM) in year t is determined by the sum of efficiency units of all full-time workers in each STEM/non-STEM-year cell:

$$L_{w_t} = \sum_{i \in Cell_{w,t}} \mathit{Efficiency\text{-}weight}_{wsag}, \tag{C.1}$$

where w is defined as above, s denotes the skill group, a the age group, and g gender.

C.1.3. Composition Constant Premium

In analogy to the adjusted STEM premium, I want to net out changes in wages that reflect compositional differences between the group of STEM and non-STEM workers. Consequently, I calculate skill, age and gender composition constant wage differentials. The composition constant wages are composed of two components: (1) The *cell specific wage* defined by the mean log wages in each STEM/non-STEM-skill-age-gender-year cell, and (2) the *fixed cell weight* defined by each cell's STEM/non-STEM group share of full-time workers averaged over all years. The annual composition constant log real wage of STEM/non-STEM workers is the weighted average of all cell specific wages and their corresponding fixed cell weights. For example, the composition constant log wage for STEM workers ($w = S$) in year t is defined by $\ln wage_{S_t} = \sum_s \sum_a \sum_g \ln wage_{w=S,s,a,g,t} \times weight_{w=S,s,a,g}$, where $w \in \{N, S\}$ (S: STEM and N: non-STEM), s denotes the skill group, a the age group, and g gender.[3]

[2] In line with the imputation method for wages, I use eight age groups defined as: 20-25; 26-30; 31-35; 36-40; 41-45; 46-50; 51-55; 56-60. Moreover, skill groups are defined as low, medium, and high.

[3] In contrast to Glitz and Wissmann (2017), I only consider the main employment spell of full-time workers in my baseline sample for both the supply and wage measures (see section 4.2.1). I test the sensitivity of my results at the end of section 4.4.2 by using a sample that is more similar to Glitz and Wissmann (2017). Notably, for the determination of supplies, I include part-time workers weighted by 1/2 and 2/3, workers undergoing training, and multiple job spells by the same worker during a year, and weight each spell by the spell-length (measured in days worked per year). Moreover, for the calculation of the composition constant wages, I take into account multiple job spells per worker during a year and further take the spell-lengths into consideration when determining the fixed cell weights.

C.2. Gelbach's Decomposition

To quantify the contribution of unobserved worker and firm effects as well as observed time-varying worker characteristics to the unadjusted STEM premium, I apply Gelbach's decomposition to a linear wage equation. In the following, I illustrate the working of Gelbach's standard decomposition approach for model (4.5) in analogy to Gelbach (2016) and Cardoso et al. (2016). The application of the approach to my auxiliary wage model (4.8) can be conducted analogously and will be discussed briefly at the end of this section.

In general, Gelbach's decomposition links the change of a coefficient of interest between a *basic model* and a *full model* using the formula of omitted variable bias (OVB).[4] Notably, in the present study, the coefficient of interest is a (time constant) STEM dummy which captures the unadjusted STEM premium in a basic model and is absorbed in the full model.

I start by defining a full model as a linear wage equation of the following form:

$$w_{it} = \alpha_i + \psi_{J(i,t)} + x_{it}'\beta + r_{it}, \tag{C.2}$$

where w_{it} is the log wage of an worker i in year t, α_i is a worker fixed effect (absorbing the time constant STEM dummy), $\psi_{J(i,t)}$ is a firm fixed effect, and β captures the effect of observable time-varying worker characteristics. The idea is to interpret the estimated coefficient on the STEM dummy s_i in a basic model of the form:

$$w_{it} = \gamma_B s_i + r_{it} \tag{C.3}$$

as a biased estimator of returns to STEM jobs.[5] Using the formula of omitted variable bias (OVB), Gelbach's approach allows for an unequivocal quantification of the portion of the difference in the STEM premium between the basic model and the full model ($\hat{\gamma}_B$) due to worker effects, firm effects, and a time-varying covariate index. To show this formally, I rewrite the stacked system of model (C.2) in matrix notation:

$$W = L\alpha + D\psi + X\beta + r, \tag{C.4}$$

where W is a $(N \times 1)$ vector of log wages, L is a $(N \times N)$ design matrix for the worker effects, D is a $(N \times F)$ design matrix for the firm effects, and X is a $(N \times k)$ matrix containing observed time-varying worker characteristics, and r is a $(N \times 1)$ vector of disturbances (assumed to be orthogonal to the design matrices due to the exogenous mobility assumption).

[4]The omitted variable bias formula is based on a least-squares identity that links estimates of a base specification $Y = X_1\beta_1^{base} + \varepsilon^{base}$ and a full specification $Y = X_1\beta_1^{full} + X_2\beta_2 + \varepsilon^{full}$ via the formula $\hat{\beta}_1^{base} = \hat{\beta}_1^{full} + (X_1'X_1)^{-1}X_1'X_2\hat{\beta}_2$. To this end, standard results imply that β_1^{full} is a consistent estimate for β_1 "without assuming anything about either β_2 or the correlation between X_1 and X_2, since all X_2 variation is partialed out in the full specification" and, accordingly, β_1^{base} is biased in the traditional sense of an omitted variable bias (Gelbach 2016).

[5]Note that $\hat{\gamma}_B$ corresponds to the average unadjusted STEM premium for the periods underlying the estimation of the model.

Further, I rewrite equation (C.4) in terms of fitted values:

$$W = L\hat{\alpha} + D\hat{\psi} + X\hat{\beta} + \hat{r}. \tag{C.5}$$

Likewise, the stacked system of model (C.3) can be written as:

$$W = \gamma_B S + r, \tag{C.6}$$

where S is a $(N \times 1)$ vector that indicates the assignment of workers to the occupational group of STEM or non-STEM workers. Next, I plug equation (C.6) into equation (C.5) and left-multiply both sides by $M_S \equiv (S'S)^{-1}S'$. Given M_s is orthogonal to $\hat{r}$ due to the assumed exogenous mobility, this yields:

$$\hat{\gamma}_B = M_S L\hat{\alpha} + M_S D\hat{\psi} + M_S X\hat{\beta}, \tag{C.7}$$

or written in the notation of Cardoso et al. (2016):

$$\hat{\gamma}_B = \hat{\delta}_\alpha + \hat{\delta}_\psi + \hat{\delta}_\beta, \tag{C.8}$$

where $\hat{\delta}_\alpha = M_S L\hat{\alpha}$ is the contribution of worker effects, $\hat{\delta}_\psi = M_S D\hat{\psi}$ is the contribution of firm effects, and $\hat{\delta}_\beta = M_S X\hat{\beta}$ is the contribution of the covariate index. Empirically, the three terms on the RHS of equation (C.8) are coefficients of regressions of $L\hat{\alpha}$, $D\hat{\psi}$, and $X\hat{\beta}$ on the vector S, whereby $L\hat{\alpha}$, $D\hat{\psi}$, and $X\hat{\beta}$ are obtained as predicted values from model (C.4).

The approach can be applied analogously to alternative wage equations. Notably, in the present study, I use an auxiliary wage model that has the following form:

$$w_{it} = \alpha_i + \sum_{q=2}^{20} \tilde{\psi}_q F^q_{J(i,t)} + x'_{it}\beta + r_{it}, \tag{C.9}$$

where w_{it} is the log daily real wage, α_i is a worker fixed effect, each $F^q_{J(i,t)}$ for $q = 2, ..., 20$ represents a dummy variable that is 1 if an individual i's weighted CHK firm effect falls into the qth 5%-percentile of the weighted CHK firm effects and 0 otherwise (with the 1st 5%-percentile as the reference category), and x_{it} is a covariate index. While the contribution of the firm effects in model (C.2) are obtained by regressing the predicted firm effects on the STEM dummy, the contribution of the firm effects in the auxiliary model (C.9) are obtained by regressing each of firm dummy covariate on the STEM dummy and then summing over the estimated coefficients or equivalently by summing over the series of firm dummy covariates for each worker, and regressing the compound covariate index on the STEM dummy (see notes to Table 4.4).

C.3. Appendix Tables

Table C.1.: Overview of Occupations Classified as STEM

	Empl. Share in 1980 (1)	Empl. Share in 2010 (2)	%-Change in Empl. Share (3)	Log Mean Wage in 1980 (4)	Log Mean Wage in 2010 (5)	%-Change in Log Mean Wage (6)
	... on total employment					
Non-STEM	0.91	0.87	−0.04	4.11	4.21	0.11
STEM	0.09	0.13	0.04	4.57	4.76	0.20
	... on STEM employment					
Engineers						
Architects, civil engineers	0.06	0.04	−0.02	4.74	4.65	−0.09
Electrical engineers	0.06	0.06	0.00	4.79	5.07	0.28
Mechanical, motor engineers	0.06	0.06	0.00	4.81	5.02	0.21
Survey engineers until other engineers	0.05	0.10	0.05	4.79	4.92	0.13
Computer scientists						0.00
Computer scientists	0.07	0.20	0.12	4.58	4.75	0.17
Technicians						
Biological, physical and math specialists	0.03	0.01	−0.01	4.28	4.50	0.23
Chemical until photo laboratory assistants	0.03	0.02	−0.01	4.22	4.54	0.32
Electrical engineering and building techn.	0.10	0.08	−0.02	4.49	4.66	0.17
Foremen, master mechanics	0.10	0.04	−0.06	4.58	4.76	0.18
Manufacturing technicians	0.06	0.03	−0.03	4.46	4.56	0.09
Mechanical engineering technicians	0.06	0.04	−0.02	4.57	4.73	0.16
Other technicians	0.14	0.13	−0.01	4.48	4.68	0.20
Technical draughtspersons	0.07	0.04	−0.03	4.17	4.38	0.20
Math, Physics, Chemistry, Economics						0.00
Chemists, physicists, mathematicians	0.02	0.02	0.00	4.82	5.00	0.18
Economics, social scientists, statisticians,	0.00	0.00	0.00	0.00	0.00	0.00
humanities, and other natural scientists[1]	0.03	0.05	0.02	4.70	4.67	−0.03
Medical workers						
Physicians and Pharmacists	0.06	0.08	0.02	4.80	4.885	0.09

Notes: Table lists the occupations defined as STEM. [1]Humanities as a non-STEM occupation within the aggregated occupation category of *Economics, social scientists, statisticians, humanities, and other natural scientists* constitutes a share of about 1 to 2% of all STEM workers between 1980 and 2010 (based on frequency counts of the SIAB 7510.). Data source: SIAB-R 7510.

Table C.2.: Shift-Share Decomposition of Changes in Share of Employment due to Changes in Industry Shares and Changes in Occupational Shares Within Industries

	Decade Change			Total Change
	1980-1990	1990-2000	2000-2010	1980-2010
	(1)	(2)	(3)	(4)
Panel A: Men				
Non-STEM				
Total Δ	−1.79	−1.86	−1.17	−4.82
Industry Δ	−0.46	−0.39	−0.72	−1.41
Occupation Δ	−1.32	−1.48	−0.45	−3.41
STEM				
Total Δ	1.79	1.86	1.17	4.82
Industry Δ	0.46	0.39	0.72	1.41
Occupation Δ	1.32	1.17	0.45	3.41
Panel B: Women				
Non-STEM				
Total Δ	−1.29	−1.24	−1.22	−3.75
Industry Δ	−0.07	−0.11	−0.13	−0.27
Occupation Δ	−1.22	−1.13	−1.09	−3.48
STEM				
Total Δ	1.29	1.24	1.22	3.75
Industry Δ	0.07	0.11	0.13	0.27
Occupation Δ	1.22	1.22	1.09	3.48

Notes: Each set of three rows presents the change in the share of employment in percentage points in the indicated occupational group and time interval and decomposes this change into between and within-industry components. The decomposition is based on 120 occupations and 13 industry groups. See section 4.3 for additional details. Data source: SIAB-R 7510.

Table C.3.: Food, Cleaning, Security and Logistics Occupation Codes in KldB 1988 and SIAB-R

Occ. Type	Label	KldB1988 Occ. Code	SIAB-R Occ. Code	Ambiguous Occ. Code
Food	Restaurant, inn, bar keepers, hotel proprietors, catering trade dealers	911	115	
Food	Waiters, stewards	912	115	
Food	Others attending on guests	913	116	
Food	Cooks	411	40	
Food	Ready-to-serve meals, fruit, vegetable preservers, preparers	412	40	
Cleaning	Other housekeeping attendants	923	117	921,922
Cleaning	Household cleaners	933	119	
Cleaning	Glass, buildings cleaners	934	119	
Cleaning	Vehicle cleaners, servicers	936	120	935
Cleaning	Machinery, container cleaners and related occupations	937	120	
Security	Factory guards, detectives	791	96	
Security	Watchmen, custodians	792	96	
Security	Doormen, caretakers	793	97	
Logistics	Motor vehicle drivers	714	81	
Logistics	Warehouse managers, warehousemen	741	84	
Logistics	Transportation equipment drivers	742	85	
Logistics	Stowers, furniture packers	743	86	
Logistics	Stores, transport workers	744	86	

Notes: Table lists occupations defined as food, cleaning, security and logistics (FCSL) occupations using the *KldB 1988* classification and the SIAB-R classification. The definition of FCSL occupations in the *KldB 1988* classification results from Table A-3 of the Appendix to Goldschmidt and Schmieder (2017).

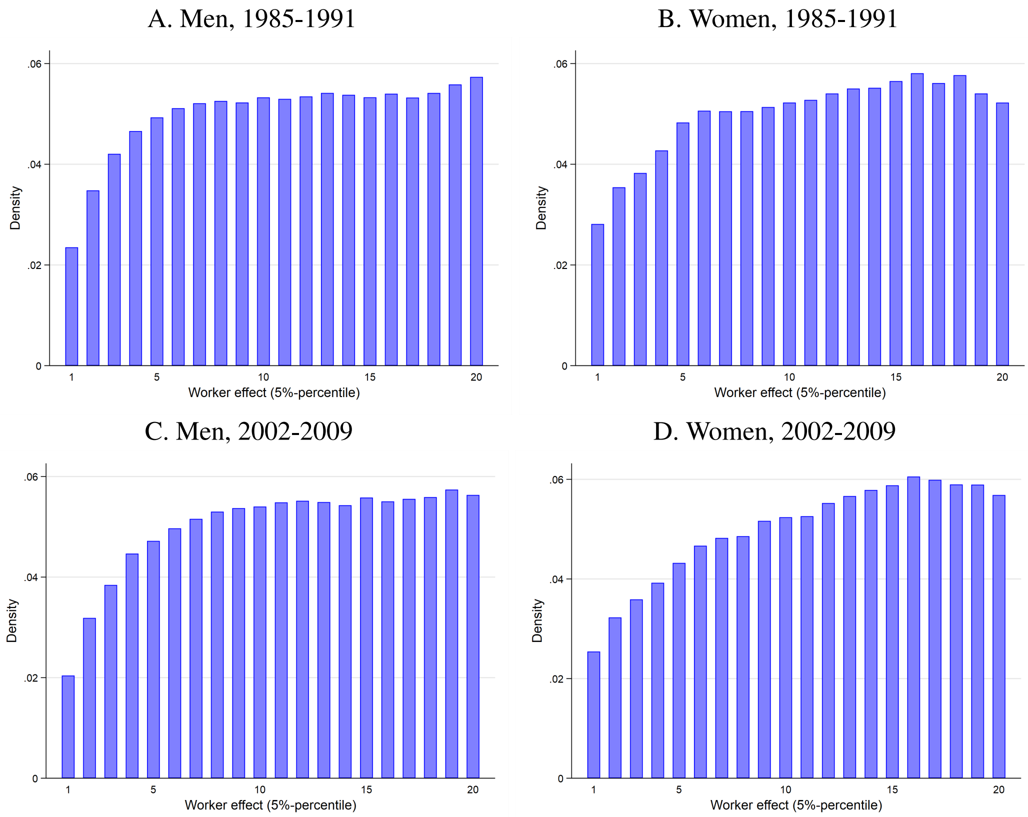
Table C.4.: Estimation Results of OLS Regressions of CHK Worker Effects on Estimates of Worker Effects Based on SIAB-R

	Men		Women	
	1985-1991	2002-2009	1985-1991	2002-2009
	(1)	(2)	(3)	(4)
Beta	1.02	0.94	0.94	0.97
(se x100)	(0.03)	(0.02)	(0.04)	(0.03)
T-test	3,405	4,593	2,315	2,849
R-squared (adjusted)	0.88	0.93	0.88	0.91
Worker-year observations	1,587,636	1,702,954	757,302	817,475

Notes: Table shows results from OLS regressions of Card et al. (2013)'s estimated worker effects (provided in the supplementary IAB data) on own predictions of worker effects as given by empirical model (4.8) using the SIAB-R. Data sources: SIAB-R 7510 and supplementary IAB data on CHK effects.

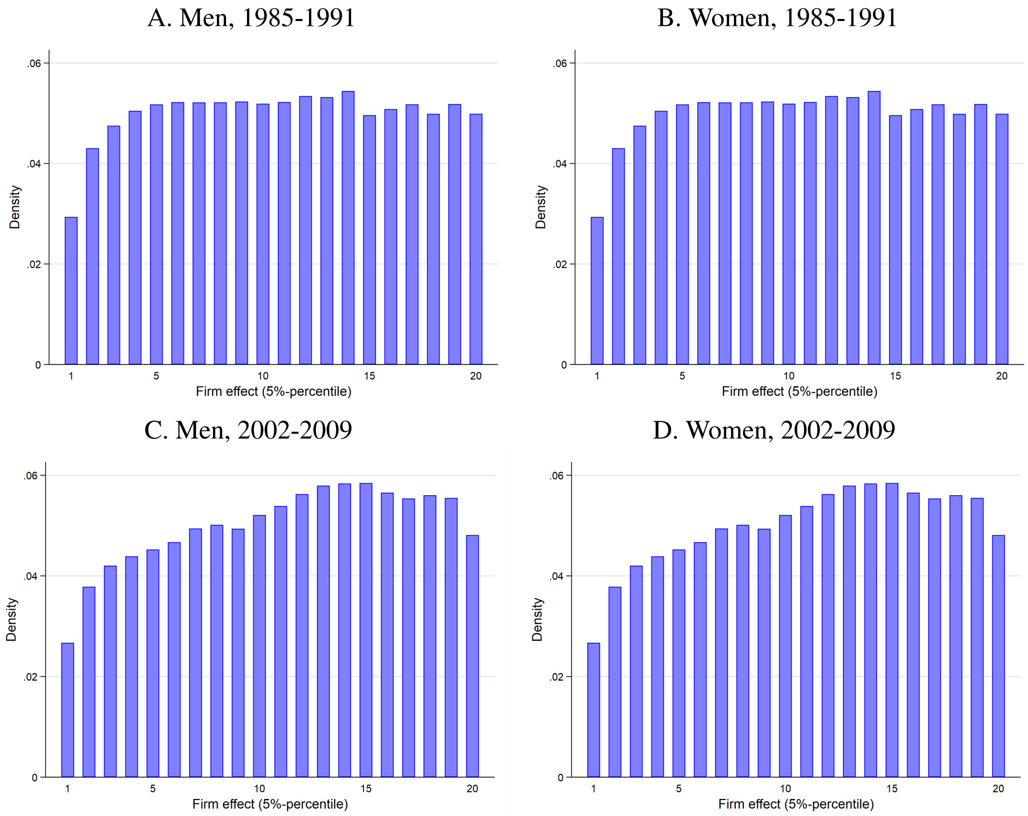
C.4. Appendix Figures

Figure C.1.: Distribution of CHK Worker Effects



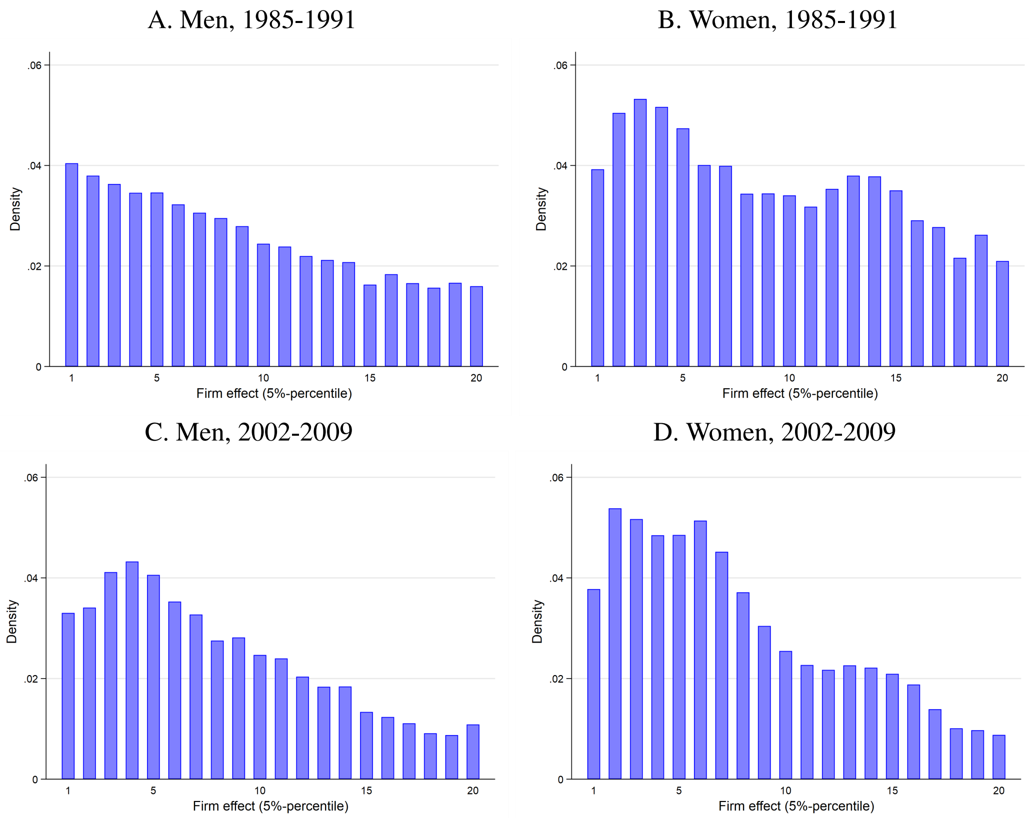
Notes: Figure shows densities of worker effects by 5%-percentiles in the first and last subinterval. The histograms are based on all worker-year observations. Data sources: SIAB-R 7510 and supplementary IAB data on CHK effects.

Figure C.2.: Distribution of CHK Firm Effects



Notes: Figure shows densities of weighted firm effects by 5%-percentiles in the first and last subinterval. The histograms are based on all worker-year observations in a given subinterval. Data sources: SIAB-R 7510 and supplementary IAB data on CHK effects.

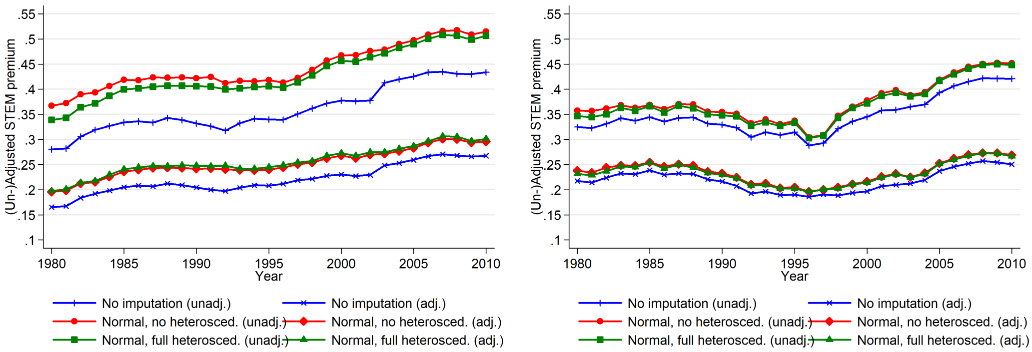
Figure C.3.: Distribution of CHK Firm Effects for FCSL Occupations



Notes: Figure shows densities of weighted firm effects by 5%-percentiles for FCSL workers in the first and last subinterval. The histograms are based on all worker-year observations in a given subinterval. Data sources: SIAB-R 7510 and supplementary IAB data on CHK effects.

Figure C.4.: Evolution of Adjusted and Unadjusted Mean Differences in Log Real Wages Between STEM and non-STEM Occupations Using Alternative Imputation Method

A. Men B. Women



Notes: Figure shows annual estimates of the coefficient on the STEM dummy s_{it} as well as the 95% confidence interval from OLS estimates of the model (4.1) controlling for linear, quadratic and cubic terms in age fully interacted with skill groups. Estimates based on right censored (non-imputed) wages are plotted with pluses and crosses. Estimates based on imputed wages following the 'normal, no heteroscedasticity' method (baseline sample) are plotted with dots and diamonds. Estimates based on wages following the 'normal, full heteroscedasticity' method are plotted with squares and triangles. See Appendix C.1.1 for additional details. Data source: SIAB-R 7510.

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Selbstständigkeitserklärung

Ich erkläre, dass ich die vorliegende Arbeit selbstständig und nur unter Verwendung der angegebenen Literatur und Hilfsmittel angefertigt habe.

Ich bezeuge durch meine Unterschrift, dass meine Angaben über die bei der Abfassung meiner Dissertation benutzten Hilfsmittel, über die mir zuteil gewordene Hilfe sowie über frühere Begutachtungen meiner Dissertation in jeder Hinsicht der Wahrheit entsprechen.

Berlin, den Vorname Nachname