Empirical Evidence on Labor Market Frictions

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Introduction

Labor market inequality

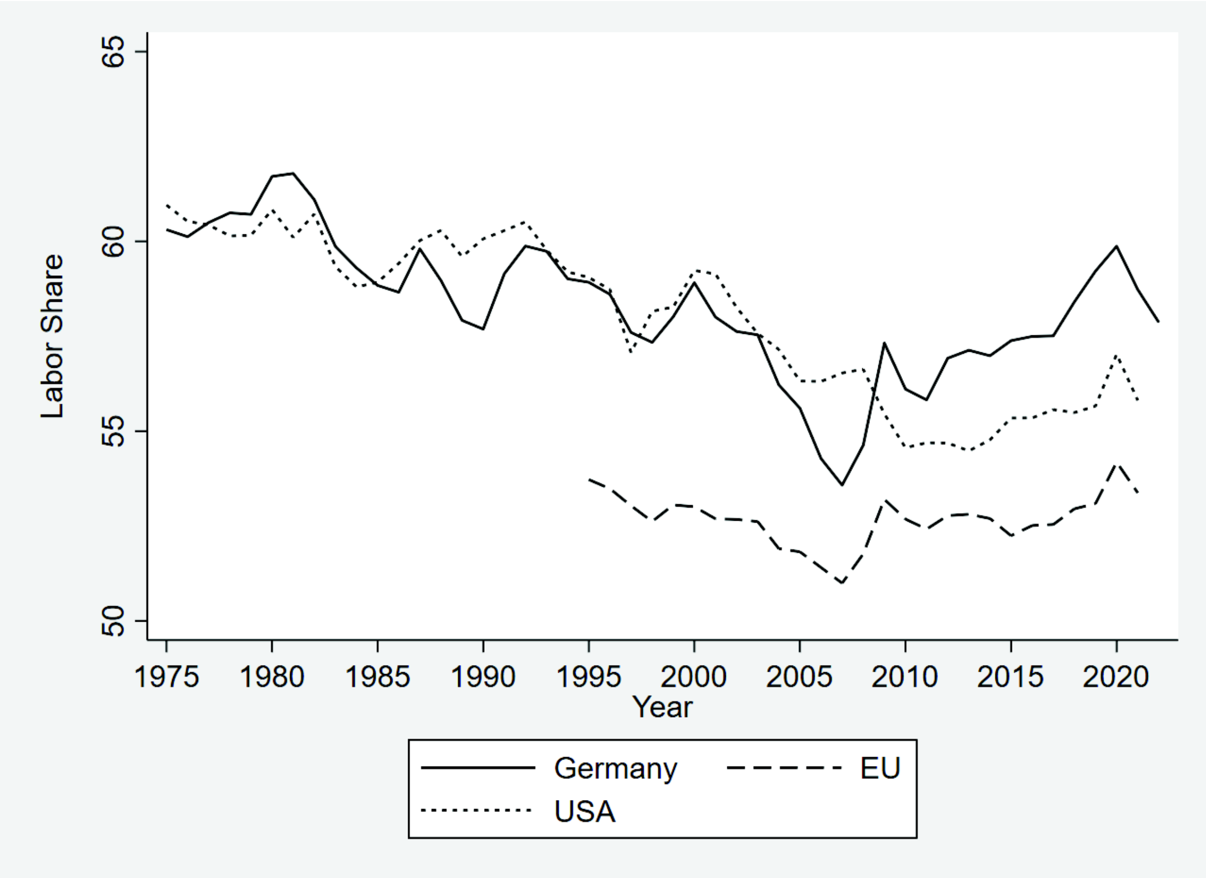
Recent developments in the labor market have cast doubt on the idea of a perfectly functioning labor market. One major issue is the globally declining labor share of income (Karabarbounis and Neiman, 2014). The labor share of income generally measures the compensation of employees as a share of gross value added. Figure 1 depicts the declining labor share of income for Germany, the USA, and Europe. Since the early 1980s, the labor share of income has declined within the large majority of countries.[1] Because the labor share of income has long been assumed to be very stable (Kaldor, 1961) and has served as a foundation for economic growth theories, its decline has attracted the interest of many scholars. A vast body of research tries to explain the reasons behind this decline (see Grossman and Oberfield, 2022, for an overview). Possible explanations include, for example, investment-specific technological change (Karabarbounis and Neiman, 2014), the rise of superstar firms (Autor et al., 2020), demand-side forces (Kehrig and Vincent, 2021), automation and robot adoption (Acemoglu and Restrepo, 2018, 2020; Dauth et al., 2021), and declining market power of workers (Stansbury and Summers, 2020).

During the last four decades, decisive changes took place in many industrialized countries. Figure 2 shows the collective bargaining coverage over time for Germany using data from OECD and AIAS (2021).[2] In the context of my dissertation, I understand collective bargaining coverage as a proxy for the bargaining power of workers. Germany has witnessed a dramatic decline in collective bargaining coverage from 85% in the 1980s to 54% in 2018. Thus, it is also very likely that the bargaining power of employees has decreased over this time. In Germany, employer

[1] Figure 1 also shows that there has been a reversal in the decline of the labor share of income since the global financial crisis (see also e.g. Andic and Burda, 2021). Nevertheless, the declining labor share of income from 1980 to 2010 remains an important object of study.

[2] OECD and AIAS (2021) draw on a variety of sources to measure collective bargaining coverage in Germany, primarily the IAB Establishment Panel (Ellguth et al., 2014).

Figure 1.: Declining Labor Share

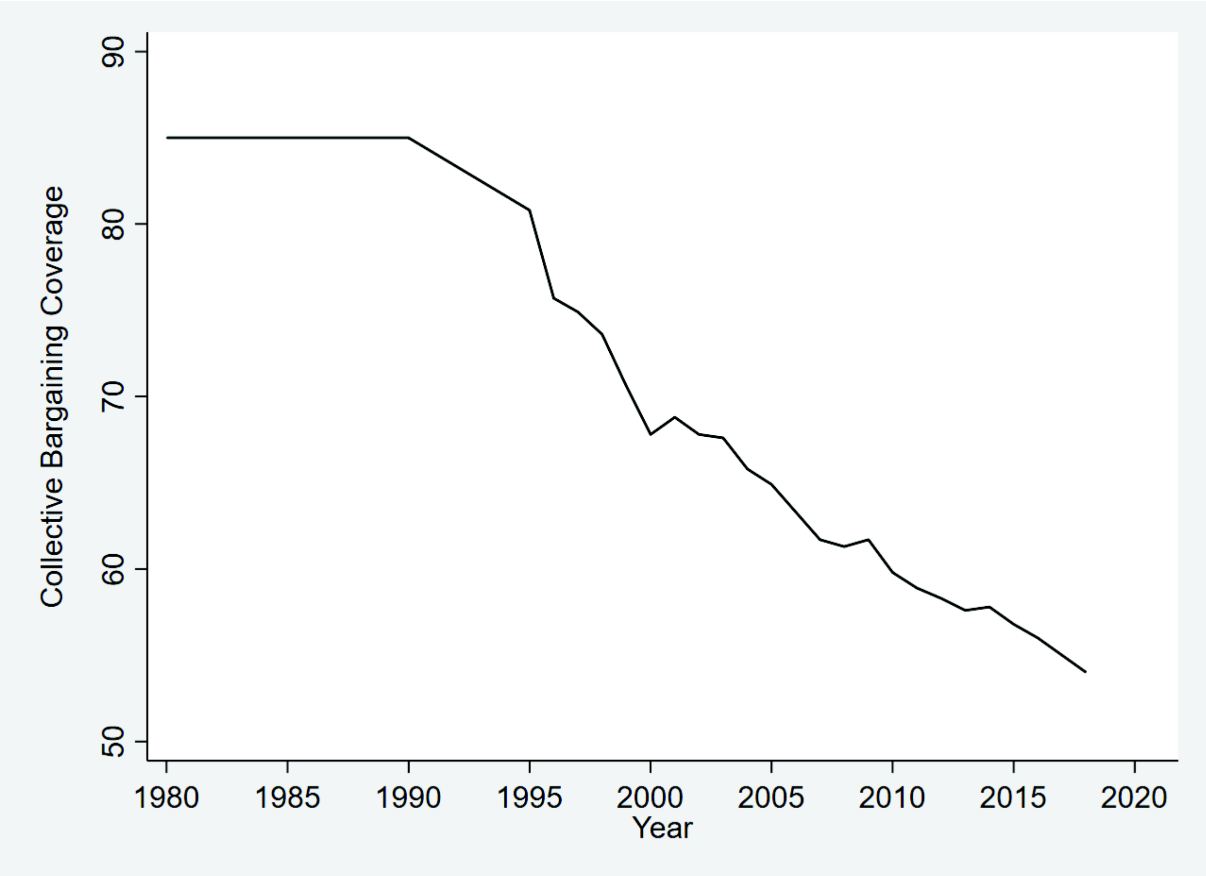


Notes: The labor share of income measures the compensation of employees as a percentage share of gross value added. Compensation of employees has two components: gross wages and salaries payable in cash or in kind, and the value of social contributions payable by employers. **Source:** OECD (2023).

associations and trade unions negotiate at the industry-region level to set standards in wages and working conditions without the government directly exerting influence (Jäger et al., 2022a). The basis of the institutional setup has stayed the same, but as it has become more decentralized, the power to negotiate wages and working conditions has shifted from the industry-region level to the level of single firms and workers for those not included in collective bargaining agreements. Although this has led to greater flexibility in wage setting and this is also claimed to be the reason for the resilience of the German economy to major economic circumstances (Dustmann et al., 2014), it could also explain why wage inequality and the number of low-wage jobs have increased (Dahl et al., 2013; Jäger et al., 2022a; Massenkoff and Wilmers, 2023).

Figure 3 depicts different measures of wage trends and wage inequality (similar to Card et al., 2013). For this exercise, I use the SIAB data and the sample in Chapter 2. Specifically,

Figure 2.: Declining Collective Bargaining Coverage in Germany



Notes: The collective bargaining coverage measures the number of employees covered by a collective agreement in force as a proportion of the number of eligible employees equipped (i.e., the total number of employees minus the number of employees legally excluded from the right to bargain). **Source:** OECD and AIAS (2021).

all measures are based on full-time male workers aged 18-65 years in their main job in West Germany. Panel (a) of Figure 3 shows the evolution of log real wage percentiles over time, indexed to a base of 1996. From 1996 to 2010, the gap between the upper wage percentiles in the figure (80th and 50th percentiles) and lower wage percentiles (20th and 10th percentiles) widened dramatically. For example, the wage gap between the 80th and 10th wage percentiles grew by approximately 26 log points between 1996 and 2010. Panel (b) of Figure 3 illustrates different measures of wage dispersion over time: standard deviation of log real daily wages, normalized 80-50 gap, normalized 80-20 gap, and normalized 50-20 gap.[3] All measures show a rise in wage inequality over time.

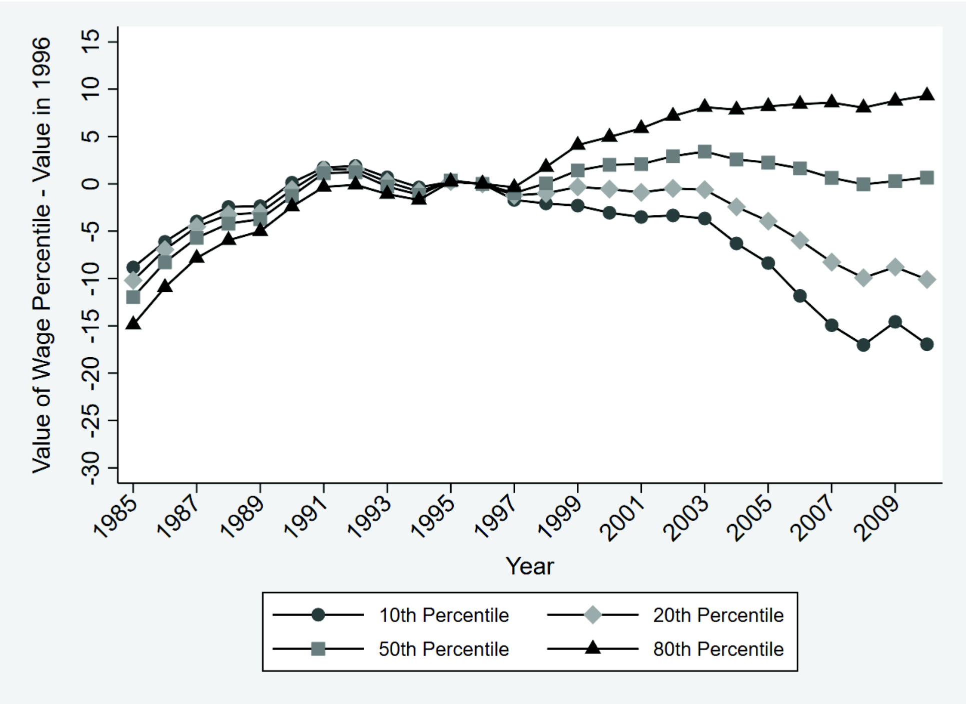
In summary, the share of gross value added for workers has declined over time (Figure 1)

[3] As in Card et al. (2013), I normalize the gap measures by dividing by the corresponding percentile gaps of a standard normal variate.

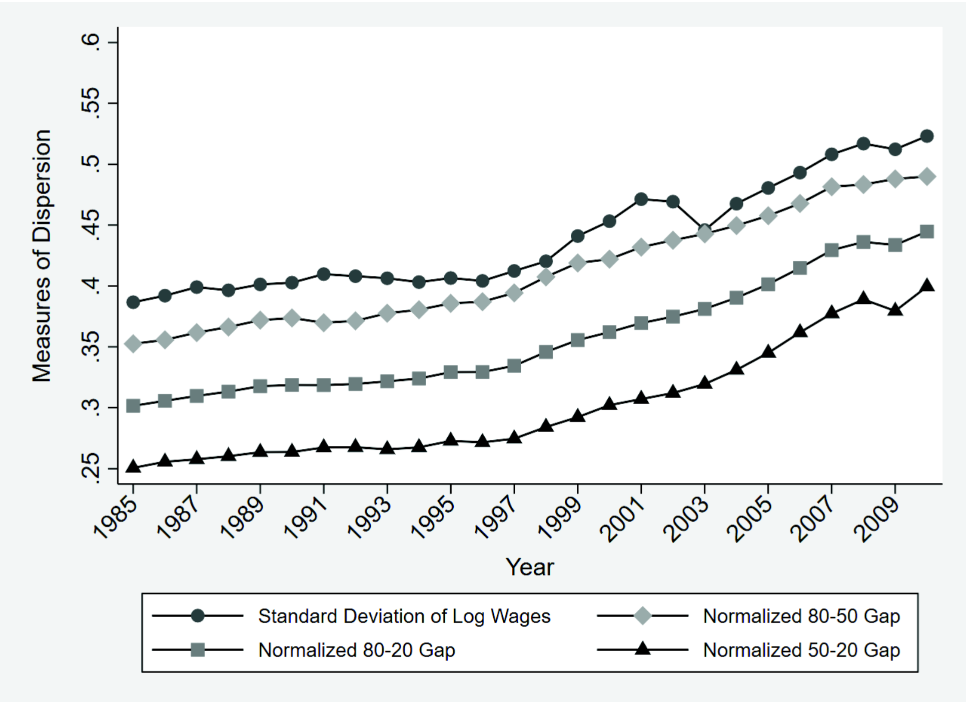
and wage inequality among employees increased (Figure 3), all while the bargaining power of employees has decreased (Figure 2).

Figure 3.: Wage Inequality over Time

(a) Trends in Percentiles of Real Log Daily Wages



(b) Trends in Wage Dispersion



Notes: For both figures, I use real log daily wages with imputations (see Chapter 2). The sample consists of full-time male workers on their main job in West Germany. In Panel (a), I generate percentiles of real log daily wages for each year and subtract it by the value of the same percentile in 1996 and multiply it by 100. In Panel (b), I illustrate different measures of wage dispersion over time. I normalize the gaps between percentiles by dividing them by the corresponding difference in percentiles of standard normal variate. **Source:** SIAB. Sample of Chapter 2 in this dissertation. Author's calculations following Card et al. (2013).

Labor market frictions

Against this backdrop, a large literature seeks to measure the sources of wage dispersion (e.g. Abowd et al., 1999; Card et al., 2013; Song et al., 2019). For Germany, Card et al. (2013) stress the importance of establishment-specific wage premiums and rising assortativeness in the assignment of workers to establishments in explaining the rising wage inequality over time. In other words, the wages that companies pay equivalent workers for comparable jobs differ significantly, and over time this difference has grown.

This raises the question of why this kind of wage dispersion exists in the first place and how establishments are able to maintain their wage differentials. Famously, in his book "Capitalism and Freedom" Friedman (1962), Milton Friedman wrote "the employee is protected from being coerced by his employer by the existence of other employers for whom he can work." So why do employees not quit their current position and go for one at an establishment that pays better? According to models with perfect competition in the labor market, the free flow of workers will require firms to offer a competitive compensation and the best working conditions. In this case, the law of one price will prevail, which states that those with equal skills in comparable jobs would be paid similarly.

As early as 1933, Joan Robinson dealt with imperfect competition in the labor market and coined the term monopsony (Robinson, 1933). Although her ideas did not catch on for a very long time, the literature on labor market frictions and monopsony power in the labor market has recently increased considerably (Card, 2022). The core idea is that labor market frictions prevent workers from moving to the best jobs, leading to wage dispersion.

Labor market frictions include, for example, search frictions (Burdett and Mortensen, 1998; Diamond, 1982; Mortensen, 1982; Pissarides, 1985), mobility frictions and heterogeneous preferences (Berger et al., 2022; Bhaskar and To, 1999; Lamadon et al., 2022), and information frictions (Belot et al., 2019; Carranza et al., 2022; Conlon et al., 2018; Jäger et al., 2022b; Skandalis, 2018; Spinnewijn, 2015). Labor market frictions lead to a labor supply curve to individual firms that is not infinitely elastic, meaning that a 1 cent reduction in the wage does not directly lead to the loss of all workers, as predicted by perfectly competitive models (Manning, 2021). Following the seminal estimation model in Manning (2003), many studies measured the labor supply elasticity to the firm in different countries and contexts and find an elasticity that is incompatible with perfect competition (Sokolova and Sorensen, 2021).

Although the literature on labor market frictions and monopsony power has grown significantly over the past decade, many questions remain. Specifically, what labor market frictions are important, who in particular is affected most by these frictions, what the consequences of these frictions are, and what can help alleviate these frictions.

This dissertation

In my dissertation, I address these questions and provide empirical evidence to help expand the overall picture of the sources and consequences of labor market frictions.

Chapter 1 (co-authored by Ronald Bachmann and Hanna Frings) is motivated by the labor market effects of technological change and the increased use in robots and artificial intelligence. The falling routine employment share in many industrialized countries (Autor and Dorn, 2013; Autor et al., 2003; Goos et al., 2009) suggests that outside options for workers in routine jobs have decreased, which may have led to an increase in monopsony power towards these workers. Furthermore, research on monopsony power in the labor market documents variation in the degree to which worker groups are exposed to potential monopsony power (e.g. Hirsch and Jahn, 2015; Hirsch et al., 2010). The overall degree of monopsony power in the labor market may change if one worker group is particularly affected by monopsony power and the importance of this worker group in the labor market rises. However, differences in monopsony power across workers performing different job tasks has not been explored yet. Therefore, we investigate whether workers who perform different job tasks are exposed to different degrees of monopsony power and whether technological change is associated with higher monopsony power towards routine workers over time.

To measure monopsony power in the labor market, we follow the semi-structural estimation procedure in Manning (2003). This estimation procedure is based on the seminal paper by Burdett and Mortensen (1998) in which quit and recruitment rates vary with wages. Manning (2003) provides a tractable method to estimate the labor supply elasticity to the firm that has been implemented in many different settings (see Sokolova and Sorensen, 2021, for a meta-analysis of this literature). Intuitively, the sensitivity of quit and recruitment rates to wage changes and wage differentials provides information on the potential monopsony power of firms. While in perfect competition every employee would quit in response to a tiny pay cut, this elasticity is lower in dynamic monopsony with frictions. We use administrative social security data from Germany, the Sample of Integrated Labor Market Biographies (SIAB), and merge this data to occupational tasks data using the BIBB Employment Surveys. These two data sets allow us to follow individuals over time, classify workers into different task groups, and assign task intensities to different occupations. We distinguish between routine, nonroutine manual, and nonroutine cognitive tasks.

We find that workers who perform mostly nonroutine cognitive tasks are exposed to a higher degree of monopsony power compared to workers who perform mostly routine or nonroutine manual tasks. We provide suggestive evidence that this result can be explained by nonpecuniary job characteristics and job-specific human capital, both of which are highest for employees who primarily perform nonroutine cognitive tasks. Furthermore, the degree of monopsony power experienced by employees who perform routine tasks has remained relatively constant over time. The results imply that job tasks are an important dimension of monopsony power in the cross-section and could explain wage gaps between workers. Nevertheless, changes in monopsony power does not contribute to rising wage inequality over time.

Chapter 2 (co-authored by Ronald Bachmann, Colin Green, and Arne Uhlendorff) follows on from Chapter 1 and asks how wages evolve when tasks within occupations change. The automation of routine labor as a consequence of advances in technology may have led to a reduction in wages for these workers. However, as a result of this process, new tasks are also developed where humans possess a comparative advantage (Acemoglu and Restrepo, 2018). Due to data limitations, earlier research ignored variations in tasks within occupations and assumed that tasks remained constant throughout time. This chapter's key contribution is to explicitly consider the changing task mixes within occupations when analyzing wage changes within occupations over time.

We use the empirical approach of Cortes (2016) to estimate wage changes of different occupation groups over time. The estimation procedure purges unobserved variation of workers' ability and occupation-specific returns to ability. As in Chapter 1, we use the SIAB data combined with the BIBB data to follow individual workers over time and measure changing task content within occupations over time. We extend the prior literature by introducing three new subgroups of routine occupations: those with high increases in nonroutine cognitive task content, those with medium increases in nonroutine cognitive task content, and those with low or even decreasing levels of nonroutine cognitive task content.

We demonstrate that wages increased for workers in routine jobs whose nonroutine cognitive task content increased the most, but fell for employees in routine jobs whose nonroutine cognitive task did not change much over time. By integrating our data sets with supplementary survey data on training participation from the SOEP, we demonstrate that as the intensity of nonroutine cognitive tasks in certain routine jobs increased, enrollment in employer-funded training also increased. This indicates that employees in these occupations were able to mitigate wage penalties by adapting to the changing task environment. Our findings suggest that technological advancements can rapidly alter the tasks within occupations, and this can also create new earning potential for workers in occupations that are heavily impacted. Additionally, we deduce that expanding occupational search, for example through online advice (Belot et al., 2019), could reduce information frictions and help exposed workers identify suitable occupations.

In **Chapter 3**, the primary motivation is to understand the underlying causes for the coexistence of good (high-wage) and bad (low-wage) jobs in the labor market. With labor market frictions, wage and information shocks on workers' potential outside options can positively affect workers' labor market behavior and outcomes. To investigate this hypothesis, I examine the spillover effects of the public discussion, announcement, and introduction of Germany's first sectoral minimum wage in the main construction sector. This paper mainly contributes to the small but growing body of literature on horizontal spillover effects, which mostly utilizes firm-level data. By combining worker-level and establishment-level data, this study goes beyond the existing literature by uncovering reallocation patterns in addition to wage and employment outcomes, as well as testing predictions from different theoretical models.

For identification, I utilize a triple differences approach, comparing over time (difference one) low- and high-wage workers (difference two), differentially for industries with high (outside option industries) versus low employment flows (non-outside option industries) to the minimum wage sector in the past (difference three). To estimate the empirical strategy, I use linked employer-employee administrative data. I expect that low-wage workers in outside option industries should be affected most by potential spillover effects of the public announcement, discussion, and introduction of the main construction sector minimum wage. The triple differences approach has the benefit of purging group-specific time shocks, such as shocks to the low-wage or high-wage labor market. It also serves as a methodological contribution in terms of measuring spillover effects at the individual level.

I find that low-wage workers in outside option industries experienced higher wage growth, driven by an increase in establishment transitions, and reallocation to better-paying establishments right at the year when the minimum wage was prominently discussed and announced in the media. Furthermore, by testing the predictions of a theoretical model of strategic interactions between employers, I find that these interactions alone cannot fully explain the spillover effects observed. Instead, the model of information frictions with biased beliefs about outside options proposed by Jäger et al. (2022b) appears to better fit the patterns present in the data. This is particularly evident in the timing of the spillover effects and the tendency for workers with arguably more information frictions in the labor market to experience higher spillover effects. These findings suggest that the reduction of information frictions may be the primary driving mechanism behind the observed spillover effects.

The findings of this study imply that labor market institutions, particularly sectoral minimum wages, may have unintended benefits through spillover effects on workers who are not directly targeted. The effectiveness of the minimum wage as a pay transparency tool may be attributed to the fact that its public discussion and announcement was unsolicited and widely publicized in the media.

Chapter 4 (co-authored by Friederike Hertweck, Malte Sandner, and Ipek Yükselen) examines the role of coworker networks in college students' transitions from education to the labor market. While there is a significant amount of research on the role of networks in labor market success, there is little known about the role of coworker networks from student jobs. We argue that college student employment is a widespread and increasing practice. Since information frictions in the labor market are particularly strong at labor market entry, coworkers in student jobs may be potentially helpful in reducing these frictions. To analyze the impact of coworker networks on students' labor market outcomes at labor market entry, we use linked social security and university records from a large German university and gather detailed information on students' labor market history, as well as their university enrollment, and coworkers. We proxy the coworker quality by coworker average wages.

To account for the potential non-random selection of high-ability students into establishments with better and more productive coworkers, we employ a comprehensive set of control variables derived from our two sources of data. These controls include detailed individual characteristics, student job characteristics, establishment characteristics and network characteristics. Of particular importance is the inclusion of the final high school GPA (*Abitur*) as a measure of ability for the student, the establishment fixed effects proposed by Abowd et al. (1999) as a proxy for establishment productivity, and a focus on close coworkers while controlling for less close coworkers. Specifically, close coworkers are defined as those who work in the same establishment and occupation as the student, while less close coworkers are those who work in the same establishment but in a different occupation. This allows us to effectively capture the quality of the establishment and any establishment-specific shocks. Additionally, we also incorporate establishment fixed effects, leveraging the variation of coworker quality within the same establishment for students who enter the establishment at different times and work with different sets of coworkers.

We find that the wages of former student job coworkers have a statistically significant positive impact on the wages of the first full-time employment of graduates, and aid in the quick transition to employment for graduates. These results are not driven by an increase in study effort or from students who begin their careers in the same establishment as their student job. Instead, our findings suggest that in particular former coworkers in full-time employment drive the results. Given that our outcome pertains to the initial full-time employment of graduates, it is reasonable to surmise that the appropriate channel within this context may be one of anchoring or guidance. Our results imply that the networks of student job coworkers are a crucial means of potentially reducing information frictions and facilitating the transition into the labor market upon graduation.

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1. Labor Market Polarization, Job Tasks, and Monopsony Power*

Abstract: Using a semi-structural approach based on a dynamic monopsony model, we examine to what extent workers performing different job tasks are exposed to different degrees of monopsony power, and whether these differences in monopsony power have changed over the last 30 years. We find that workers performing mostly non-routine cognitive tasks are exposed to a higher degree of monopsony power than workers performing routine or non-routine manual tasks. Job-specific human capital and non-pecuniary job characteristics are the most likely explanations for this result. We find no evidence that labor market polarization has increased monopsony power over time.

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1.1. Introduction

The labor market effects of technological change through digitalization and the increased use of robots and artificial intelligence have raised major concerns amongst the public, politicians, and academic economists in recent years. Indeed, workers performing jobs with a high degree of routine task intensity (RTI) are most at risk because their jobs are relatively easily substitutable by computers and robots; as a result, routine employment has strongly fallen over the past decades, both in Europe and in the US (Goos et al., 2009; Autor et al., 2003; Autor and Dorn, 2013). As routine jobs are concentrated in the middle of the wage distribution, this trend has led to job polarization. However, it remains unclear whether – and if so, how – technological change and the ensuing polarization of the labor market have changed the wage-setting power of employers, that is, monopsony power. Furthermore, there is clear evidence that monopsony power matters for wage gaps between worker groups such as men and women or migrants and natives (Hirsch and Jahn, 2015; Hirsch et al., 2010). Differences in monopsony power between workers performing different job tasks have, in contrast, not been investigated yet. These issues are important because monopsony power is a crucial determinant of wages and therefore of workers' welfare.

In this paper, we therefore investigate the link between labor market polarization, job tasks, and the degree of monopsony power. We do so by answering three research questions. First, are workers who perform different job tasks exposed to different degrees of monopsony power? Second, how did the degree of monopsony power evolve over time for workers performing different job tasks? Third, which factors can explain the differences in monopsony power between workers performing jobs with different job tasks? We thus contribute to the literature on monopsony power by providing the first evidence on the relation between the task content of jobs and the market power of employers, both in a cross-sectional setting and over time.

For potential cross-sectional differences of monopsony power between workers performing different job tasks, two sources of monopsony power seem particularly relevant from a theoretical point of view: job-specific human capital and non-pecuniary job characteristics. As discussed in more detail in the next section, job-specific human capital is likely to be more important for high-skilled workers working in non-routine cognitive (NRC) jobs; these workers are also likely to have stronger preferences for non-pecuniary factors such as working conditions or job satisfaction.

Furthermore, the differences in monopsony power by job task intensities could have changed over time, especially as job opportunities have declined for workers with highly routine jobs in industrialized countries during the last decades (Cortes, 2016; Goos et al., 2014). A decline in job opportunities, that is, lower labor demand, has been shown in a business-cycle context to increase the degree of monopsonistic competition (Depew and Sørensen, 2013; Hirsch et al., 2018; Webber, 2022). Given the decline in job opportunities of workers performing highly routine tasks, one could therefore expect higher monopsony power towards theses workers over time.

To empirically answer our three research questions, we use the semi-structural estimation method proposed by Manning (2003) which has frequently been applied in the literature to assess the degree of monopsony power in the labor market.[1] This estimation method is based on the Burdett and Mortensen (1998) model of the labor market which includes wage posting by firms and on-the-job search by workers who can be employed or unemployed. Workers are searching for higher wages, which implies that their mobility decisions depend on the wage differences between jobs. Firms try to attract workers through their wage offers. The resulting monopsony power of firms is captured by the wage elasticity of labor supply to the firm. A low wage elasticity implies that firms can set wages without having to fear strong mobility reactions by workers; therefore, monopsony power is high. Conversely, a high wage elasticity implies low monopsony power. The wage elasticity to the firm is estimated indirectly by estimating its components: On the worker side, the wage elasticities of workers' separation decisions (to employment and to nonemployment) indicate how strongly workers react to wage differences; the share of hires from employment weights these two separation elasticities. On the firm side, the wage elasticity of the share of recruits hired from employment indicates how easy it is for firms to poach workers from other firms.

We apply two approaches to determine the task content each worker performs in his job. First, we follow the international literature on labor market polarization which differentiates between relatively broad task groups that are fixed over time (see for example Cortes, 2016; Goos and Manning, 2007; Goos et al., 2009). To facilitate the comparability of our results with this well-established literature, we estimate the wage elasticity separately for task groups, in our case routine, non-routine cognitive (NRC), and non-routine manual (NRM) workers. A disadvantage of this classification of workers via occupations into task groups is that it is rather

[1] See Sokolova and Sorensen (2021) for a recent meta-analysis of studies on labor market monopsony. Section 1.3 describes the estimation approach in detail.

broad and fixed over the entire observation period. As our main approach, we therefore use a survey data set on job tasks. This allows us to include continuous measures of task intensities as explanatory variables, as Bachmann et al. (2019) do for routine task intensity. In contrast to the first approach, we are therefore able to employ time-varying intensity measures for routine, non-routine cognitive, and non-routine manual tasks. These time-varying task intensity measures mitigate potential measurement errors due to changing occupational task contents over time. Furthermore, this approach allows us to quantify the importance of job task intensities for differences in monopsony power between workers.

Our analysis is based on two unique data sets from Germany. First, we use administrative data on individual labor-market histories spanning the years 1985–2014. This data set includes several socio-demographic worker characteristics as well as firm characteristics and is particularly well suited to identify labor market transitions, including job-to-job transitions. Second, we use survey data that contains time-varying information on individual job tasks. From this data set, we compute the intensities of routine, non-routine cognitive, and non-routine manual job tasks at the occupational level, which we merge to the administrative data set.

Our analysis is closely related to the recent literature on routine-biased technological change (RBTC) and worker flows. Cortes and Gallipoli (2017), using data from the Current Population Survey (CPS) and the Dictionary of Occupational Titles (DOT), examine the importance of task distance between occupations (as in Gathmann and Schönberg, 2010) for the corresponding worker flows in the US. They show that for most occupation pairs, task-specific costs account for up to 15 percent of the costs that arise when individuals move between occupations. Bachmann et al. (2019) analyze the link between labor market transitions and job tasks for the German labor market. They find differences in the mobility patterns of workers belonging to different task groups, and that RTI plays an important role for worker mobility.

Our results can be summarized as follows: First, workers with high routine task content in their occupation display a higher wage elasticity of labor supply to the firm than workers with a high non-routine cognitive task content in their occupation. This indicates that workers with high non-routine cognitive task content are subject to higher monopsony power by employers. A decomposition analysis of the components of this wage elasticity shows that this result mainly arises because workers with high non-routine cognitive task content are much less likely to separate to employment than routine workers. Second, the differences in monopsony power between workers performing jobs with low RTI and workers performing jobs with high RTI stay relatively constant over time, and we do not find pronounced long-run trends for any worker group. This can be seen as an indication that technological progress and the corresponding polarization of the labor market has not increased monopsony power over time. Third, we provide evidence on explanations for the higher monopsony power towards NRC workers: these workers dispose of more job-specific human capital, and they assign a higher importance to non-pecuniary benefits than workers performing jobs with higher routine or non-routine manual task content. Finally, we find that collective bargaining coverage matters for the overall degree of monopsony power of the labor market, but that collective bargaining coverage cannot explain differences between workers performing jobs with different tasks.

Our paper therefore makes two important contributions to the literature. First, we provide evidence on the link between job tasks and monopsonistic competition, and especially to quantify the importance of job task intensities in this context. Furthermore, we investigate potential reasons for the differences in monopsony power that is faced by workers performing different job tasks. Second, we analyze the degree of monopsonistic competition over a long time period using time-varying measures of job task intensities.

1.2. Task Groups, Technological Progress, and Monopsony Power

Workers' job search and mobility behavior in the labor market, as well as the ensuing monopsony power of firms, can be analyzed using the Burdett and Mortensen (1998) equilibrium search model, which is also the theoretical foundation of our empirical approach described in Section 1.3. The model features firms which post wages to fill jobs, and workers who can be employed or unemployed, and who search on the job when employed.[2] In this model, the wage elasticities of workers' separations to employment and unemployment are two key determinants of monopsony power.[3] If workers react strongly to wage differences, firms have little discretion in setting wages, and monopsony power is low. By contrast, if workers hardly react to wage differences, firms have high monopsony power. The job mobility of workers depends on the job offer arrival rate, given the wage offer distribution, as well as on factors that can give rise to monopsony power:

[2] The key assumption of the model is that wages are posted by firms, and workers decide on whether to accept or decline a wage offer. In line with this assumption, Brenzel et al. (2014) showed that wage posting is the predominant mode of wage determination in Germany.

[3] As described in more detail in Section 1.3, the hiring function of firms also plays a role.

job-specific human capital (Webber, 2015), preferences for non-pecuniary job characteristics, search frictions, and mobility costs (Manning, 2003).

Our first research question is whether workers who perform different job tasks are exposed to different degrees of monopsony power. We therefore discuss for each source of monopsony power if and why we expect this source to have a different effect on monopsony power across task groups. The first source of monopsony power, job-specific human capital, implies that a job change leads to a loss of human capital. The existence of job-specific human capital therefore decreases workers' incentives to switch jobs to improve their wage, that is, it increases monopsony power of employers. Importantly for our purpose, one reason why human capital is job-specific, and therefore gets lost with a job change, is that job tasks often change when a worker changes job (Gathmann and Schönberg, 2010).

There are two reasons why the job-specificity of human capital, and thereby the degree of monopsonistic competition stemming from this source, is highest for workers performing NRC tasks. First, the production of output generally requires the combination of tasks into task bundles, and more highly skilled workers can perform more complex tasks. For example, in the labor market model of Acemoglu and Autor (2011) with high-, medium-, and low-skilled workers, each task can be performed by every skill type, but the comparative advantage of skill types differs across tasks. Thus, more complex tasks can be better performed by high-skilled workers than medium-skilled workers, while intermediate tasks can be better performed by medium-skilled workers than low-skilled workers. Furthermore, it costs strictly less to perform simpler tasks with low-skilled rather than medium-skilled or high-skilled workers. As a result, more complex tasks are performed by high-skilled workers, less complex tasks by low-skilled workers (Acemoglu and Autor, 2011). As high-skilled workers perform more complex tasks, they are more likely to lose human capital when they change job, which increases the monopsony power of firms.

Second, complex tasks often require collaboration. This has been shown in the model of Booth and Zoega (2008), where the range of tasks firms can perform is determined by the collective ability of its entire workforce. Therefore, worker heterogeneity translates into firm heterogeneity when collective abilities within firms are not identical. In this model, only firms characterized by workforces of higher ability can perform complex tasks, and complex tasks can be performed in a smaller number of firms than simpler tasks. As a result, high-skilled workers are only able to perform the most complex tasks in relatively few firms with a very specific workforce, and therefore these workers only have few outside options. Firms performing complex tasks therefore have high monopsony power towards their workers, particularly the high-skilled ones who predominantly perform NRC tasks.

The second source of monopsony power consists of preferences for non-pecuniary job characteristics such as working conditions or job satisfaction. The importance of non-pecuniary job characteristics has been stressed in the compensating wage differentials literature (Rosen, 1986). More recently, it has been shown that workers in the US are willing to give up part of their compensation to avoid unfavorable working conditions (Mas and Pallais, 2017), and that high-wage workers and college-educated workers have uniformly better job characteristics (Maestas et al., 2018). Non-pecuniary job characteristics also play an important role in explaining job mobility. Sullivan and To (2014) show that there are substantial gains to workers from job search based on non-pecuniary factors and that workers have a tendency to sort into jobs with better non-pecuniary job characteristics that they are willing to pay for. Sorkin (2018) shows that workers systematically sort into lower-paying firms which provide better non-pecuniary job characteristics. Finally, Lamadon et al. (2022) show that worker preferences over non-pecuniary job characteristics lead to imperfect competition in the US labor market. Given these results from the literature, we expect non-pecuniary job characteristics to be most important for workers performing NRC tasks, implying a higher degree of monopsony power faced by these workers.

For the two remaining sources of monopsony power, search frictions through information imperfections and mobility costs leading to limited regional mobility, the literature does not provide strong indications why these should differ between task groups. In our empirical analysis of mechanisms leading to differences in monopsony power between task groups in Section 1.5.3, we therefore focus on the first two mechanisms: job-specific human capital and non-pecuniary job characteristics.

Our second research question is how the degree of monopsony power evolved over time for workers performing different job tasks. It seems likely that the differences in monopsony power between task groups have changed over time because the general labor market situation of workers belonging to different task groups has evolved very differently in recent decades. There is ample evidence for the US and many European countries that routine work has strongly declined (see for example Autor and Dorn, 2013, and Goos et al., 2014), and that this has had adverse effects on routine workers' long-term employment probabilities (see Bachmann et al., 2019 for Germany and Cortes, 2016 for the US) and wages (Cortes, 2016).

These general developments are likely to have affected the evolution of monopsony power in the labor market for workers performing routine tasks. As shown by Depew and Sørensen (2013) and Hirsch et al. (2018) in a business-cycle context, the degree of monopsonistic competition in the labor market increases at times in which labor demand is relatively low. The most important explanation for this is that workers' job separations are less wage-driven when unemployment is high. Intuitively, a higher unemployment rate leads to worse outside opportunities for workers. Therefore, job security becomes more important for workers which increases search frictions and thus monopsony power (Hirsch et al., 2018).

Extending this argument to a long-run analysis, we expect that the labor supply elasticity to the firm has decreased for routine workers. This is so because labor market polarization has led to a reduction of jobs with predominantly routine task content, which means that outside options decreased for workers specialized in performing routine tasks. Within the Burdett and Mortensen (1998) model, this demand-side effect would mainly feature as a reduction in the job offer arrival rate. Workers performing routine tasks will therefore be limited in their ability to separate from a job to find a better-paying one.

It is important to point out that this demand-side effect may in turn be amplified and hence lead to changes in monopsony power. Similarly to the business-cycle studies cited above, an important reason for this is that routine workers become more risk averse in their mobility decision given limited outside options. Consequently, workers will prefer job stability over a wage raise. This would reduce the wage elasticity of job separations, thus amplifying the initial demand shock.

By contrast, we expect the wage elasticity of labor supply to the firm for workers performing NRC tasks to increase over time, because labor market polarization has led to an increase of outside options for this task group. This increase could for example be caused by the emergence of new tasks that can be performed best by high-skilled (NRC) workers as in the model by Acemoglu and Restrepo (2018). Again, one should distinguish between a pure demand-side effect and an amplification effect. In the case of NRC workers, this amplification effect would further increase the wage elasticity of job separations, even for a constant job offer arrival rate, because NRC workers have increasingly good labor-market prospects and therefore become less risk averse in their mobility decisions.

Finally, there exists another long-run trend that could have affected the evolution of monopsony power in the labor market: the rise of superstar firms. As Autor et al. (2020) point out, technological change and globalization benefit the most productive firms in each industry. This leads to product market concentration as industries become increasingly dominated by superstar firms with high profits and a low share of labor in firm value-added and sales. This increased product market concentration is likely to be accompanied by stronger labor market concentration and thus to lead to monopsony power in the labor market, as shown for the US by Azar et al. (2022). Therefore, this long-run trend can be viewed as a change in the composition of firms towards more firms with high monopsony power, which raises overall monopsony power in the labor market.

1.3. Empirical Methodology

In the following, we briefly summarize the method to empirically estimate the wage elasticity of labor supply to the firm, the measure of monopsony power pioneered by Manning (2003). This method is based on the Burdett and Mortensen (1998) model introduced in Section 1.2, where workers leave the firm at a rate $s(w_t)$ that depends negatively on the wage paid. The number of new recruits $R(w_t)$ depends positively on the wage paid. The law of motion for labor supply to the firm can therefore be expressed as

$$L_t = R^e(w_t) + R^n(w_t) + [1 - s^e(w_t) - s^n(w_t)]L_{t-1}, \tag{1.1}$$

with firms paying wage w_t at time t. The exponents e and n indicate the destination states (for separations) or states of origin (for recruitments) corresponding to employment and non-employment, respectively. Considering the steady state in which total separations must equal recruits and $L_t \equiv L$ and $w_t \equiv w$, we have

$$L(w) = \frac{R^e(w) + R^n(w)}{s^e(w) + s^n(w)}, \tag{1.2}$$

which results in a positive long-run relationship between employment and wages. Equation 1.2 implies that the long-term elasticity of labor supply to the individual firm ϵ_{Lw} is the difference of a weighted average between the wage elasticities of recruitment from employment (ϵ^e_{Rw}) and non-employment (ϵ^n_{Rw}), and the wage elasticities of the separation rates to employment (ϵ^e_{sw}) and non-employment (ϵ^n_{sw}), that is,

$$\epsilon_{Lw} = \theta_R \epsilon^e_{Rw} + (1-\theta_R)\epsilon^n_{Rw} - \theta_s \epsilon^e_{sw} - (1-\theta_s)\epsilon^n_{sw} \tag{1.3}$$

where the weights are given by θ_R, the share of recruits hired from employment, and θ_s, the share of separations to employment.

Estimating the separation rate elasticities using data on job durations is relatively straightforward but estimating the recruitment elasticities requires information that is typically not available in data sets. Specifically, we do not have information on the firms' applicants and the wages offered to them. A solution is to impose additional structure on the model by assuming a steady state which implies that $\theta \equiv \theta_R = \theta_s$ holds. Imposing this on Equation 1.3 gives the following relation[4]

$$\epsilon_{Lw} = -(1+\theta)\epsilon^e_{sw} - (1-\theta)\epsilon^n_{sw} - \epsilon_{\theta w} \tag{1.4}$$

where $\epsilon_{\theta w}$ is the wage elasticity of the share of recruits hired from employment and θ is the overall share of hires from employment. The four components of the wage elasticity of labor supply to the firm are thus the wage elasticity of the separation rate to employment, the wage elasticity of the separation rate to nonemployment, the wage elasticity of the share of recruits from employment, and the share of recruits from employment. One can therefore estimate these four components to arrive at the wage elasticity of labor supply to the firm. This estimation approach is widely used in the literature (Booth and Katic, 2011; Hirsch et al., 2022, 2018, 2010; Webber, 2022).

Intuitively, lower wage elasticities of the two separation rates mean that workers react less strongly to wage differences by moving to a new job or to non-employment. This implies that firms have more discretion in setting their wage in this case. Therefore, lower separation rate elasticities lead to a lower labor supply elasticity to the firm, that is, higher monopsony power, in Equation 1.4. The two separation rate elasticities are weighted by θ, the share of hires from

[4]See the Appendix for a derivation of the equation.

employment, to capture the relative contribution of these two rates to the overall wage elasticity of labor supply.[5] By contrast, the wage elasticity of the share of hires from employment takes into account the hiring function of the firm. If this elasticity is high, firms find it relatively easy to poach workers from other firms. In this sense, market power of firms is high if this elasticity is high. Therefore, a high wage elasticity of the share of hires in employment in Equation 1.4 reduces the wage elasticity of labor supply to the firm, that is, it increases monopsony power.

Although this estimation approach is widespread, it has recently been criticized for using all variation in wages for the identification of the separation rate elasticities in specific and the labor supply elasticity to the firm in general (Bassier et al., 2022). We expect workers to react to the firm-specific and the match-specific components of pay in their decision to separate, but not so much to the worker-specific component or any idiosyncratic shock. Keeping all variation in wages instead of focusing on components that are influenced by firm-level wage policies adds noise to the data, and therefore leads to an attenuation bias. Unfortunately, the data used in this study do not allow us to isolate the firm-specific component of pay. We therefore recognize that the estimated elasticities constitute a lower bound, and that the true degree of monopsonistic competition is probably lower than suggested by our estimates. However, this limitation is unlikely to apply to our main research questions dealing with differences in monopsonistic competition over time and between task groups. Therefore, we focus on interpreting the differences between task groups and their evolution over time, rather than the absolute level of monopsony power.

To estimate the components of Equation 1.4, we proceed as follows. For the separation rate elasticities to employment and non-employment, we model the instantaneous separation rate of employment spell i at duration time t as a Cox proportional hazard model:

$$s_i^{\rho}\Big(t|x_i^{\rho}(t)\Big) = h_0(t)\, exp\Big(x_i^{\rho}(t)'\beta^{\rho}\Big), \tag{1.5}$$

where $\rho = e, n$ indicates a separation to employment or non-employment respectively, $h_0(t)$ is a baseline hazard with no assumptions on its shape, $x_i^{\rho}(t)$ is a vector of time-varying covariates with

[5] Note that in steady state, the share of hires from employment is equivalent to the share of separations to employment.

β^{ρ} as a corresponding vector of coefficients.[6] $x_i^{\rho}(t)$ includes log wage as our key independent variable. The corresponding coefficient β^{ρ} can directly be interpreted as the wage elasticity of separations to employment or non-employment, respectively. Furthermore, we include the following covariates to control for individual- and plant-level as well as economy-wide factors which may affect labor supply to the firm: dummy variables for age and education groups, immigrant status, occupation fields (54 fields, following Tiemann et al. 2008), economic sector (15 sectors, following Eberle et al. 2014), worker composition of the firm (shares of low-skilled, high-skilled, female, part-time and immigrant workers in the plant's workforce), plant size (four dummies), the average age of its workforce, as well as year and federal state fixed effects and the unemployment rate by year and federal state.

Estimating Cox proportional hazard models, which place no restrictions on the baseline hazard, forces us to control for job tenure. There are arguments for and against the inclusion of job tenure. On the one hand, Manning (2003, 103) argues that including tenure reduces the estimated wage elasticity as high-tenure workers are less likely to leave the firm and are more likely to have high wages. Thus, tenure is itself partly determined by wages, and including it would take away variation from wages and therefore bias the estimated wage elasticity. On the other hand, considering the existence of seniority wage scales, Manning (2003) also argues that the exclusion of job tenure would lead to a spurious relationship between wages and separations. The empirical literature on seniority wage schedules in the German labor market suggests that controlling for tenure is appropriate in our application (see for example Zwick, 2011, 2012).[7]

To arrive at an estimate of the wage elasticity of the share of recruits hired from employment, $\epsilon_{\theta w}$, we model the probability that a worker is hired from employment (as opposed to non-employment) using a logit model:

$$Pr[y_i = 1|x_i] = \Lambda\Big(x_i'\beta\Big), \tag{1.6}$$

where the dependent variable is a dummy, which takes the value 1 if it is a recruit from employment and 0 if the recruit comes from non-employment. Λ denotes the cumulative distribution

[6] We follow Manning (2003, 100-101) and assume that, conditional on x, the two types of separations are independent. Thus, one can estimate the separation rates separately. To estimate the elasticity of separations to non-employment, we use the whole sample (all jobs). We only use those jobs that do not end in non-employment when estimating the separation rate to employment.

[7] However, as a robustness check we also use exponential models in Appendix Table 1.A.1. This increases the estimated elasticities as expected.

function of the standard logistic distribution. Again, our key independent variable in this equation is log wages. The coefficient of log wages in this model gives the wage elasticity of the share of recruits hired from employment $\epsilon_{\theta w}$ divided by $1 - \theta$. Multiplying the coefficient by $1 - \theta$ yields the estimate of $\epsilon_{\theta w}$ in Equation 1.4. To obtain the weights used in Equation 1.4, we calculate the share of hires coming from employment θ from the data.

To analyze differences in the wage elasticity of labor supply to the firm between workers performing different job tasks, we proceed in two ways. First, we estimate the respective wage elasticities separately by task group. We follow Cortes (2016) and distinguish three different task categories: (1) Routine: administrative support, operatives, maintenance and repair occupations, production and transportation occupations (among others); (2) Non-Routine Cognitive (NRC): professional, technical management, business and financial occupations; (3) Non-Routine Manual (NRM): service workers. These task groups are rather broad and fixed over time, but the classification allows a direct comparison with the US literature using this type of classification. Second, we use a time-varying measure of task intensities (TI), which we explain in detail in Section 1.4.2. Here, we include the interaction of the log wage and $TI_i(t)$ to estimate the separation rate elasticities in Equation 1.5. The respective separation rate elasticity is given by $\epsilon^{\rho}_{sw} = \beta^{\rho}_{w} + \beta^{\rho}_{TI \times w} \times TI_i(t)$. Similarly, the wage elasticity of the share of recruits hired from employment, $\epsilon_{\theta w}$, is given by $\beta_w + \beta_{TI \times w} \times TI_i(t)$ divided by $1 - \theta$. As this second approach allows us to exactly quantify the link between TI and monopsony power, and because it allows us to control for changes in TI by occupation over time, this is our preferred approach in the empirical analyses in Section 1.5.

1.4. Data

1.4.1. The Sample of Integrated Labor Market Biographies (SIAB) 1975-2014

This study uses the weakly anonymized Sample of Integrated Labor Market Biographies (SIAB) for the years 1975-2014.[8] We combine this data with the Establishment History Panel (BHP), also provided by the Research Data Centre of the BA at the IAB. A detailed description of the Sample of Integrated Labor Market Biographies is provided in Antoni et al. (2016).

[8]Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently remote data access.

The SIAB is a representative 2 percent random sample of the population of the Integrated Employment Biographies (IEB). The IEB includes the universe of individuals with either employment subject to social security, marginal part-time employment (mini-job), registered unemployment benefits, job-seeker status at the Federal Employment Agency, participation in active labor market policy measures or other training measures. The information on the corresponding labor market spells is exact to the day.

The most important data source of the IEB for this paper is the Employee History (BeH). The BeH is based on the integrated notification procedure for health, pension, and unemployment insurances. Employers have the legal obligation to notify the responsible social security agencies about all of their employees covered by social security at the beginning and at the end of an employment spell, and to update the information at least once a year. Misreporting is a legal offense (for more information on the notification procedure see Bender et al., 1996). Civil servants and self-employed individuals or spells are not recorded in the BeH, as it only covers employees subject to social security. To identify spells of registered unemployment, we use the Benefit Recipient History (LeH) and the Unemployment Benefit 2 Recipient History (LHG). The data provides us with personal information such as age, gender, nationality, and place of residence, as well as job information such as the daily wage and the occupation. The information on the daily wage is censored at the yearly varying social security contribution ceiling. We explain in Section 1.4.3 how we deal with this issue.

Using the establishment identifier that is included in the data, we can link the individual-level data with the Establishment History Panel (BHP). The BHP data consists of BeH data which is aggregated at the establishment-year level on 30 June of a year. The BHP provides information on the industry of the establishment and other establishment characteristics such as worker group shares with respect to skill, gender, part-time employment, and nationality, as well as the establishment size and the average age of its workforce. Furthermore, it is possible to identify plant closures with the BHP data (see Hethey and Schmieder, 2010).

1.4.2. BIBB/IAB and BIBB/BAuA Employment Surveys

To compute task intensities for occupations, we use the BIBB/IAB and BIBB/BAuA Employment Surveys (herein BIBB data) that provide a representative sample of German workers (BIBB

– Federal Institute for Vocational Education and Training, 2021).[9] The BIBB data consists of repeated cross-sections on approximately 20,000 to 30,000 employees in Germany for each survey wave that we use in this paper (1985-6, 1991-2, 1998-9, 2006, and 2012). The BIBB data are representative of the core labor force in Germany, that is, for persons who are at least 15 years old and work at least 10 hours per week. The dataset contains questions about the workplace concerning, for example, job tasks, working conditions, satisfaction with current job, and other non-pecuniary job characteristics.

Among others, Antonczyk et al. (2009), and Baumgarten (2015) use these data to generate measures of relative task intensities at the occupational level. We follow the approach of Antonczyk et al. (2009) and categorize the activities employees perform at the workplace into routine (R), non-routine manual (NRM) and non-routine cognitive (NRC). This allows us to compute task intensities at the individual level. We aggregate these individual task intensities for 54 occupational categories following Tiemann et al. (2008), and for each occupation-time period combination provide a R, NRM, and NRC share that sums to 100 percent.[10] The ensuing task intensity measure (TI) at the individual level i can be expressed as

$$Task_{ijt} = \frac{\text{number of activities in category j performed by i in cross section t}}{\text{total number of activities performed by i over all categories at time t}}, \tag{1.7}$$

where t= 1985-6, 1991-2, 1998-9, 2006, and 2012 and j indicates routine (R), non-routine manual (NRM), and non-routine cognitive (NRC) tasks, respectively. Taking averages over individuals task intensities by occupational categories provides a continuous measure of routine task intensity (RTI), non-routine manual task intensity (NRMTI), and non-routine cognitive task intensity (NRCTI) over time for a given occupational group. We merge the TI measures to the worker-level SIAB data based on occupation and year combinations.

A key advantage of BIBB is that the survey is conducted at regular six- to seven-year intervals throughout our period of analysis. This allows us to have time-varying task intensities by occupational groups. Doing so allows us to fully exploit the BIBB data to update occupation

[9] Between 1979 and 1999, the Federal Institute for Vocational Education and Training (BIBB) conducted the surveys in cooperation with the Institute for Employment Research (IAB). Since 2006 the BIBB cooperated with the Federal Institute for Occupational Safety and Health (BAuA) to administer the surveys.

[10] Using a finer occupational classification is not possible given the relatively small sample size of the BIBB data.

task intensities over time. This has the advantage that our analysis considers task intensities which are regularly updated and therefore reflect the actual task composition at the time of observation. Thus, computing task intensities with the usage of additional data sources is in contrast to the more parsimonious approach, which assigns workers to routine, non-routine manual, and non-routine cognitive categories at one point in time based on groups of standardized occupational codes (see for example Cortes, 2016; Goos and Manning, 2007; Goos et al., 2009). A cost of relying on the time-varying task measures computed from the BIBB data consists in discontinuities in these measures from one survey wave to the next. However, as shown by Bachmann et al. (2019), these discontinuities are not large.

1.4.3. Sample Construction

The SIAB provides information on workers' employment biographies from 1975 onwards. However, for our analysis, it is only possible to use the data set from 1985 because the wage variable does not include bonus payments before 1985 but does so afterwards. As this results in a strong break in measured wages from 1984-5, we restrict our observation period to 1985-2014. As our observation period includes the pre-unification period, we focus on West Germany only. Including observations for East German workers from 1992 onwards and therefore restricting our analysis to the post-unification period would considerably reduce our period of observation and thus the long time period needed to properly answer our research questions.

The SIAB data includes the daily wage of every employment spell, but no information on working hours. We therefore focus on full-time workers, as this ensures comparability between daily wage rates. Wages are top-coded at the social security contribution limit. To avoid possible biases in the estimated wage elasticity of labor supply, we exclude all job spells with wages that are at this limit at least once during the observation period.[11] Further, we convert gross daily wages into real daily wages by using the consumer price index of the Federal Statistical Office.

In our empirical analysis, we focus on the core labor force in dependent employment and therefore exclude apprentices, trainees, homeworkers, and individuals older than 55. We further restrict our analysis to male workers to avoid selectivity issues regarding female labor force participation. Information on workers' education is provided by employers and is therefore inconsistent

[11] In robustness checks, we include job spells with censored wages and impute the wages of these spells following the imputation procedure outlined in Dustmann et al. (2009), Card et al. (2013) and Gartner (2005). More details are provided in the Appendix. This yields very similar results to our estimations excluding top-coded wages.

or missing for some workers. To correct for the inconsistent education information, we impute the missing information on workers' education by using the procedure proposed by Fitzenberger et al. (2006). Furthermore, we exclude plants during their closing year, thus mitigating biases resulting from involuntary, demand-side driven separations from a job.[12] Specifically, excluding plants in their closing year helps to mitigate the possible spurious relationship between wages and separations that is not driven by workers' labor supply behavior.

Following the theoretical model based on Manning (2003), we distinguish between two labor market states: employment and non-employment. However, the reports and notifications of establishments and individuals are not always exactly consistent with the actual change of labor market state. For example, workers might report to the unemployment office only a few days after they are laid off. To deal with these potential measurement errors, we define our main dependent variables in the following way:

(i) *Separation to employment/ job-to-job transitions*: If the time gap between two employment spells at different establishments (that is, an establishment with a different establishment identifier) does not exceed 30 days.

(ii) *Separation to registered unemployment or non-employment*: If the time gap between two employment spells at different establishments exceeds 30 days, we define this time gap as a non-employment spell. A separation to non-employment is also defined as a job spell ending in registered unemployment or no spell in the data at all. Further, we take care of recalls in the following way: Recalls are defined as one single employment spell if the time gap between two employment notifications at the same firm does not exceed 120 days. If the time gap between two employment notifications at the same firm is equal to or larger than 120 days, we define this gap as an additional non-employment spell. Treating recalls as continuous employment spells ensures that seasonal effects that differ between industries and task groups and may affect wages and transitions into/from non-employment simultaneously do not distort the results.

(iii) *Recruitment from employment relative to non-employment*: Similar to (i) and (ii), we define a recruitment from employment if the time gap between two employment spells at different establishments (that is, an establishment with a different establishment identifier) does not exceed 30 days. A recruitment from non-employment is defined if the time gap between two

[12] We cannot fully focus on the voluntary supply-side driven separation behavior of workers, because firings are still included in the data, as we cannot identify and distinguish firings from voluntary separations.

employment spells at different establishments exceeds 30 days, the individual is hired from registered unemployment, the time gap between two employment notifications at the same firm is equal to or larger than 120 days, or the individual has no spell in the data (prior to recruitment) at all.

Table 1.1 gives an overview on our final sample which consists of 5,641,241 employment spells from 465,131 workers with 444,864 separations to employment and 742,690 separations to non-employment. The descriptive evidence is in line with the expectations and shows that NRM workers are in the lower, routine workers in the middle and NRC workers in the higher end of the wage and skill distribution (see for example Acemoglu and Autor, 2011; Cortes, 2016). Our task intensity measures are in line with the task group classification of Cortes (2016). Specifically, the means of the task intensity measures by task groups show that RTI is highest for routine workers, NRMTI is highest for NRM workers, and NRCTI is highest for NRC workers. The share of censored spells in our sample amounts to 12.62 percent. In comparison, most censored spells come from NRC workers, where the share of censored spells amounts to 32.42 percent (the share of censored spells of routine workers amounts to 5.65 percent, while the share of censored spells of NRM workers amounts to only 2.47 percent). The share of foreign workers among all NRM workers is relatively high compared to the other task groups. NRM workers are also more likely to work with foreign workers and low-skilled workers in their respective firms, while NRC workers have more high-skilled co-workers. In comparison to the other task groups, a relatively high share of routine workers is in small firms and a distinctively high share of routine workers work in manufacturing, while a high share of NRC workers is employed in large or very large firms. A relatively high share of NRC workers works in district-free cities. A high share of routine workers works in urban districts, but in comparison to the other task groups are also relatively likely to work in rural districts.

Table 1.1.: Sample Description

	Routine		NRM		NRC		All workers	
	Mean	sd	Mean	sd	Mean	sd	Mean	sd
Log(daily wage)	4.32	0.33	4.14	0.43	4.48	0.39	4.32	0.38
Imputed log(daily wage)	4.37	0.38	4.16	0.45	4.75	0.52	4.44	0.48
Share censored	5.65		2.47		32.42		12.62	
RTI	0.43	0.15	0.36	0.16	0.26	0.11	0.38	0.16
NRMTI	0.36	0.17	0.38	0.15	0.17	0.13	0.32	0.17
NRCTI	0.21	0.18	0.26	0.19	0.57	0.18	0.30	0.23
Job tenure in years	6.36	6.56	4.96	6.01	5.68	6.16	5.97	6.41
Share of high-skill workers in firm	5.78	8.91	5.52	8.31	17.66	20.56	8.29	13.26
Share of low-skill workers in firm	17.36	14.56	20.21	16.02	13.45	13.16	17.01	14.69
Share of foreign workers in firm	9.89	13.84	13.41	17.33	8.01	13.69	10.09	14.57
Share of female workers in firm	21.29	19.20	30.03	23.03	36.93	23.54	26.18	21.93
Share of part-time workers in firm	5.09	9.15	8.81	14.15	10.98	14.02	7.00	11.59
Share in small firms (0-19 employees)	24.98		19.50		22.73		23.55	
Share in medium firms (20-250 employees)	41.61		44.87		39.75		41.77	
Share in large firms (251-999 employees)	17.65		18.46		19.35		18.16	
Share in very large firms (1000+ employees)	15.13		16.52		17.58		15.90	
Missing	0.63		0.66		0.60		0.63	
Share in agriculture and forestry	0.19		0.16		0.12		0.17	
Share in fishery	0.01		0.00		0.00		0.01	
Share in mining industry	1.48		0.34		0.38		1.04	
Share in manufacturing industry	42.66		30.92		26.23		37.09	
Share in energy and water supply industry	1.38		0.29		0.86		1.08	
Share in construction industry	17.46		3.09		2.72		11.80	
Share in trade and repair industry	13.66		17.62		12.75		14.15	
Share in catering industry	0.45		4.63		5.64		2.30	
Share in transport and news industry	7.72		10.07		2.72		7.05	
Share in finance and insurance industry	0.56		0.35		9.67		2.48	
Share in economic services industry	6.56		17.26		16.68		10.59	
Share in public services industry	4.25		4.46		4.52		4.35	

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Table 1.1 – continued from previous page

	Routine	NRM	NRC	All workers
	Mean sd	Mean sd	Mean sd	Mean sd
Share in education industry	0.42	1.07	4.21	1.35
Share in health industry	0.82	4.75	8.30	3.12
Share in other industry	1.74	4.33	4.58	2.80
Missing	0.64	0.66	0.61	0.64
Share in top 3 industries with highest collective bargaining commitment	22.27	7.90	16.91	18.62
Share in bottom 3 industries with lowest collective bargaining commitment	21.83	32.31	21.11	23.49
Share of foreign workers	11.60	18.72	6.87	11.82
Share without vocational training	11.21	20.46	2.70	10.97
Share with upper secondary school leaving certificate or vocational training	84.48	73.81	69.40	79.38
Share with university degree or university of applied sciences degree	2.17	1.63	25.33	7.07
Missing	2.15	4.09	2.57	2.58
Share in age group 18-25	15.87	18.76	10.45	15.20
Share in age group 26-35	30.42	31.19	38.43	32.28
Share in age group 36-45	28.45	27.30	30.02	28.59
Share in age group 46-55	25.27	22.75	21.11	23.94
Share in district-free cities	29.93	35.96	41.47	33.47
Share in urban districts	44.39	43.02	39.72	43.15
Share in rural districts, some densely populated areas	14.16	12.22	10.48	13.03
Share in rural districts, sparsely populated	10.88	8.14	7.73	9.73
Missing	0.64	0.66	0.60	0.63
Number of separations to employment	258,284	84,761	101,819	444,864
Number of separations to non-employment	450,502	168,768	123,420	742,690
Number of employment spells	3,448,117	976,905	1,216,219	5,641,241
Number of workers	338,384	164,654	171,454	465,131

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Table 1.1 – continued from previous page

	Routine		NRM		NRC		All workers	
	Mean	sd	Mean	sd	Mean	sd	Mean	sd

Notes: Employment spells are split by calendar year. Shares are expressed in percent. All statistics are estimated after dropping censored spells (except imputed wages and the share of censored spells). NRM, nonroutine manual; NRC, nonroutine cognitive; RTI, routine task intensity; NRMTI; nonroutine manual task intensity; NRCTI, nonroutine cognitive task intensity.

Source: SIAB and BHP, 1985-2014. Authors' calculations.

1.5. Results

1.5.1. Monopsony Power by Task Groups

As described in Section 1.3, we start by estimating the labor supply elasticities to the firm for three task groups (routine, NRM and NRC workers) for the whole observation period. Thus, we estimate Cox models for the separation rates to employment and non-employment, and logit models for the probability that a worker is hired from employment (as opposed to non-employment) separately for these three groups. Our key independent variable in each of these estimations is log wages. Inserting the estimated wage elasticities from these models as well as the share of hires from employment into Equation 1.4 yields estimates of the firm-level labor supply elasticity.

Table 1.2 shows that the wage elasticity of labor supply to the firm is distinctly smaller for NRC workers (0.958) than for the other task groups (1.696 for routine workers and 1.659 for NRM workers), which implies a higher degree of monopsony power towards NRC workers.[13] The results in Table 1.2 also indicate that the components of the estimated labor supply elasticities differ considerably between task groups.

To quantify the contribution of the individual components to the overall differences in the labor supply elasticity between task groups, we apply the decomposition proposed by Hirsch

[13]We use imputed wages in Table 1.C.1 in the Appendix. All estimated labor supply elasticities are lower here, because of the addition of idiosyncratic variation to wages. The main results do not change.

Table 1.2.: Labor Supply Elasticity to the Firm by Task Group

	Routine	*NRM*	*NRC*
Separation rate to employment			
log wage (ϵ^e_{sw})	-1.271***	-1.203***	-0.905***
	(0.012)	(0.019)	(0.020)
Observations	1,766,919	497,460	733,684
Separation rate to non-employment			
log wage (ϵ^n_{sw})	-1.628***	-1.610***	-1.302***
	(0.008)	(0.013)	(0.015)
Observations	3,351,798	930,594	1,177,920
Hiring probability from employment			
log wage ($\frac{\epsilon_{\theta w}}{1-\theta}$)	1.737***	1.519***	1.887***
	(0.013)	(0.020)	(0.022)
$\epsilon_{\theta w}$	1.065	1.021	1.079
Observations	574,157	199,582	205,774
Share of hires from employment (θ)	0.387	0.328	0.428
Firm-level labor supply elasticity (ϵ_{Lw})	1.696	1.659	0.958

Notes: Clustered standard errors at the person level in parentheses. Covariates included (see Section 1.3 for details): dummy variables for age and education groups, immigrant status, occupation fields, economic sector, worker composition of the firm (shares of low-skilled, high-skilled, female, part-time, and immigrant workers in the plant's workforce), dummy variables for plant size, the average age of its workforce, year and federal state fixed effects, unemployment rate by year and federal state. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. NRM, nonroutine manual; NRC, nonroutine cognitive.

Source: SIAB and BHP, 1985-2014. Authors' calculations.

and Jahn (2015). In doing so, we focus on the routine-NRC and NRM-NRC differences.[14] We find that the most important component driving the difference in the firm-level labor supply elasticities between NRC workers and the other task groups is the separation rate elasticity to employment (see Table 1.3). This component contributes almost 69 percent to the lower firm-level labor supply elasticity of NRC workers relative to routine workers, and about 56 percent to the difference between NRM and NRC workers. Hence, job-to-job transitions of NRC workers are much less wage-driven than is the case for other task groups. Separations to non-employment are also less wage-elastic for NRC workers than for routine and NRM workers (see Table 1.2). This component accounts for almost 27 percent of the difference in firm-level labor supply elasticities between routine and NRC workers, and for almost 30 percent of the difference between NRM and NRC workers.

The wage elasticity of the share of recruits hired from employment is highest for NRC workers. It thereby contributes to the lower labor supply elasticity of NRC workers in comparison to the other two task groups. However, the magnitude of the contribution differs: For the routine-NRC difference, it accounts for only 12.5 percent, while the contribution is significantly higher at 35 percent for the NRM-NRC difference in labor supply elasticities. Thus, by increasing the wage, employers raise the share of hires from employment to a greater extent for NRC workers than for routine and (especially) NRM workers.

Finally, the share of hires from employment which is used to weight the different components in the firm-level labor supply elasticity equation mitigates the difference between NRC workers and the other task groups. This mitigating effect of the share of hires from employment for the difference in firm-level labor supply elasticities is much more pronounced for the NRM-NRC than for the routine-NRC difference. NRC workers are more likely to be hired from employment than routine and particularly than NRM workers.

[14] We do not decompose the routine-NRM difference, because the firm-level labor supply elasticities are fairly similar in Table 1.2.

Table 1.3.: Decomposition of the Difference in the Firm-Level Labor Supply Elasticity

Component	*Routine Workers' Estimated Firm-Level Labor Supply Elasticity*	*Change in % of the Routine-NRC Difference in the Labor Supply Elasticity*	*NRM Workers' Estimated Firm-Level Labor Supply Elasticity*	*Change in % of the NRM-NRC Difference in the Labor Supply Elasticity*
Routine/NRM workers' estimated firm-level labor supply elasticity	1.696		1.659	
... when using NRC workers' estimated **separation rate elasticity to employment** (ϵ^e_{sw})	1.188	−68.83	1.263	−56.49
... when additionally using NRC workers' estimated **separation rate elasticity to non-employment** (ϵ^n_{sw})	0.989	−26.97	1.056	−29.53
... when additionally using NRC workers' estimated **wage elasticity of the share of hires from employment** ($\frac{\epsilon_{\theta w}}{1-\theta}$)	0.897	−12.50	0.809	−35.28
... when additionally using NRC workers' estimated **share of hires from employment** (= NRC workers' estimated labor supply elasticity) (θ)	0.958	+8.30	0.958	+21.30

Notes: The decomposition is based on estimates from Table 1.2. NRM, nonroutine manual; NRC, nonroutine cognitive.
Source: SIAB and BHP, 1985-2014. Authors' calculations.

Summarizing, our results based on the approach using three task groups with a fixed classification over time are as follows. First, the lowest wage elasticity of labor supply to the firm, that is, the highest degree of monopsony power, can be observed for NRC workers. Second, this result is mainly due to the lower separation rate elasticity to employment of NRC workers. Third, the share of hires from employment acts as a mitigating factor in the difference of the firm-level labor supply elasticity between NRC workers and workers in other task groups.

1.5.2. Monopsony Power and Task Intensities

Table 1.4.: Labor Supply Elasticity to the Firm by Task Intensities (TI)

	RTI	*NRMTI*	*NRCTI*
Separation rate to employment			
log wage (ϵ^e_{sw} mean TI)	-1.273***	-1.199***	-1.241***
	(0.009)	(0.009)	(0.009)
log wage × TI	-0.315***	-0.181***	0.359***
	(0.007)	(0.007)	(0.007)
ϵ^e_{sw} (high TI)	-1.588	-1.380	-0.882
ϵ^e_{sw} (low TI)	-0.958	-1.018	-1.600
Observations	2,998,063	2,998,063	2,998,063
Separation rate to non-employment			
log wage (ϵ^n_{sw} mean TI)	-1.612***	-1.570***	-1.582***
	(0.006)	(0.006)	(0.006)
log wage × TI	-0.227***	-0.075***	0.222***
	(0.005)	(0.005)	(0.005)
ϵ^n_{sw} (high TI)	-1.839	-1.645	-1.360
ϵ^n_{sw} (low TI)	-1.385	-1.495	-1.804
Observations	5,460,312	5,460,312	5,460,312
Hiring probability from employment			
log wage ($\frac{\epsilon_{\theta w}}{1-\theta}$)	1.725***	1.724***	1.717***
	(0.010)	(0.010)	(0.010)
log wage × TI	-0.114***	-0.098***	0.160***
	(0.008)	(0.008)	(0.009)

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Table 1.4 – continued from previous page

	RTI	*NRMTI*	*NRCTI*
$\epsilon_{\theta w}$ (high TI)	1.052	1.085	1.045
$\epsilon_{\theta w}$ (mean TI)	1.066	1.069	1.082
$\epsilon_{\theta w}$ (low TI)	1.059	1.028	1.104
Observations	979,514	979,514	979,514
Share of hires from employment (θ)			
with high TI	0.347	0.333	0.443
with mean TI	0.382	0.380	0.370
with low TI	0.424	0.436	0.291
Firm-level labor supply elasticity (ϵ_{Lw})			
with high TI	2.288	1.852	0.985
with mean TI	1.689	1.559	1.615
with low TI	1.103	1.277	2.241

Notes: Clustered standard errors at the person level in parentheses. Routine task intensity (RTI), nonroutine manual task intensity (NRMTI), and nonroutine cognitive task intensity (NRCTI) are standardized with mean zero and standard deviation one. Thus, for instance, workers with low RTI are workers with RTI one standard deviation below the mean, and workers with high RTI are workers with RTI one standard deviation above the mean. Same control variables as in Table 1.2. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Source: SIAB and BHP, 1985-2014. Authors' calculations.

In our second estimation approach, we estimate a model including all workers, and interact the wage variable with three task intensity (TI) measures: routine TI (RTI), non-routine manual TI (NRMTI), and non-routine cognitive TI (NRCTI). These time-varying TI measures are assigned to individual workers according to their occupation. This allows us to study the influence of the TI on the labor supply elasticity to the firm on a continuous scale. More details on how we construct task intensities are provided in Section 1.4.2.

The results obtained from this estimation approach (Table 1.4) are in line with those based on the separate estimations by task group presented in the preceding section: The labor supply elasticity of workers performing jobs with high RTI, that is, workers with one standard deviation above the mean RTI value in the sample, equals 2.288. In contrast, the labor supply elasticity of workers performing jobs with low RTI, that is, workers with one standard deviation below the mean RTI value in the sample, is much lower and equals 1.103.[15] Next, we use our continuous measures NRMTI and NRCTI in Table 1.4 to distinguish between non-routine jobs that are cognitive in nature and non-routine jobs that are manual in nature. Workers with high NRMTI have a labor supply elasticity of 1.852, while workers with high NRCTI have a significantly lower labor supply elasticity of 0.985.[16] Again, the results show that all components, apart from the share of hires from employment, contribute to the lower labor supply elasticity for workers with high NRCTI in comparison to workers that have a high RTI or NRMTI. Similarly to the results in Table 1.3, especially the separation rate elasticity to employment is much smaller for workers with high NRCTI than for high RTI or high NRMTI workers.

We perform multiple robustness checks for the estimations in Table 1.4 that are presented in the Appendix (Section 1.E).[17] First, we estimate a full-interaction model in which we interact the TI variable with every control variable in the specification. We find that the results are robust to this specification and that the main results still hold when the coefficients of all covariates are allowed to vary with TI. Second, we use sector-year (interacted) fixed effects so that identification uses only wage variation within sector-year cells. The results in Table 1.4 are robust to this specification. Third, we analyze if the estimated differences in monopsony power for workers with different task intensities are simply driven by the workers' location in the wage distribution. The different location in the wage distribution is relevant as the theoretical model of Burdett and Mortensen (1998) suggests that the labor supply elasticity is falling in wages. To alleviate this concern, we estimate the labor supply elasticity to the firm separately by wage brackets and task intensities. Hence, we compare workers with different task intensities at the same points of the wage distribution. Reassuringly, we find that our general result (workers in

[15] The full set of regression coefficients for the estimations with RTI can be found in Table 1.D.1 in the Appendix.

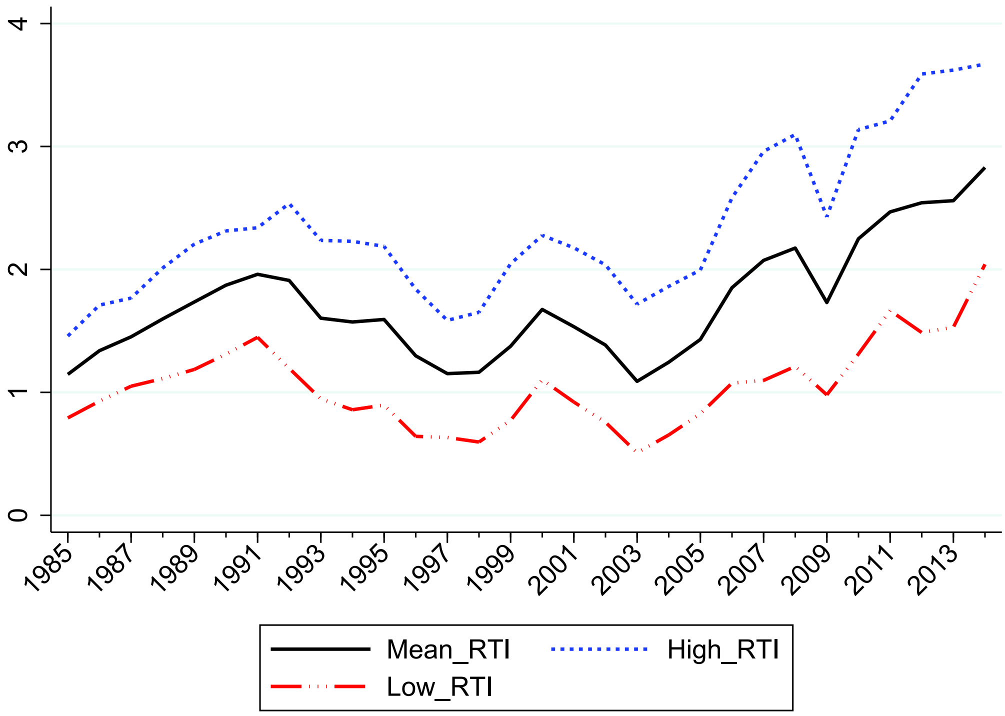
[16] We use imputed wages in Table 1.C.2 in the Appendix. Specifically, we keep all censored wage spells instead of dropping them and apply the imputation procedure outlined in the Appendix to those spells. All estimated labor supply elasticities are lower here, because of the addition of idiosyncratic variation to wages. Furthermore, we use exponential models in Appendix Table 1.A.1. As exponential models do not control for tenure, the estimated elasticities are higher (see Section 1.3 for more details).

[17] We thank two anonymous reviewers for the suggestions.

occupations with high NRCTI have lower labor supply elasticities to the firm) holds even when we compare workers at the same position of the wage distribution.

Given that separate estimations by task groups or interacting wages with task intensities lead to qualitatively similar results, we focus on task intensities in the remaining estimations for two reasons. First, the TI variables are continuous and therefore contain more information on the task content of the worker. Second, the TI measures are updated over time, taking into account that the task content of each occupation changes during the observation period, possibly to a different degree (see Section 1.4.2).

Figure 1.1.: Yearly Labor Supply Elasticities for Workers with Different Routine Task Intensity (RTI)



Notes: The estimates are derived from the same specification as in Table 1.4. Further, a three-way interaction with year dummies is added to analyze the development over time, that is, log wages, RTI and year dummies are interacted. The plotted lines correspond to the sum of the relevant coefficients for workers with mean RTI as well as workers with RTI one standard deviation below ("low RTI") and above ("high RTI") the mean.
Source: Authors' calculations based on SIAB 1985-2014, for West Germany.

We now turn to the question to which extent the estimated labor supply elasticity to the firm changes over time and if there are differences in this trend by RTI. To do so, we add a three-way interaction to the model using RTI (Table 1.4). That is, we interact the wage variable, RTI, and year dummies[18], which allows us to trace the evolution of $log\, wage{*}RTI$ over time. For ease of interpretation, Figure 1.1 plots the obtained yearly labor supply elasticities for workers with low, mean, and high RTI. Clearly, the level differences between workers with low and high RTI found for the pooled sample in Table 1.4 persist, that is, workers with low RTI have lower yearly labor supply elasticities to the firm than workers with high RTI. These differences vary over time, and the labor supply elasticities display a markedly procyclical variation, which confirms the results in Depew and Sørensen (2013) and Hirsch et al. (2018).

Overall, cyclical movements in the elasticity of labor supply to the firm appear to be more important than long-run trends. There is some indication in Figure 1.1 that the labor supply elasticity has been increasing from 2003 onwards. However, it would be premature to interpret this rise as a structural shift in labor market competition, as the German labor market experienced no significant downturn during this time period. This rise could therefore simply be due to good economic conditions, which have generally been found to reduce monopsony power. Even more importantly for our purpose, the increase in the labor supply elasticity is of equal magnitude for workers with low and high RTI. We therefore conclude that labor market polarization, in terms of decreasing outside options for workers with high RTI, has not influenced the degree of monopsony power faced by routine workers to an important degree.[19]

Looking at the components of the labor supply elasticity over time for workers with different RTI levels, we also find no pronounced long-run trend for the separation rate elasticities and the elasticity of the share of recruits from employment.[20] The only component that changes more strongly, the share of recruits from employment, plays the least important role for differences between task groups. Therefore, the relative contributions of the components of the labor supply elasticity to the firm are rather unchanged over time.

We provide two robustness checks for the results obtained in Figure 1.1. First, instead of estimating yearly labor supply elasticities, we use time windows of three years, thereby smoothing the estimates and making them less vulnerable to short-term fluctuations. Appendix Figure 1.A.1 shows that the general pattern over time is comparable to our yearly estimates, and

[18]To be complete, we include the base variables (log wages, RTI, year dummies), the three two-way interactions, and the three-way interaction in the model. In deriving the labor supply elasticities shown in Figure 1.1, we take the sum of the appropriate coefficients.

[19]Theoretically, one could also observe no long-run trend in monopsony power if technological change did have a significant impact that was, however, counterbalanced by one or several other macro factors. However, we do not see an obvious suspect in this context and therefore regard this as an unlikely explanation.

[20]See Figure 1.D.2 in the Appendix.

that the differences by RTI still persist.[21]

Second, up to this point, in our estimations we have used all the variation in wages and transition rates, both across and within workers. The separation rate elasticities may alternatively be estimated with stratified Cox models, in which the baseline hazard $h_{m(i)0}(t)$ is stratified at the worker level. Similarly to the within estimator in linear fixed-effects models, this cancels out the worker-specific effect (Ridder and Tunalı, 1999)[22]. Furthermore, in this robustness test we also use a conditional logit (or fixed-effects logit) model to arrive at an estimate of the wage elasticity of the share of recruits hired from employment.[23] Appendix Figure 1.A.2 shows the estimated labor supply elasticities for each year and by RTI using only within-worker variation. There are two important differences to the results from our baseline model. First, the estimated labor supply elasticities for workers of all RTI levels are higher at the beginning of the observation period and decline sharply from 1985-98 and increase thereafter. Second, differences between workers with low and high RTI are smaller. However, we still find workers with high RTI at their job to show higher labor supply elasticities than workers with low RTI. Our general findings are therefore robust to using only within-worker variation.

Generally, we prefer the estimates based on the Cox model over those obtained from the stratified Cox model for two reasons. First, the stratified Cox model only includes workers in the estimation sample that have at least two employment spells ending in the same transition, which implies that the estimation sample is smaller, and possibly more selective, than the estimation sample of the Cox model without stratification. As workers with different RTI levels could well differ in this respect - for example there may be more non-routine workers who display the required transitions - this kind of sample selection is likely to lead to an estimation bias. Therefore, using the entire sample, that is, estimating without stratification, seems more appropriate. Second, the variation used in the stratified Cox model is purely within-worker variation. Given

[21] Figure 1.D.1 in the Appendix provides an additional robustness test by estimating the labor supply elasticities separately for 3-year-intervals. Thereby, all covariates - and not only RTI and log wages - may have time-variant effects on the separation probabilities. The main results are the same as in Appendix Figure 1.A.1 and Figure 1.1.

[22] The stratified Cox model is a modification of the Cox model. The main difference between the estimators from the two models is that the stratified Cox model allows for the stratification of a predictor, that is, the stratified partial likelihood estimator conditions on the employment spells in the same stratum (worker). The stratified predictors in the stratified Cox model only need to satisfy the proportional hazard assumption for employment spells belonging to the same worker and therefore improve the identification argument in comparison to the Cox model (Kalbfleisch and Prentice, 2011).

[23] We estimate the wage elasticity of the share of recruits hired from employment $\epsilon_{\theta w}$ using the relation $Pr[y_i = 1|x_i, \upsilon_{m(i)}] = \Lambda\Big(x_i'\beta + \upsilon_{m(i)}\Big)$, where $\upsilon_{m(i)}$ is a worker fixed effect. This estimator controls for worker fixed effects by conditioning on those workers who are hired from employment at one point in time and from non-employment at another, and discarding those always hired from the same labor market status.

that workers generally change to jobs with a low task distance (Gathmann and Schönberg, 2010), the within-worker variation in RTI is much smaller than the between-worker variation used in the Cox model without stratification. However, to answer our research questions, comparing workers with different RTI levels seems crucial. Based on these considerations and because the results obtained using between-worker and within-worker variation do not differ qualitatively, we analyze the mechanisms potentially driving differences in monopsony power by task intensities using the Cox model.

1.5.3. Mechanisms

In this section, we explore different mechanisms that may explain our results on the level differences in monopsony power between task groups: collective bargaining agreements, job-specific human capital, and non-pecuniary job characteristics.

Differences by collective bargaining coverage

An important labor-market institution that potentially influences level differences in monopsony power is collective bargaining. Collective bargaining agreements typically increase wages of low-wage workers and compress the industry's wage distribution. This does not necessarily influence any of the sources of monopsony but prevents firms from exercising their monopsony power (Manning, 2003), thereby increasing the estimated labor supply elasticities. Bachmann and Frings (2017) confirm this idea by showing that the estimates of the labor supply elasticity are larger in industries with higher collective bargaining coverage in Germany.

Collective bargaining coverage varies to a large degree at the industry level in Germany. For example, collective bargaining coverage amounts to 91 percent in the public services industry and 38 percent in transportation and logistics for West Germany in 2014 (WSI, 2018). This might affect our estimates of the labor supply elasticity by TI in two ways. First, to the extent that workers with different TI are not randomly distributed across industries, these differences might be driving the link between TI and the labor supply elasticity to the firm. In this case, we should observe much smaller differences in labor supply elasticities by TI within industries than in the whole sample. Second, differences in monopsony power by TI might be influenced by collective bargaining coverage at the industry level, because for instance routine workers are much more often low-wage workers compared to non-routine cognitive workers. Additionally, due to their public nature, collective bargaining agreements can decrease information asymmetries with respect to wages, but not necessarily with respect to non-pecuniary job characteristics that are not part of the collective bargaining process. Thus, we expect collective bargaining agreements to increase the labor supply elasticity of routine workers, but not so much for NRC workers. In this case, we should observe an increase in the labor supply elasticity for routine workers only in industries with a high coverage rate of collective bargaining.

To differentiate between these two channels through which collective bargaining coverage influences the estimated labor supply elasticities by TI, we choose three industries with high[24] and three industries with low[25] collective bargaining coverage, while ensuring that each industry employs workers with varying TI. We omit industries with average collective bargaining coverage because possible differences in the relationship between TI and monopsony power will be easier to detect in the tails of the collective bargaining coverage distribution. Also, this allows us to neglect changes over time in bargaining coverage. We then run our baseline model for both groups of industries separately.[26] We summarize our results in Table 1.5.[27]

In line with theoretical expectations, Table 1.5 shows that the labor supply elasticity to the firm is much lower in industries with a low coverage rate of collective bargaining. The labor supply elasticity decreases by about 63 percent for workers with high NRCTI from high collective bargaining coverage industries to low collective bargaining coverage industries, while it decreases by about 31 percent for workers with high RTI and 18 percent for workers with high NRMTI. This indicates that collective bargaining status has a strong counteracting effect on the monopsony power of firms, especially for workers with high NRCTI. However, the differences in labor supply elasticities for workers with high RTI, high NRMTI, and high NRCTI persist independently of collective bargaining coverage. We can thus draw two conclusions: First, our main results are not strongly driven by composition effects with respect to industries. Second, collective bargaining coverage does not influence differences in monopsony power between task groups.

[24]These are the finance and insurance, public administration, and construction industry with coverage rates of 73-89 percent, 83-91 percent, and 67-83 percent in the years 1998-2014 (WSI, 2018), respectively.

[25]These are the trade and repair, transport and communications as well as the catering and hotel industry with coverage rates of 37-65 percent, 38-61 percent, and 40-48 percent in the years 1998-2014 (WSI, 2018), respectively.

[26]The industry variable indicates the economic activity as a 3-digit code and provides time-consistent information. We use the generated time-consistent industry codes in Eberle et al. (2014).

[27]In Tables 1.D.2, 1.D.3, and 1.D.4 in the Appendix we show the full estimation results of all the components of the labor supply elasticity for industries with different collective bargaining coverage by RTI, NRMTI and NRCTI respectively.

Table 1.5.: Labor Supply Elasticity to the Firm by Task Intensities and Collective Bargaining Coverage

	High coverage	Low coverage	Baseline
Firm-level labor supply elasticity (ϵ_{Lw})			
with high RTI	2.010	1.379	2.288
with high NRMTI	1.510	1.237	1.852
with high NRCTI	1.044	0.387	0.985

Notes: Clustered standard errors at the person level in parentheses. Routine task intensity (RTI), nonroutine manual task intensity (NRMTI), and nonroutine cognitive task intensity (NRCTI) are standardized with mean zero and standard deviation one. Thus, for instance, workers with low RTI are workers with RTI one standard deviation below the mean, and workers with high RTI are workers with RTI one standard deviation above the mean. Same control variables as in Table 1.2.
Source: SIAB and BHP, 1985-2014. Authors' calculations.

The role of job-specific human capital

In Section 1.2 we argue that job-specific human capital is an important source of monopsony power in the labor market. Workers who have accumulated a high amount of job-specific human capital can be expected to have a relatively low incentive to switch jobs to improve their wage. Hence, as workers do not want to lose their accumulated job-specific human capital, the labor supply elasticity to the firm with respect to wages can be expected to decrease with higher job-specific human capital, thereby increasing the monopsony power of employers.

Job-specific human capital should be more important as a source of monopsony power for NRC workers than for other task groups as NRC workers perform more complex tasks at their job (Acemoglu and Autor, 2011; Booth and Zoega, 2008). Gathmann and Schönberg (2010) propose the concept of task-specific human capital - which is strongly related to job-specific human capital - and show that workers generally move to occupations with similar task requirements. Workers lose task-specific human capital if the tasks in the new job are very different from the old one. We expect that NRC workers have a lower arrival rate of job offers suiting their current task profile which is relatively complex. We further expect that NRC workers have a low incentive to switch to a new job in which they perform different tasks than in their current job because this would imply a relatively large loss of job-specific human capital. In consequence, the labor supply elasticity to the firm with respect to wages is likely to be lower for NRC workers than for other task groups, that is NRC workers are likely to be exposed to a higher degree of monopsony power.

To provide evidence regarding these hypotheses, we estimate the separation rate elasticities for workers in different job tenure brackets - proxying different degrees of accumulated job-specific human capital - and with different task intensities. We focus on the separation rate elasticities because all job-specific human capital is lost once a worker quits his job. Therefore, the separation rate elasticities are the components of the labor supply elasticity to the firm in Equation 1.4 which are most directly related to job-specific human capital. Table 1.6 presents the results for the separation rate elasticities for different job tenure brackets and workers with different task intensities.[28]

All estimated elasticities are small in comparison to the baseline results in Appendix Table 1.A.1, because the correlation between separations and log wages is - by construction - smaller within tenure brackets than across all tenure brackets.[29] It is therefore not possible to interpret the size of the elasticities, but it is possible to compare differences in the elasticities between task groups within each tenure bracket. Analyzing the separation rate elasticity to employment in more detail, we find for the first tenure bracket (0-3 years) that the elasticity is twice as high for high-RTI workers than for high-NRCTI workers. In the last tenure bracket (10+ years), the separation rate elasticity of high-RTI workers is 3.6 times higher than the elasticity of high-NRCTI workers. This means that the relative difference in the separation rate elasticities to employment almost doubles as tenure increases. Noticeably, there are hardly any differences between high-RTI and high-NRMTI workers. For the elasticity of the separation rate to non-employment, we generally find the same pattern but the differences between high-NRCTI and high-RTI/NRMTI workers do not increase as strongly across tenure brackets.

In sum, this exercise provides suggestive evidence that high-NRCTI workers value job-specific human capital more strongly when considering a separation to employment than workers performing routine or NRM tasks. At the same time, job-specific human capital is less important to high-NRCTI workers when considering a separation to non-employment. Therefore, for high-NRCTI workers, job-specific human capital has an important impact on separations to

[28]The coefficients, standard errors and number of observations used for the estimations can be found in Table 1.D.5 in the Appendix. We use exponential models in Table 1.6, because by estimating the separation rate elasticities for different job tenure brackets we already control for job tenure. Appendix Table 1.A.1 shows our baseline results with exponential models without differentiating tenure brackets.

[29]The underlying reason is that tenure itself is determined by wages. See Section 1.3 for a detailed discussion.

Table 1.6.: Separation Rate Elasticities by Task Intensities and Tenure Brackets

	High RTI	High NRMTI	High NRCTI
Separation rate elasticity			
to employment (ϵ_{sw}^{e})			
Job Tenure: 0-3 years	-1.066	-0.891	-0.505
Job Tenure: 3-10 years	-0.916	-0.783	-0.293
Job Tenure: 10+ years	-0.698	-0.678	-0.191
Separation rate elasticity			
to non-employment (ϵ_{sw}^{n})			
Job Tenure: 0-3 years	-1.446	-1.254	-1.058
Job Tenure: 3-10 years	-1.251	-1.132	-0.803
Job Tenure: 10+ years	-1.092	-1.006	-0.705

Notes: We use exponential models for this table. The table shows separation rate elasticities for high routine task intensity (RTI), high nonroutine manual task intensity (NRMTI), and high nonroutine cognitive task intensity (NRCTI) workers. To compute the elasticity of high TI workers we add the coefficient of the interaction term to the coefficient of the log wage in the respective estimations. RTI, NRMTI and NRCTI are standardized with mean zero and standard deviation one. Thus, for instance, workers with low RTI are workers with RTI one standard deviation below the mean, and workers with high RTI are workers with RTI one standard deviation above the mean. Same control variables as in Table 1.2.
Source: SIAB and BHP, 1985-2014. Authors' calculations.

employment which contributes to the relatively high monopsony power these workers are facing.

The role of non-pecuniary job characteristics

As we discuss in Section 1.2, non-pecuniary job characteristics are likely to differ between workers performing different job tasks, and therefore to lead to different levels of monopsony power between these workers. In the following, we therefore analyze the prevalence of specific non-pecuniary job characteristics by task intensities and the change in these job characteristics over time. The BIBB data described in Section 1.4.2 allow us to do so because the dataset not only contains information on job tasks, but also on various non-pecuniary job characteristics and on workers' satisfaction with those characteristics. Specifically, we construct several dependent variables which capture to what extent a non-pecuniary characteristic (for example satisfaction with promotion opportunities) is present. This generally results in ordinal discrete variables with more than two outcomes and natural ordering. We regress these dependent variables on task group dummies and additional control variables separately for each BIBB wave.[30]

The results from this exercise are presented in Table 1.7. Panel A shows that NRC workers are less likely to work in unfavorable physical working conditions such as extreme temperatures, noise, and unfavorable body positions relative to routine workers, which is in line with expectations. For example, the odds ratio of answering the question of whether one works in a physically unfavorable position with high approval versus the combined lower approval categories is 0.456 times lower for NRC workers than for routine workers in 1985.

Panel B of Table 1.7 features questions on the mental working conditions of workers such as working under strong deadline or performance pressure, perceiving the workplace as part of a community, and cooperation with colleagues. Here we find that NRC workers are generally more likely to work under strong deadline or performance pressure than routine workers. For the 2006 wave, we also find that NRC workers are more likely to perceive the workplace as part of a community and to appreciate the cooperation with colleagues.

Panel C of Table 1.7 shows the satisfaction with different non-pecuniary job characteristics of workers in different task categories. In all BIBB waves where this question was asked, we find that NRC workers are generally more likely to be satisfied with their job than routine workers. For example, the odds ratio of being very satisfied with the current job versus the combined lower satisfaction categories is 1.242 times higher for NRC workers than for routine workers in 2012. Looking at sub-categories of job satisfaction, we find that NRC workers, relative to routine workers, are generally more likely to be satisfied with their promotion opportunities, the work climate (significant for one survey wave only), the type and content of tasks at the job, the ability to use own skills, and the available training opportunities. At the same time, we do not find any higher likelihood for NRM workers in panel C, indicating that they are either equally or less satisfied than routine workers.

[30] As the main advantage in using our TI measures lies in its continuous updating over time and the separate estimation by BIBB wave cancels this variation, we opt to focus on task groups here. Moreover, using task groups in this context facilitates the interpretation of the results.

Table 1.7.: Nonpecuniary Job Characteristics by Task Group. Odds Ratios from Regression Analysis

Dependent variable	1985		1992		1999		2006		2012	
	NRM	NRC	NRM	NRC	NRM	NRC	NRM	NRC	NRM	NRC
Panel A: Physical Working Conditions										
Work in cold, hot, humid, wet or draught conditions	1.259***	0.484***	0.698***	0.576***	1.109	0.448***	1.219*	0.258***	2.467***	0.815*
	(0.078)	(0.027)	(0.063)	(0.053)	(0.077)	(0.026)	(0.127)	(0.018)	(0.234)	(0.097)
Work under noisy conditions	1.095	0.470***	0.655***	0.512***	1.019	0.395***	0.735***	0.241***	2.845***	0.992
	(0.067)	(0.026)	(0.059)	(0.047)	(0.071)	(0.022)	(0.077)	(0.017)	(0.268)	(0.115)
Work in a physically unfavourable position	0.845***	0.456***	0.571***	0.626***	0.869**	0.409***	0.880	0.329***	2.917***	1.023
	(0.053)	(0.027)	(0.052)	(0.059)	(0.060)	(0.024)	(0.093)	(0.024)	(0.292)	(0.131)
Panel B: Mental Working Conditions										
Work under strong deadline or performance pressure	0.805***	1.311***	0.723***	1.629***	0.650***	1.381***	0.998	1.502***	0.888	1.115
	(0.049)	(0.067)	(0.054)	(0.127)	(0.045)	(0.073)	(0.115)	(0.114)	(0.086)	(0.138)
Perceiving the workplace as part of a community					0.890	0.918	0.896	1.319***	0.774**	0.977
					(0.070)	(0.056)	(0.121)	(0.119)	(0.096)	(0.151)

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Table 1.7 – continued from previous page

Dependent variable	1985		1992		1999		2006		2012	
	NRM	NRC	NRM	NRC	NRM	NRC	NRM	NRC	NRM	NRC
Cooperation with colleagues							0.987	1.315**	0.883	1.245
							(0.171)	(0.156)	(0.136)	(0.251)
Panel C: Satisfaction										
Satisfied with job overall	0.650***	1.550***	0.888	1.484***			0.910	1.273***	0.959	1.242*
	(0.045)	(0.090)	(0.079)	(0.131)			(0.111)	(0.097)	(0.099)	(0.155)
Satisfied with the promotion opportunities			0.748***	1.350***	0.804***	1.491***	0.870	1.402***	0.969	0.980
			(0.059)	(0.112)	(0.057)	(0.085)	(0.094)	(0.099)	(0.093)	(0.115)
Satisfied with the work climate			0.855*	1.003	0.776***	1.013	0.968	1.310***	0.916	1.037
			(0.070)	(0.085)	(0.058)	(0.058)	(0.107)	(0.092)	(0.087)	(0.121)
Satisfied with the type and content of tasks			0.652***	1.608***	0.717***	1.568***	0.922	1.570***	0.947	1.292**
			(0.058)	(0.140)	(0.057)	(0.093)	(0.113)	(0.120)	(0.098)	(0.162)

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Table 1.7 – continued from previous page

Dependent variable	1985		1992		1999		2006		2012	
	NRM	NRC	NRM	NRC	NRM	NRC	NRM	NRC	NRM	NRC
Satisfied with the possibility to use own skills			0.620***	1.598***	0.697***	1.597***	0.919	1.603***	1.019	1.267**
			(0.051)	(0.137)	(0.053)	(0.095)	(0.107)	(0.119)	(0.100)	(0.152)
Satisfied with the training opportunities			0.742***	1.424***	0.801***	1.542***	0.794**	1.627***	1.128	1.292**
			(0.058)	(0.119)	(0.057)	(0.088)	(0.086)	(0.114)	(0.107)	(0.150)
Number of observations	10,384		5,949		8,619		4,405		3,274	

Notes: Odds ratios from ordered logit and logit models. Results are from ordered logit models except for the 1992 wave, where logit models are used for all dependent variables in Panel A. Missing cells indicate questions that were not asked in a the particular BIBB wave. We recoded the dependent variables such that the lowest value of a variable shows a low level of approval while the highest value shows the highest level of approval. Standard errors are provided in parentheses. We include controls for federal state, sector, education, age, establishment size, immigrant worker, job tenure, and job tenure squared in the estimation. Routine workers are the base category. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Source: BIBB 1985, 1992, 1999, 2006 and 2012 waves. Authors' calculations.

Summarizing, the descriptive evidence in Table 1.7 shows that NRC workers enjoy better non-pecuniary job characteristics and there is no indication for trends over time. Our results complement the literature on non-pecuniary job characteristics which shows that workers are willing to accept lower wages in exchange for better non-pecuniary working conditions (see for example Maestas et al., 2018; Mas and Pallais, 2017) and sometimes make a transition to jobs with lower wages compensated with better non-pecuniary job characteristics (Sorkin, 2018; Sullivan and To, 2014). Our estimation results indicate that non-pecuniary job characteristics are more important for NRC workers than for other worker groups. This in turn implies that wages play a smaller role in the mobility decisions of NRC workers. Therefore, employers have higher wage-setting power towards NRC workers because of non-pecuniary job characteristics. These job characteristics are therefore an important source of monopsony power for NRC workers.

1.6. Conclusion

In this paper, we investigate the link between technological change and job tasks on the one hand, and the degree of monopsony power on the other hand. To estimate the degree of monopsony power, we use the semi-structural estimation approach proposed by Manning (2003), which allows us to identify the wage elasticity of labor supply to the firm. Our analysis is based on two unique data sets from Germany: an administrative data set on individual labor market histories spanning the years 1985-2014 which provides exact information on wages and labor market transitions; and worker-level survey data on job tasks which allows us to compute time-varying measures of job task intensities at the occupational level, and which we merge to the administrative data set. This approach goes beyond many papers in the job task literature as we are able to measure intensities for routine, non-routine cognitive (NRC) and non-routine manual (NRM) job tasks on a continuous scale, and to account for changes in task intensities over time.

Our results indicate that workers who perform jobs with a high routine task content face a higher wage elasticity of labor supply to the firm than workers performing mainly NRC tasks. This means that workers specializing in NRC tasks are subject to higher monopsony power by employers. When decomposing the wage elasticities for routine, NRC, and NRM workers, we find that this result mainly arises because NRC workers react much less to wages in their decision to separate to employment than routine workers.

When analyzing the evolution of monopsony power over time, we find no long-run trends in the labor supply elasticity to the firm for any worker group, including high-RTI workers, and therefore conclude that the de-routinization of the labor market has not influenced the degree of monopsony power faced by routine workers to a significant degree. This result is somewhat surprising: as explained in Section 1.2, in a Burdett and Mortensen (1998)-type of labor market, we would have expected the lower demand for routine workers to decrease the job offer arrival rate for these workers resulting in less job mobility, with additional amplification effects reinforcing the original demand factors and leading to an increase in monopsony power. Such amplification effects can arise because workers in declining task groups become more risk averse in their mobility decisions. Given that we observe a relatively constant labor supply elasticity over time, we can conclude that there are no amplification effects in the long run.

There are two possible explanations for our result of a relatively constant monopsony power. First, there could be composition effects, which are neglected in the Burdett and Mortensen (1998) model which assumes ex ante identical workers. As shown by Böhm et al. (2019) recently, workers leaving shrinking occupations and entering growing occupations are predominantly low-wage (relative to their peer group). These labor-market transitions have a composition effect for occupations: In shrinking occupations, average worker quality rises. Therefore, the job-offer arrival rate to workers in shrinking occupations can be expected not to decline as strongly, because firms know that the workers remaining in these occupations are (relatively) high-skilled workers with high productivity, and hence try to poach them from rivals. While this seems a potential explanation in this context, the results on our analysis on non-wage job characteristics do not indicate large composition effects. Second, our research question relates to long-run developments as opposed to the studies on the cyclicality of monopsony power such as Hirsch et al. (2018) or Webber (2022) who find monopsony power to react to changes in demand. It seems conceivable that workers react very differently to short-term changes in demand such as business cycle developments than they do in response to long-run changes such as the polarization of the labor market. Analyzing these two potential explanations for our finding are therefore important avenues for future research.

In the final part of our analysis, we explore potential mechanisms leading to level differences in monopsony power between workers performing different job tasks, especially to explain the higher monopsony power towards NRC workers. An analysis of the separation elasticity to employment by tenure bracket indicates that job-specific human capital plays a more important role for NRC workers, which increases firms' monopsony power towards these workers. Furthermore, non-pecuniary job characteristics such as working conditions and job satisfaction seem to play a much more important role for NRC workers, again increasing firms' monopsony power towards these workers. Finally, we find that the labor supply elasticity to the firm is much lower in industries with a low coverage rate of collective bargaining than in industries with a high coverage rate of collective bargaining. However, the differences in monopsony power between worker groups are not driven by composition effects in terms of industries employing workers with varying levels of task intensities. Therefore, unions do not seem to play a role for differences in monopsony power between workers performing different job tasks.

Our results have two important implications. First, the cross-sectional differences in monopsony power show that job tasks are another individual-level dimension in explaining wage gaps between worker groups, similar to earlier results in the literature, for instance with respect to gender or nationality. Our results suggest that controlling for job tasks could provide an additional explanation for monopsony power workers face, and hence for the resulting wage gaps. Second, our finding that monopsony power does not display a long-run trend may come as a surprise, particularly with respect to routine workers, as the job opportunities of routine workers have declined strongly in recent decades with ongoing labor market polarization caused by technological progress. Nevertheless, our results imply that changes in monopsony power do not seem to be a factor contributing to increased labor-market inequality in Germany in recent decades.

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Appendix

1.A. Additional Tables and Figures

Table 1.A.1.: Labor Supply Elasticity to the Firm by Task Intensities (TI) – Exponential Model

	RTI	*NRMTI*	*NRCTI*
Separation rate to employment			
log wage (ϵ^e_{sw} mean TI)	-1.454***	-1.376***	-1.420***
	(0.011)	(0.011)	(0.011)
log wage × TI	-0.333***	-0.195***	0.383***
	(0.009)	(0.009)	(0.009)
ϵ^e_{sw} (high TI)	-1.787	-1.571	-1.037
ϵ^e_{sw} (low TI)	-1.121	-1.181	-1.803
Observations	2,998,063	2,998,063	2,998,063
Separation rate to non-employment			
log wage (ϵ^n_{sw} mean TI)	-1.849***	-1.802***	-1.816***
	(0.008)	(0.008)	(0.008)
log wage × TI	-0.255***	-0.106***	0.266***
	(0.007)	(0.007)	(0.007)
ϵ^n_{sw} (high TI)	-2.104	-1.908	-1.550
ϵ^n_{sw} (low TI)	-1.594	-1.696	-2.082
Observations	5,460,312	5,460,312	5,460,312
Hiring probability from employment			
log wage ($\frac{\epsilon_{\theta w}}{1-\theta}$)	1.725***	1.724***	1.717***
	(0.010)	(0.010)	(0.010)
log wage × TI	-0.114***	-0.098***	0.160***

Continued on next page

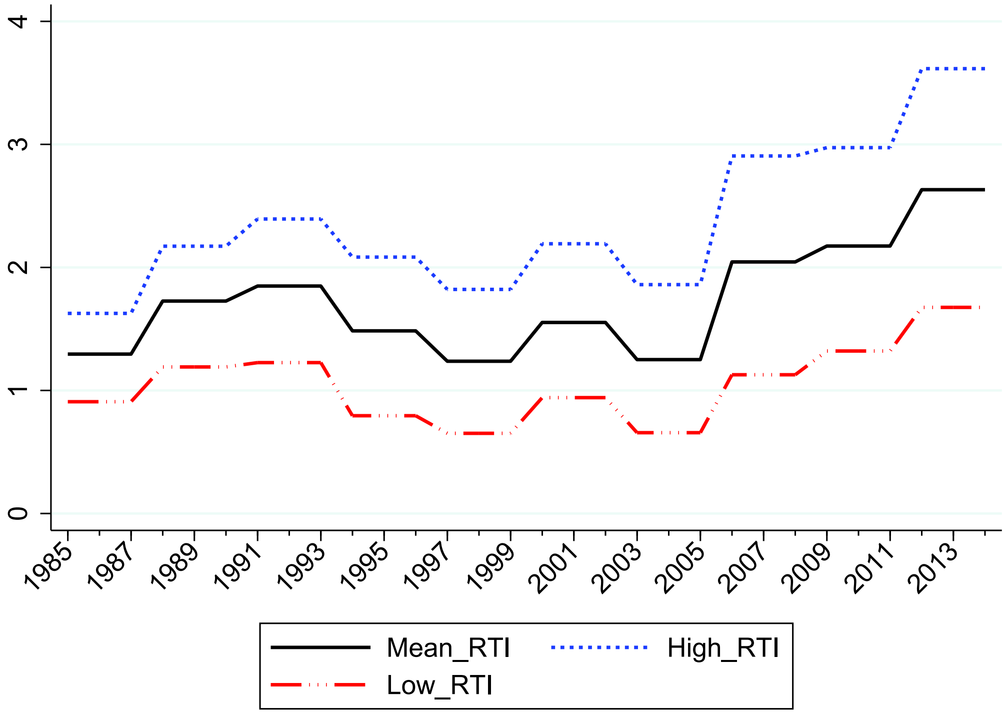
Table 1.A.1 – continued from previous page

	RTI	*NRMTI*	*NRCTI*
	(0.008)	(0.008)	(0.009)
$\epsilon_{\theta w}$ (high TI)	1.052	1.085	1.045
$\epsilon_{\theta w}$ (mean TI)	1.066	1.069	1.082
$\epsilon_{\theta w}$ (low TI)	1.059	1.028	1.104
Observations	979,514	979,514	979,514
Share of hires from employment (θ)			
with high TI	0.347	0.333	0.443
with mean TI	0.382	0.380	0.370
with low TI	0.424	0.436	0.291
Firm-level labor supply elasticity (ϵ_{Lw})			
with high TI	2.729	2.282	1.314
with mean TI	2.086	1.947	2.008
with low TI	1.455	1.625	2.700

Notes: Clustered standard errors at the person level in parentheses. Routine task intensity (RTI), nonroutine manual task intensity (NRMTI), and nonroutine cognitive task intensity (NRCTI) are standardized with mean zero and standard deviation one. Thus, for instance, workers with low RTI are workers with RTI one standard deviation below the mean, and workers with high RTI are workers with RTI one standard deviation above the mean. Same control variables as in Table 1.2. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

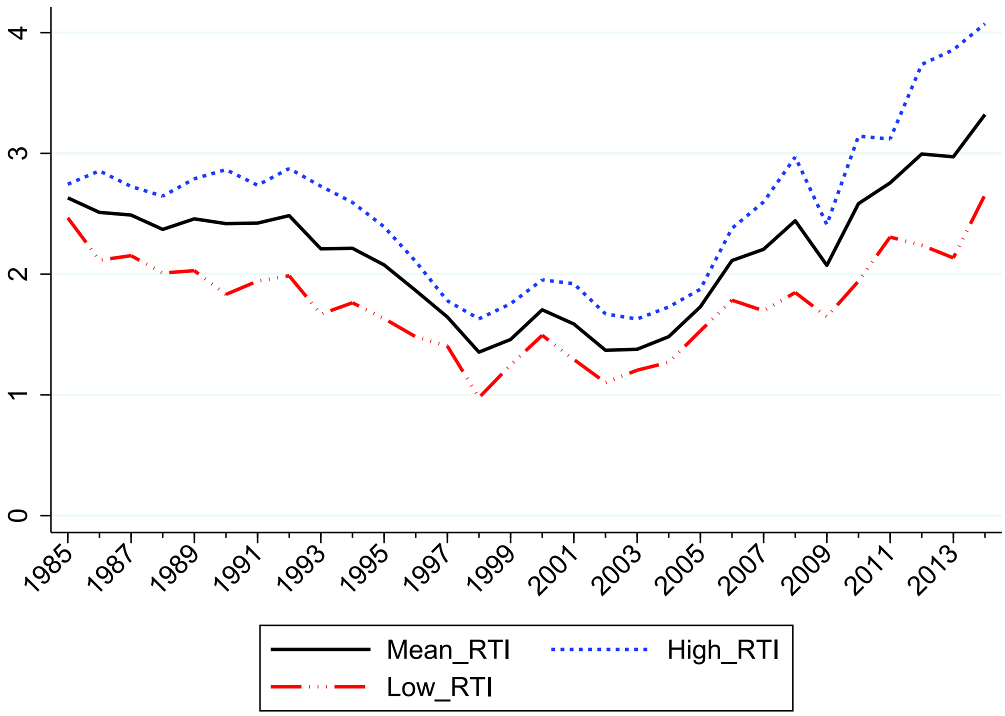
Source: SIAB and BHP, 1985-2014. Authors' calculations.

Figure 1.A.1.: Labor Supply Elasticities for Workers with Different Routine Task Intensity (RTI) over Three-Year-Intervals



Notes: The estimates are derived from the same specification as in Table 1.4. Further, a three-way interaction with three-year dummies is added to analyze the development over time; that is, log wages, RTI, and three-year dummies are interacted. The plotted lines correspond to the sum of the relevant coefficients for workers with mean RTI as well as workers with RTI one standard deviation below ("low RTI") and above ("high RTI") the mean.
Source: Authors' calculations based on SIAB 1985-2014, for West Germany.

Figure 1.A.2.: Yearly Labor Supply Elasticities for Workers with Different Routine Task Intensity (RTI) – Within-Worker Variation



Notes: The estimates are derived from a stratified Cox model using the same control variables as in Table 1.4. Further, a three-way interaction with year dummies is added to analyze the development over time; that is, log wages, RTI and year dummies are interacted. The plotted lines correspond to the sum of the relevant coefficients for workers with mean RTI as well as workers with RTI one standard deviation below ("low RTI") and above ("high RTI") the mean.
Source: Authors' calculations based on SIAB 1985-2014, for West Germany.

1.B. Derivation of Equation 1.4

In order to get from Equation 1.3 to Equation 1.4, we need to replace ϵ_R^e and ϵ_R^n from Equation 1.3. First, we show how to express ϵ_R^n in terms of ϵ_R^e and $\epsilon_{\theta R}$. Starting with the definition of the share of recruits coming from employment, θ_R, it follows:

$$\begin{aligned}
\theta^R &= \frac{R^e}{R^e + R^n} \\
\theta^R(R^e + R^n) &= R^e \\
R^n &= \frac{R^e}{\theta^R} - R^e \\
R^n &= \frac{1-\theta^R}{\theta^R} R^e
\end{aligned}$$

Taking logs and differentiating with respect to w yields[31]

$$\begin{aligned}
logR^n &= log\frac{1-\theta^R}{\theta^R} + logR^e \\
\frac{R^{n\prime}}{R^n} &= \frac{\theta^R}{1-\theta^R}\left(\frac{-\theta^{R\prime}\theta^R - \theta^{R\prime}(1-\theta^R)}{(\theta^R)^2}\right) + \frac{R^{e\prime}}{R^e} \\
\frac{R^{n\prime}}{R^n} &= \frac{R^{e\prime}}{R^e} - \frac{1}{1-\theta^R}\left(\frac{\theta^{R\prime}}{\theta^R}\right)
\end{aligned}$$

From the definition of a wage elasticity ($\epsilon_x = \frac{x'}{x}/\frac{w'}{w} = w\frac{x'}{x}$), we have

$$\begin{aligned}
\frac{1}{w}\epsilon_R^n &= \frac{1}{w}\epsilon_R^e - \frac{1}{1-\theta^R}\left(\frac{\theta^{R\prime}}{\theta^R}\right) \\
\epsilon_R^n &= \epsilon_R^e - w\frac{1}{1-\theta^R}\left(\frac{\theta^{R\prime}}{\theta^R}\right) \\
\epsilon_R^n &= \epsilon_R^e - \frac{1}{1-\theta^R}\epsilon_{\theta^R}
\end{aligned} \tag{1.B.8}$$

Second, we show how to express ϵ_R^e in terms of ϵ_S^e and θ_R, i.e. $\epsilon_R^e = \frac{-\theta_S \epsilon_S^e}{\theta_R}$. In doing so, we follow Hirsch (2010).

Let $\varphi(x/w)$ be the probability that an employed worker who currently receives wage w accepts a job which offers wage x, and let $F(x)$ be the distribution of wage offers. The separation rate to employment of a firm paying wage w can then be expressed as

[31] Note that R^e, R^n and θ depend on w.

$$s^e(w) = \lambda^e \int_{\underline{w}}^{\overline{w}} \varphi(x/w)\ dF(x)$$

with derivative

$$\frac{ds^e\,(w)}{dw} = -\lambda^e \int_{\underline{w}}^{\overline{w}} \frac{\varphi'(x/w)x}{w^2}\ dF(x).$$

The firm's number of recruits from employment is

$$R^e(w) = \lambda^e \int_{\underline{w}}^{\overline{w}} \varphi(x/w)\, L(x)\, dF(x)$$

with derivative

$$\frac{dR^e(w)}{dw} = \lambda^e \int_{\underline{w}}^{\overline{w}} \frac{\varphi'(x/w)\, L(x)}{x}\ dF(x).$$

Using this result, the separations-weighted separation elasticity can be written as follows:

$$\begin{aligned}
\int_{\underline{w}}^{\overline{w}} \varepsilon^e_{sw}(x)\, s^e(x)\, L(x)\, dF(x) &= \int_{\underline{w}}^{\overline{w}} \frac{ds^e(x)}{dx} \frac{x}{s^e(x)} s^e(x)\, L(x)\, dF(x) \\
&= \int_{\underline{w}}^{\overline{w}} \left(-\lambda^e \int_{x}^{\overline{w}} \frac{\varphi'(z/x)\, z}{x^2}\ dF(z) \right) x\, L(x)\, dF(x) \\
&= -\lambda^e \int_{\underline{w}}^{\overline{w}} \int_{x}^{\overline{w}} \frac{\varphi'(z/x) z L(x)}{x} dF(z) dF(x) \\
&= -\int_{\underline{w}}^{\overline{w}} \frac{dR^e(x)}{dx} x\, dF(x) \\
\int_{\underline{w}}^{\overline{w}} \varepsilon^e_{sw}(x)\, s^e(x)\, L(x)\, dF(x) &= -\int_{\underline{w}}^{\overline{w}} \varepsilon^e_{Rw}(x)\, R^e(x)\, dF(x). \qquad (1.B.9)
\end{aligned}$$

Note that in steady state, for the aggregate economy it holds that $s^e(x)L(x) = \theta_s S(x)$ for separations to employment and $R^e(x) = \theta_R R(x)$ for hirings from employment. It follows for

Equation 1.B.9:

$$\begin{aligned}
\int_{\underline{w}}^{\overline{w}} \varepsilon_{sw}^{e}(x)\, s^{e}(x)\, L(x)\, dF(x) &= -\int_{\underline{w}}^{\overline{w}} \varepsilon_{Rw}^{e}(x)\, R^{e}(x)\, dF(x) \\
\int_{\underline{w}}^{\overline{w}} \varepsilon_{sw}^{e}(x)\, \theta_s S(x)\, dF(x) &= -\int_{\underline{w}}^{\overline{w}} \varepsilon_{Rw}^{e}(x)\, \theta_R R(x)\, dF(x) \\
\int_{\underline{w}}^{\overline{w}} \varepsilon_{Rw}^{e}(x)\, R(x)\, dF(x) &= -\frac{\theta_s}{\theta_R}\int_{\underline{w}}^{\overline{w}} \varepsilon_{sw}^{e}(x)\, S(x)\, dF(x)
\end{aligned}$$

which can be written as $\epsilon_R^e = -\frac{\theta_s}{\theta_R}\epsilon_s^e$.

Substituting ϵ_R^n (from Equation 1.1) and ϵ_R^e into Equation 1.3 in the article yields the following:

$$\begin{aligned}
\epsilon_{Lw} &= \theta_R\epsilon_R^e + (1-\theta_R)\epsilon_R^n - \theta_s\epsilon_s^e - (1-\theta_s)\epsilon_s^n \\
&= \theta_R\left(\frac{-\theta_s\epsilon_s^e}{\theta_R}\right) + (1-\theta_R)\left[\epsilon_R^e - \frac{w\theta_R'}{\theta_R(1-\theta_R)}\right] - \theta_s\epsilon_s^e - (1-\theta_s)\epsilon_s^n \\
&= -2\theta_s\epsilon_s^e - (1-\theta_R)\frac{\theta_s\epsilon_s^e}{\theta_R} - (1-\theta_R)\frac{w\theta_R'}{\theta_R(1-\theta_R)} - (1-\theta_s)\epsilon_s^n
\end{aligned}$$

Note that in steady state, $\theta_R = \theta_s$. It follows:

$$\begin{aligned}
\epsilon_{Lw} &= -2\theta\epsilon_s^e - (1-\theta)\epsilon_s^e - \frac{w\theta'}{\theta} - (1-\theta)\epsilon_s^n \\
&= -(1+\theta)\epsilon_s^e - (1-\theta)\epsilon_s^n - \frac{w\theta'}{\theta} \\
&= -(1+\theta)\epsilon_s^e - (1-\theta)\epsilon_s^n - \epsilon_\theta
\end{aligned}$$

where the last equality follows from the definition of the wage elasticity of θ: $\epsilon_\theta = \frac{w\theta'}{\theta}$, and we have shown that Equation 1.3 follows from Equation 1.4.

1.C. Imputation of Wages

To examine whether the high incidence of censoring for NRC jobs affects our main results, we implement robustness checks by keeping all censored spells in the sample and imputing the daily wage of these censored spells. In doing so, we use the procedure outlined in Dustmann et al. (2009); Gartner (2005), and Card et al. (2013). In the following we use the notation of Card et al. (2013). We assume that the error term in the wage regression is normally distributed with a variance which differs by year, education and age group. Then we draw a random value of y (i.e. ln(wage)) from a normal distribution $\mathcal{N}(x'\hat{\beta}, \sigma^2)$. In other words, we add an error term with the standard deviation σ to the expected wage. We use the σ from the Tobit estimation

$$y_i = x_i'\hat{\beta} + \eta_i. \tag{1.C.10}$$

In order to draw the imputed wage so that it is above the social security contribution limit, we draw from a truncated distribution. Let c be the censoring point. We use $k = \Phi[(c - x_i'\hat{\beta})/\sigma]$, where Φ represents the standard normal density. Also, let $u \sim U[0, 1]$ represent a uniform random variable. Then we impute an uncensored value for y as

$$y_i = x_i'\hat{\beta} + \sigma\Phi^{-1}[k + u \times (1 - k)]. \tag{1.C.11}$$

We fit a series of Tobit models to log daily wages separately by year for the years 1985-2014, age group (years 18-25, 26-35, 36-45, 46-55) and education group (without vocational and school degree lower than Abitur, with vocational training or Abitur or with a university degree) and impute an uncensored value for each censored observation using the estimated parameters from the model and a random draw from the associated (left-censored) distribution (Card et al., 2013). As in Card et al. (2013) we include the following variables in the Tobit estimations: age, mean log wage in other years, fraction of censored wages in other years, number of full time male employees at the current firm and its square, dummy for 11 or more employees in the firm, fraction of university graduates at the current firm, dummy for individuals observed only 1 year between 1985 and 2014, dummy for employees of 1-worker firm. Thus, as in Card et al. (2013), we replace each censored wage value with a random draw from the upper tail of the appropriate conditional wage distribution. We display the results in Tables 1.C.1 and 1.C.2. Comparing these results with the baseline specification excluding jobs spells with censored wages (displayed in Tables 2 and 4) shows that the results are similar with respect to the differences between task groups. The labor supply elasticities are smaller in size when including imputed wages, mainly because of the additional idiosyncratic variation in wages introduced by the imputation procedure.

Table 1.C.1.: The Labor Supply Elasticity to the Firm by Task Group with Imputed Wages

	Routine	*NRM*	*NRC*
Separation rate to employment			
log wage (ϵ_{sw}^{e})	-1.153***	-1.140***	-0.640***
	(0.012)	(0.018)	(0.015)
Observations	1,866,139	510,170	1,053,137
Separation rate to non-employment			
log wage (ϵ_{sw}^{n})	-1.523***	-1.555***	-1.097***
	(0.008)	(0.013)	(0.013)
Observations	3,554,950	954,905	1,753,047
Hiring probability from employment			
log wage ($\frac{\epsilon_{\theta w}}{1-\theta}$)	1.578***	1.443***	1.585***
	(0.011)	(0.019)	(0.015)
$\epsilon_{\theta w}$	0.953	0.965	0.804
Observations	593,383	202,110	264,820
Share of hires from employment (θ)	0.396	0.331	0.493
Firm-level labor supply elasticity (ϵ_{Lw})	1.576	1.592	0.708

Notes: Cox model. Clustered standard errors at the person level in parentheses. Same control variables as in Table 1.2. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level respectively.
Source: SIAB and BHP, 1985-2014. Authors' calculations.

Table 1.C.2.: The Labor Supply Elasticity to the Firm by Task Intensities (TI) with Imputed Wages

	RTI	*NRMTI*	*NRCTI*
Separation rate to employment			
log wage (ϵ^e_{sw} mean TI)	-1.110***	-1.028***	-1.106***
	(0.008)	(0.008)	(0.009)
log wage × TI	-0.353***	-0.281***	0.428***
	(0.006)	(0.007)	(0.006)
ϵ^e_{sw} (high TI)	-1.463	-1.309	-0.678
ϵ^e_{sw} (low TI)	-0.757	-0.747	-1.534
Observations	3,429,446	3,429,446	3,429,446
Separation rate to non-employment			
log wage (ϵ^n_{sw} mean TI)	-1.474***	-1.421***	-1.442***
	(0.006)	(0.006)	(0.006)
log wage × TI	-0.264***	-0.150***	0.295***
	(0.005)	(0.005)	(0.005)
ϵ^n_{sw} (high TI)	-1.738	-1.571	-1.147
ϵ^n_{sw} (low TI)	-1.210	-1.271	-1.737
Observations	6,262,902	6,262,902	6,262,902
Hiring probability from employment			
log wage ($\frac{\epsilon_{\theta w}}{1-\theta}$)	1.565***	1.556***	1.549***
	(0.008)	(0.008)	(0.008)
log wage × TI	-0.053***	-0.087***	0.097***
	(0.007)	(0.007)	(0.007)
$\epsilon_{\theta w}$ (high TI)	0.984	0.956	0.811

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Table 1.C.2 – continued from previous page

	RTI	*NRMTI*	*NRCTI*
$\epsilon_{\theta w}$ (mean TI)	0.937	0.946	0.959
$\epsilon_{\theta w}$ (low TI)	0.838	0.812	0.971
Observations	1,060,314	1,060,314	1,060,314
Share of hires from employment (θ)			
with high TI	0.349	0.349	0.507
with mean TI	0.401	0.392	0.381
with low TI	0.482	0.506	0.331
Firm-level labor supply elasticity (ϵ_{Lw})			
with high TI	2.121	1.832	0.776
with mean TI	1.501	1.349	1.461
with low TI	0.911	0.941	2.232

Notes: Cox model. Clustered standard errors at the person level in parentheses. RTI, NRMTI and NRCTI are standardized with mean zero and standard deviation one. Thus, e.g. workers with low RTI are workers with RTI one standard deviation below the mean, and workers with high RTI are workers with RTI one standard deviation above the mean. Same control variables as in Table 1.2. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level respectively.

Source: SIAB and BHP, 1985-2014. Authors' calculations.

1.D. Supplementary Tables and Figures

Table 1.D.1.: Routine Task Intensity (RTI) and its Influence on the Separation Rate Elasticities and the Wage Elasticity of the Share of Recruits Hired from Employment

	Separation rate to employment	*Separation rate to non-employment*	*Hiring probability from employment*
log wage	-1.273***	-1.612***	1.725***
	(0.009)	(0.006)	(0.010)
RTI	1.228***	0.908***	0.443***
	(0.032)	(0.021)	(0.034)
log wage × RTI	-0.315***	-0.227***	-0.114***
	(0.007)	(0.005)	(0.008)
Skill group			
Upper secondary school leaving certificate or vocational training	0.468***	0.206***	0.251***
	(0.011)	(0.007)	(0.009)
University degree or university of applied sciences degree	1.168***	0.743***	-0.233***
	(0.017)	(0.014)	(0.015)
Age group			
26-35	-0.610***	-0.742***	0.650***
	(0.006)	(0.005)	(0.006)
36-45	-1.037***	-1.208***	0.626***
	(0.008)	(0.007)	(0.008)
46-55	-0.921***	-0.856***	0.456***
	(0.010)	(0.006)	(0.009)
Firm size			
Medium (20-250)	-0.001	-0.094***	0.067***

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Table 1.D.1 – continued from previous page

	Separation rate to employment	*Separation rate to non-employment*	*Hiring probability from employment*
	(0.007)	(0.005)	(0.006)
Large (250-999)	-0.297***	-0.344***	0.066***
	(0.009)	(0.007)	(0.008)
Very large (1000+)	-0.709***	-0.548***	-0.155***
	(0.011)	(0.008)	(0.010)
Foreign	0.078***	0.218***	-0.128***
	(0.010)	(0.007)	(0.009)
Share of high skill workers in firm	-0.170***	-0.166***	-0.236***
	(0.025)	(0.021)	(0.022)
Share of low skill workers in firm	-0.240***	-0.213***	-0.165***
	(0.021)	(0.015)	(0.018)
Share of foreign workers in firm	0.945***	0.691***	-0.048***
	(0.024)	(0.016)	(0.018)
Share of female workers in firm	0.276***	0.257***	0.061***
	(0.017)	(0.012)	(0.014)
Share of part-time workers in firm	-0.312***	-0.267***	0.054**
	(0.027)	(0.020)	(0.024)
Mean age of workers in firm	-0.016***	-0.012***	0.004***
	(0.001)	(0.000)	(0.001)
Unemployment rate	-0.003	0.010***	-0.015***
	(0.003)	(0.002)	(0.004)
Industry dummies	yes	yes	yes
Occupation dummies	yes	yes	yes
Year dummies	yes	yes	yes

Continued on next page

Table 1.D.1 – continued from previous page

	Separation rate to employment	*Separation rate to non-employment*	*Hiring probability from employment*
Federal state dummies	yes	yes	yes
Observations	2,998,063	5,460,312	979,514

Notes: Clustered standard errors at the person level in parentheses. RTI is standardized with mean zero and standard deviation one. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level respectively.

Source: SIAB and BHP, 1985-2014. Authors' calculations.

Table 1.D.2.: The Labor Supply Elasticity to the Firm by RTI and Collective Bargaining Coverage

	High coverage	Low coverage	Baseline
Separation rate to employment			
log wage (ϵ^e_{sw} mean RTI)	-1.331***	-0.876***	-1.273***
	(0.022)	(0.016)	(0.009)
log wage × RTI	-0.190***	-0.225***	-0.315***
	(0.022)	(0.015)	(0.007)
ϵ^e_{sw} (high RTI)	-1.521	-1.101	-1.588
ϵ^e_{sw} (low RTI)	-1.141	-0.651	-0.958
Observations	519,173	730,598	2,998,063
Separation rate to non-employment			
log wage (ϵ^n_{sw} mean RTI)	-1.635***	-1.294***	-1.612***
	(0.015)	(0.012)	(0.006)
log wage × RTI	-0.059***	-0.178***	-0.227***
	(0.017)	(0.011)	(0.005)
ϵ^n_{sw} (high RTI)	-1.694	-1.472	-1.839
ϵ^n_{sw} (low RTI)	-1.576	-1.116	-1.385
Observations	1,029,019	1,274,113	5,460,312
Hiring probability from employment			
log wage ($\frac{\epsilon_{\theta w}}{1-\theta}$)	2.053***	1.750***	1.725***
	(0.027)	(0.019)	(0.010)
log wage × RTI	-0.305***	0.002	-0.114***
	(0.031)	(0.018)	(0.008)

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Table 1.D.2 – continued from previous page

	High coverage	Low coverage	Baseline
$\epsilon_{\theta w}$ (high RTI)	1.145	1.044	1.052
$\epsilon_{\theta w}$ (mean RTI)	1.347	1.026	1.066
$\epsilon_{\theta w}$ (low RTI)	1.099	1.019	1.059
Observations	186,490	270,115	979,514
Share of hires from employment (θ)			
with high RTI	0.345	0.404	0.347
with mean RTI	0.344	0.414	0.382
with low RTI	0.534	0.417	0.424
Firm-level labor supply elasticity (ϵ_{Lw})			
with high RTI	2.010	1.379	2.287
with mean RTI	1.515	0.971	1.690
with low RTI	1.386	0.554	1.103

Notes: Clustered standard errors at the person level in parentheses. RTI is standardized with mean zero and standard deviation one. Thus, workers with low RTI are workers with RTI one standard deviation below the mean, and workers with high RTI are workers with RTI one standard deviation above the mean. Same control variables as in Table 1.2. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level respectively.

Source: SIAB and BHP, 1985-2014. Authors' calculations.

Table 1.D.3.: The Labor Supply Elasticity to the Firm by NRMTI and Collective Bargaining Coverage

	High coverage	Low coverage	Baseline
Separation rate to employment			
log wage (ϵ^e_{sw} mean NRMTI)	-1.234***	-0.770***	-1.199***
	(0.021)	(0.015)	(0.009)
log wage × NRMTI	-0.083***	-0.266***	-0.181***
	(0.014)	(0.013)	(0.007)
ϵ^e_{sw} (high NRMTI)	-1.317	-1.036	-1.380
ϵ^e_{sw} (low NRMTI)	-1.151	-0.504	-1.018
Observations	519,173	730,598	2,998,063
Separation rate to non-employment			
log wage (ϵ^n_{sw} mean NRMTI)	-1.650***	-1.219***	-1.570***
	(0.016)	(0.011)	(0.006)
log wage × NRMTI	0.047***	-0.126***	-0.075***
	(0.011)	(0.009)	(0.005)
ϵ^n_{sw} (high NRMTI)	-1.603	-1.345	-1.645
ϵ^n_{sw} (low NRMTI)	-1.697	-1.093	-1.495
Observations	1,029,019	1,274,113	5,460,312
Hiring probability from employment			
log wage ($\frac{\epsilon_{\theta w}}{1-\theta}$)	2.214***	1.742***	1.724***
	(0.028)	(0.018)	(0.010)
log wage × NRMTI	-0.283***	-0.142***	-0.098***
	(0.021)	(0.016)	(0.008)
$\epsilon_{\theta w}$ (high NRMTI)	1.319	1.035	1.085

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Table 1.D.3 – continued from previous page

	High coverage	Low coverage	Baseline
$\epsilon_{\theta w}$ (mean NRMTI)	1.393	1.038	1.069
$\epsilon_{\theta w}$ (low NRMTI)	1.331	0.999	1.028
Observations	186,490	270,115	979,514
Share of hires from employment (θ)			
with high NRMTI	0.317	0.353	0.333
with mean NRMTI	0.371	0.404	0.380
with low NRMTI	0.467	0.470	0.436
Firm-level labor supply elasticity (ϵ_{Lw})			
with high NRMTI	1.510	1.237	1.852
with mean NRMTI	1.337	0.769	1.559
with low NRMTI	1.262	0.322	1.277

Notes: Clustered standard errors at the person level in parentheses. NRMTI is standardized with mean zero and standard deviation one. Thus, workers with low NRMTI are workers with NRMTI one standard deviation below the mean, and workers with high NRMTI are workers with NRMTI one standard deviation above the mean. Same control variables as in Table 1.2. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level respectively.

Source: SIAB and BHP, 1985-2014. Authors' calculations.

Table 1.D.4.: The Labor Supply Elasticity to the Firm by NRCTI and Collective Bargaining Coverage

	High coverage	Low coverage	Baseline
Separation rate to employment			
log wage (ϵ^e_{sw} mean NRCTI)	-1.229***	-0.846***	-1.241***
	(0.020)	(0.016)	(0.009)
log wage × NRCTI	0.209***	0.304***	0.359***
	(0.016)	(0.013)	(0.007)
ϵ^e_{sw} (high NRCTI)	-1.020	-0.542	-0.882
ϵ^e_{sw} (low NRCTI)	-1.438	-1.150	-1.600
Observations	519,173	730,598	2,998,063
Separation rate to non-employment			
log wage (ϵ^n_{sw} mean NRCTI)	-1.629***	-1.257***	-1.582***
	(0.015)	(0.011)	(0.006)
log wage × NRCTI	-0.019	0.189***	0.222***
	(0.013)	(0.009)	(0.005)
ϵ^n_{sw} (high NRCTI)	-1.648	-1.068	-1.360
ϵ^n_{sw} (low NRCTI)	-1.610	-1.446	-1.804
Observations	1,029,019	1,274,113	5,460,312
Hiring probability from employment			
log wage ($\frac{\epsilon_{\theta w}}{1-\theta}$)	2.170***	1.726***	1.717***
	(0.027)	(0.018)	(0.010)
log wage × NRCTI	0.422***	0.096***	0.160***
	(0.025)	(0.016)	(0.009)
$\epsilon_{\theta w}$ (high NRCTI)	1.314	0.980	1.045

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Table 1.D.4 – continued from previous page

	High coverage	Low coverage	Baseline
$\epsilon_{\theta w}$ (mean NRCTI)	1.437	1.032	1.082
$\epsilon_{\theta w}$ (low NRCTI)	1.176	1.214	1.104
Observations	186,490	270,115	979,514
Share of hires from employment (θ)			
with high NRCTI	0.493	0.462	0.443
with mean NRCTI	0.338	0.402	0.370
with low NRCTI	0.327	0.255	0.291
Firm-level labor supply elasticity (ϵ_{Lw})			
with high NRCTI	1.044	0.387	0.985
with mean NRCTI	1.286	0.906	1.615
with low NRCTI	1.815	1.306	2.241

Notes: Clustered standard errors at the person level in parentheses. NRCTI is standardized with mean zero and standard deviation one. Thus, workers with low NRCTI are workers with NRCTI one standard deviation below the mean, and workers with high NRCTI are workers with NRCTI one standard deviation above the mean. Same control variables as in Table 1.2. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level respectively.

Source: SIAB and BHP, 1985-2014. Authors' calculations.

Table 1.D.5.: Separation Rate Elasticities by Task Intensities and Tenure Brackets

	RTI	NRMTI	NRCTI
Separation rate elasticity to employment (ϵ^{e}_{sw})			
Job Tenure: 0-3 years			
log wage	-0.814***	-0.756***	-0.783***
	(0.008)	(0.007)	(0.008)
log wage × TI	-0.251***	-0.135***	0.278***
	(0.006)	(0.006)	(0.006)
Observations	1,359,344	1,359,344	1,359,344
Job Tenure: 3-10 years			
log wage	-0.612***	-0.553***	-0.626***
	(0.015)	(0.015)	(0.016)
log wage × TI	-0.303***	-0.229***	0.333***
	(0.013)	(0.013)	(0.013)
Observations	1,028,293	1,028,293	1,028,293
Job Tenure: 10+ years			
log wage	-0.478***	-0.479***	-0.499***
	(0.029)	(0.029)	(0.029)
log wage × TI	-0.220***	-0.199***	0.308***
	(0.024)	(0.026)	(0.026)
Observations	610,426	610,426	610,426
Separation rate elasticity to non-employment (ϵ^{n}_{sw})			
Job Tenure: 0-3 years			
log wage	-1.249***	-1.222***	-1.222***

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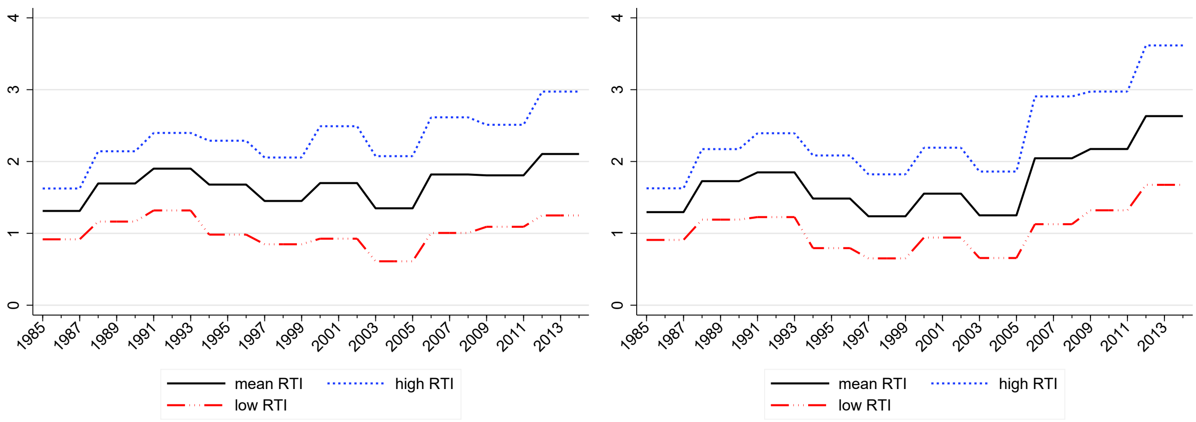
Table 1.D.5 – continued from previous page

	RTI	NRMTI	NRCTI
	(0.006)	(0.006)	(0.006)
log wage × TI	-0.196***	-0.031***	0.164***
	(0.005)	(0.005)	(0.005)
Observations	2,504,538	2,504,538	2,504,538
Job Tenure: 3-10 years			
log wage	-1.035***	-0.989***	-1.031***
	(0.013)	(0.012)	(0.013)
log wage × TI	-0.216***	-0.143***	0.228***
	(0.011)	(0.011)	(0.010)
Observations	1,683,269	1,683,269	1,683,269
Job Tenure: 10+ years			
log wage	-0.905***	-0.906***	-0.917***
	(0.017)	(0.017)	(0.016)
log wage × TI	-0.187***	-0.100***	0.212***
	(0.014)	(0.015)	(0.015)
Observations	1,272,505	1,272,505	1,272,505

Notes: Clustered standard errors at the person level in parentheses. We use exponential models for this table. The table shows coefficients of the estimation of separation rate elasticities for high RTI, high NRMTI and high NRCTI workers. RTI, NRMTI and NRCTI are standardized with mean zero and standard deviation one. Thus, e.g. workers with low RTI are workers with RTI one standard deviation below the mean, and workers with high RTI are workers with RTI one standard deviation above the mean. Same control variables as in Table 1.2. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level respectively.

Source: SIAB and BHP, 1985-2014. Authors' calculations.

Figure 1.D.1.: Labor Supply Elasticities for Workers with Different RTI over 3-Year-Intervals

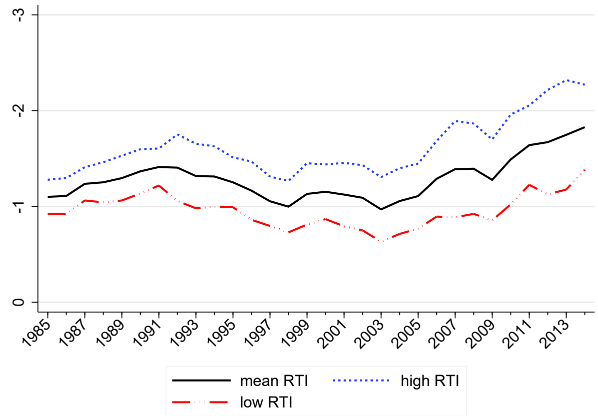


(a) Using subsamples

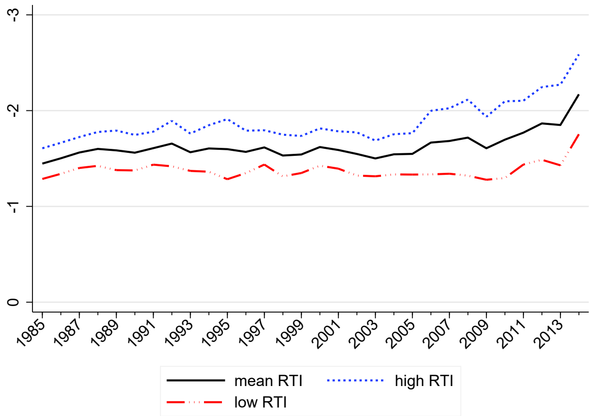
(b) Baseline

Notes: The estimates are derived from the same specification as in Table 1.4 of the paper. Further, in panel (a) we estimate the main specification separately for 3-year sub-samples. Panel (b) is a pure reproduction of Figure 1.A.1. That is, in panel (b) a three-way interaction with year dummies is added to analyze the development over time, i.e. log wages, RTI and year dummies are interacted. The plotted lines correspond to the sum of the relevant coefficients for workers with mean RTI as well as workers with RTI one standard deviation below ("low RTI") and above ("high RTI") the mean.
Source: Authors' calculations based on SIAB 1985-2014, for West Germany.

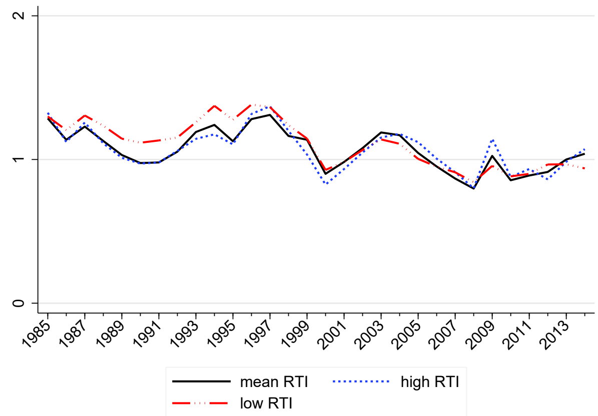
Figure 1.D.2.: Components of the Labor Supply Elasticity to the Firm over Time



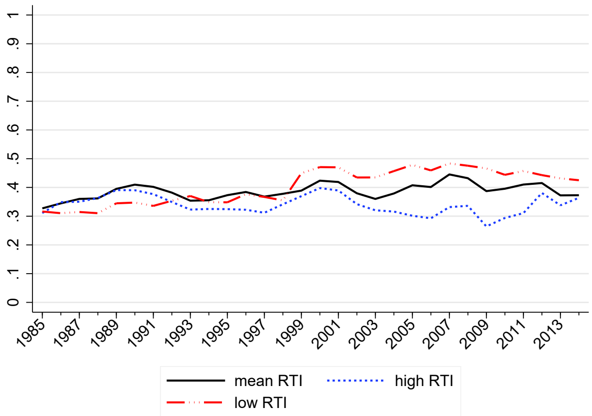
(a) Separation rate elasticity to employment



(b) Separation rate elasticity to non-employment



(c) Elasticity of the share of recruitments from employment



(d) Share of recruitments from employment

Notes: The estimates are derived from the same specification as in Table 1.4. Further, a three-way interaction with year dummies is added to analyze the development over time, i.e. log wages, RTI and year dummies are interacted. The plotted lines correspond to the sum of the relevant coefficients for workers with mean RTI as well as workers with RTI one standard deviation below ("low RTI") and above ("high RTI") the mean.
Source: Authors' calculations based on SIAB 1985-2014, for West Germany.

1.E. Further Robustness Checks

In the following, we provide additional tests of the robustness of our results. In contrast to the main paper, we use exponential models for these robustness tests for two reasons. First, we show in Table 1.A.1 of the paper that the main results do not change qualitatively when using exponential models. Workers with high NRCTI still have a distinctively smaller labor supply elasticity to the firm than workers with high RTI or high NRMTI. The main difference between the two models is that the exponential model does not control for tenure. This increases all estimated elasticities, but does not change the results qualitatively as just described. Second, exponential model are much more feasible in terms of computation times. Cox models need a substantially higher amount of computation time to estimate the same specification.

Full-Interaction Model

It might be a concern that the task-specific features of our control variables, e.g. the age/education profile of workers in different task groups, could bias our estimated elasticities in Table 1.4. By interacting our TI measures only with the log wage, we do not account for task-specific features of the covariates, such as e.g. the age/education profile of separations. To circumvent this concern, we repeat our main analysis with a full interaction model. In addition to the variables of the baseline model, the full-interaction model includes the interaction of the task intensities (RTI, NRMTI and NRCTI) with every control variable. Therefore, this model accounts for task-specific features of the control variables such as e.g. the age/education profile of separations. Also, as this model fully interacts the task intensity measures with every other variable, it is equivalent to estimating separate regressions by task group. We display the result in Table 1.E.1. Our main results hold: Workers with high NRCTI have a distinctively lower firm-level labor supply elasticity and therefore are exposed to a higher degree of monopsony power than workers with high RTI and high NRMTI.

Table 1.E.1.: The Labor Supply Elasticity to the Firm by Task Intensities (TI). Full-Interaction Model

	RTI	*NRMTI*	*NRCTI*
Separation rate to employment			
log wage (ϵ^e_{sw} mean TI)	-1.436***	-1.376***	-1.406***
	(0.011)	(0.011)	(0.011)
log wage × TI	-0.288***	-0.160***	0.303***
	(0.011)	(0.011)	(0.010)
ϵ^e_{sw} (high TI)	-1.724	-1.536	-1.103
ϵ^e_{sw} (low TI)	-1.148	-1.216	-1.709
Observations	2,998,063	2,998,063	2,998,063
Separation rate to non-employment			
log wage (ϵ^n_{sw} mean TI)	-1.848***	-1.813***	-1.819***
	(0.008)	(0.008)	(0.008)
log wage × TI	-0.253***	-0.071***	0.219***
	(0.008)	(0.008)	(0.008)
ϵ^n_{sw} (high TI)	-2.101	-1.884	-1.600
ϵ^n_{sw} (low TI)	-1.595	-1.742	-2.038
Observations	5,460,312	5,460,312	5,460,312
Hiring probability from employment			
log wage ($\frac{\epsilon_{\theta w}}{1-\theta}$)	1.733***	1.715***	1.710***
	(0.010)	(0.010)	(0.010)
log wage × TI	-0.106***	-0.094***	0.135***
	(0.009)	(0.009)	(0.010)
$\epsilon_{\theta w}$ (high TI)	1.062	1.081	1.028

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Table 1.E.1 – continued from previous page

	RTI	*NRMTI*	*NRCTI*
$\epsilon_{\theta w}$ (mean TI)	1.071	1.063	1.077
$\epsilon_{\theta w}$ (low TI)	1.059	1.020	1.117
Observations	979,514	979,514	979,514
Share of hires from employment (θ)			
with high TI	0.347	0.333	0.443
with mean TI	0.382	0.380	0.370
with low TI	0.424	0.436	0.291
Firm-level labor supply elasticity (ϵ_{Lw})			
with high TI	2.632	2.223	1.455
with mean TI	2.056	1.960	1.995
with low TI	1.494	1.708	2.535

Notes: We use exponential models in this table. Clustered standard errors at the person level in parentheses. RTI, NRMTI and NRCTI are standardized with mean zero and standard deviation one. Thus, e.g. workers with low RTI are workers with RTI one standard deviation below the mean, and workers with high RTI are workers with RTI one standard deviation above the mean. Covariates included in the estimations are education, age, immigrant worker, occupation, sector, year and federal state of the plant controls. Further, we include the shares of low-skilled, high-skilled, female, part-time and immigrant workers in the plant's workforce, dummy variables for plant size, the average age of its workforce and the unemployment rate by year and federal state. We interact RTI, NRMTI and NRCTI with every control variable. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level respectively.

Source: SIAB and BHP, 1985-2014. Authors' calculations.

Sector-Year Fixed Effects

We check for the robustness of our results by including interacted sector-year fixed effects, so that identification comes from variation in wages within sector-year cells, rather than between them. We display the results in Table 1.E.2 and find that our main results hold. Namely, workers with high NRCTI face a higher degree of monopsony power than workers with high RTI and high NRMTI.

Table 1.E.2.: The Labor Supply Elasticity to the Firm by Task Intensities (TI) with Sector-Year Fixed Effects

	RTI	*NRMTI*	*NRCTI*
Separation rate to employment			
log wage (ϵ^e_{sw} mean TI)	-1.445***	-1.368***	-1.413***
	(0.011)	(0.011)	(0.011)
log wage × TI	-0.324***	-0.199***	0.370***
	(0.009)	(0.009)	(0.009)
ϵ^e_{sw} (high TI)	-1.769	-1.567	-1.043
ϵ^e_{sw} (low TI)	-1.121	-1.169	-1.783
Observations	2,998,063	2,998,063	2,998,063
Separation rate to non-employment			
log wage (ϵ^n_{sw} mean TI)	-1.851***	-1.804***	-1.818***
	(0.008)	(0.008)	(0.008)
log wage × TI	-0.255***	-0.107***	0.267***
	(0.007)	(0.007)	(0.007)
ϵ^n_{sw} (high TI)	-2.106	-1.911	-1.551
ϵ^n_{sw} (low TI)	-1.596	-1.697	-2.085
Observations	5,460,312	5,460,312	5,460,312
Hiring probability from employment			
log wage ($\frac{\epsilon_{\theta w}}{1-\theta}$)	1.728***	1.727***	1.720***
	(0.010)	(0.010)	(0.010)
log wage × TI	-0.109***	-0.104***	0.157***
	(0.008)	(0.008)	(0.009)
$\epsilon_{\theta w}$ (high TI)	1.057	1.083	1.045

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Table 1.E.2 – continued from previous page

	RTI	*NRMTI*	*NRCTI*
$\epsilon_{\theta w}$ (mean TI)	1.068	1.071	1.084
$\epsilon_{\theta w}$ (low TI)	1.058	1.033	1.108
Observations	979,495	979,495	979,495
Share of hires from employment (θ)			
with high TI	0.347	0.333	0.443
with mean TI	0.382	0.380	0.370
with low TI	0.424	0.436	0.291
Firm-level labor supply elasticity (ϵ_{Lw})			
with high TI	2.701	2.281	1.323
with mean TI	2.073	1.936	1.998
with low TI	1.457	1.603	2.672

Notes: We use exponential models in this table. Clustered standard errors at the person level in parentheses. RTI, NRMTI and NRCTI are standardized with mean zero and standard deviation one. Thus, e.g. workers with low RTI are workers with RTI one standard deviation below the mean, and workers with high RTI are workers with RTI one standard deviation above the mean. Covariates included in the estimations are education, age, immigrant worker, occupation, sector-year, federal state of the plant controls. Further, we include the shares of low-skilled, high-skilled, female, part-time and immigrant workers in the plant's workforce, dummy variables for plant size, the average age of its workforce and the unemployment rate by year and federal state. ***, ** and * indicate statistical significance at the 1%, 5% and 10% level respectively.

Source: SIAB and BHP, 1985-2014. Authors' calculations.

Analysis by Wage Brackets

To alleviate the concern that our main results are simply driven by the different location of task groups in the wage distribution, we perform different analyses separately by wage brackets. Specifically, we include six 20-Euro wage brackets for (deflated) daily wages (this would amount to 400 Euro monthly wages, given a month of 20 working days): 10-30 Euros, 30-50 Euros, 50-70 Euros, 70-90 Euros, 90-110 Euros and 110-130 Euros. We choose the wage brackets such that they are large enough to include a sufficiently high number of observations and distinct enough so that an estimation by separate wage brackets is meaningful.

Table 1.E.3 shows the number of observations together with the row and column percentages by wage bracket and task intensity. The row percentages display the proportions of each task intensity group within a wage bracket, while the column percentages show the proportion in different wage brackets within task intensity groups. As expected, we find that workers with high NRCTI are much more likely in the upper wage brackets in terms of row and column percentages. Workers with high RTI and high NRMTI are more likely in the middle wage brackets.

Table 1.E.3.: Number of Observations and Row/Column Percentages by Wage Brackets and Task Intensities

Daily Wage Bracket	High RTI	High NRMTI	High NRCTI	Total
10-30	17,182	27,989	27,494	72,665
row percentage	23.65	38.52	37.84	100
column percentage	1.93	2.14	2.59	2.23
30-50	67,492	102,158	69,846	239,496
row percentage	28.18	42.66	29.16	100
column percentage	7.56	7.82	6.57	7.34
50-70	205,288	396,823	173,233	775,344
row percentage	26.48	51.18	22.34	100
column percentage	23	30.38	16.3	23.77
70-90	345,582	514,595	259,338	1,119,515
row percentage	30.87	45.97	23.17	100
column percentage	38.72	39.39	24.41	34.33
90-110	191,561	208,996	282,070	682,627
row percentage	28.06	30.62	41.32	100
column percentage	21.47	16	26.55	20.93
110-130	65,313	55,816	250,600	371,729
row percentage	17.57	15.02	67.41	100
column percentage	7.32	4.27	23.58	11.4

Continued on next page

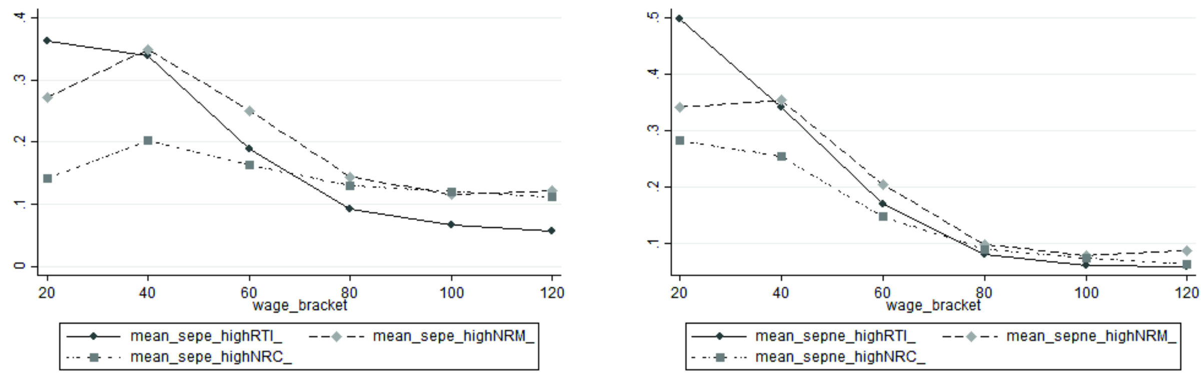
Table 1.E.3 – continued from previous page

Daily Wage Bracket	High RTI	High NRMTI	High NRCTI	Total
Total	892,418	1,306,377	1,062,581	3,261,376
row percentage	27.36	40.06	32.58	100
column percentage	100	100	100	100

Notes: RTI, NRMTI and NRCTI are standardized with mean zero and standard deviation one. Thus, e.g. workers with low RTI are workers with RTI one standard deviation below the mean, and workers with high RTI are workers with RTI one standard deviation above the mean.

Source: SIAB and BHP, 1985-2014. Authors' calculations.

Figure 1.E.1.: Fitted Values of Separation Rates by Wage Brackets and Task Intensities



(a) Separation to employment

(b) Separation to non-employment

We proceed by illustrating mean separation rates by wage brackets for workers with different task intensities. Specifically, for each wage bracket and worker type (high RTI, high NRMTI, high NRCTI), we estimate the fitted values of separations to employment and separations to non-employment using the covariates of our baseline estimations. We then estimate the mean separation rate for each wage bracket and task intensity. We plot the results in Figure 1.E.1.

In Figure 1.E.1a we observe that the mean separation rate to employment of high RTI workers is relatively high in the lower wage brackets, but decreases strongly for higher wage brackets. In contrast, workers with high NRCTI have relatively low separation rates to employment in the lower wage brackets and do have a relatively smaller decline in the separation rate to employment for the higher wage brackets. Thus, while workers with high RTI have a much higher separation rate to employment than workers with high NRCTI for the lower wage brackets, this relation reverses for the higher wage brackets as workers with high NRCTI have a slightly higher separations to employment. Workers with high NRMTI show similar separation rates as workers with high RTI in low wage brackets, but the decline in separation rates is less strong. Figure 1.E.1b show similar plots as before, but here we use the separation rate to non-employment. Again, workers with high RTI and high NRMTI have a relatively higher separation rate to non-employment for the lower wage brackets than workers with high NRCTI. However, the mean separation rates to non-employment become similar for the different task intensities in the higher wage brackets.

Overall we can conclude from this exercise that separation rates generally decline for workers in higher wage brackets. The level differences between workers with different task intensities in the mean separation rates are relatively high for the lower to middle wage brackets and equalize for higher wage brackets. Therefore, we cannot exclude that composition effects with respect to wage brackets influence our results for the labor supply elasticity to the firm.

To further analyze whether composition effects with respect to wage brackets influence our results, we estimate the labor supply elasticity for workers with different task intensities who are in the same position of the wage distribution. That is, we re-estimate our baseline specification by wage brackets. This exercise shows whether the heterogeneity in high versus low RTI jobs is simply reflecting the different location of workers in the wage distribution or whether it is also present within the same wage bracket.[32] Specifically, we perform our baseline estimations of Table 1.4 in the paper for the 6 wage brackets defined above (by daily wages: 10-30 Euro, 30-50 Euro, 50-70 Euro, 70-90 Euro, 90-110 Euro and 110-130 Euro).

We summarize the estimation results by wage brackets in Figure 1.E.2. It becomes apparent that the labor supply elasticity is increasing from the lowest wage brackets to the middle and then is declining again for the higher wage brackets. Thus, we observe an inverted U-shape for the labor supply elasticity to the firm in wages and the labor supply elasticity is indeed falling in wages (at least for the higher wage brackets) as the Burdett and Mortensen (1998) model suggests. More importantly, we find that high RTI and high NRMTI workers have higher labor supply elasticities for half of the wage brackets and almost equal labor supply elasticities in the other wage brackets. Specifically, the labor supply elasticities of high RTI and high NRMTI workers are higher than the labor supply elasticity of high NRCTI workers for the 10-30 Euro, 50-70 Euro and 70-90 Euro wage brackets. The labor supply elasticities by wage brackets and task intensities are almost equal in the 30-50 Euro, 90-110 Euro and 110-130 Euro wage brackets.

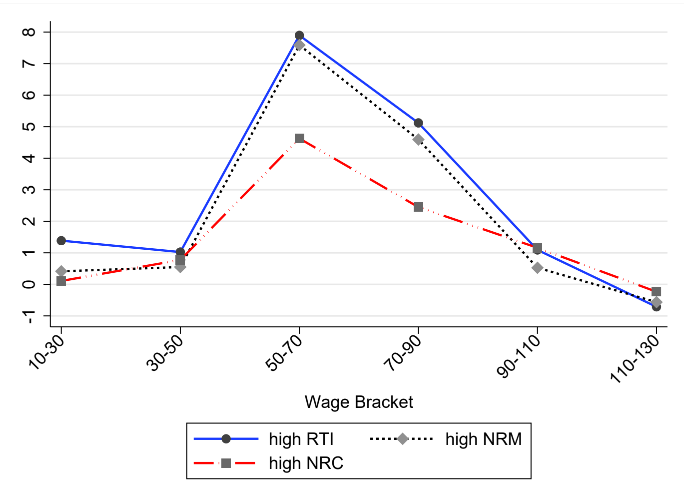
Table 1.E.3 shows that almost 45% of high NRCTI workers are located in wage brackets, where we indeed observe lower labor supply elasticities for this group of workers. At the same time, 50% of high NRCTI workers receive daily wages exceeding 90 Euros, corresponding to wage brackets where the labor supply elasticities are generally low and we do not observe differences between workers with different task intensities. Therefore, the workers' location in the wage distribution may indeed overstate the estimated differences in the degree of monopsony the workers face to a certain extent, but they are not pronounced enough to explain these differences completely.

Overall, we thus conclude that workers in occupations with high NRCTI have lower labor

[32] Note that Table 1.E.3 shows that this analysis is feasible in terms of observation numbers as workers with different task intensities are sufficiently represented in all wage brackets.

supply elasticities to the firm compared to workers with high RTI or high NRMTI even when we compare workers in the same position of the wage distribution. Thus, the heterogeneity in the labor supply elasticity of workers with different task intensities is not just simply reflecting the different location in the wage distribution.

Figure 1.E.2.: Labor Supply Elasticities by Wage Brackets



Notes: The estimates are derived from the same specification as in Table 1.4 of the paper separately by (daily) wage brackets (in Euro). We use exponential models here.
Source: Authors' calculations based on SIAB 1985-2014, for West Germany.

Declaration of Contribution

Hereby I, Gökay Demir, declare that the Chapter "Labor Market Polarization, Job Tasks, and Monopsony Power" is co-authored by Ronald Bachmann and Hanna Frings. All authors contributed equally to the chapter.

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2. The Role of Within-Occupation Task Changes in Wage Development*

Abstract: We examine how changes in task content over time condition occupational wage development. Using survey data from Germany, we document substantial heterogeneity in within-occupational changes in task content. Combining this evidence with administrative data on individual employment outcomes over a 25-year period, we find important heterogeneity in wage penalties amongst initially routine intensive jobs. While occupations that remain (relatively) routine intensive generate substantial wage penalties, occupations with a decreasing routine intensity experience stable or even increasing wages. These findings cannot be explained by composition or cohort effects.

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2.1. Introduction

The shift away from middle skill, routine intensive, jobs is a pervasive feature of structural change in the labour market over the past four decades. A large reduction in the employment shares of these jobs has been documented across a range of developed economies (Autor et al., 1998; Bachmann et al., 2019; Goos and Manning, 2007; Goos et al., 2009). These losses of routine work have implications for individual welfare. Routine task workers who lose jobs face welfare losses through the loss of firm specific human capital along with reductions in overall industry and economy wide demand for their skills. Along these lines, Cortes (2016) demonstrates that the US wage premium associated with routine intensive occupations reduced by 17% over the period between 1972 and the mid-2000s. However, the existing literature does not consider the fact that occupations may evolve over time, enabling individual workers to adapt to technological change.

In this paper, we re-examine whether routine workers face worse labour market prospects, and in particular, suffer greater wage losses when compared to other workers. Our main contribution to the existing literature is that we explicitly take into account that task mixes within occupations are likely to change over time. The standard approach has been to use the initial task content of occupations to define a job as routine-intensive.[1] This has advantages in terms of data requirements, ease of estimation, and interpretation. Yet, it misses an important component of the adaptation process to the de-routinization of work - within-occupation changes in task mixes. Our research builds on previous work which demonstrates large changes in task mixes within occupations over time in Germany and the US (Atalay et al., 2020; Bachmann et al., 2019; Spitz-Oener, 2006). Our main contribution is to demonstrate the consequences of these task changes for wage development. We do so using detailed task data for Germany matched with administrative wage data spanning 3 decades.

Specifically, we estimate the effect of exposure to different task mixes on wages for Germany for 1985 to 2010. Using combined social security data and survey data on occupational task mixes we go beyond estimates of, for instance, the effect of exposure to routinisation on wages, and decompose this according to within and across occupational changes in task mixes. We document large heterogeneity in within occupation task mix changes. For those jobs that are

[1] An exception to this is Ross (2017), who explores variation of tasks within occupations over time to estimate the returns to routine and abstract tasks in the US for the time period 2004 to 2013. He documents increasingly negative returns to routine tasks and increasingly positive returns to abstract tasks over time.

initially routine task intensive the magnitude of these within changes dwarf across-occupation task changes.

Our empirical strategy is based on the estimation of wage equations with person-occupation fixed effects. This approach controls for workers' time constant unobserved heterogeneity, which is allowed to vary across different types of occupations. We are mainly interested in the estimation of the time varying occupation specific wage components. If unobserved skills and their occupation specific returns are constant over time, this approach identifies yearly occupation-specific wage premia which are common to all workers in a specific occupation group (Cortes, 2016). However, a change in workers' unobserved skills and a changing task mix within an occupation might violate the assumption of time constant skills and their occupation specific returns. In this case, the estimated occupation specific wage component reflects both, the wage premia common to all workers, and the impact of the change in skills and their potentially changing return in an occupation group.

While previous work demonstrates marked wage penalties associated with routine work for the US and no routinisation penalty for Germany (Cortes, 2016; Wang, 2020), we present large heterogeneity in the development of wages of initially routine jobs that reflects changes in within-occupation task mix. Occupations that remain (relatively) routine intensive over time generate substantial wage penalties. Yet, as we show, a range of initially routine occupations that changed task mix over time and became more intensive in non-routine cognitive tasks, are instead associated with substantial wage increases. These increases are comparable in magnitude to those experienced by workers who perform primarily non-routine cognitive tasks, and lead to sizeable differences in wage growth amongst initially routine task-intensive occupations. If task changes within occupations are not taken into account, the growth in occupation-specific wage components would be understated by up to 16 percentage points for those routine occupations with a growing importance of non-routine cognitive tasks and overstate the growth in occupation-specific wage components by up to 10.9 percentage points for routine occupations with relatively constant non-routine cognitive task intensity. This heterogeneity in wage development amongst routine workers has not been documented in the previous literature. It is, however, consistent with evidence for the US by Deming and Noray (2020) who show for the time period 2007-2019 that faster-changing occupations display lower returns to experience.

This novel fact raises a range of additional questions regarding the source of these differences.

As an initial step, we rule out a range of potential explanations. For instance, we demonstrate that this does not reflect the occupation specific changes in worker composition that have been shown to be important features of the routinisation process (Böhm et al., 2019). We also demonstrate that it does not simply reflect cohort effects.

This leaves the question of which factors, in addition to changes in task mix, have changed in these specific jobs in a way that increases worker productivity, and through this, wages. We explore one likely factor, receipt of training. It seems probable that worker skills must evolve along with the changing nature of the job. We demonstrate that those initially routine intensive jobs that changed in task mix to become more demanding of cognitive tasks are associated with greater training receipt. This paints a picture of a group of occupations that changed markedly in nature, and where workers through training were able to avoid wage penalties associated with routinisation.

Finally, we provide descriptive evidence based on those workers who change their task group. First, we find that workers who switch from routine occupations to occupations with non-routine cognitive tasks experience a higher wage growth than those who stay in routine occupations, and we observe a similar pattern for workers switching from routine occupations to initially routine occupations experiencing an increase in non-routine cognitive tasks. Second, we observe that workers in initially routine occupations who experience an increase in non-routine cognitive tasks have a relatively high probability to switch to occupations with non-routine cognitive tasks, and vice versa. This suggests that these occupations are relatively close to each other in terms of human capital transferability.

Taken together our results provide a more nuanced view of the wage and welfare consequences of exposure to routinisation than has been presented before, stressing the role of changing occupations and worker adaptability to technological change. The role of changing occupations is important as it implies that occupations which are routine at some point may – contrary to results from the previous literature – offer good prospects for workers if these occupations manage to increase their intensity in non-routine cognitive tasks. Furthermore, our results provide an indication of which occupations may be most promising for routine workers. This type of information may feed into advice given to job seekers e.g. through on-line advice as in Belot et al. (2019), thus improving workers' job search outcomes. Our results also offer a potential explanation for conflicting results from the literature that during the last decades, routine workers have experienced declining wage premia in the US but not in Germany.

The paper proceeds as follows. Section 2.2 introduces the datasets that allow us to follow workers over time as well as to capture the changing task content of occupations and presents approach to measuring task content along with the definition of the sample. Section 2.3 describes the econometric approach. Section 2.4 presents the main results, provides robustness checks and evidence on mechanisms, and analyses the role of job training. Section 2.5 concludes.

2.2. Data

Our analysis is based on the Sample of Integrated Labour Market Biographies (SIAB). The SIAB is a representative 2 percent random sample from the Integrated Employment Biographies (IEB) which covers the universe of individuals in Germany in employment subject to social security contributions or with registered unemployment spells (Dauth and Eppelsheimer, 2020; Frodermann et al., 2021). Civil servants and self-employed workers are not included in the data. The data contain individual information such as age, gender, nationality, education, and place of residence, as well as job information such as the daily wage and the occupation. We combine these worker-level data with the Establishment History Panel (BHP) containing information on the industry of the establishment.

We match the SIAB to survey data that provides information on occupational task intensities. Specifically, we use the BIBB/IAB and BIBB/BAuA Employment Surveys (herein BIBB data) that provide a representative sample of German employees working at least 10 hours per week (BIBB – Federal Institute for Vocational Education and Training, 2021). The BIBB data consists of repeated cross-sections on approximately 20,000 to 30,000 employees in Germany for each survey wave that we use in this paper (1985-6, 1991-2, 1998-9, 2006).

We use the information on the job tasks performed by a worker to compute individual level task intensities, imposing the same sample restrictions as for the SIAB data. We follow the approach of Antonczyk et al. (2009) and categorize the activities employees perform at the workplace into routine (R), non-routine manual (NRM) and non-routine cognitive (NRC). These individual level task intensities are calculated as follows

$$Task_{ijt} = \frac{\text{number of activities in category j performed by i in cross section t}}{\text{total number of activities performed by i over all categories at time t}}, \qquad (2.1)$$

where t= 1985-6, 1991-2, 1998-9 and 2006 and j indicates routine (R), non-routine manual (NRM), and non-routine cognitive (NRC) tasks, respectively. Using the occupation field classification in Tiemann et al. (2008), we aggregate these individual task intensities for 53 occupation fields. The shares of task intensities for each occupation-time period combination sum to 100 percent. As a result, these measures provide a continuous measure of routine task intensity (RTI), non-routine manual task intensity (NRMTI), and non-routine cognitive task intensity (NRCTI) over time for a given occupational group. We merge the task intensity measures to the worker-level SIAB data based on occupation and year combinations. Together this allows us to create time-varying task intensities by occupational group.

Before 1985 the wage variable in the SIAB does not include bonus payments but does so afterwards. This results in large inconsistencies in measured wages across these periods and as a result we restrict our observation period to start from 1985. While the occupational classification data in the SIAB is consistent until 2010, as highlighted by Böhm et al. (2019), there is a change in occupational classifications from 2011 onwards. Critically for our purposes, there is no approach available that allows for consistent classification of occupations before and after this change. Consequently, we only use data until 2010. The SIAB data includes no information on working hours, however it allows us to distinguish between full-time and part-time workers. We focus on full-time workers as this increases the comparability of daily wage rates. Wages are top-coded at the social security contribution limit. We deal with this issue by imputing censored wages following the imputation procedures outlined in Gartner (2005), Dustmann et al. (2009) and Card et al. (2013). We convert gross daily wages into real daily wages by using the consumer price index of the Federal Statistical Office. We create a yearly panel and select all employment spells that include June 30th as the cutoff date.

We exclude observations for East German workers who were registered in the data only from 1992 onwards. We further exclude apprentices, trainees, homeworkers, and individuals older than 65. Additionally, we restrict our analysis to male workers to avoid selectivity issues regarding female labour force participation and corresponding changes over time.[2]

We use, and contrast, two approaches to estimating the effect of job tasks on occupation specific wage components over time. First, we use a *fixed group definition* of task groups. Specifically, we define occupation fields as routine if the RTI of that occupation field is in the highest tercile of the employment weighted RTI distribution in 1985. We classify the remaining occupation fields as NRM (NRC) occupations if the NRMTI (NRCTI) of an occupation field in 1985 is higher than its NRCTI (NRMTI) in 1985.[3]

Next, we exploit the time variation in task intensities in the BIBB data to generate our *dynamic group definition* of task groups. Specifically, we use the routine task category from the *fixed group definition* and split it into three subcategories by using the time variation in NRCTI. To do so, for each occupation field in the routine task category we calculate the difference in NRCTI from the first to the last BIBB wave that we use ($NRCTI_{2006-1985} = NRCTI_{2006} - NRCTI_{1985}$). The routine occupation fields which are in the highest tercile of the 1985 employment weighted $NRCTI_{2006-1985}$ distribution are then classified as routine – Δ NRC high, those in the middle tercile as routine – Δ NRC middle and those in the lowest tercile as routine – Δ NRC low.

Table 2.A.1 presents descriptive statistics using the *fixed group definition* of task groups. The NRM task group has the highest share in our sample. The routine and NRC task groups have similar shares. In line with other studies examining task and labour market polarization (see e.g. Autor and Dorn, 2013), NRC workers are at the top, routine workers in the middle and NRM workers at the end of the wage and skill distribution. The average job tenure is highest for routine workers and much lower for NRM workers who also have on average lower full-time labour market experience compared to the other task groups. Routine workers are more likely to work in the manufacturing industry compared to the other task groups. Table 2.A.2 uses the *dynamic group definition* of task groups in which we split the routine task group into three subgroups: routine – Δ NRC high, routine – Δ NRC middle and routine – Δ NRC low. By design, all three routine task groups begin in 1985 (first BIBB wave) with a relatively high

[2]Individuals can hold more than one job in the data. We keep the main job, defined as the job with the highest daily wage or, in case of a tie, the spell with the longest tenure.

[3]As an alternative version of this approach, we classify 3-digit occupations into three task groups based on the approach in Acemoglu and Autor (2011) and Cortes (2016): (1) Routine: administrative support, operatives, maintenance and repair occupations, production and transportation occupations (among others); (2) Non-Routine Cognitive (NRC): professional, technical management, business and financial occupations; (3) Non-Routine Manual (NRM): service workers. These task groups are rather broad and fixed over time. However, this classification allows comparisons with the US literature on the evolution of wage premia over time (Cortes, 2016).

share of RTI. In 2006 (last BIBB wave), the share of RTI in subgroup routine – Δ NRC high decreased, while the share of NRC increased sharply. For the other two routine subgroups, the task mix remained about the same. For the whole observation period, workers in the routine – Δ NRC high task category earn on average more and are better educated compared to the other routine subgroups. Workers in routine – Δ NRC middle and routine – Δ NRC low are more likely to work in the manufacturing industry.

2.3. Estimation Approach

Our starting point follows the empirical approach outlined in Cortes (2016) which in turn builds on the theoretical model of Jung and Mercenier (2014). The main aim of this approach is to retrieve occupational wage premia over time.

Consider 3 occupations: routine (R), nonroutine manual (NRM) and nonroutine cognitive (NRC). Workers receive a potential wage which is equal to:

$$w_j(z) = \lambda_j \varphi_j(z), j \in \{R, NRM, NRC\} \tag{2.2}$$

where λ_j is the wage per efficiency unit in that occupation and $\varphi_j(z)$ is the productivity of a worker of skill z performing task $j \in \{R, NRM, NRC\}$.

Workers sort into tasks in the following way: High skilled workers are more productive at all tasks but have a comparative advantage in more complex tasks. Nonroutine cognitive tasks are assumed to be the most complex and nonroutine manual tasks the least complex. More formally:

$$0 < \frac{d\varphi_{NRM}(z)}{dz} < \frac{d\varphi_R(z)}{dz} < \frac{d\varphi_{NRC}(z)}{dz}.$$

Consider, as an example $\lambda_{NRC} = \lambda_R = \lambda_{NRM}$, meaning that the wages per efficiency unit are the same for all three tasks. In this case, all workers would sort into the nonroutine cognitive occupation where they are most productive and receive the highest wage. However, in equilibrium, λ_{NRC} is relatively low, while λ_{NRM} is relatively high, with λ_R in the middle. The low λ_{NRC} makes it optimal only for the most skilled workers to select into the nonroutine cognitive occupation, while the high λ_{NRM} attracts the least skilled workers to the nonroutine manual occupation, as their productivity in the other tasks is relatively small.

In logs the wage can be expressed as:

$$ln\, w_j(z) = ln\, \lambda_j + ln\, \varphi_j(z). \tag{2.3}$$

An intuitive way to think about the productivity term is:

$$ln\, \varphi_j(z_i) = z_i a_j. \tag{2.4}$$

Hence, the individual's occupation-specific productivity $\varphi_j(z_i)$ consists of individual's ability or skill z_i and occupation-specific return to skills a_j. Assuming that z_i and a_j are time constant while the wage premia might change, we can express the log wage of individual i in period t in the following way:

$$ln\, w_{ijt} = \theta_{jt} + z_i a_j, \tag{2.5}$$

where $\theta_{jt} \equiv ln\, \lambda_{jt}$ is the occupation wage premium in occupation j in year t. Intuitively, NRC occupations have a relatively low level of occupation wage premium, but a high level of occupation-specific return to skills. Therefore, workers with a high skill level are better off in NRC occupations, as their high skills have a higher reward in those occupations. On the other hand, nonroutine manual occupations have a relatively high level of occupation wage premium, but low occupation-specific returns to skills ($a_{NRM} < a_R < a_{NRC}$). Thus, for highly skilled workers, it is not rational to sort into nonroutine manual occupations, because the returns to skills are low there.

With routine-biased technical change (RBTC), and a skill level such that it is not optimal for a worker to switch, wages will fall for routine workers as θ_{jt} declines due to RBTC. Automation technology substitutes routine workers and complements NRC workers. Due to demand factors, routine workers loose wages and NRC workers gain. Thus, while $z_i a_j$ stays fixed over time, θ_{jt} does not. The prediction is that θ_{jt} will fall for routine jobs once we account for the selection mechanisms described above.

The assumption that z_i and a_j are time constant may not hold. For example, z_i might change over time if workers invest in their human capital through training. While a_j may change if the task mix in occupation j changes over time. An increase in the occupation-specific return to skills a_j for initially routine jobs – for example due to a change in the task mix to more non-routine cognitive tasks – would imply a less negative or even a positive impact of RBTC on the evolution of θ_{jt} over time.

We use the following empirical specification as in Cortes (2016):

$$ln\, w_{it} = \sum_j D_{ijt}\theta_{jt} + \sum_j D_{ijt}\gamma_{ij} + Z_{it}\zeta + u_{it}. \tag{2.6}$$

The dependent variable is the log wage of worker i at year t. θ_{jt} is the occupation specific wage component in occupation j in year t. We capture the occupation specific wage component by using occupation-year dummies. The reference task group is non-routine manual (NRM). D_{ijt} is an occupation indicator that equals one if individual i works in occupation j at year t and is zero otherwise. γ_{ij} is composed of an individual's time-invariant skills and the occupation-specific returns to those skills. It varies for an individual across occupations, but it stays constant whenever the individual stays in the same occupation. We estimate γ_{ij} by using person-occupation fixed effects. Z_{it} includes the region type, federal state dummies, sector dummies, a dummy for nationality and year dummies.

In our empirical specification, we control for occupation-individual fixed effects, which capture time constant unobserved heterogeneity. This implies that a change in occupation-specific skill returns or individual human capital over the time being employed in a specific occupation, for example due to technological changes or work-orientated training, will contribute to our estimate of θ_{jt}. In other words, estimates $\hat{\theta}_{jt}$ based on this approach will reflect occupation wage premia *and* changes in individuals' occupation-specific productivity over time if occupation-specific productivity is not constant over time. We therefore interpret our results as reflecting occupation-specific wage changes which go beyond occupation-specific wage premia in the strict sense.

As discussed above, workers sort into occupations based on their skills and the occupation-specific returns to those skills. By using person-occupation fixed effects, we aim to eliminate a bias that arises from different types of workers selecting into occupations that benefit them (positive selection). Specifically, occupation specific wage components are identified from variation in wages for workers who have stayed within specific occupation groups over time. Any bias that arises from time-constant unobserved variation across persons, occupations or person-occupation combinations is eliminated with this approach. Therefore, this approach explicitly exploits the shocks to which workers who have stayed in their occupation group are exposed. We use 1985 as our base year and the NRM task group as the reference category. Hence, the occupation-year dummies identify the changes over time relative to the base year and relative to the analogous change experienced by the NRM task group.

We estimate several variants of Equation 2.6 to explore potential heterogeneity in the development of occupations over time. To achieve this, we use the different classifications described in Section 2.2. First, we estimate Equation 2.6 by using our *fixed group definition.* This approach classifies occupations into routine, NRM and NRC task groups according to their initial task intensities.

Second, we estimate Equation 2.6 by using our *dynamic group definition.* This approach aims to capture changes in the task composition of occupations over time. Intuitively, we follow Acemoglu and Autor (2011) and Acemoglu and Restrepo (2020) in understanding occupations as a bundle of tasks. Thus, each occupation consists of a share of tasks that is routine, NRM and NRC. The composition of tasks within occupations can change and adapt to changes in technology. For example, occupations in finance and accounting have experienced a strong decrease in their RTI between 1985 and 2006, which was mostly compensated by an increase in their NRCTI (see Table 2.A.3). While workers in this occupation field mostly performed routine tasks initially, such as measuring, calculating and operating, this has changed to more NRC tasks such as investigating, consulting and organizing. We expect that routine occupations which experience an increase in their NRC task content over time also experience an increase in their occupation specific wage components. The reasoning goes as follows. As more automating technologies are used in these occupations which substitute for routine tasks, for some occupations the share of NRC tasks increases. This also has implications for the type of worker, or the skill level required for this job. Hence, next to a potential change in the return to skills a_j, the wage per efficiency unit λ_j increases for those occupations as the relative demand for NRC tasks increases.

A change in the task mix will change the occupation specific returns to skills, a_j, meaning that more skilled workers select and stay in those occupations over time. For occupations that continue to use a relatively high share of routine tasks, such as occupations in metal production and processing, the λ_j decreases as the relative demand for routine tasks decreases over time due to technological change. Specifically, we estimate the wage changes for the 5 task categories routine – Δ NRC high, routine – Δ NRC middle, routine – Δ NRC low, NRM and NRC. Again, we use the year 1985 and the NRM task category as base categories in our estimation.

2.4. Results

2.4.1. The Evolution of Task Wages

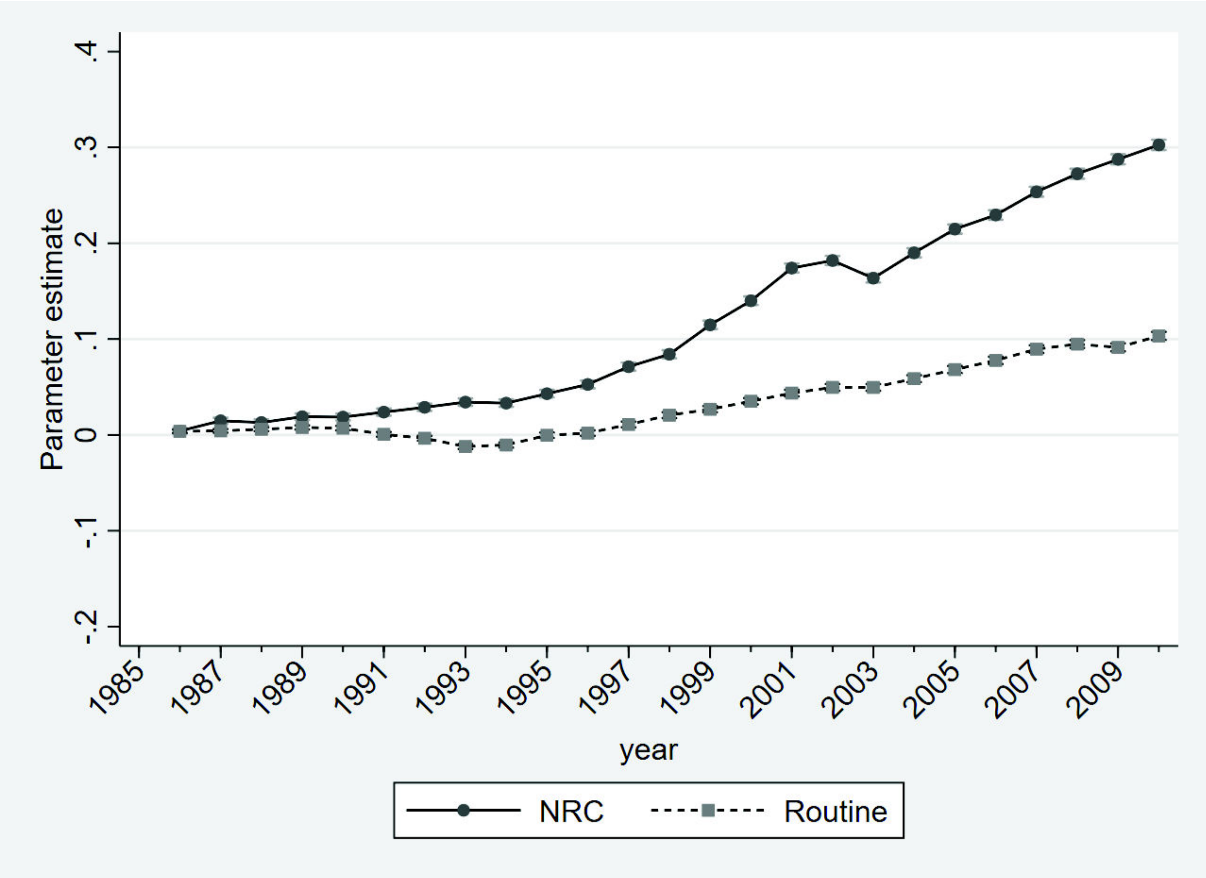
Figure 2.1 plots the annual evolution of occupation-specific wage components relative to non-routine manual jobs associated with working in a routine and non-routine cognitive job, respectively (see Table 2.A.4 for details). It does so by fixing initial task mixes at 1985 such that NRC and routine jobs reflect those occupations that in 1985 were most intensive in those tasks. This displays the development of a much larger wage growth for non-routine cognitive work that by the late 2000s leads to a wage difference to non-routine manual work of 20%. This is consistent in general pattern and magnitude to that reported, for instance, for the US (Cortes, 2016). This pattern, however, takes longer to develop, with substantial wage differences between task groups only becoming apparent in the mid to late 1990s. This is some 10 years after similar patterns for the US and fits with the suggestion in previous research that routinisation occurred later in continental Europe (Goos et al., 2009).

One striking feature of Figure 2.1 is the complete absence of the deterioration in wages for German routine workers. While this contrasts with the quite marked wage penalties for these groups that have been demonstrated elsewhere, this pattern has been noted in other research for Germany using other data sources across shorter time periods (Wang, 2020). Nonetheless, the lack of a wage penalty for routine workers in Germany, relative to non-routine manual jobs, remains a puzzle and runs against the general view of the impact of technological change on workers.[4]

An issue with fixing occupational tasks content at initial values is that it may miss important changes in task content within occupations over time that increasingly make the occupations

[4]As a robustness check, we use a similar classification of task groups as in the US literature (see e.g. Acemoglu and Autor, 2011; Cortes, 2016) in Figure 2.A.1 and find similar results as for our baseline specification.

Figure 2.1.: Task-group specific wages over time (fixed task groups using BIBB 1985 data)



Notes: NRC: non-routine cognitive occupations. Reference category: NRM = non-routine manual occupations.

within given task groups heterogeneous. For example, consider auxiliary office occupations such as secretaries and typists. These are jobs impacted strongly by routine biased technological change as they involved a set of tasks that were largely replaceable by algorithm. However, these occupations still exist, albeit with markedly different task mixes (see Table 2.A.3 for examples of occupational groups with a strong change in task content over time). To explore this process, our next step is to utilise the strength of our task data to examine within occupational changes in task mix, and the implications of accounting for this on our understanding of the evolution of occupational wages over time.

Using the BIBB data, our initial descriptive step is to use our two end points in this data, 1985 and 2006, and decompose occupational changes in routine task intensity across this period. We perform a simple shift-share analysis of changes (decline in RTI) over time into that component explained by changes in employment shares of given occupations (between differences) and changes in the routine task intensity of given occupations (within differences). As shown in Table 2.1, within occupational changes in task mix dominate the overall decline in RTI over this period, comprising some 75% of total reductions in RTI. This highlights a key point, holding occupational employment shares constant at 1985 values, RTI of given occupations have changed substantially over this 21-year period. This suggests that technological change induced large shifts in the task content of occupations.

Table 2.1.: Shift-share analysis of RTI, different time periods

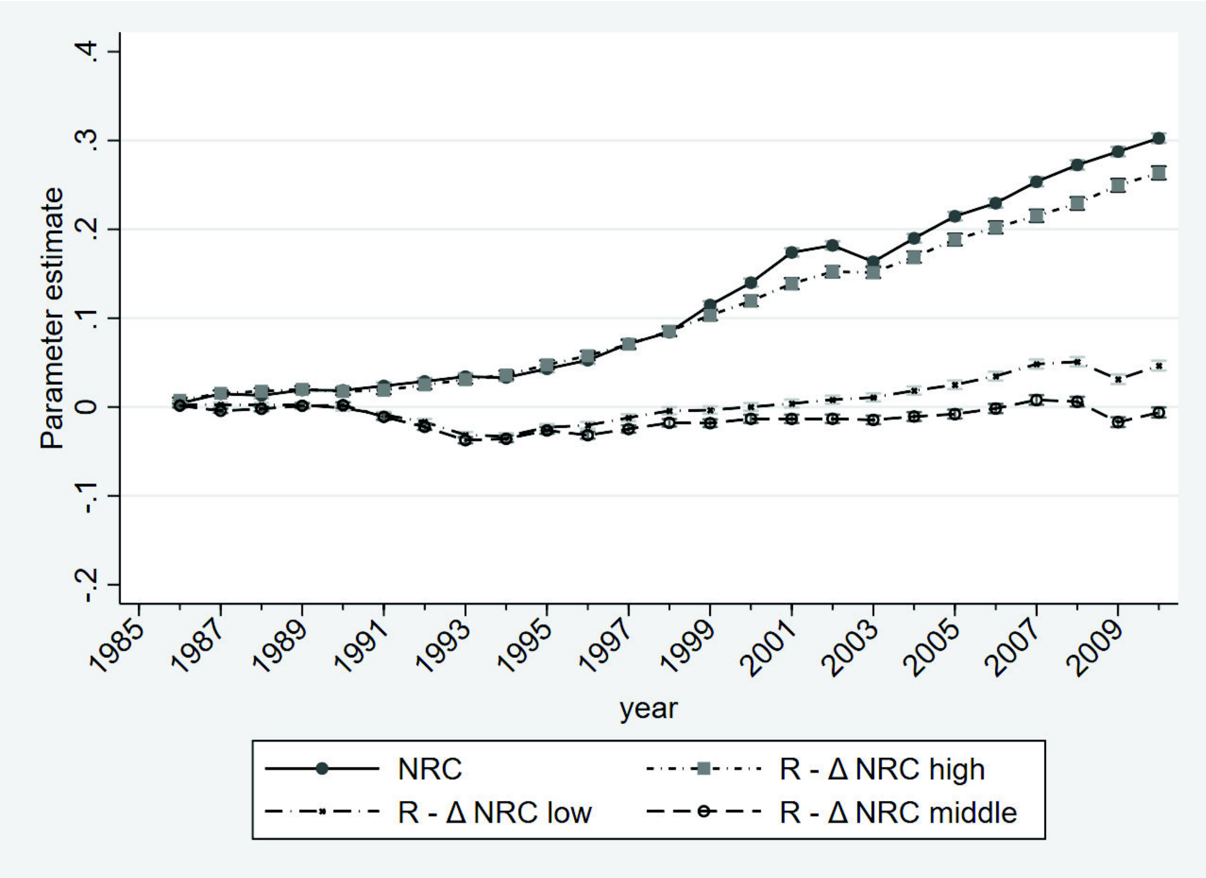
	Total	**Between**	**Within**
1985-1992	-0.87	-1.01	0.14
1992-1999	-3.73	-1.54	-2.20
1999-2006	-3.17	-0.58	-2.59
1985-2006	-7.78	-1.97	-5.81

Notes: This table shows the change in overall RTI as well as the importance for this overall change of the composition of occupations in total employment holding RTI within occupations constant (Composition Change) and of the RTI change within occupations holding composition constant (Change in RTI). Results are 100 × annual changes in task measures.

Using this information, we return to estimating task group-wages over time where we now allow task content to vary over time. Our first step is to re-estimate Equation 2.6 separately for occupations which were intensive in routine tasks in 1985, but then evolved differently in terms of their task content over time. We thus use our dynamic group definition of task groups, in which we create sub-categories within the initially routine task jobs, those with very high increases in NRC, those with only small increases in NRC and those with very low increases or even decreases in NRC over the 21-year period. Figure 2.2 plots the evolution of wages for these disaggregated categories (see Table 2.A.5 for details).

What is immediately clear is how dramatically the evolution of wages for routine task intensive workers is contingent on subsequent changes in within-job task content. In particular, the lack of any wage growth of routine task intensive workers relative to non-routine manual workers demonstrated earlier reflects two very different patterns. For those initially routine intensive

Figure 2.2.: Task-group specific wages over time (routine subgroups by change in NRCTI between 1985 and 2006)



Notes: This figure shows the task-group specific wage component over time for occupations which were routine or non-routine cognitive in 1985 (according to the BIBB data). Additionally, the routine task group is divided into three further subgroups by change in NRCTI over time: Routine – Δ NRC high, Routine – Δ NRC middle and Routine – Δ NRC low. Reference category= NRM.

occupations that do not experience increases in non-routine cognitive task content, we observe relative wage stagnation, and small wage increases or decreases contingent on the period. This broadly fits with previous evidence across a range of settings, routine task intensive jobs are associated with wage stagnation and/or losses. However, this is simply not true for those jobs that increased in NRC content, and in fact these jobs are associated with marked increases in wages over time. These are only slightly smaller than those present for non-routine cognitive occupations over this period and often overlap.

The small difference in wage trends between R - Δ NRC middle and R - Δ NRC low occupations and the large gap with R - Δ NRC high occupations can most likely be attributed to the non-monotonic difference in the change in NRCTI between these task groups, as reported in

Table 2.A.3. While we observe a very large increase in NRCTI for R - Δ NRC high occupations, the change in NRCTI was similar for R - Δ NRC middle and R - Δ NRC low.

These findings indicate very different wage effects across jobs that initially had similar routine intensity, and it is quantitatively sizeable: over the time period under consideration, the wage growth of routine occupations with a growing importance of NRC tasks amounts to 10.3% (relative to NRM occupations) when using the fixed task group definition (Table 2.A.4), but to 26.3% when using the dynamic task group definition, i.e. taking into account within-occupation changes in task intensity (Table 2.A.5). Not taking into account task changes within occupations, we would therefore understate the growth in the occupation-specific wage component by up to 16 percentage points. By contrast, we would overstate the wage growth for routine occupations with relatively constant non-routine cognitive task intensity by 10.9 percentage points, as a similar comparison makes clear.

2.4.2. Robustness

Naturally, these results raise questions regarding their robustness. First, is the observed change in task content likely to be driven by changes in worker composition? Second, can worker composition explain wage growth within task groups? Third, do workers with different occupational tenure, who are otherwise observationally equivalent, perform different job tasks, and do we therefore observe cohort effects for wages?

Regarding the first question, we start with the observation that workers entering and leaving the five task groups considered differ quite markedly in terms of observable characteristics, but that the differences are much smaller between the NRC and the R - Δ NRC high task groups (see Table 2.A.6). Furthermore, the differences between these two task groups shrink over the time period under consideration, particularly with respect to education.

We therefore analyze whether the change in task content of our task groups over time is driven by changing worker composition in terms of education or by changing task content within education groups. Specifically, using the BIBB data, we perform two decompositions of the change in mean NRC task content over time with education as the explanatory variable. The first decomposition compares the R - Δ NRC high and R - Δ NRC middle task groups, the second decomposition compares the R - Δ NRC high and R - Δ NRC low task groups.

We use the decomposition method of Smith and Welch (1989). This method allows us to decompose the difference in the change in mean NRCTI between two task groups over time. For example, the mean NRCTI of R - Δ NRC high workers increased by 0.133 more than the mean NRCTI of R - Δ NRC middle workers from 1985 to 2006 (see Table 2.A.7). We can decompose this total change into four components: the main effect, i.e. the change in education groups within the task groups valued at base year 1985 in R - Δ NRC middle (or R - Δ NRC low); the group interaction, i.e. the change in education groups within R - Δ NRC high that is valued differently between task groups in base year 1985; the time interaction, i.e. the returns to education in R - Δ NRC middle (or R - Δ NRC low) given the education difference in 2006 between R - Δ NRC high vs. R - Δ NRC middle (or R - Δ NRC low); and the group-time interaction, i.e. the returns to education over time given the 2006 level in education of group R - Δ NRC high. If changing composition in terms of education within task groups explains the relative increase in mean NRCTI for R - Δ NRC high, we would find that the main effect of the decomposition dominates the total change.

The decomposition results show, however, that almost all the change in mean NRCTI for R - Δ NRC high can be explained by a change in returns to education. In other words, the increase in mean NRCTI within the R - Δ NRC high task group cannot be explained by an inflow of highly educated workers. Instead, highly educated workers do more NRC tasks within the R - Δ NRC high task group. In Section 2.4.4, we present suggestive evidence that more job training for R - Δ NRC high is a likely driver of increasing NRC task content over time.

Regarding the second question on whether worker composition can explain wage growth within task groups, it is worth recalling that our estimates come from variation within person $\times$ occupation cells such that they should not reflect returns to an individuals' time-invariant skill level or occupation-specific returns to skill. However, as reported in Table 2.A.8, there are initial differences in both the composition of these jobs and the workers in these occupations. Most notably, there are differences in terms of industry structure (those occupations where NRC did not increase are disproportionately in the manufacturing industry), and differences in terms of the educational profiles of the workers (those occupations where NRC did increase have a markedly larger share of workers with university level education). There are few if any other differences. Our approach to exploring this uses more homogeneous workers groups (in terms of observables) while maintaining sufficient sample sizes. We do this by re-estimating our main models first (a) only including manufacturing industry workers and then separately (b) excluding all workers with university education.

The resultant estimates are reported in two panels as Figure 2.A.2. As can be seen, the reported patterns of occupation-specific wage growth essentially match those for our main results. This provides supportive evidence that the differential patterns in the evolution of routine worker wages we present do not simply reflect observable differences across these occupations.

As noted in Section 2.3, we assume that changes in occupation-specific skill returns stay constant over time or that any changes in occupation-specific skill returns do not affect our estimates. One approach to relaxing this assumption is to allow changes in *observable* occupation-specific skills to vary over time. To do so, we follow Cortes (2016) in assuming that the time variation in the return to education is the same for all occupations and additionally include education $\times$ year fixed effects in our baseline estimation Equation 2.6. We report the results in Figure 2.A.3 and find very similar results compared to our baseline estimation in Figure 2.2. Thus, these results provide supportive evidence that observable returns to skills are not driving our results.

The third question is whether workers with low occupational tenure do not, in practice, conduct the same average task mix as workers with higher tenure they are joining or replacing, and whether this is an important determinant of wage growth. Examining this is equivalent to asking whether our main result that task change within occupations is a key determinant of wage growth is driven by age and/or cohort effects. For example, one may suspect that young workers are best able to reap the benefits of technological change, whereas older workers have difficulties adapting and are therefore particularly vulnerable to technological change. In this case, one would observe strongly differing wage growth of task groups between young and older workers, with young NRC workers displaying the highest, older R– Δ NRC low workers the lowest wage growth. Furthermore, looking at different cohorts allows us to examine whether our results are driven by specific time periods where technological change may have had a particularly strong effect on workers.

We therefore analyse the wage growth of workers in different task groups by age group and start year. We separately estimate the wage growth for young workers (age 25-34) and older workers (age 35-50) who in a specific year t (1985, 1990, 1995, 2000) were in one of the task groups R- Δ NRC high, R - Δ NRC middle, R - Δ NRC low or NRC occupations.[5] We estimate a regression with wage growth from t to $t+1$, $t+2$, $t+4$ or $t+10$ as the dependent variable and dummies for being in one of the task groups as independent variables with NRM as the reference category.

The results of our wage growth regressions by age and start year are displayed in Figure 2.3. Two features become apparent. First, for young and older workers, we observe two task groups with increasing wage growth over time (R- Δ NRC high and NRC), and two task groups with decreasing wage growth over time (R- Δ NRC low and R- Δ NRC middle), where the reference group are NRM workers. Second, this first feature is observable for all start years, and it is quantitatively similar across start years.

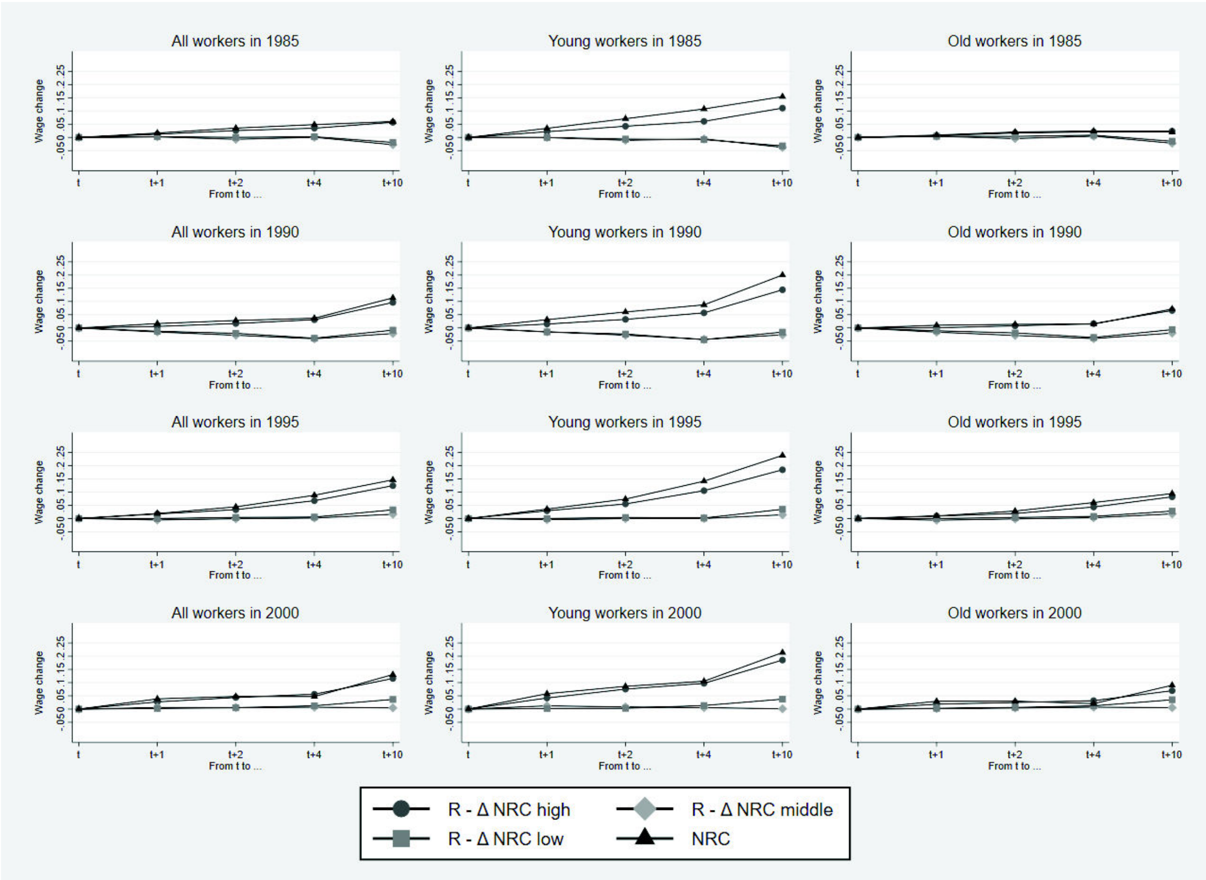
Thus, in line with Figure 2.2, wages grow over time for occupations with higher NRC task content. Most importantly, wage growth in these occupations is not driven by young workers who start those jobs and do something different than older workers, but rather by higher wage growth in R – Δ NRC high and NRC occupations for all workers across all years. This result is in line with the additional observation that average task intensities for young and older workers are very similar (Table 2.A.9), i.e. that young and older workers perform roughly the same tasks within any given task group. Therefore, the higher wage growth of younger workers in Figure 2.3 is unlikely to reflect differences in job task between young and older workers. Instead, job ladder effects, which are more important early in the life cycle, are a more likely explanation.

2.4.3. Wage changes and selection of workers who switch task groups

Our main results come from regressions in which we control for selection into task groups using worker × occupation fixed effects (see Equation 2.6). Here, we provide descriptive evidence on the consequences on wage growth of switching between task groups. Our working hypothesis is that switching out of occupations with falling labour demand, R- Δ NRC low and R- Δ NRC middle, to occupations with growing labour demand, NRC or R- Δ NRC high is associated with subsequent positive wage growth. By contrast, switching out of R- Δ NRC high or NRC is expected to be associated with subsequent negative wage growth unless workers switch to either NRC or R- Δ NRC high.

[5] Note that ”start year” denotes the year where we start analysing these workers, not the year where they start a job or enter a task group.

Figure 2.3.: Wage Growth by Age and Cohort



Notes: This figure shows the wage growth for different task groups over time and for young workers (25-34 years) vs. older workers (35-50 years). We subsample different years and regress wage growth on workers who in starting year t were in one of the task groups. Reference category: NRM.

This leads us to analyse the wage growth of workers who in year t were in one of these five task groups and switched to another task group in year $t+1$. To do so, we regress wage growth from year t to year $t+1$, $t+2$, $t+4$ and $t+10$ on dummy variables which indicate whether a worker has switched out of his or her original task group to another specific task group. The regression therefore yields the wage growth in year $t+1$, $t+2$, $t+4$ or $t+10$, conditional on switching from one task group to another, and relative to staying in the original task group. In the regression, we include as control variables dummies for the year, region type, federal state, 1-digit industry, nationality (German vs. non-German), age group (18-25, 26-35, 36-45, 46-55, 56-65) and three skill group dummies (no vocational training, vocational training, university, or university of applied sciences).

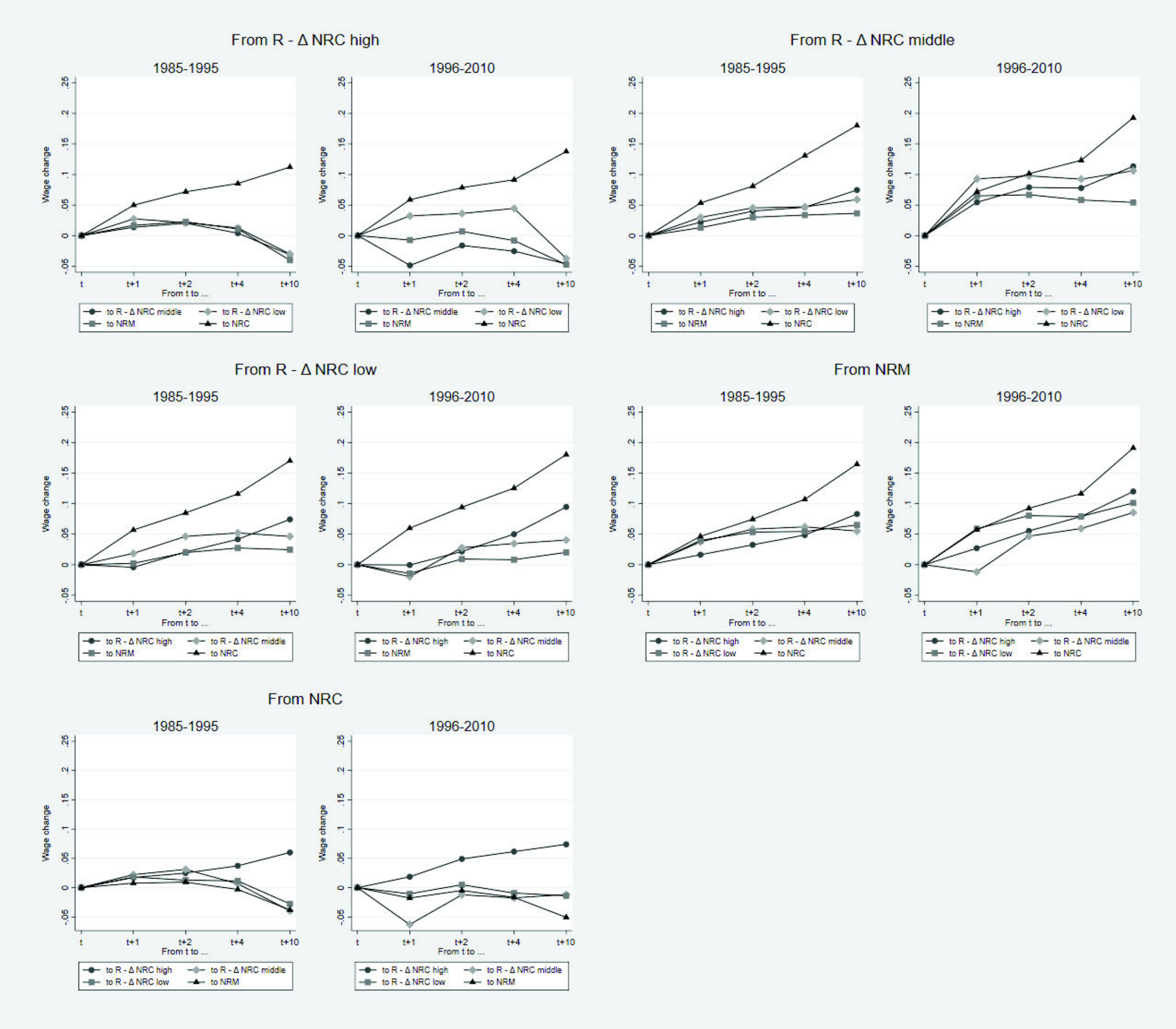
The analysis of task group switches yields several insights (Figure 2.4). First, in line with our working hypothesis, switching out of one's task group to NRC occupations is always associated with positive subsequent wage growth. Second, switching out of ones' task group to R- Δ NRC high is also associated with positive wage growth. This effect even increases over time and is therefore most pronounced for long periods ($t + 10$). Third, switching out of R- Δ NRC high to the other routine occupations is associated with negative wage growth over the long time horizon for the time period 1985-1995 and immediate wage decline even in the short time horizon ($t + 1$) for the later time period 1996-2010. A similar pattern is observable for the NRC task group. Thus, over time it becomes more and more profitable to stay in the R- Δ NRC high (NRC) occupations rather than switching out of it, unless a switch to NRC (R- Δ NRC high) occupations occurs.

Switching between task groups does not occur at random. Instead, workers purposefully select into task groups (Böhm et al., 2019), and this has important consequences for wage development (Gathmann and Schönberg, 2010). We therefore investigate in more detail which workers switch to which task group, and whether this selection into task groups has changed over time. We are particularly interested in which workers switch to NRC or R - Δ NRC high and therefore experience wage gains.

In our analysis, we focus on unobservable skills which we proxy with workers' ability quintile. More specifically, we follow Cortes (2016) and use the predicted occupation spell fixed effects ($\hat{\gamma}_{ij}$) from Equation 2.6, i.e. the estimation equation for Figure 2.2. As γ_{ij} in Equation 2.6 is monotonically increasing in underlying ability z, we refer to the quintiles of the estimated occupation spell fixed effects as ability quintiles (see Section 2.3). To construct ability quintiles, we rank workers according to their position in the ability distribution of the estimated occupation spell fixed effects for a given task group and for each year separately. To capture changes over time, we perform the estimation of switching probabilities for two time periods, 1985-98 and 1999-2010.

The results of this exercise are displayed in Figure 2.5 and can be summarised as follows. First, workers with higher ability have a higher likelihood of switching to NRC, workers with lower ability have a higher likelihood of switching to NRM. Second, workers in R- Δ NRC high across all ability quintiles have a relatively high probability of switching to NRC occupations, this likelihood becomes higher with higher ability. In the initial time period (1985-98) workers in the lowest ability quintiles of R- Δ NRC high workers have the highest likelihood of switching

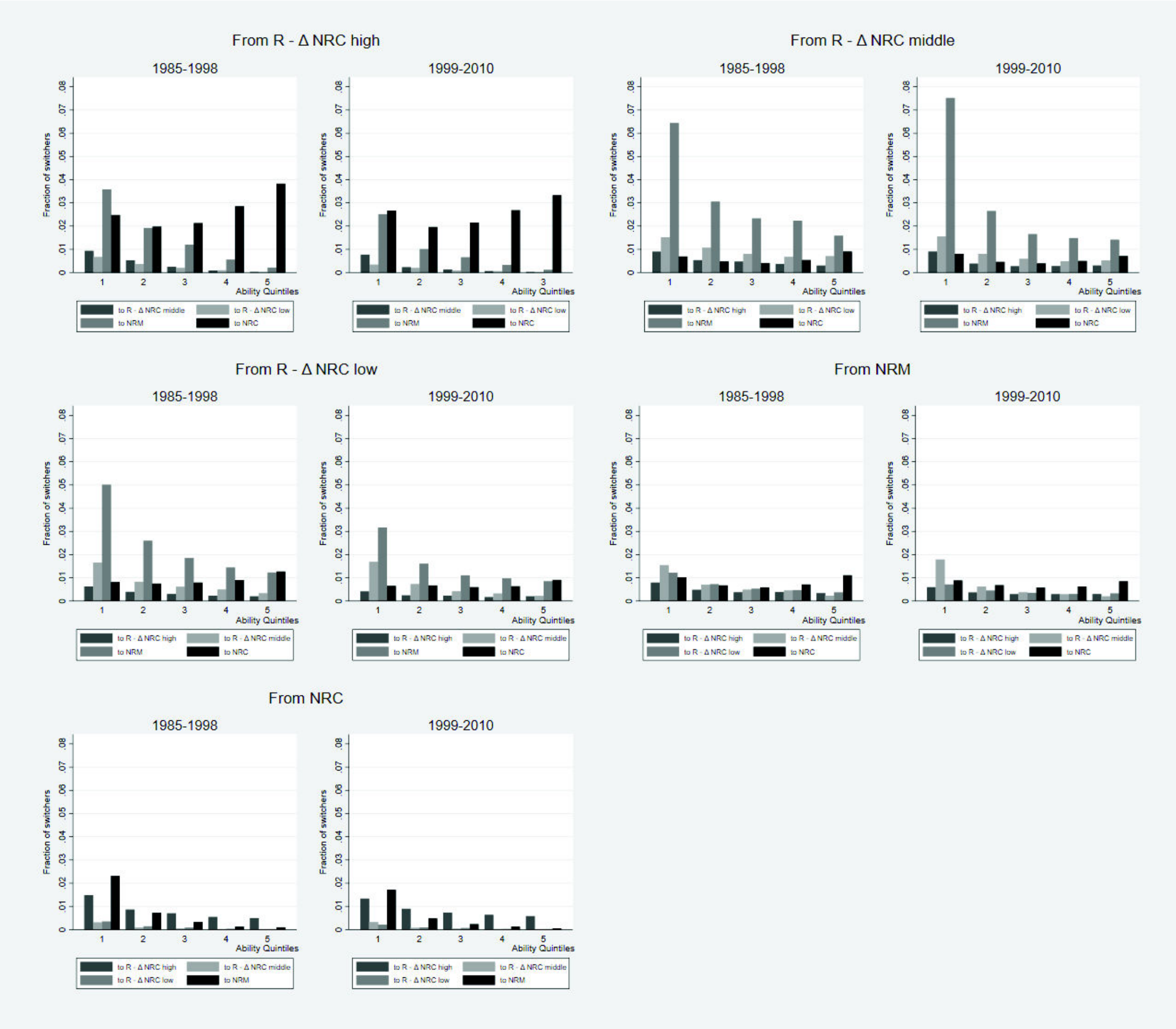
Figure 2.4.: Wage Growth by Task Group Switchers



Notes: This figure shows the wage growth over time for workers who switch out of their task group from t to $t+1$. Workers who stay in their respective task group are the omitted category. The wage changes are taken over the horizons 1985-1995 and 1996-2010. All regressions include dummies for year, region type, federal state, 1-digit industry, nationality (German vs. non-German), age group (18-25, 26-35, 36-45, 46-55, 56-65) and three skill group dummies (no vocational training, vocational training, university, or university of applied sciences).

to NRM. This changes over time as even R- Δ NRC high workers with lower ability in 1999-2010 have a higher likelihood of switching to NRC and lower likelihood of switching to NRM. Third, the probability that R- Δ NRC middle and R- Δ NRC low stay within their task group increases over time (only implicit in the graph). Other than this, the switching patterns do not change much over time for R- Δ NRC middle and R- Δ NRC low occupations. Fourth, there is a high likelihood of switching into NRM occupations, which likely reflects the large size of this task group (see Table 2.A.2). Fifth, despite the small size of the R- Δ NRC high task group, NRC workers have a relatively high probability of moving into this task group.

Figure 2.5.: Fraction of Switchers by Ability Quintiles



Notes: This figure illustrates the probability of switching out of a task group between years t and $t + 1$, according to a workers' ability quintile.

Our results imply that R- Δ NRC high and NRC occupations are relatively close in terms of human capital transferability. If workers in R- Δ NRC high occupations switch, they are more likely to switch to NRC, and vice-versa for NRC workers. This pattern is stronger for workers with higher ability. R- Δ NRC middle, R- Δ NRC low and NRM occupations are also relatively close to each other in terms of human capital transferability. Thus, these results are in line with our other findings: NRC and R- Δ NRC high occupations feature high wage growth and attract workers with better skills and ability; workers in R- Δ NRC middle, R- Δ NRC low and NRM occupations feature relatively low wage growth and attract workers with lower skills and ability.

2.4.4. The role of training

To this point, we have demonstrated robust differences in the occupation-specific wage component attached to initially routine intensive occupations that are a function of the evolution of the task mix of these occupations over time. If, as we contend, there is wage growth in routine jobs that increased markedly in their NRC content, a natural question is what happened to the skills of workers in these jobs. To examine this, we explore the role of job training in occupation task mix changes over time.[6] Specifically, if the change in task mixes for initially routine intensive occupations is a process of individual adaptation to the new task environment rather than a change in the workforce composition, this should also be reflected in the likelihood of on-the-job training over time. In terms of our task groups, we hypothesize that the share of workers participating in job training has distinctively increased over time for R- Δ NRC high occupations relative to the other routine occupations.

To test this hypothesis, we use an additional data source, the German Socio-Economic Panel (SOEP). The SOEP is a representative longitudinal data set of private households in Germany which includes information regarding on-the-job training over time.[7] In Figure 2.6, we illustrate the shares of workers in training courses financed by the employer over time and for each task group. The following features become apparent.[8] First, NRC workers have relatively high shares of training participation which remains relatively stable over time. Second, the share of training participation for R- Δ NRC high workers increased strongly from 1989 to 2000 and decreases in 2004 and 2008. In particular, the share of R – Δ NRC high workers in training financed by the employer increased abruptly from 1989 to 1993 (from 9.5 percent to 23.4 percent). Third, training participation for the other two routine task groups (R – Δ NRC middle and R – Δ NRC low) increased steadily over time, however not as strongly and abruptly as for the R – Δ NRC high task group. Fourth, training participation of the NRM task group also increased steadily over time with a stronger increase from 2004 to 2008. Together, these

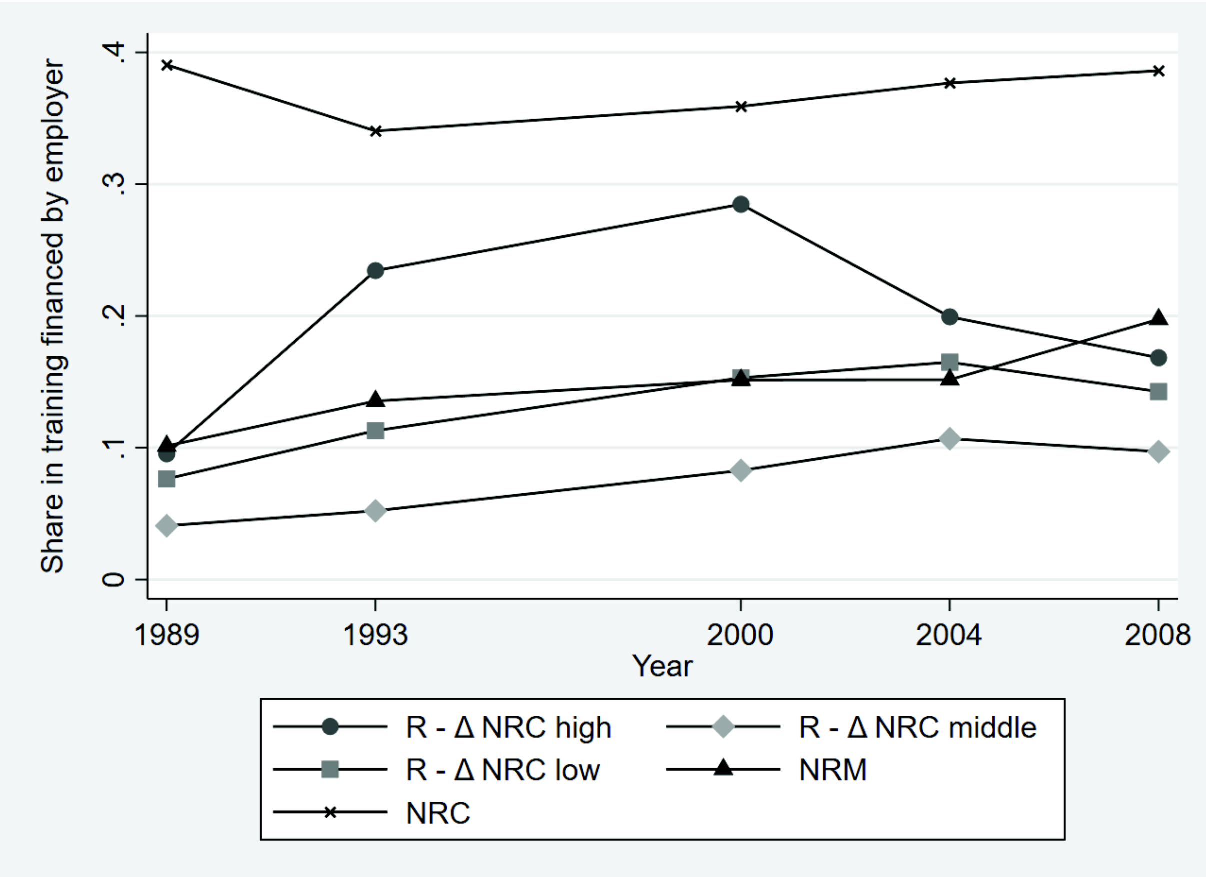
[6] Other papers studying the relation of job tasks and job training include e.g. Görlitz and Tamm (2016a), Görlitz and Tamm (2016b), Mohr et al. (2016), Tamm (2018), Feng and Graetz (2020), and Lukowski et al. (2021).

[7] Specifically, the SOEP asked in the years 1989, 1993, 2000, 2004 and 2008: "How many professional development courses or classes have you taken in the last three years?" The SOEP also asks respondents when these courses started, how long they took, whether the courses took place during working time, who organized these courses and who financed these courses. We only focus on courses which took place in the interview year or the year before. We classify courses as "financed by employer" if the course took place during working time or was organized by the employer or financed by the employer. More information on the SOEP can be found in Goebel et al. (2019).

[8] In Figure 2.A.4, we illustrate the shares in any type of training course. In general, most training course, conditional on employment, are in some way financed by the employer.

results suggest that employers and workers adapted to changing tasks by increasing training participation in a manner that was particularly pronounced for workers in R – Δ NRC high occupations. In particular, we observe a sharp increase in training participation for the R – Δ NRC high occupation in the 1990s, when the decline in routine tasks and the increase in more complex tasks were most pronounced (see Table 2.1).

Figure 2.6.: Shares in Training Course Financed by Employer



Notes: This figure illustrates the shares of workers in training courses financed by the employer by task group and year. Source: SOEP

The raw changes in training participation in Figure 2.6 could be driven by compositional changes within the task groups over time. To check whether these results still hold once we control for observable characteristics, we estimated, by pooled linear probability models, the relationship between our task group dummies and training financed by the employer, respectively. In doing so we control for age, education, marital status, migration background, federal state, industry, firm size, and year dummies.

Table 2.A.10 illustrates the results using the NRM task group as reference category. We find

a statistically insignificant positive coefficient for R – Δ NRC high workers and negative statistically significant coefficients for R – Δ NRC middle and R – Δ NRC low workers. Furthermore, we find that the coefficients between R – Δ NRC high workers vs. R – Δ NRC middle and R – Δ NRC high vs. R – Δ NRC low are statistically different from each other.[9] NRC workers have a statistically significant positive coefficient on training participation. Overall, we conclude that our main results in Figure 2.6 still hold once we control for observable characteristics. Specifically, R – Δ NRC high workers have substantially higher participation in training compared to the other routine task groups which experienced smaller changes in their task intensities.

2.5. Conclusion

There have been dramatic changes in the nature of job tasks over the past decades. A focus has been on how the workers in routine jobs, most readily replaced by computing, have suffered wage losses over this period. We provide evidence on the importance of an adaptation process at the intensive margin of employment: changes of within-occupational task mixes over time, which we are able to analyse using unique data for Germany. Looking at a 25-year period, we show that many initially routine intensive occupations have changed markedly in terms of their task mix. This has substantive implications for our understanding of the effect of routinisation on the welfare outcomes of workers.

We demonstrate that how these occupations changed over time has important consequences for the evolution of wages, and that only those jobs that remain routine task intensive over this period are associated with wage losses or stagnation. By contrast, jobs that increase the content of non-routine cognitive tasks feature significant wage gains. These effects are quantitatively sizeable. For example, initially routine occupations with a strong increase in non-routine cognitive task content over our 25-year observation period experience wage growth nearly 27 percentage points higher than initially routine occupations with relatively constant non-routine cognitive task content. These results do not appear to reflect factors such as worker composition or cohort effects within occupations. We also provide evidence that on-the-job training is a likely driver of these wage effects.

[9] The coefficients of R – Δ NRC middle and R – Δ NRC low are also statistically different from each other. This difference is entirely driven by two occupation fields within the R – Δ NRC low task group: "Occupations in mechanics and tool making" and "Precision engineering and related occupations".

Our results have a number of implications. First, some occupations that are considered initially rather inefficient can adapt over time by changing their production technology. Workers may therefore be better off staying in an occupation rather than switching to another one, even as technological progress continues or becomes more intensive in the future, e.g. with the growing importance of artificial intelligence. Second, the importance of adaptability within a given occupation highlights the relevance of a good education system, and particularly the relevance of lifelong learning and on-the-job training. This means that workers, firms, and policy makers should devote even more attention to this part of the education system. Third, our result that some initially routine occupations provide good prospects for their workers could be an important piece of information for job seekers which could e.g. be provided in online job advice. Finally, our results indicate that accounting for within-occupation task change is crucial for understanding the wage effects of technological change. In particular, differences in the evolution of the task content of occupations could explain why during the last decades, routine workers have experienced a relative decline in wages in the US but not in Germany. This conjecture is left for further research.

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Appendix

2.A. Additional Tables and Figures

Table 2.A.1.: Sample descriptives, task classification according to task intensity (BIBB data)

	Routine		**Nonroutine Manual**		**Nonroutine Cognitive**	
No. of observations	1,589,127		2,079,037		1,534,333	
Share	30.55		39.96		29.49	
No. of individuals	188,821		228,073		154,875	
Averages:						
Log (daily) wage	4.65	(0.31)	4.58	(0.28)	4.92	(0.30)
Log (daily) imputed wage	4.68	(0.36)	4.59	(0.31)	5.07	(0.50)
Age	39.70	(10.98)	39.70	(11.17)	41.74	(10.15)
Job tenure (in years)	8.19	(7.12)	7.25	(6.77)	7.68	(6.96)
Labour market experience (in years)	13.23	(7.93)	12.85	(7.88)	13.77	(7.78)
Task measures:						
RTI	0.52	(0.18)	0.35	(0.08)	0.24	(0.09)
NRM	0.22	(0.10)	0.48	(0.10)	0.12	(0.06)
NRC	0.26	(0.23)	0.16	(0.09)	0.64	(0.11)
Fractions within the task group:						
No vocational training	14.96		13.10		2.55	
Vocational training	79.43		83.35		63.60	
University or university of applied science	4.61		2.55		33.31	
Missing	0.99		1.01		0.55	
Mining industry	2.66		0.68		0.64	
Manufacturing industry	63.87		30.97		35.04	
Energy and water supply industry	1.43		1.66		1.52	
Construction industry	1.78		23.02		2.71	
Trade and repair industry	8.69		13.36		18.26	

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Table 2.A.1 – continued from previous page

	Routine	**Nonroutine Manual**	**Nonroutine Cognitive**
Catering industry	2.39	1.50	0.37
Transport and news industry	2.54	11.38	2.99
Finance and insurance industry	0.79	0.24	10.89
Real estate and housing, renting of movable property, business service industry	6.79	5.35	14.03
Public services industry	5.41	4.13	4.36
Education industry	0.52	0.54	2.67
Health industry	1.54	4.78	2.73
Other services industry	1.57	2.40	3.79
Missing	0.01	0.01	0.01
Foreign workers	12.10	11.21	3.86
Censored wages	7.04	3.16	37.40

Notes: Standard deviation in parentheses. Tasks groups are defined by using the fixed group definition described in Section 2.2.

Table 2.A.2.: Sample descriptives, task classification according to task intensity (BIBB data) for task subgroups

	Routine - Δ NRC high		**Routine - Δ NRC middle**		**Routine - Δ NRC low**		**Nonroutine Manual**		**Nonroutine Cognitive**	
No. of observations	549,951		503,845		535,331		2,079,037		1,534,333	
Share	10.57		9.68		10.29		39.96		29.49	
No. of individuals	74,297		75,356		63,548		228,073		154,875	
Averages										
Log (daily) wage	4.74	(0.33)	4.56	(0.31)	4.66	(0.25)	4.58	(0.28)	4.92	(0.30)
Log (daily) imputed wage	4.79	(0.43)	4.56	(0.32)	4.67	(0.27)	4.59	(0.31)	5.07	(0.50)
Age	40.71	(10.75)	39.07	(11.12)	39.25	(10.99)	39.70	(11.17)	41.74	(10.15)
Job tenure (in years)	8.13	(7.18)	7.78	(7.04)	8.64	(7.11)	7.25	(6.77)	7.68	(6.96)
Labour market experience (in years)	13.56	(7.94)	12.55	(7.92)	13.53	(7.88)	12.85	(7.88)	13.77	(7.78)
Task measures										
RTI	0.34	(0.18)	0.65	(0.10)	0.57	(0.07)	0.35	(0.08)	0.24	(0.09)
NRM	0.15	(0.10)	0.22	(0.07)	0.29	(0.07)	0.48	(0.10)	0.12	(0.06)
NRC	0.51	(0.22)	0.13	(0.08)	0.14	(0.05)	0.16	(0.09)	0.64	(0.11)
RTI in 1985	0.49	(0.07)	0.65	(0.07)	0.58	(0.06)	0.31	(0.08)	0.30	(0.06)

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Table 2.A.2 – continued from previous page

	Routine - Δ NRC high		**Routine - Δ NRC middle**		**Routine - Δ NRC low**		**Nonroutine Manual**		**Nonroutine Cognitive**	
NRM in 1985	0.15	(0.15)	0.26	(0.08)	0.28	(0.05)	0.58	(0.08)	0.12	(0.07)
NRC in 1985	0.36	(0.16)	0.09	(0.05)	0.13	(0.04)	0.12	(0.06)	0.58	(0.10)
RTI in 2006	0.20	(0.11)	0.56	(0.08)	0.53	(0.03)	0.37	(0.07)	0.19	(0.07)
NRM in 2006	0.15	(0.07)	0.26	(0.04)	0.30	(0.05)	0.38	(0.05)	0.11	(0.06)
NRC in 2006	0.66	(0.18)	0.18	(0.06)	0.17	(0.04)	0.26	(0.07)	0.70	(0.10)
Fractions within the task group										
No vocational training	8.92		23.01		13.59		13.10		2.55	
Vocational training	78.38		74.64		85.03		83.35		63.60	
University or university of applied science	11.96		0.69		0.76		2.55		33.31	
Missing	0.74		1.67		0.62		1.01		0.55	
Mining industry	0.56		0.46		6.89		0.68		0.64	
Manufacturing industry	38.70		77.24		77.15		30.97		35.04	
Energy and water supply industry	1.77		0.25		2.19		1.66		1.52	

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Table 2.A.2 – continued from previous page

	Routine - Δ NRC high	**Routine - Δ NRC middle**	**Routine - Δ NRC low**	**Nonroutine Manual**	**Nonroutine Cognitive**
Construction industry	2.32	1.37	1.62	23.02	2.71
Trade and repair industry	17.40	2.75	5.32	13.36	18.26
Catering industry	0.35	6.91	0.22	1.50	0.37
Transport and news industry	5.14	0.66	1.64	11.38	2.99
Finance and insurance industry	2.11	0.15	0.05	0.24	10.89
Real estate and housing, renting of movable property, business service industry	12.01	5.95	2.22	5.35	14.03
Public services industry	13.44	1.38	0.96	4.13	4.36
Education industry	0.82	0.39	0.35	0.54	2.67
Health industry	2.03	1.70	0.88	4.78	2.73
Other services industry	3.34	0.78	0.50	2.40	3.79
Missing	0.01	0.01	0.01	0.01	0.01
Foreign workers	6.06	20.07	10.80	11.21	3.86
Censored wages	15.90	1.61	3.04	3.16	37.40

Notes: Standard deviation in parentheses. Tasks groups are defined by using the dynamic group definition described in Section 2.2.

Table 2.A.3.: RTI and NRCTI of Occupation Fields in 1985 and 2006

Occupational Field	**Classified as**	**RTI in 1985**	**RTI in 2006**	**NRCTI in 1985**	**NRCTI in 2006**
Occupations in spinning and rope-making	R - Δ NRC high	0.63	0.57	0.11	0.29
Textile processing, leather manufacture	R - Δ NRC high	0.49	0.32	0.15	0.38
Goods examiners, Packagers, despatchers	R - Δ NRC high	0.47	0.43	0.13	0.26
Occupations in finance and accounting	R - Δ NRC high	0.68	0.14	0.32	0.76
Commercial office occupations	R - Δ NRC high	0.45	0.14	0.48	0.74
Auxiliary office occupations, telephone operators	R - Δ NRC high	0.53	0.19	0.15	0.64
Occupations in production and processing of glass- and ceramic	R - Δ NRC middle	0.70	0.59	0.06	0.13
Paper manufacture, paper processing, printing	R - Δ NRC middle	0.69	0.55	0.14	0.22
Metal productions and processing	R - Δ NRC middle	0.65	0.63	0.07	0.15
Bakers, pastry cooks, production of confectionary goods	R - Δ NRC middle	0.82	0.51	0.08	0.19
Cooks	R - Δ NRC middle	0.51	0.35	0.24	0.34
unskilled workers	R - Δ NRC middle	0.54	0.55	0.03	0.16
Miners and mineral extraction workers	R - Δ NRC low	0.56	0.46	0.15	0.07
Occupations in plastic and chemistry -making and –processing	R - Δ NRC low	0.64	0.58	0.13	0.14
Occupations in mechanics and tool making	R - Δ NRC low	0.57	0.51	0.11	0.18
Precision engineering and related occupations	R - Δ NRC low	0.46	0.55	0.25	0.27
Butchers	R - Δ NRC low	0.71	0.51	0.14	0.19
Production of beverages, foods and tobacco, other nutrition occupations	R - Δ NRC low	0.53	0.50	0.25	0.21
Metal, plant, and sheet metal construction, installation, fitters	NRM	0.34	0.45	0.09	0.23

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Table 2.A.3 – continued from previous page

Occupational Field	Classified as	RTI in 1985	RTI in 2006	NRCTI in 1985	NRCTI in 2006
Vehicle and aircraft construction, maintenance occupations	NRM	0.24	0.33	0.15	0.29
Occupations in mechatronics, energy electronics and electrical engineering	NRM	0.26	0.39	0.17	0.28
Construction occupations, wood and plastics manufacture and processing occupations	NRM	0.27	0.41	0.08	0.25
Transport occupations	NRM	0.41	0.38	0.09	0.18
Occupations in aircraft and ship operation	NRM	0.40	0.45	0.25	0.18
Packers, warehouse operatives, transport processors	NRM	0.42	0.34	0.13	0.24
Personal protection, guards	NRM	0.15	0.21	0.17	0.31
Building caretakers	NRM	0.29	0.26	0.06	0.22
Medical and health care occupations with medical licence	NRM	0.11	0.19	0.42	0.54
Medical and health care occupations without medical medical licence	NRM	0.15	0.17	0.28	0.43
Body care occupations	NRM	0.07	0.06	0.33	0.47
Hotel and restaurant occupations, housekeeping	NRM	0.15	0.20	0.32	0.48
Cleaning and disposal occupations	NRM	0.22	0.24	0.03	0.27
Engineers	NRC	0.21	0.18	0.70	0.74
Chemists, physicists, scientists	NRC	0.17	0.21	0.79	0.73
Technicians	NRC	0.32	0.29	0.48	0.54
Technical draughtsmen/draughtswomen, related occupations	NRC	0.32	0.10	0.66	0.90
Surveying and mapping	NRC	0.26	0.45	0.59	0.50
Specialist skilled technicians	NRC	0.40	0.41	0.49	0.45
Sales occupations (retail)	NRC	0.31	0.16	0.48	0.66
Occupations in wholesale and retail	NRC	0.36	0.13	0.57	0.77

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Table 2.A.3 – continued from previous page

Occupational Field	**Classified as**	**RTI in 1985**	**RTI in 2006**	**NRCTI in 1985**	**NRCTI in 2006**
Occupations in insurance and financial services	NRC	0.36	0.14	0.62	0.82
Other commercial occupations (not including wholesale, retail, banking)	NRC	0.34	0.09	0.55	0.80
Advertising specialists	NRC	0.21	0.21	0.61	0.77
Managing directors, auditors, management consultants	NRC	0.25	0.14	0.69	0.77
Administrative occupations in the public sector	NRC	0.27	0.12	0.70	0.82
IT professions	NRC	0.41	0.21	0.53	0.67
Occupations in security	NRC	0.26	0.14	0.43	0.56
Legal occupations	NRC	0.24	0.09	0.58	0.78
Artists and musicians	NRC	0.41	0.25	0.33	0.52
Designers, photographers, advertising creators	NRC	0.24	0.31	0.50	0.56
Social occupations	NRC	0.20	0.09	0.62	0.68
Teachers	NRC	0.21	0.15	0.75	0.75
Journalists, librarians, translators, related academic research occupations	NRC	0.31	0.18	0.59	0.78

Source: BIBB/IAB and BIBB/BAuA Employment Surveys 1985 and 2006.

Table 2.A.4.: Task-group specific wage growth by fixed task group definitions

	Fixed group definition - BIBB data approach		**Fixed group definition - Cortes (2016) approach**	
Routine x 1986	0.004***	(0.001)	-0.004***	(0.001)
Routine x 1987	0.004***	(0.001)	-0.006***	(0.001)
Routine x 1988	0.006***	(0.001)	-0.002	(0.001)
Routine x 1989	0.008***	(0.001)	0.000	(0.001)
Routine x 1990	0.007***	(0.001)	0.002	(0.002)
Routine x 1991	0.001	(0.001)	0.004**	(0.002)
Routine x 1992	-0.003***	(0.001)	0.005***	(0.002)
Routine x 1993	-0.012***	(0.001)	0.007***	(0.002)
Routine x 1994	-0.010***	(0.001)	0.011***	(0.002)
Routine x 1995	0.000	(0.001)	0.013***	(0.002)
Routine x 1996	0.002	(0.001)	0.013***	(0.002)
Routine x 1997	0.011***	(0.002)	0.011***	(0.002)
Routine x 1998	0.021***	(0.002)	0.009***	(0.002)
Routine x 1999	0.027***	(0.002)	0.011***	(0.002)
Routine x 2000	0.035***	(0.002)	0.012***	(0.002)
Routine x 2001	0.044***	(0.002)	0.016***	(0.002)
Routine x 2002	0.050***	(0.002)	0.016***	(0.002)
Routine x 2003	0.050***	(0.002)	0.013***	(0.002)
Routine x 2004	0.059***	(0.002)	0.017***	(0.002)
Routine x 2005	0.068***	(0.002)	0.019***	(0.002)
Routine x 2006	0.078***	(0.002)	0.024***	(0.003)
Routine x 2007	0.090***	(0.002)	0.028***	(0.003)
Routine x 2008	0.095***	(0.002)	0.028***	(0.003)
Routine x 2009	0.091***	(0.002)	0.027***	(0.003)
Routine x 2010	0.103***	(0.002)	0.027***	(0.003)
NRC x 1986	0.004***	(0.001)	-0.002	(0.002)
NRC x 1987	0.015***	(0.002)	0.008***	(0.002)
NRC x 1988	0.013***	(0.002)	0.010***	(0.002)
NRC x 1989	0.019***	(0.002)	0.012***	(0.002)
NRC x 1990	0.019***	(0.002)	0.015***	(0.002)

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Table 2.A.4 – continued from previous page

	Fixed group definition - BIBB data approach		**Fixed group definition - Cortes (2016) approach**	
NRC x 1991	0.024***	(0.002)	0.027***	(0.002)
NRC x 1992	0.029***	(0.002)	0.033***	(0.002)
NRC x 1993	0.034***	(0.002)	0.044***	(0.002)
NRC x 1994	0.033***	(0.002)	0.046***	(0.003)
NRC x 1995	0.043***	(0.002)	0.052***	(0.003)
NRC x 1996	0.053***	(0.002)	0.062***	(0.003)
NRC x 1997	0.071***	(0.002)	0.074***	(0.003)
NRC x 1998	0.084***	(0.002)	0.080***	(0.003)
NRC x 1999	0.115***	(0.002)	0.108***	(0.003)
NRC x 2000	0.140***	(0.002)	0.131***	(0.003)
NRC x 2001	0.174***	(0.002)	0.164***	(0.003)
NRC x 2002	0.182***	(0.002)	0.169***	(0.003)
NRC x 2003	0.164***	(0.002)	0.148***	(0.003)
NRC x 2004	0.190***	(0.002)	0.174***	(0.003)
NRC x 2005	0.215***	(0.002)	0.196***	(0.003)
NRC x 2006	0.229***	(0.003)	0.210***	(0.003)
NRC x 2007	0.254***	(0.003)	0.233***	(0.003)
NRC x 2008	0.273***	(0.003)	0.250***	(0.003)
NRC x 2009	0.288***	(0.003)	0.266***	(0.003)
NRC x 2010	0.303***	(0.003)	0.276***	(0.003)
Region type				
Urban districts	-0.009***	(0.001)	-0.010***	(0.001)
Rural districts, some densely populated areas	-0.033***	(0.002)	-0.036***	(0.002)
Rural districts, sparsely populated	-0.052***	(0.003)	-0.056***	(0.003)
Missing	-0.043***	(0.011)	-0.039***	(0.011)
Foreign	0.010***	(0.002)	0.013***	(0.002)
Missing	0.006	(0.011)	0.006	(0.012)
Year dummies	yes		yes	

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Table 2.A.4 – continued from previous page

	Fixed group definition - BIBB data approach	**Fixed group definition - Cortes (2016) approach**
Federal state dummies	yes	yes
Industry dummies	yes	yes
Occupation-person fixed effects	yes	yes
Observations	5,202,497	5,202,497

Notes: This table illustrates the results of our estimation of the task-group specific wage component for the fixed group definition in table form. Standard errors (in parentheses) are clustered at the worker level. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.A.5.: Task-group specific wage growth by dynamic task group definition

	Dynamic group definition - BIBB data approach with Routine subcategories	
Routine - Δ NRC high x 1986	0.008***	(0.001)
Routine - Δ NRC high x 1987	0.015***	(0.002)
Routine - Δ NRC high x 1988	0.018***	(0.002)
Routine - Δ NRC high x 1989	0.020***	(0.002)
Routine - Δ NRC high x 1990	0.017***	(0.002)
Routine - Δ NRC high x 1991	0.019***	(0.002)
Routine - Δ NRC high x 1992	0.025***	(0.002)
Routine - Δ NRC high x 1993	0.031***	(0.002)
Routine - Δ NRC high x 1994	0.036***	(0.002)
Routine - Δ NRC high x 1995	0.047***	(0.003)
Routine - Δ NRC high x 1996	0.058***	(0.003)
Routine - Δ NRC high x 1997	0.071***	(0.003)
Routine - Δ NRC high x 1998	0.085***	(0.003)
Routine - Δ NRC high x 1999	0.103***	(0.003)
Routine - Δ NRC high x 2000	0.119***	(0.003)
Routine - Δ NRC high x 2001	0.139***	(0.003)
Routine - Δ NRC high x 2002	0.152***	(0.003)
Routine - Δ NRC high x 2003	0.151***	(0.003)
Routine - Δ NRC high x 2004	0.169***	(0.003)
Routine - Δ NRC high x 2005	0.188***	(0.003)
Routine - Δ NRC high x 2006	0.202***	(0.003)
Routine - Δ NRC high x 2007	0.215***	(0.004)
Routine - Δ NRC high x 2008	0.229***	(0.004)
Routine - Δ NRC high x 2009	0.249***	(0.004)
Routine - Δ NRC high x 2010	0.263***	(0.004)

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Table 2.A.5 – continued from previous page

	Dynamic group definition - BIBB data approach with Routine subcategories	
Routine - Δ NRC middle x 1986	0.001	(0.001)
Routine - Δ NRC middle x 1987	-0.004***	(0.001)
Routine - Δ NRC middle x 1988	-0.002	(0.001)
Routine - Δ NRC middle x 1989	0.001	(0.001)
Routine - Δ NRC middle x 1990	0.002	(0.002)
Routine - Δ NRC middle x 1991	-0.011***	(0.002)
Routine - Δ NRC middle x 1992	-0.022***	(0.002)
Routine - Δ NRC middle x 1993	-0.037***	(0.002)
Routine - Δ NRC middle x 1994	-0.036***	(0.002)
Routine - Δ NRC middle x 1995	-0.027***	(0.002)
Routine - Δ NRC middle x 1996	-0.032***	(0.002)
Routine - Δ NRC middle x 1997	-0.025***	(0.002)
Routine - Δ NRC middle x 1998	-0.018***	(0.002)
Routine - Δ NRC middle x 1999	-0.018***	(0.002)
Routine - Δ NRC middle x 2000	-0.013***	(0.002)
Routine - Δ NRC middle x 2001	-0.013***	(0.002)
Routine - Δ NRC middle x 2002	-0.013***	(0.002)
Routine - Δ NRC middle x 2003	-0.015***	(0.002)
Routine - Δ NRC middle x 2004	-0.011***	(0.003)
Routine - Δ NRC middle x 2005	-0.008***	(0.003)
Routine - Δ NRC middle x 2006	-0.002	(0.003)
Routine - Δ NRC middle x 2007	0.008***	(0.003)
Routine - Δ NRC middle x 2008	0.006**	(0.003)
Routine - Δ NRC middle x 2009	-0.017***	(0.003)
Routine - Δ NRC middle x 2010	-0.006**	(0.003)
Routine - Δ NRC low x 1986	0.003**	(0.001)

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Table 2.A.5 – continued from previous page

	Dynamic group definition - BIBB data approach with Routine subcategories	
Routine - Δ NRC low x 1987	0.003**	(0.001)
Routine - Δ NRC low x 1988	0.002	(0.001)
Routine - Δ NRC low x 1989	0.002	(0.001)
Routine - Δ NRC low x 1990	-0.001	(0.002)
Routine - Δ NRC low x 1991	-0.009***	(0.002)
Routine - Δ NRC low x 1992	-0.017***	(0.002)
Routine - Δ NRC low x 1993	-0.032***	(0.002)
Routine - Δ NRC low x 1994	-0.033***	(0.002)
Routine - Δ NRC low x 1995	-0.023***	(0.002)
Routine - Δ NRC low x 1996	-0.021***	(0.002)
Routine - Δ NRC low x 1997	-0.012***	(0.002)
Routine - Δ NRC low x 1998	-0.004**	(0.002)
Routine - Δ NRC low x 1999	-0.004*	(0.002)
Routine - Δ NRC low x 2000	0.000	(0.002)
Routine - Δ NRC low x 2001	0.004*	(0.002)
Routine - Δ NRC low x 2002	0.008***	(0.002)
Routine - Δ NRC low x 2003	0.011***	(0.002)
Routine - Δ NRC low x 2004	0.018***	(0.002)
Routine - Δ NRC low x 2005	0.025***	(0.002)
Routine - Δ NRC low x 2006	0.035***	(0.003)
Routine - Δ NRC low x 2007	0.048***	(0.003)
Routine - Δ NRC low x 2008	0.051***	(0.003)
Routine - Δ NRC low x 2009	0.031***	(0.003)
Routine - Δ NRC low x 2010	0.046***	(0.003)
NRC x 1986	0.004***	(0.001)
NRC x 1987	0.015***	(0.002)

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Table 2.A.5 – continued from previous page

	Dynamic group definition - BIBB data approach with Routine subcategories	
NRC x 1988	0.013***	(0.002)
NRC x 1989	0.019***	(0.002)
NRC x 1990	0.019***	(0.002)
NRC x 1991	0.024***	(0.002)
NRC x 1992	0.029***	(0.002)
NRC x 1993	0.034***	(0.002)
NRC x 1994	0.033***	(0.002)
NRC x 1995	0.043***	(0.002)
NRC x 1996	0.053***	(0.002)
NRC x 1997	0.071***	(0.002)
NRC x 1998	0.084***	(0.002)
NRC x 1999	0.115***	(0.002)
NRC x 2000	0.140***	(0.002)
NRC x 2001	0.174***	(0.002)
NRC x 2002	0.182***	(0.002)
NRC x 2003	0.163***	(0.002)
NRC x 2004	0.190***	(0.002)
NRC x 2005	0.215***	(0.002)
NRC x 2006	0.229***	(0.003)
NRC x 2007	0.254***	(0.003)
NRC x 2008	0.272***	(0.003)
NRC x 2009	0.287***	(0.003)
NRC x 2010	0.302***	(0.003)
Region type		
Urban districts	-0.009***	(0.001)
Rural districts, some densely populated areas	-0.033***	(0.002)

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Table 2.A.5 – continued from previous page

	Dynamic group definition - BIBB data approach with Routine subcategories	
Rural districts, sparsely populated	-0.051***	(0.003)
Missing	-0.041***	(0.011)
Foreign	0.006***	(0.002)
Missing	0.005	(0.012)
Year dummies	yes	
Federal state dummies	yes	
Industry dummies	yes	
Occupation-person fixed effects	yes	
Observations	5,202,497	

Notes: This table illustrates the results of our estimation of the task-group specific wage component for the dynamic group definition in table form. Standard errors (in parentheses) are clustered at the worker level. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 2.A.6.: Averages on Task Group Leavers and Task Group Entrants by Time Period

	Routine - Δ NRC high		**Routine - Δ NRC middle**		**Routine - Δ NRC low**		**Nonroutine Manual**		**Nonroutine Cognitive**	
	1986-1990	2005-2009	1986-1990	2005-2009	1986-1990	2005-2009	1986-1990	2005-2009	1986-1990	2005-2009
Panel A: Task Group Leaver										
Age	38.99	40.70	35.83	36.97	36.07	40.83	36.88	40.07	41.08	43.33
	(14.25)	(12.34)	(13.74)	(12.47)	(13.80)	(13.24)	(13.96)	(12.96)	(13.91)	(12.66)
No vocational training	15.71	9.28	27.73	25.81	17.90	12.08	17.83	14.34	4.59	4.95
	(36.39)	(29.01)	(44.77)	(43.76)	(38.34)	(32.59)	(38.28)	(35.05)	(20.92)	(21.70)
Vocational training	75.54	70.82	70.13	70.13	80.75	85.35	78.64	80.63	73.61	60.92
	(42.99)	(45.46)	(45.77)	(45.77)	(39.43)	(35.36)	(40.99)	(39.52)	(44.08)	(48.79)
University degree	7.67	18.58	0.54	1.67	0.66	1.70	2.19	3.64	21.05	32.56
	(26.61)	(38.90)	(7.34)	(12.81)	(8.11)	(12.92)	(14.63)	(18.72)	(40.76)	(46.86)
Log daily wage	4.58	4.67	4.45	4.20	4.52	4.54	4.45	4.38	4.77	4.86
	(0.42)	(0.56)	(0.32)	(0.42)	(0.29)	(0.36)	(0.32)	(0.40)	(0.52)	(0.60)
Job tenure (in years)	5.47	6.25	4.75	4.43	5.24	7.91	4.37	5.79	5.42	6.83

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Table 2.A.6 – continued from previous page

	Routine - Δ NRC high		Routine - Δ NRC middle		Routine - Δ NRC low		Nonroutine Manual		Nonroutine Cognitive	
	(5.17)	(8.33)	(5.03)	(7.38)	(5.11)	(9.23)	(4.86)	(7.96)	(5.19)	(8.68)
No. of observations	13,431	11,931	15,690	15,210	15,696	8,593	53,821	40,742	23,130	24,791
Panel B: Task Group Entrant										
Age	32.49	35.80	30.27	33.96	29.45	34.15	30.72	35.33	32.82	36.04
	(11.03)	(10.32)	(10.61)	(10.91)	(10.32)	(11.52)	(11.03)	(11.43)	(10.13)	(10.09)
No vocational training	12.66	8.65	23.36	23.51	15.93	10.39	16.70	13.67	4.06	5.50
	(33.25)	(28.11)	(42.31)	(42.41)	(36.60)	(30.52)	(37.30)	(34.35)	(19.74)	(22.80)
Vocational training	78.01	67.35	75.15	72.71	82.97	86.34	80.47	80.95	73.40	60.86
	(41.42)	(46.89)	(43.21)	(44.55)	(37.59)	(34.35)	(39.64)	(39.27)	(44.19)	(48.81)
University degree	8.75	22.93	0.59	1.92	0.72	2.46	2.08	4.18	22.04	32.17
	(28.25)	(42.04)	(7.63)	(13.71)	(8.48)	(15.48)	(14.28)	(20.01)	(41.45)	(46.71)
Log (daily) wage	4.48	4.58	4.38	4.12	4.44	4.47	4.36	4.28	4.63	4.68
	(0.42)	(0.54)	(0.31)	(0.39)	(0.28)	(0.35)	(0.31)	(0.36)	(0.45)	(0.51)

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Table 2.A.6 – continued from previous page

	Routine - Δ NRC high		**Routine - Δ NRC middle**		**Routine - Δ NRC low**		**Nonroutine Manual**		**Nonroutine Cognitive**	
No. of observations	11,354	10,559	13,494	13,048	13,546	6,843	46,603	34,708	22,536	21,636

Notes: Task group leavers are workers who change the task group from one year to another, switch to non-employment or leave the sample. Task group entrants are workers who came from another task group either from employment or non-employment, entered the labor market, or entered full-time employment. Standard deviation in parentheses. Tasks groups are defined by using the dynamic group definition described in Section 2.2.

Table 2.A.7.: Decomposition of the Change in NRC Task Content

	R - Δ NRC high vs. R - Δ NRC middle	R - Δ NRC high vs. R - Δ NRC low
Total Change	0.133***	0.205***
	(0.025)	(0.020)
Main Effect	-0.002	0.014*
	(0.005)	(0.006)
Group Interaction	-0.002	-0.006
	(0.010)	(0.008)
Time Interaction	-0.013	-0.007
	(0.018)	(0.014)
Group-Time Interaction	0.150***	0.205***
	(0.036)	(0.025)
Observations	17,994	17,994

Notes: This table shows the decomposition of the mean NRC task intensity between for the task groups R - Δ NRC high vs. R - Δ NRC middle and R - Δ NRC high vs. R - Δ NRC low using the BIBB waves 1985 and 2006. We use the methodology of Smith and Welch (1989) and the Stata code provided by Kröger and Hartmann (2021). We estimate standard errors via bootstrapping with 100 iterations. Significance: $*p < 0.05$, $**p < 0.01$, $***p < 0.001$. Source: BIBB.

Table 2.A.8.: Sample descriptives, task classification according to task intensity (BIBB data) for task subgroups. Only 1985 – 1989

	Routine - Δ NRC high		**Routine - Δ NRC middle**		**Routine - Δ NRC low**		**Nonroutine Manual**		**Nonroutine Cognitive**	
No. of observations	104,928		106,623		120,438		427,922		273,238	
Share	10.16		10.32		11.66		41.42		26.45	
No. of individuals	30,270		31,821		34,399		119,305		71,713	
Averages										
Log (daily) wage	4.65	(0.29)	4.56	(0.25)	4.61	(0.23)	4.54	(0.25)	4.84	(0.27)
Log (daily) imputed wage	4.70	(0.38)	4.56	(0.26)	4.62	(0.25)	4.55	(0.28)	5.00	(0.47)
Age	40.52	(11.49)	38.46	(11.82)	38.01	(11.69)	38.87	(11.74)	41.40	(10.44)
Job tenure (in years)	7.26	(4.68)	6.74	(4.74)	6.97	(4.69)	6.16	(4.69)	6.97	(4.73)
Labour market experience (in years)	9.66	(3.92)	9.15	(4.12)	9.29	(4.07)	9.12	(4.05)	9.76	(3.88)
Task measures										
RTI	0.49	(0.07)	0.65	(0.07)	0.58	(0.06)	0.31	(0.08)	0.30	(0.06)
NRM	0.15	(0.15)	0.26	(0.08)	0.28	(0.05)	0.58	(0.08)	0.12	(0.07)
NRC	0.36	(0.16)	0.09	(0.05)	0.13	(0.04)	0.12	(0.06)	0.58	(0.10)

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Table 2.A.8 – continued from previous page

	Routine - Δ NRC high	**Routine - Δ NRC middle**	**Routine - Δ NRC low**	**Nonroutine Manual**	**Nonroutine Cognitive**
Fractions within the task group					
No vocational training	12.53	27.80	17.19	16.29	2.89
Vocational training	80.08	69.87	81.53	80.60	70.22
University or university of applied science	6.33	0.40	0.45	1.86	26.33
Missing	1.06	1.93	0.83	1.25	0.56
Mining industry	0.88	0.51	10.72	0.92	1.10
Manufacturing industry	45.68	84.45	73.53	32.35	38.73
Energy and water supply industry	1.85	0.29	2.38	1.85	1.72
Construction industry	2.11	1.20	1.60	25.43	2.84
Trade and repair industry	16.01	2.39	5.02	12.40	18.55
Catering industry	0.32	4.94	0.23	1.31	0.33
Transport and news industry	3.44	0.82	2.07	10.58	2.81
Finance and insurance industry	2.05	0.21	0.07	0.37	11.20

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Table 2.A.8 – continued from previous page

	Routine - Δ NRC high	**Routine - Δ NRC middle**	**Routine - Δ NRC low**	**Nonroutine Manual**	**Nonroutine Cognitive**
Real estate and housing, renting of movable property, business service industry	7.05	1.78	1.57	3.64	9.27
Public services industry	15.50	1.40	1.21	4.94	5.33
Education industry	0.69	0.21	0.30	0.49	2.38
Health industry	1.75	1.23	0.89	3.63	1.91
Other services industry	2.67	0.57	0.41	2.09	3.83
Missing	0.00	0.00	0.00	0.00	0.00
Foreign workers	5.62	19.27	10.52	9.98	3.04
Censored wages	15.04	2.07	3.41	3.29	40.47

Notes: Standard deviation in parentheses. Tasks groups are defined by using the dynamic group definition described in Section 2.2.

Table 2.A.9.: Mean Task Intensities over Time and by Age Groups

	RTI		**NRMTI**		**NRCTI**	
	young	old	young	old	young	old
1985	0.37	0.37	0.32	0.32	0.31	0.31
1992	0.38	0.34	0.30	0.26	0.32	0.40
1999	0.35	0.33	0.28	0.25	0.37	0.42
2006	0.32	0.30	0.23	0.23	0.45	0.47

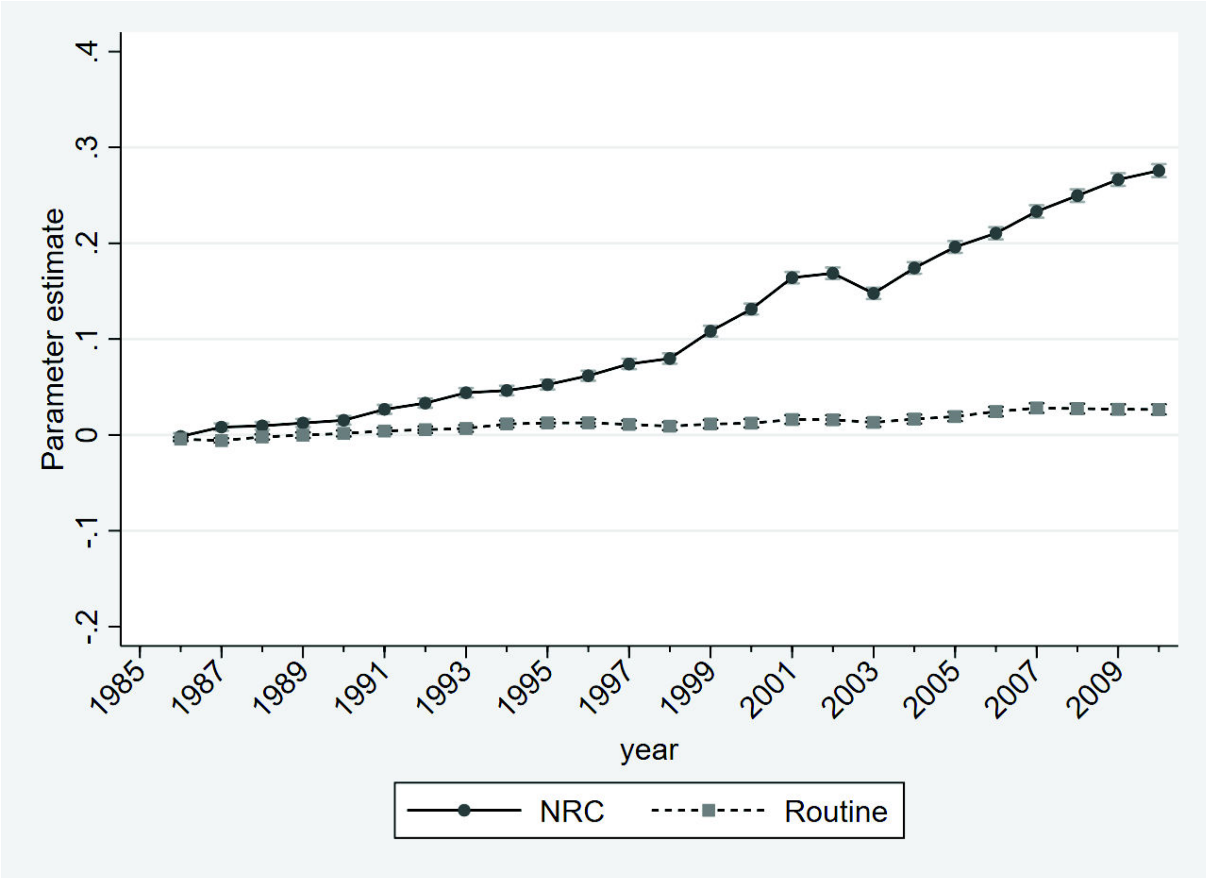
Notes: This table shows the mean routine task intensity (RTI), mean nonroutine manual task intensity (NRMTI) and mean nonroutine cognitive task intensity (NRCTI) for young (age 25-34 years) vs. older (age 35-50 years) workers.

Table 2.A.10.: Linear Probability Model of Training Participation Financed by Employer

	Financed by Employer	
R – Δ NRC high	0.015	(0.017)
R – Δ NRC middle	-0.051***	(0.009)
R – Δ NRC low	-0.022**	(0.010)
NRC	0.140***	(0.012)
Controls	yes	
No. of observations	12,429	

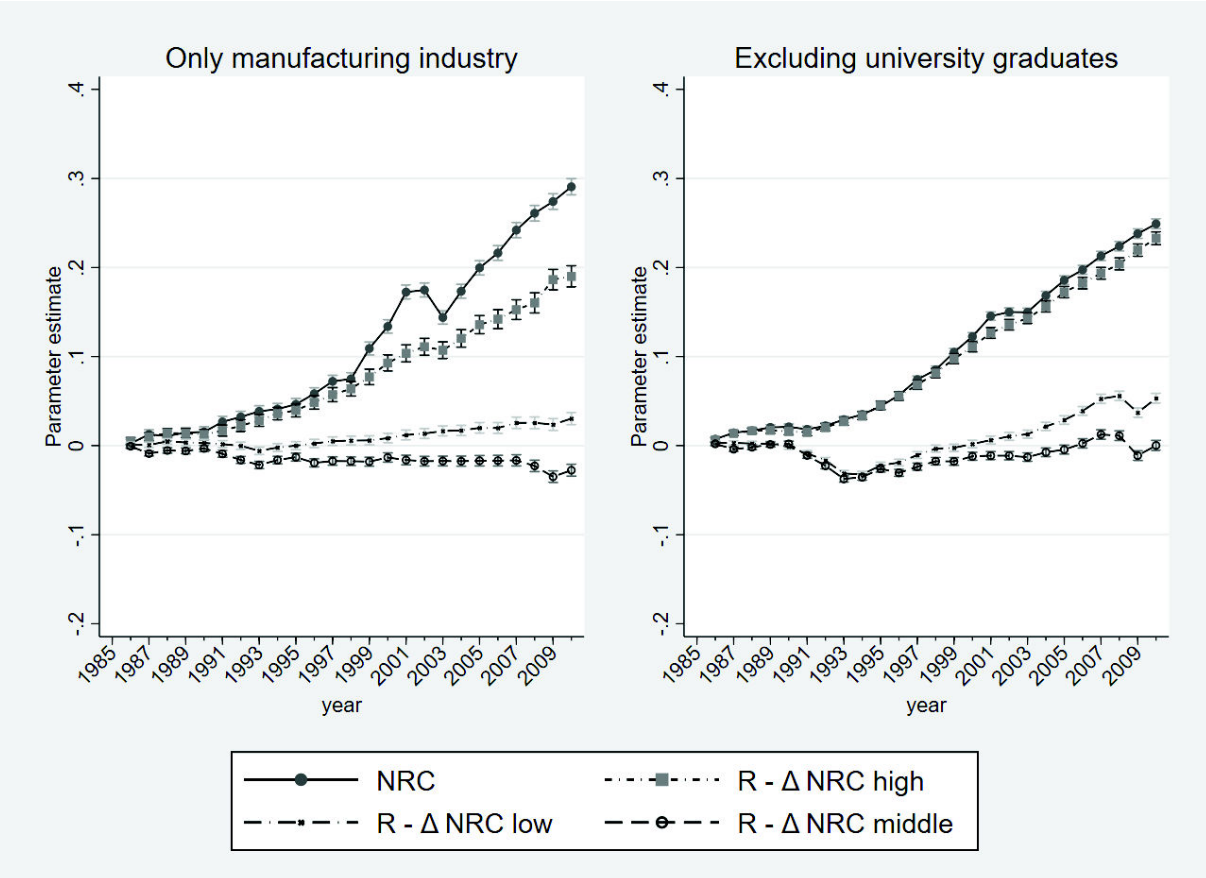
Notes: This table illustrates the results of a linear probability model using the training participation financed by the employer as the outcome variable and task group dummies as the key independent variables. NRM is the reference category. We control for age, education, marital status, migration background, federal state, industry, firm size, and year dummies. Heteroscedasticity-robust standard errors are reported in brackets. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Source: SOEP.

Figure 2.A.1.: Task-group Specific Wages Over Time (Cortes approach)



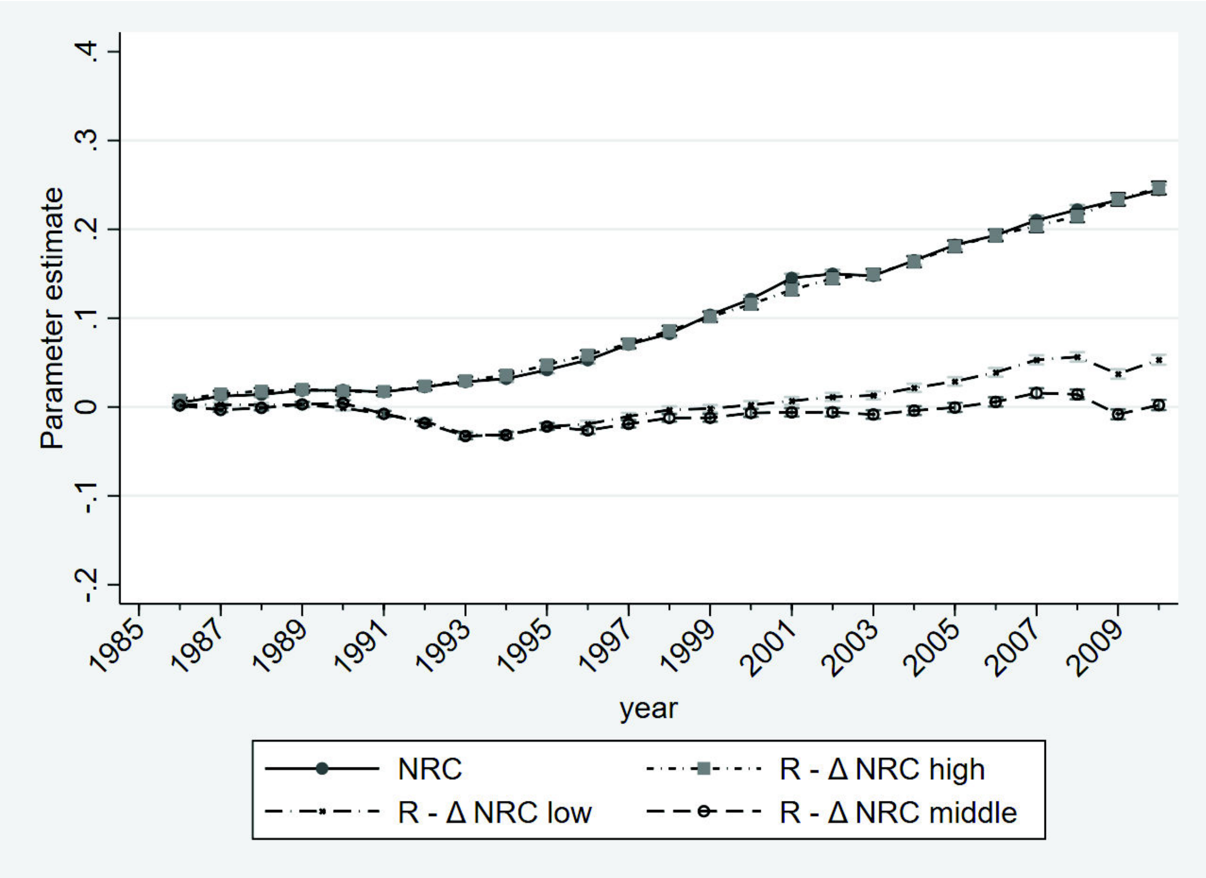
Notes: Evolution of occupation-specific wage growth over time using the task classification of Cortes (2016). NRC: non-routine cognitive occupations. Reference category: NRM = non-routine manual occupations.

Figure 2.A.2.: Robustness Checks: Task-group specific wages over time



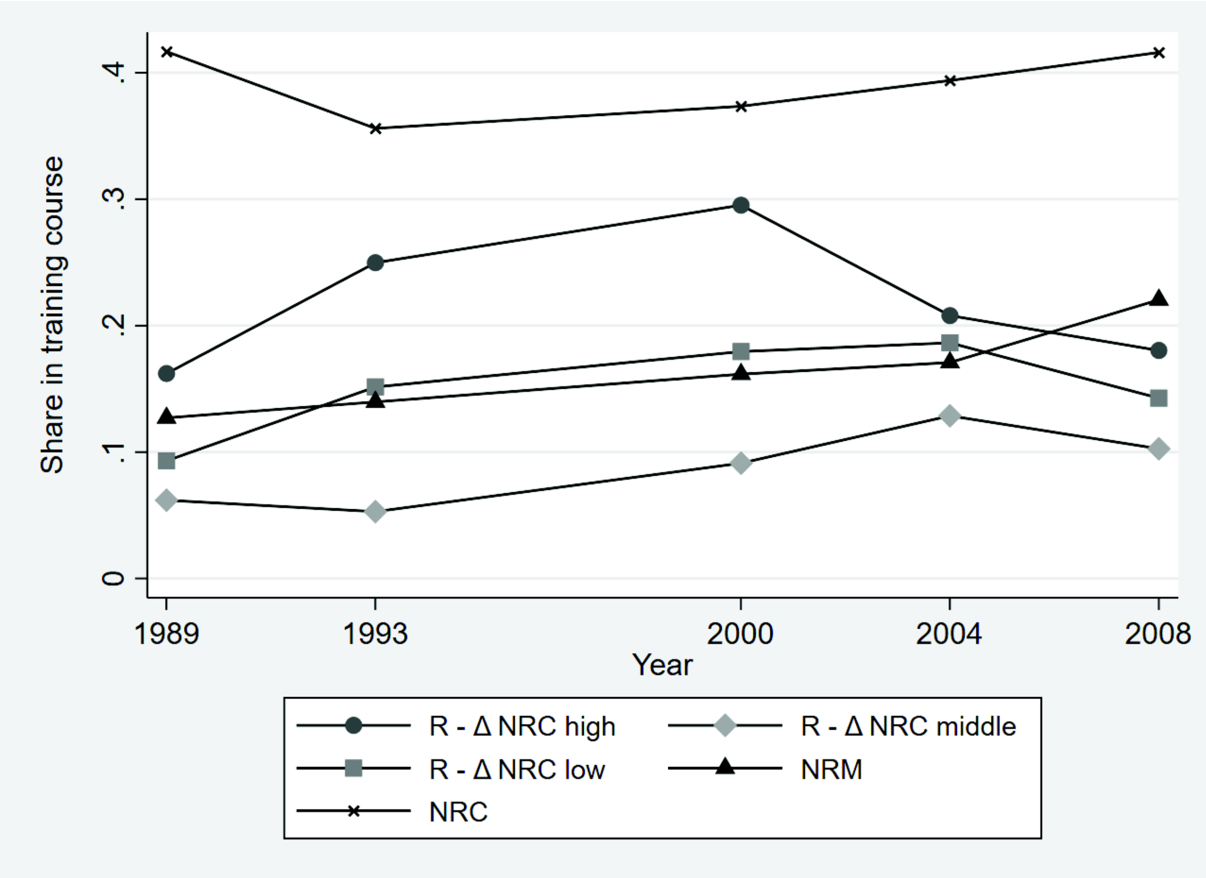
Notes: This figure shows the task-group specific wage component over time for our dynamic group definition separately only for the manufacturing industry and excluding university graduates. Reference category= NRM.

Figure 2.A.3.: Robustness Checks: Occupation Wage Growth by Task Groups using Education x Year Fixed Effects



Notes: These figures show the occupation-specific wage component over time for our dynamic group definition including education x year fixed effects. Reference category= NRM.

Figure 2.A.4.: Shares in Any Training Course



Notes: This figure illustrates the shares of workers in any training course by task group and year. Source: SOEP

Declaration of Contribution

Hereby I, Gökay Demir, declare that the Chapter "The Role of Within-Occupation Task Changes in Wage Development" is co-authored by Ronald Bachmann, Colin Green, and Arne Uhlendorff. All authors contributed equally to the chapter.

Signatures of the co-authors:

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3. Labor Market Frictions and Spillover Effects from Publicly Announced Sectoral Minimum Wages*

Abstract: This paper analyzes the spillover effects of the first sectoral minimum wage in Germany. Using a triple differences estimation, the study examines the impact of public discussion and announcement of the minimum wage on workers and industries outside the minimum wage sector. The results show that the public discussion and announcement led to an increase in wages, job-to-job transitions and reallocation from low-paying to high-paying establishments among sub-minimum wage workers in similar jobs outside the minimum wage sector. The main mechanism for these effects appears to be the reduction of information frictions, rather than strategic interaction of employers.

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3.1. Introduction

One of the most striking findings in labor economics is the coexistence of good (high-wage) and bad (low-wage) jobs. Firms differ in the wages they pay to equally skilled workers in similar jobs (Abowd et al., 1999; Card et al., 2013; Slichter, 1950). Although the continuing existence of bad jobs can be generally explained by labor market frictions, it is an open question what kind of labor market frictions are at work here. One way to reveal the presence and types of labor market frictions that are important for the existence of bad jobs is using wage and information shocks on the potential outside options of workers. Publicly announced sectoral minimum wages are such wage and information shocks to workers with wages below the minimum wage in similar jobs outside the targeted sector and may therefore have spillover effects on these workers. If publicly announced sectoral minimum wages result in wage increases in other sectors merely because of the strategic responses of firms in these other sectors to the minimum wage, legislating additional sectoral minimum wages might be a good policy tool to raise wages of bad jobs. However, if the main mechanism for spillovers is public disclosure of the sectoral minimum wage and sharing of relevant wage information for workers outside the minimum wage sector, unsolicited and widely publicized wage information on specific sectors might be a better approach.

Although examining spillover effects from sectoral minimum wages on firms and workers outside the targeted sector would contribute significantly to our understanding of the existence, types, and consequences of labor market frictions, the empirical evidence on spillover effects and its mechanisms is scarce (Bassier, 2021; Derenoncourt et al., 2021; Staiger et al., 2010).[1] Three challenges have prevented researchers from studying spillover effects. First, there was little theoretical and empirical interest on labor market frictions until recently (Card, 2022; Manning, 2021; Sokolova and Sorensen, 2021).[2] Second, large linked employer-employee data were not available, which would be necessary to uncover relevant mechanisms of spillover effects. Third, identification challenges have impeded researchers from examining spillover effects. It is difficult to find exogenous wage increases targeted at specific firms or sectors, identify groups of economic actors who are subject to their spillover effects, and then find a proper control group for them.

In this paper, I study the spillover effects of publicly discussed and announced sectoral min-

[1]A broader literature examines vertical spillover effects of minimum wages on the wage distribution within a targeted sector, state, or country (Autor et al., 2016; Cengiz et al., 2019; Fortin et al., 2021; Gopalan et al., 2021; Gramlich et al., 1976; Gregory and Zierahn, 2022; Grossman, 1983; Lee, 1999; Neumark et al., 2004).

[2]Robinson (1933) was the first to study monopsony power in the labor market. However, her ideas did not catch on for reasons laid out in Card (2022).

imum wages in Germany on workers outside the targeted sector in similar jobs. Because of its relatively large size and the fact that it was the first sectoral minimum wage in Germany, I focus on the spillover effects from the main construction sector minimum wage. This minimum wage's negotiations were extensively reported in the media throughout 1996 and its final wage level was publicly announced in 1996 and introduced in 1997. This minimum wage was introduced to curb wage competition from the posting practices of foreign firms within the European Union and was set below the entry-level wages of firms covered by collective bargaining agreements. Moreover, it had little or no effect on employment within the main construction sector (Frings, 2013; König and Möller, 2009; Möller, 2012; Möller et al., 2011; Vom Berge and Frings, 2020). Spillovers resulting from reallocation from the main construction sector to other sectors were consequently minimal, making spillovers resulting from strategic responses or information transmissions more observable. I am able to address earlier challenges in the literature by utilizing high-quality administrative linked employer-employee data, a triple differences design, and the most recent theoretical developments on frictional labor markets.[3]

The triple differences design exploits three dimensions of comparison. First, I compare sub-minimum wage workers to workers with higher wages outside of the main construction sector. Second, spillover effects are particularly relevant in industries[4] with sub-minimum wage employees for whom the minimum wage sector represents an outside option. I classify these "outside option industries" as industries which had high outflows of low-wage workers to the minimum wage sector. I compare outside option industries to industries which had low outflows of low-wage workers to the minimum wage sector, referred to as "non-outside option industries". I assume that the minimum wage sector and outside option industries share one common labor market with similar tasks and transferable skills. Non-outside option industries are outside this common labor market and can therefore be used as a proxy for the counterfactual scenario, i.e., the absence of the minimum wage introduction. Third, I compare the labor market outcomes of sub-minimum wage workers to workers with higher wages in outside option versus non-outside option industries before and after the public discussion and announcement of the minimum wage.[5]

I find that the main construction sector minimum wage led to an average increase in wage

[3] To understand the broader economic contexts of spillover effects, I use a similar triple differences strategy to examine the wage spillover effects from other sectoral minimum wages in Germany in Appendix 3.E.

[4] "Industries" refer to individual 3-digit entries in the German Classification of Economic Activities, while "sectors" refer to multiple (3-digit or 5-digit) industries that are collectively covered by a minimum wage regulation.

[5] Appendix Figure 3.A.1 illustrates the intuition for the identification strategy.

growth of 2.1% and an average increase in job-to-job transitions of 3.7 percentage points for sub-minimum wage workers in outside option industries. The wage spillover effects are about one-third of the wage effects within the main construction sector, which I estimate using the same data and identification strategy. The results are robust to controlling for region- and industry-specific shocks, international trade, and are not driven by an increase in establishment closure. In addition, the results are robust to different definitions of the key independent variables which indicate the exposure to the main construction sector minimum wage. For example, by using occupation flows instead of industry flows to define outside and non-outside options, I account for the possibility that occupations, not industries, form one labor market. I additionally analyze the spillover effects at the establishment level.[6] Using a similar triple differences specification, I find that more exposed establishments on average increased mean wages and lost workers. The effects for establishments appear much later than the effects for workers, which suggests that worker behavior, not establishment behavior, is driving the spillover effects.

One prominent channel to explain these spillover effects are models of strategic spatial complementarity (Bhaskar et al., 2002; Bhaskar and To, 1999; Staiger et al., 2010). I use a simple version of these theoretical models in which firms respond to wage changes from other firms to retain workers, with the intensity of firms' reactions depending on their geographic proximity to other firms. By definition, outside option industries are already "close" to the main construction sector in terms of task similarity and transferability of skills. Therefore, I assume that only geographic proximity is relevant for my empirical tests of this model to be conclusive on strategic complementarity. If strategic complementarity were at play, firms that are closer together would be more responsive to one another's wage changes and the wage spillovers should be driven by remaining within the same establishment or moving to the main construction sector. However, I find that the intensity of spillover effects did not increase with geographic proximity to the main construction sector and that wage spillover effects were mainly driven by switching establishments but not moving to the main construction sector. To test whether the reduction of information frictions can explain the results, I use the simple equilibrium model in Jäger et al. (2022). In this model, workers can have information costs resulting in biased beliefs about their outside options in the labor market, no incentive to search for jobs, and receiving a marked-down wage while staying in low-paying firms. Consistent with an information shock story, I find an

[6] Because I only observe establishments and not firms in the data, I refer to establishments when discussing the empirical analysis and firms when discussing theoretical and institutional considerations.

increase in wage spillovers and job-to-job transitions right at the year of public discussion and announcement of the minimum wage in 1996, and before its introduction in 1997, reallocation from low-paying to high-paying establishments, and a larger wage response for workers with arguably higher information costs about their outside options in the labor market.

My findings imply that information frictions play a significant role for the coexistence of good and bad jobs. The public discussion and announcement of sectoral minimum wages can result in unanticipated benefits from the dissemination of relevant pay information for workers doing similar jobs. Therefore, providing unsolicited and publicly published wage information would be the optimal course of action to break the coexistence of good and bad jobs, reallocate workers from less productive to more productive establishments, and thereby raise the welfare of the economy as a whole. Nevertheless, the conclusion that information frictions are the main driver of the results in this paper remains hypothetical, as I cannot provide direct tests but only indirect tests of this mechanism.

This paper contributes to an emerging literature on cross-employer spillover effects of wage-setting changes at major employers in three ways (Bassier, 2021; Derenoncourt et al., 2021; Staiger et al., 2010).[7] First, I am able to analyze the supply side spillover response to sectoral minimum wages using social security administrative data, which reveals reallocation effects that were previously obscured in firm-level studies. Second, this paper proposes a new research design to study individual-level spillover effects using a triple-differences strategy. Third, the paper uses different theoretical models to test for the mechanisms of the spillover effects.

My paper is related to three other strands of the literature. First, a growing literature studies the role of workers' outside options and their impact on wages (Beaudry et al., 2012; Caldwell and Danieli, 2018; Caldwell and Harmon, 2019; Schubert et al., 2021). Methodologically, I use this literature to define industries for which the minimum wage sectors are potential outside options. Empirically, I add to this literature by showing that after minimum wages were publicly discussed and announced in their potential outside options, employees moved to better paying establishments and experienced positive wage spillovers. Second, this paper relates to the lit-

[7] Other related papers include second-order wage spillover effects of decentralized wage bargaining for teachers (Willén, 2021), wage spillovers across establishments within the same firm (Hjort et al., 2020), and market-level effects of privatization of state-owned enterprises (Arnold, 2022). Furthermore, an older literature analyzes the spillover effects of unionization on non-union wages in the same industry due to a "threat effect" or a labor supply shock from workers of the unionized firms reallocating to the non-unionized firms (Farber et al., 2021; Fortin et al., 2021; Freeman and Medoff, 1981; Lewis, 1963; Moore et al., 1985; Neumark and Wachter, 1995; Podgursky, 1986).

erature on the role of labor market institutions in disrupting the coexistence of good and bad jobs (Acemoglu, 2001; Dustmann et al., 2022). I show that labor market institutions can also have a signaling effect that goes far beyond the actual target group. Third, this paper relates to the literature on pay transparency (Baker et al., 2019; Brütt and Yuan, 2022; Card et al., 2012; Cullen and Perez-Truglia, 2022; Mas, 2017; Perez-Truglia, 2020; Roussille, 2022), information frictions in the labor market (Belot et al., 2019; Carranza et al., 2022; Conlon et al., 2018; Jäger et al., 2022; Skandalis, 2018; Spinnewijn, 2015), and fairness concerns at the workplace (Breza et al., 2018; Dube et al., 2019). I add to this literature by showing that wage transparency can be particularly effective in reducing information frictions when it is unsolicited and published prominently in the media.

This paper is structured as follows. Section 3.2 provides an overview of the institutional setting for sectoral minimum wages in Germany. Section 3.3 presents the linked employer-employee data and the sampling procedure. In Section 3.4, I detail the empirical strategy to estimate spillover effects. Section 3.5 presents the main results, robustness checks, and mechanisms. Section 3.6 discusses the findings and concludes.

3.2. Institutional Background

Due to European trade integration, sectors in Germany that had been largely spared from international trade up to the beginning of the 1990's were then facing fierce wage competition. European firms could send workers to another EU member state on the terms and conditions of its country of domicile, while domestic firms had to continue to comply with internal regulations (Bosch and Zühlke-Robinet, 2000; Muñoz, 2022). The main construction sector in particular was affected by foreign wage competition. Although there were of course beneficiaries from cheaper construction products in Germany, an opposition to the European posting practice formed relatively quickly with the demand to limit the market opening in order to prevent low-wage competition within the main construction sector.

The main construction sector already had a relatively high collective bargaining coverage in 1995 of approximately 80% in West Germany and 40% in East Germany (Möller et al., 2011). To curb wage competition within the main construction sector and set a minimum wage in this sector, collective bargaining agreements could be declared generally binding under Section 5 of the Collective Bargaining Agreement Act[8]. Sectoral minimum wages can be extended to foreign firms through the Posting of Workers Law which came into force in March 1996.

Since there was no minimum wage in the main construction sector to make the Posting of Workers Law effective, representatives of employers and unions in the construction sector debated an appropriate minimum wage rate in 1996.[9] This issue received significant media attention[10] and at times became quite contentious, with various values ranging from 6.14 Euro to 10.35 Euro being proposed and repeatedly publicized in the media as potential minimum wage levels. Negotiations between unions and employers on the level of the minimum wage have broken down several times. The unions even threatened strikes and organized large demonstrations to draw attention to the situation.[11] It is noteworthy for the purposes of this paper that wage levels were a topic of discussion and media attention throughout the year 1996. An agreement on the wage level was eventually reached and the minimum wage was announced on November 16, 1996 in the German Federal Bulletin (No. 215, p. 12102), as required by law, and covered by Germany's most watched news program, the *Tagesschau* (Zubayr and Gerhard, 2005), on November 12, 1996.[12] The two sides (trade union and employer association) agreed on a minimum hourly wage of 8.69 Euro in West Germany and 8.00 Euro in East Germany, which came into force at the beginning of 1997. In mid-1997, the minimum wage in the main construction sector was lowered slightly to 8.18 Euro in West and 7.74 Euro in East Germany and raised again to 9.46 Euro in West and 8.32 Euro in East Germany in mid-1999.

Taking stock, two features of the sectoral minimum wage in the main construction sector make it particularly valuable for this paper. First, the main construction sector minimum wage

[8]§5 of the Collective Bargaining Agreement Act (*Tarifvertragsgesetz*) states that on request of the collective bargaining parties a collective agreement can be declared generally binding by the federal ministry of labor and social affairs (BMAS). This law requires an agreement of the majority of a bargaining committee of the federal ministry, which consists of three representatives of the employer association and three representatives of the trade union, to pass the general binding declaration. Furthermore, the general binding declaration has to be of public interest and until 2014, the employers bound by the collective agreement must at least employ 50% of the workers in the scope of the collective agreement.

[9]For example, Klaus Schmidt, who was one of three representatives of the employer association in the bargaining committee of the federal ministry of labor, stated in February of 1996 that he would agree even to a minimum wage of 6.14 Euro possibly with reservations (Glabus, 1996).

[10]For example, the largest national daily newspapers in Germany such as the Frankfurter Allgemeine Zeitung, the Süddeutsche Zeitung and other newspapers reported on this topic throughout the year.

[11]As reported by the Frankfurter Allgemeine Zeitung, a newspaper of record in Germany, the relevant trade union for construction (IG Bauen-Agrar-Umwelt) threatened a strike and organized a demonstration with 2,000 officials in North Rhine-Westphalia to push for a minimum wage of at least 10.01 Euro at that time ("Die IG Bau bereitet sich auf Streik vor", *Frankfurter Allgemeine Zeitung*, March 14, 1996, No. 63, p.15). Furthermore, the Süddeutsche Zeitung, another important newspaper of record in Germany, reported of another big demonstration with up to 20,000 construction workers from Bavaria, Baden-Wuerttemberg, and Hesse in Munich citing the trade unions' core demand of 10.01 Euro as a minimum wage ("Der Krieg am Bau weitet sich aus", *Süddeutsche Zeitung*, March 20, 1996, p.33).

[12]https://www.tagesschau.de/multimedia/video/video-229995.html around minute 4:55.

was introduced because of within-sector concerns, making it an exogenous variation in outside wages for workers and firms not in the minimum wage sector. Second, extensive public attention to the minimum wage (e.g., through news broadcasts and newspapers) is likely to represent an information shock for individuals who were previously not aware of wages in other sectors. I am able to provide suggestive evidence for information shocks as the driving mechanism for spillover effects in the mechanisms Section 3.5.4.

In the years following the introduction of the minimum wage in the main construction sector, other sectoral minimum wages were also introduced.[13] The Temporary Work Law (*Arbeitnehmerüberlassungsgesetz*) is another piece of legislation which, since changes in the law in 2011, allows enacting a minimum wage in the temporary work sector to prevent misuse of temporary work. Table 3.1 gives an overview of all sectoral minimum wages in Germany that were enacted using the Collective Bargaining Agreement Act, Posting of Workers Law, Temporary Work Law, or combinations of these three pieces of legislation, and whose spillover effects I also study in Appendix 3.E of this paper.[14]

3.3. Data

3.3.1. The Sample of Integrated Employer-Employee Data (SIEED) 1975–2018

The Sample of Integrated Employer-Employee Data (SIEED) 1975–2018, together with additional establishment level information from the Establishment History Panel (BHP), provides high quality administrative variables. By using the information on establishments, detailed industry codes, wages, and employment biographies of individuals, this data allows me to convincingly estimate spillover effects of sectoral minimum wages in Germany. The SIEED and BHP are provided by the Research Data Centre of the BA at the IAB. Schmidtlein et al. (2020) provide a detailed description of the SIEED.

The main data source of the SIEED is the Employee History (Beschäftigtenhistorik - BeH). The BeH in turn is based on the integrated notification procedure for health, pension and

[13] See e.g. Popp (2021) for an overview of prerequisites for all sectoral minimum wages in Germany. For the context of this study it is only important that sectoral minimum wages were exogenous from the perspective of workers and firms outside the targeted sectors.

[14] The sectoral minimum wages in industrial laundries (introduced 2009), specialized hard coal mining (introduced 2009), public training services (introduced 2012) and money and value services (introduced 2015) cannot be studied as the 5-digit industry classification that I use in this paper is not granular enough to identify these sectors.

Table 3.1.: Sectoral Minimum Wages in Germany

Sector	First MW	Hourly Wage (in Euro)
Main Construction	01/1997	West (incl. Berlin) 8.69; East 8.00
Electrical Trade	06/1997	West 8.03; East (incl. Berlin) 6.41
Roofing	10/1997	West (incl. Berlin) 8.18; East 7.74
Painting & Varnishing	12/2003	West (incl. Berlin) 7.69; East 7.00
Commercial Cleaning	07/2007	West (incl. Berlin) 7.87; East 6.36
Waste Removal	01/2010	8.02
Nursing Care	08/2010	West (incl. Berlin) 8.50; East 7.50
Security	06/2011	Federal states: ranges from 6.53 to 8.60
Temporary Work	01/2012	West 7.89; East (incl. Berlin) 7.01
Scaffolding	08/2013	10.00
Stonemasonry	10/2013	West (incl. Berlin) 11.00; East 10.13
Hairdressing	11/2013	West 7.5; East (incl. Berlin) 6.5
Chimney Sweeping	04/2014	12.78
Slaughtering & Meat Processing	08/2014	7.75
Textile & Clothing	01/2015	West 8.5; East (incl. Berlin) 7.5
Agriculture, Forestry & Gardening	01/2015	West 7.4; East (incl. Berlin) 7.2

unemployment insurance. This notification procedure started on 1 January 1973 (1 January 1991 in East Germany) and made it mandatory for employers to report information on all of their employees covered by social security to the responsible social security agencies at least once a year. Misreporting is a legal offense. For further details on the notification procedure see Bender et al. (1996); Wermter and Cramer (1988). Because the BeH only covers employees subject to social security, civil servants and self-employed individuals or unemployment spells are not included in it.

The SIEED is constructed in a three-step procedure. A 1.5% random sample of the population of establishments in the BeH is taken in the first step. All individuals who worked at least one day in one of these establishments between 1975 and 2018 are drawn in the second step. The full employment biographies for these individuals are taken from the BeH in the third step. The employment biographies span the years 1975–2018 and cover employment spells in both sampled and non-sampled establishments. Due to the sampling procedure, the SIEED is representative for establishments in Germany but not for persons. The data contains information on the exact (to the day) spell time period, person and establishment identifiers, personal information such as age, gender, nationality, place of residence, education, detailed occupation codes, the daily wage[15] and type of job (e.g., part-time vs. full-time). To this data, I merge additional establishment level information on the place of work and detailed industry codes from the BHP.

3.3.2. Sample Construction

Sectoral minimum wages are hourly wages. A drawback of the SIEED is that it does not record an employees' hours worked, which in turn means that exact hourly wages are unknown. To ensure comparability between daily wage rates as an outcome variable and to calculate hourly wages for the definition of treated workers or establishments, I proceed in two steps. First, I focus on full-time workers who in general have similar working hours. Second, I set the weekly working hours to 40 hours and then use the daily wages and the imputed weekly working hours to calculate the nominal hourly wages. Using the consumer price index of the Federal Statistical Office, I convert gross daily wages into real wages when using wages as an outcome variable in the analysis.

To identify the national minimum wage sectors, I use the 1973 3-digit, 1993 5-digit, 2003 5-digit and 2008 5-digit German Classification of Economic Activities (WZ). The first four digits in the WZ are based on the Statistical Classification of Economic Activities in the European Community (NACE). Appendix Table 3.A.1 summarizes the industry codes that I use to identify and classify the minimum wage industries. If an establishment has one of the industry codes listed in Appendix Table 3.A.1 during the observation period, I classify it as belonging to the respective minimum wage sector. I use the evaluation studies on sectoral minimum wages in Germany, which were commissioned by the Federal Ministry of Labor and Social Affairs, as aids for delimiting the minimum wage sectors in Appendix Table 3.A.1 (Aretz et al., 2011; Bosch et al., 2011; Egeln et al., 2011; Kirchmann et al., 2011a,b,c; Möller et al., 2011). Appendix Table 3.A.2 presents descriptive statistics on the minimum wage sectors. The minimum wage sectors vary widely in terms of their bite (share of workers within a sector with wages below the minimum wage), share of full-time workers, and composition of workforce.

In the data preparation, I largely follow the guide in Dauth and Eppelsheimer (2020). In the

[15]The information on the daily wage is censored at the yearly varying social security contribution.

empirical analysis, I focus on workers aged 18 to 65. Since I am interested in spillover effects of sectoral minimum wages and not in the effects on the minimum wage sectors themselves, I omit all observations of establishments belonging to a minimum wage sector. To include East Germany in the data, I restrict the main analysis period to start from the year 1992 onward. I create an annual panel by selecting all employment spells that include June 30 as the cutoff date, since this date coincides with the measurement of the variables in the BHP. I deal with multiple employment spells of a worker in a year by keeping her main job, defined as the employment spell with the highest wage or longest tenure in case of a tie. I trim extremely low daily wages of full-time workers by dropping observations with real daily wages below the mean real daily wage of the first percentile of real daily wages.

For the mechanisms analyses, I calculate the share of the main construction sector in a labor market region. I proceed in four steps and use the delineation of labor market regions from Kosfeld and Werner (2012). First, I use the raw data and keep only panel establishments. Second, for each labor market region, I calculate the relative share of full-time workers in the main construction sector using only the pre-introduction years 1992–96. Third, I split the distribution of shares of main construction sector full-time workers across labor market regions into terciles, weighted by the number of full-time workers in each labor market region. Fourth, I merge this information to my sample. In a similar way, I calculate the main construction sectors' first minimum wage bite in each labor market region. Again, I only use panel establishments and calculate the share of workers earning a wage below 8.69 Euro in West Germany and 8.00 Euro in East Germany in each labor market region for the years 1992–96 within the main construction sector.

Abowd et al. (1999) (hereafter AKM) introduced an estimation strategy to isolate worker-specific and establishment-specific wage premia by using additive fixed effects for workers and establishments. Card et al. (2013) use the AKM estimation strategy to study the role of establishment-specific wage premia in generating recent increases in wage inequality in West Germany. The establishment-specific wage premia can be interpreted as a proportional pay premium or discount that is paid by an establishment to all employees, e.g., due to rent-sharing, efficiency wage premium, or strategic wage posting behavior (Card et al., 2013). The estimation strategy of AKM requires a connected set of establishments linked by worker mobility to identify the fixed effects. I use the AKM establishment fixed effects provided by Bellmann et al. (2020)

and estimated for the universe of workers and establishments in the German social security data. These estimated AKM establishment fixed effects are available for the five sub-periods 1985–92 (for West Germany only), 1993–99, 1998–2004, 2003–10, and 2010–17.

My analysis estimates spillover effects on the worker as well as the establishment level. To estimate establishment level responses to sectoral minimum wages, I keep only panel establishments that were sampled in a first step for the data (see Section 3.3.1) and collapse the worker level data to the establishment level. Thus, in my analyses I use a worker-year panel and an establishment-year panel. In the respective analysis samples, I only keep workers or establishments that appeared at least once before and once after the treatment (the public announcement of the sectoral minimum wages).

3.3.3. Exposed Groups and Descriptives

Workers

I begin by assigning workers outside the main construction sector to different groups, based on the expected intensity of their exposure to the minimum wage from the main construction sector. Formally, I assign workers to three wage groups based on their nominal hourly wage in year t. Using the nominal minimum wages in West Germany (including Berlin) and East Germany as thresholds, I define the groups in the following way:

Definition of Wage Groups

	Treated Group	**Partially Treated Group**	**Control Group**
Hourly Wage (in Euro) West	$h_{i,t} < 8.69$	$8.69 \leq h_{i,t} < 8.69 + 40\%$	$8.69 + 40\% \leq h_{i,t} < 8.69 + 80\%$
Hourly Wage (in Euro) East	$h_{i,t} < 8.00$	$8.00 \leq h_{i,t} < 8.00 + 40\%$	$8.00 + 40\% \leq h_{i,t} < 8.00 + 80\%$

The variable $h_{i,t}$ refers to the nominal hourly wage of worker i in year t. Although the main construction sector minimum wage was adjusted several times during my observation period, I use only the introductory minimum wage to define the groups because it was mainly this wage that was publicly announced and received greater media attention. I use a partially treated group in this paper mainly for three reasons. First, the adjustments to the minimum wage are covered by the partially treated group, the range of which was defined large enough. Second, because I use imputed hours to calculate hourly wages, the partially treated group could include workers in the treated group who were incorrectly assigned to the partially treated group due to measurement error. Third, the minimum wage in the main construction sector could also affect workers who are just above the minimum wage threshold, for example, because the increased wage in the main construction sector, together with already better non-pecuniary characteristics for some workers, now represents a better deal for these workers.[16] I try different bandwidths to define the partially treated and control group in Section 3.5.2 and find no qualitative change in the patterns of my results. Using data on the years prior to its introduction (1992–95), Table 3.2 illustrates descriptive statistics for worker groups affected by the minimum wage outside the main construction sector. These groups differed widely from each other. Workers in the treated groups had a higher share of women, non-German nationality, young and low-educated workers and were more likely to work in smaller establishments in rural districts, compared to the control group. In Section 3.4, I describe how my methodology deals with these issues.

Table 3.2.: Descriptives for Main Construction Sector Spillover Groups (1992–95)

	Treated Group		**Partially Treated Group**		**Control Group**	
No. of observations	878,392		1,502,064		1,203,169	
Share	24.51		41.91		33.57	
Averages						
Daily wage (in Euro)	52.57	(11.38)	82.27	(8.69)	107.30	(8.71)
Log (daily) wage	3.93	(0.25)	4.40	(0.11)	4.67	(0.08)
Log (daily) two-year wage growth	0.11	(0.24)	0.03	(0.15)	0.01	(0.14)
Shares within group (in percent)						
Women	59.47		39.58		25.93	
Non-German nationality	8.37		8.83		8.33	

Continued on next page

[16]This theoretical consideration stems from a model with strategic complementarity that I sketch in Appendix 3.C and whose predictions I review in Section 3.5.4.

Table 3.2 – continued from previous page

	Treated Group	Partially Treated Group	Control Group
By age			
18-25 years old	26.75	20.02	7.67
26-35 years old	34.81	42.67	43.26
36-45 years old	24.42	23.10	29.94
46-55 years old	12.36	12.05	16.14
56-65 years old	1.66	2.15	3.00
By education			
No vocational training	12.98	11.68	9.04
Vocational training	84.01	83.82	82.59
University or university of applied sciences	2.25	4.11	8.03
Missing education	0.75	0.39	0.35
By industry			
Agriculture and Forestry	2.47	1.05	0.42
Fishing and Fish Farming	0.02	0.01	0.01
Mining	0.39	1.65	2.86
Manufacturing	23.55	30.80	37.72
Energy and Water Supply	0.23	0.88	1.65
Construction	2.77	3.35	2.50
Trade and Repair	24.64	20.24	13.06
Catering	10.77	2.18	0.84
Transport and News	7.15	10.27	10.73
Finance and Insurance	0.69	2.12	3.96
Real Estate and Housing	8.80	6.14	6.15
Public Services	3.78	8.87	7.99
Education	1.08	2.15	2.80
Health	7.70	7.53	6.45
Other Services	5.11	2.39	2.65
Private Household	0.42	0.33	0.19
Missing industry	0.41	0.05	0.02

Continued on next page

Table 3.2 – continued from previous page

	Treated Group	Partially Treated Group	Control Group
By plant size			
Very small (1-4 workers)	21.88	7.50	3.84
Small (5-19 workers)	29.21	19.93	13.44
Medium (20-249 workers)	35.57	40.31	37.01
Large (250-999 workers)	8.66	18.30	22.49
Very large (1000+ workers)	4.68	13.96	23.22
By region type			
District-free cities	30.28	36.84	43.08
Urban districts	27.05	33.40	36.65
Rural districts, some densely populated areas	20.49	15.21	11.30
Rural districts, sparsely populated	22.19	14.56	8.98

Notes: Observations are worker-year combinations. Standard deviation in parentheses. The groups are defined by using the nominal hourly wage of a worker at year t. Daily wages are deflated using the consumer price index of the Federal Statistical Office. For workers in West Germany, I use the nominal main construction minimum wage of 8.69 Euro and for workers in East Germany 8.00 Euro as a threshold (see Table 3.1).

Source: SIEED and BHP, 1992–1995. Authors' calculations.

Establishments

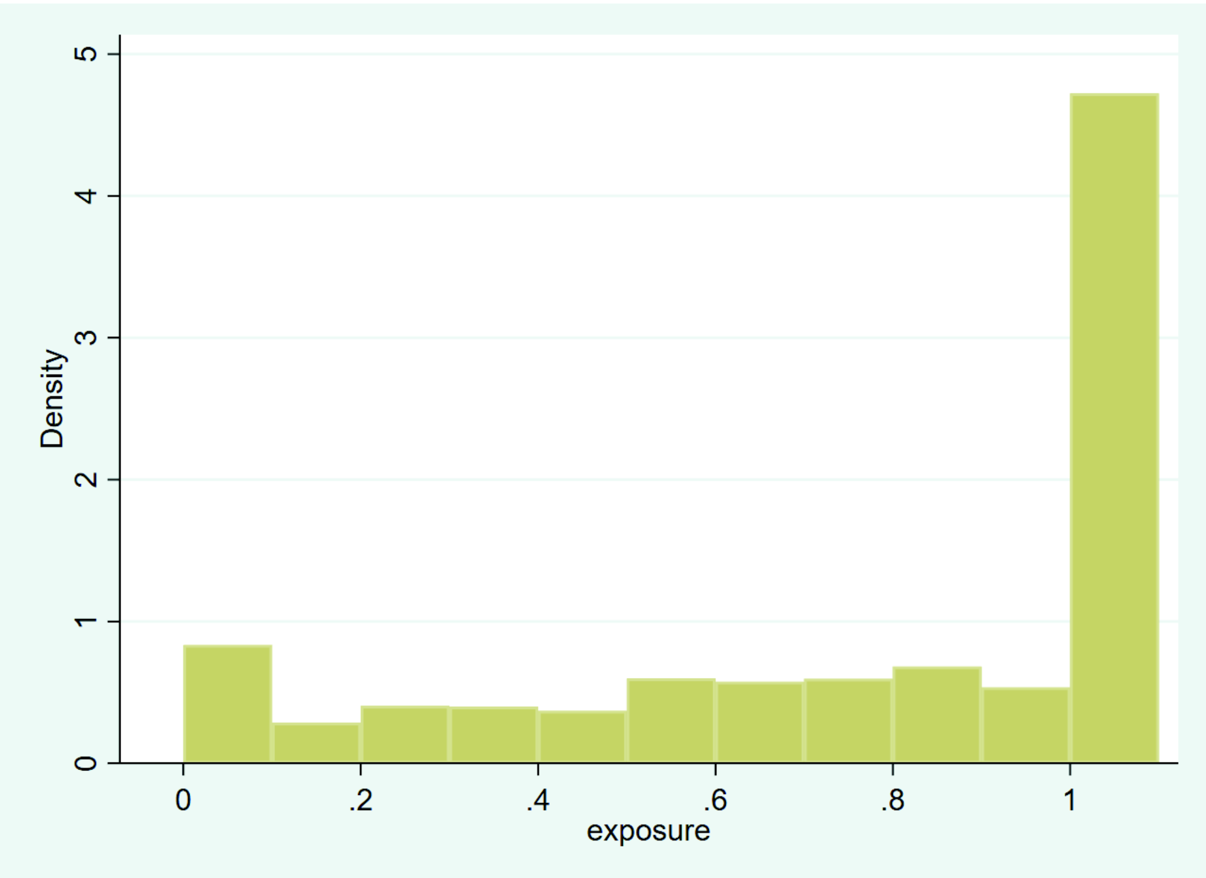
In the establishment level approach, I exploit the continuous variation in the exposure to the main construction sector minimum wage across establishments. This approach is based on a large literature exploiting regional variation in the bite of federal minimum wages (e.g. Bailey et al., 2021; Card, 1992; Dustmann et al., 2022). Derenoncourt et al. (2021) and Bossler and Gerner (2020) recently employed this method to examine exposure to minimum wages across employer-by-occupation-by-commuting-zone cells and establishments, respectively. Formally, I define the exposure $D_{j(i)}$ of an establishment j to the main construction minimum wage as

$$D_{j(i)} = \frac{\sum_{i \in j(i)} \sum_{t \in [1992,1995]} \mathbb{1}(h_{i,t} < MW + 40\%)}{N_{j(i),t \in [1992,1995]}}, \tag{3.1}$$

where MW refers to the minimum wage and $N_{j(i),t\in[1992,1995]}$ is the number of workers in an establishment for the time period 1992–95. Thus, I define exposure of an establishment to the main construction sector minimum wage as the fraction of workers paid a nominal hourly wage below the threshold for partially treated workers in the pre-introduction period of 1992-95.

Figure 3.1 shows the distribution of the exposure measure across establishments. Many establishments pay all of their workers an hourly wage below the cutoff. These establishments are characterized by a very small number of workers (1–4 workers), which naturally makes it more likely to have an exposure value of 1. Apart from this, the figure shows a continuous and relatively uniform distribution across exposure bins.

Figure 3.1.: Density of the Continuous Establishment Exposure Measure



Notes: For this figure, I keep only one observation per establishment in the period 1992–95. **Source:** SIEED and BHP 1992–95. Authors' calculations.

Industries

Furthermore, I also classify industries with workers for whom the main construction sector was considered an outside option (herein: outside option industries) and were therefore more likely exposed to the main construction sector minimum wage. In the empirical analysis, I compare the outcomes of workers in these industries with those of workers in other industries for whom the main construction sector was not considered as an outside option (herein: non-outside option industries). To define outside option and non-outside option industries, I use an employment flows approach as in Schubert et al. (2021). I begin with constructing the share of separations from a 3-digit industry k to the main construction sector as follows

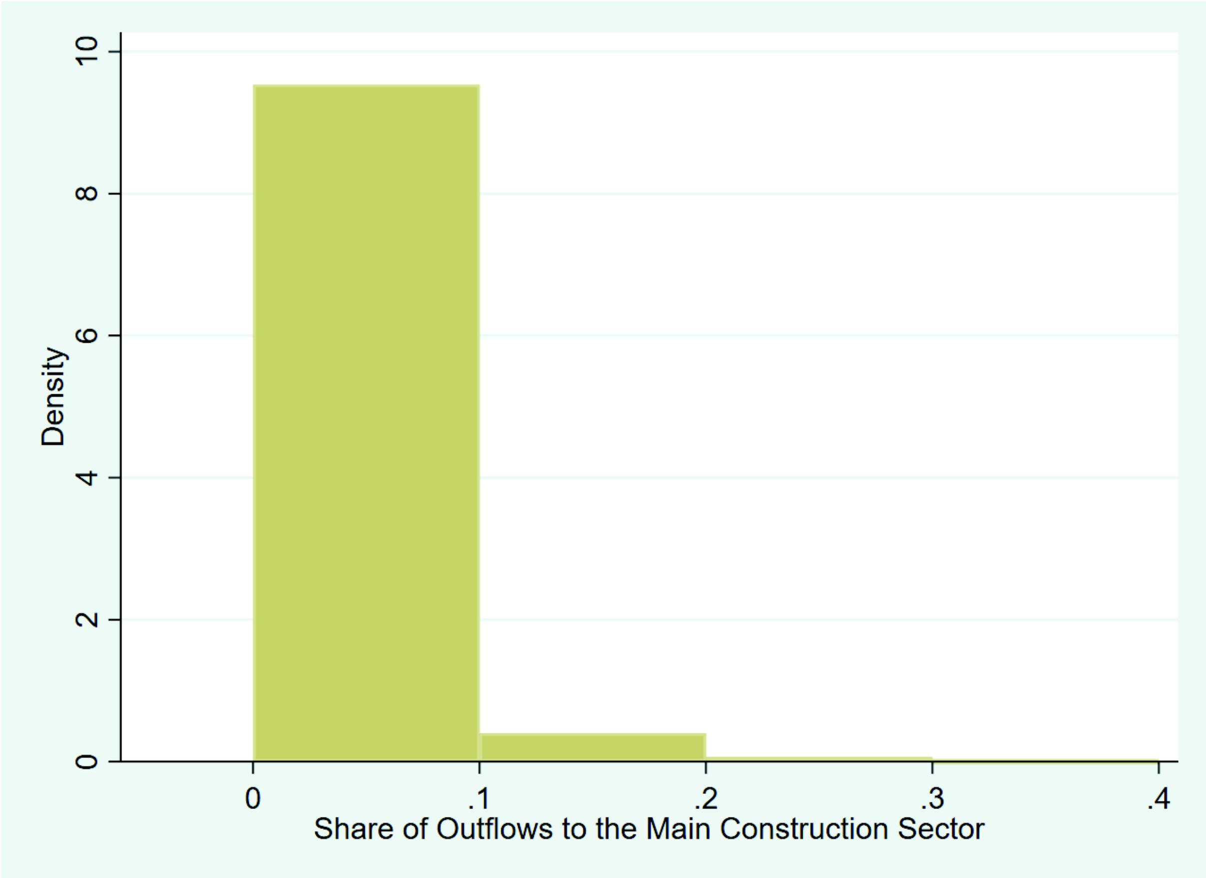
$$\pi_{k \to \text{main construction}} = \frac{\#\,\text{of separations from industry}\,k\,\text{to the main construction sector in year}\,t\,\text{to}\,t{+}1}{\#\,\text{of separations from industry}\,k\,\text{in year}\,t\,\text{to}\,t{+}1}. \tag{3.2}$$

I define separations as any employer transition.[17] To construct $\pi_{k \to \text{main construction}}$, I only use separations of workers who are in the treated or partially treated group at year t. I also choose the longest possible time period from 1985 to 1994.[18] This means that I construct $\pi_{k \to \text{main construction}}$ for West Germany in a first step and extrapolate it to East Germany. Figure 3.2 illustrates the distribution of $\pi_{k \to \text{main construction}}$ for the 1992–95 period, weighted by the number of workers in each industry in that time period. This distribution is heavily skewed to the left, with many industries having a low or no share of outflows to the main construction sector. This is as expected, because I use employer transitions instead of industry transitions and the share of the main construction sector in the economy (see Appendix Table 3.A.2) is not too high.

[17]This accounts for the possibility that for some industries only employers within the same industry are considerable outside options. Defining separations as industry transitions, instead of employer transitions, would thus overstate the role of some industries for workers' job choice.

[18]For consistency, I restrict the West German sample to 1985, since information in the variables was changed from that year onward.

Figure 3.2.: Density of the Share of Outflows to the Main Construction Sector by 3-digit Industries



Notes: For this figure, I only keep observations from the period 1992–95 and drop all observations with missing two-year wage growth or treatment assignment. The figure shows the share of outflows to the main construction sector by 3-digit industries weighted by the number of workers in each industry from 1992–95. **Source:** SIEED and BHP 1992–95. Authors' calculations.

I proceed by classifying industries in the top 10^{th} percentiles of the employment weighted distribution (whole sample in 1992–95) of $\pi_{k \rightarrow \text{main construction}}$ as outside option industries and industries in the lowest 10^{th} percentiles as non-outside option industries. Appendix Table 3.A.3 lists the 3-digit industries in the outside option industries classification and Appendix Table 3.A.4 lists the 3-digit industries in the non-outside option industries classification. Appendix Table 3.A.3 shows that workers from industries which rely more on manual tasks (e.g., "manufacture of wooden containers") are classified as outside option industries, whereas industries which are more service-oriented (e.g., "Telecommunications") are classified as non-outside option industries for the main construction sector.

I use this binary approach of different industry groups in my analysis instead of continuous variation of $\pi_{k \to \text{main construction}}$ for two reasons. First, because non-outside option industries are an additional control group in my analyses, they should not be affected by spillover effects from the minimum wage in the main construction sector. Therefore, I use the lowest part of the distribution in $\pi_{k \to \text{main construction}}$ by still keeping a large number of observations. Second, the main construction sector has shown to be an important outside option for workers in outside option industries, as evidenced by the fact that these industries are at the top of the $\pi_{k \to \text{main construction}}$ distribution. The main construction sector and outside option industries share one common labor market with transferable skills and similar (manual) tasks. Therefore, these industries should be affected by spillover effects from the minimum wage in the main construction sector.

3.4. Empirical Strategy

3.4.1. Worker Level Analysis

In my empirical strategy, I focus on the *changes* in outcomes over time rather than shifts in the level of the outcome for two reasons. First, when comparing the evolution of outcomes, e.g. wages, for workers with lower wages versus higher wages over time, one will typically observe higher wage growth for workers with lower wages, e.g. due to mean reversion (Ashenfelter and Card, 1982). Using changes in outcomes can alleviate this worry, since even with triple differences, the mean reversion could be different for workers in outside option industries than for workers in non-outside option industries. Second, because one possible mechanism for the spillover effects on the individual-level could be switching to better-paying jobs driven by information shocks, the relevant outcome is a shift in wage growth and not wage levels. In other words, switching to better-paying establishments could shift the wage-tenure profile of a worker and not only the level of the wage.

My main empirical strategy is a triple differences estimator (DiDiD). I compare the *changes* in outcomes for treated and control group workers in outside option vs. non-outside option industries over two-year windows (between t and $t + 2$), similar to e.g., Burauel et al. (2020); Clemens and Wither (2019); Currie et al. (1996); Dustmann et al. (2022). In the following, I describe the estimation approach using wages as the dependent variable, but the same arguments apply for other outcome variables as well. Formally, I estimate the following DiDiD specification around the time of the public discussion and announcement of the main construction sector minimum wage:

$$
\begin{aligned}
w_{i,t+2} - w_{i,t} = \alpha_i \, + \, \zeta_t \, + & \sum_{t=1992, t\neq 1993}^{1997} \beta_t Treated_{i,t} \times Option_{i,t} \times Year_t \\
& + \sum_{t=1992, t\neq 1993}^{1997} \gamma_t Partial_{i,t} \times Option_{i,t} \times Year_t \, + \, \delta X_{i,t} \, + \, \epsilon_{i,t}.
\end{aligned} \tag{3.3}
$$

Here $w_{i,t}$ refers to the log (deflated daily) wage of worker i in year t. In Equation 3.3, I regress (deflated daily) log wage growth of worker i between the years t and $t+2$ on the triple interaction of an indicator variable $Treated_{i,t}$, which is equal to 1 if worker i falls into the treated group and 0 if worker i falls into the control group at the baseline year t, the variable $Option_{i,t}$, and a year indicator $Year_t$. The variable $Option_{i,t}$ is equal to 1 if worker i is employed at an outside option industry (Appendix Table 3.A.3) in year t and 0 if she is employed at a non-outside option industry (Appendix Table 3.A.4). I include a similar triple interaction term with the indicator variable $Partial_{i,t}$ which is equal to 1 if worker i falls into the partially treated group at baseline year t and 0 if the worker is in the control group. I include all respective double interactions and indicators in $X_{i,t}$. Furthermore, in $X_{i,t}$, I include additional controls. Specifically, I include 1-digit industry, federal state, and region type dummies measured at baseline year t.[19] ζ_t are year fixed effects. The reference period is 1993 to 1995. I estimate the DiDiD specifications including one pre-introduction period $t = 1992$ and four post-introduction periods $t \geq 1994$. Thus, the change in wage growth for treated relative to control group workers in outside option versus non-outside option industries from 1992-94 serves as a placebo test. I cluster the standard errors at the worker level.

I include worker fixed effects with α_i. The inclusion of worker fixed effects α_i is very important in the context of this study for two reasons. First, the worker fixed effects purge time-invariant unobserved worker-specific effects on wage growth, such as e.g. ability or motivation to climb up the job ladder. Second, around the time of the introduction of the main construction sector minimum wage, many macroeconomic trends affected the treated and control groups differently, such as e.g. technological change (Dustmann et al., 2009; Goos et al., 2009), deepening trade relations with China and Eastern Europe (Dauth et al., 2014, 2021), and migration (D'Amuri

[19] I also estimate different specifications of Equation 3.3 without worker fixed effects. In this case, I additionally control for age, education, gender and nationality.

et al., 2010; Glitz, 2012). Worker fixed effects, which, in a regression with a differenced outcome, is analogous to controlling for worker-specific linear trends in a non-differenced regression (Allegretto et al., 2017), help to account for these group-specific macroeconomic trends.[20]

The coefficients of interest, (γ_t) β_t, now essentially compare the difference-in-differences (DiD) of (partially) treated versus control group workers in outside option industries relative to the DiD of (partially) treated versus control group workers in non-outside option industries. By excluding all sectors that have introduced minimum wages in t, the specification addresses other minimum wages implemented at the same time. Furthermore, in Section 3.5.2, I show that flows to industries that have introduced minimum wages at the same time or later were relatively low in $t+2$ and cannot explain the spillover effects found in this paper.

The DiDiD estimates of Equation 3.3 primarily have two advantages over simple difference-in-differences specifications. First, the DiDiD specification confirms the working hypothesis that after the minimum wage was discussed and announced in 1996, workers in industries similar to main construction (outside option industries) should also experience a larger change in their wage growth than workers in industries less similar to main construction (non-outside option industries). Second, the DiDiD estimates also remove any group-specific time shocks. Olden and Møen (2022) derive the formal identifying assumptions of the triple differences estimator and show that the estimator does not require two parallel trends assumptions, but only one parallel trends assumption, to have a causal interpretation. Intuitively, any contemporaneous shock to the outcome variable that affects all workers in the treated groups or all workers in the control group across outside option and non-outside option industries will be differenced out. In Section 3.5.1, statistically and/or economically insignificant effects for β_t and γ_t in the pre-announcement period indicate that the DiDiD parallel growth assumption holds. The spillover effect from the main construction sector minimum wage should have only affected workers in the treated group and to a larger extent within outside option industries and therefore does not get filtered out by the DiDiD specification.[21]

In addition to the event-study analysis in Equation 3.3, I also estimate the triple differences by pooling pre- and post-announcement periods:

[20] In the robustness checks, I drop the assumption that the mentioned economic factors can be viewed as group-specific macroeconomic trends and instead treat them as region-specific and industry-specific shocks.

[21] For further intuition, Appendix Figure 3.A.1 illustrates the identification strategy.

$$w_{i,t+2} - w_{i,t} = \alpha_i + \zeta_t + \beta\, Treated_{i,t} \times Option_{i,t} \times Post + \gamma\, Partial_{i,t} \times Option_{i,t} \times Post + \delta X_{i,t} + \epsilon_{i,t}. \tag{3.4}$$

The dummy *Post* equals 0 for the years of 1992 and 1993, and equals 1 for the years 1994, 1995, 1996 and 1997. All other variables remain the same as in Equation 3.3.

3.4.2. Establishment Level Analysis

To analyze the spillover effects from the main construction sector minimum wage on establishments, I exploit the continuous variation in the exposure $D_{j(i)}$ of an establishment j in the following event-study DiD specification:

$$y_{j,t} = \alpha_j + \zeta_t + \sum_{t=1992, t\neq 1995}^{1999} \gamma_t D_{j(i)} \times Year_t + \epsilon_{j,t}. \tag{3.5}$$

$y_{j,t}$ denotes the outcome of interest, α_j are establishment fixed effects and ζ_t are year fixed effects. At the establishment level, there is not a comparable issue of mean reversion as there is at the worker level, which allows for a focus on the growth of outcomes rather than changes in growth. The coefficients γ_t trace out how establishments with higher exposure to the main construction sector minimum wage responded to it relative to establishments with lower exposure and relative to the base year 1995. For the years $t > 1995$, the coefficients estimates for γ_t yield the causal spillover effect of the main construction sector minimum wage if the parallel trends assumption holds. Specifically, the underlying assumption for the DiD specification in Equation 3.5 is that more exposed establishments would have evolved similarly, in terms of the potential outcomes, compared to less exposed establishments in the absence of the main construction sector minimum wage. In Section 3.5.3, I provide suggestive evidence of this parallel trends assumption by visualizing the coefficient estimates for γ_t for the years prior to the minimum wage announcement $t < 1995$. Coefficient estimates of $t < 1995$ which are statistically and/or economically insignificant hint towards a plausible parallel trends assumption.

To further validate the hypothesis that the spillover effects stem from the main construction sector minimum wage rather than contemporaneous shocks to low-wage jobs, I estimate a DiDiD specification. I use the same intuition as for the individual-level analysis. Formally, I estimate the following DiDiD specification:

$$y_{j,t} = \alpha_j + \zeta_t + \sum_{t=1992, t\neq 1995}^{1999} \gamma_t D_{j(i)} \times Option_{j(i),t} \times Year_t + \delta X_{j,t} + \epsilon_{j,t}. \quad (3.6)$$

I estimate a triple interaction and include all respective double interactions as well as the $Option_{j(i),t}$ variable in $X_{j,t}$.[22] The DiDiD specification in Equation 3.6 has the additional advantage of filtering out any group-specific time shocks to establishments with different levels of exposure, while at the same time supporting the hypothesis that the main construction sector minimum wage should have a larger spillover effect to establishments in outside option industries.

Similar to Equation 3.3, the underlying parallel trends assumption in Equation 3.6 is that the gap in the potential outcome variable between outside and non-outside option industries would have evolved similarly for establishments with different levels of exposure, in the absence of the main construction sector minimum wage (Cunningham, 2021). In other words, any contemporaneous shock to the outcome variable, not induced by the minimum wage, which affects establishments with high levels of exposure but not low levels of exposure or vice versa, should be similar within outside option industries as in non-outside option industries. Again, in Section 3.5.3, I provide suggestive evidence for this assumption in an event-study figure, by showing that the coefficient estimates of $\gamma_t < 1995$ are statistically insignificant. If this assumption holds, $\gamma_t > 1995$ identifies the causal spillover effect of the main construction sector minimum wage on establishments in outside option industries with higher exposure.

I weight both, DiD and DiDiD, regressions by using the average number of full-time employees within each establishment in the 1992–95 pre-period. I cluster the standard errors at the establishment level.

3.5. Results

3.5.1. Wages and Reallocation

In Figure 3.3, I estimate my baseline DiDiD specification from Equation 3.3 using the change in wage growth as the outcome variable. Here, the y-axis shows the DiDiD coefficients from the

[22] To be more specific, $X_{j,t}$ includes: $Option_{j(i),t}$, $Year_t \times Option_{j(i),t}$, $D_{j(i)} \times Option_{j(i),t}$, and $\sum_{t=1992,t\neq 1995}^{1999} \gamma_t D_{j(i)} \times Year_t$.

triple interaction in which I, intuitively, compare the DiD in outside option industries with the DiD in non-outside option industries. In contrast to simple DiD estimators, the DiDiD estimator has the advantage of removing biases due to group-specific time shocks, such as shocks affecting the wage growth of all low-wage workers (in outside and non-outside option industries). I find a small positive and statistically significant coefficient in the pre-period of 1992–94 for treated workers in outside option industries. The coefficient quadruples in size from 1992–94 to 1994–96, right at the public discussion and announcement of the main construction sector minimum wage. Specifically, the relative wage growth of treated workers in outside option industries increased by 2% in 1994–96 relative to 1993–95. For the time periods of 1995–97, 1996–98, and 1997–99 the size of the DiDiD coefficient increases slightly more.[23] In column 4 of Appendix Table 3.A.5, I present the baseline specification illustrated in Figure 3.3, together with standard errors, the number of observations, and the partially treated group.[24] Without the inclusion of worker fixed effects in columns 1 and 2 of Appendix Table 3.A.5, I find similar patterns compared to the specification with worker fixed effects, with no statistically and economically significant coefficient in the pre-period of 1992–94. Appendix Table 3.A.5 further shows that the DiDiD estimates are similar with or without the inclusion of additional controls such as industry or federal state fixed effects. In Appendix Figure 3.B.1, I estimate the DiDiD specification excluding (ancillary) construction industries from the outside option industry classification and find that the wage spillover effects were not driven by these construction industries.[25]

To gain intuition on the validity of the triple differences specification, I estimate DiD specifications separately by non-outside option and outside option industries in Appendix Table 3.A.6.[26]

[23] In Appendix 3.E, I analyze the wage spillover effects of other sectoral minimum wages in Germany that were introduced either at the same time as the main construction sector minimum wage or later. I find that only minimum wages in sectors with a relatively high share of full-time workers had positive wage spillover effects for sub-minimum wage workers in outside option industries, while minimum wage sectors with a relatively high share of part-time female workers had negative wage spillover effects. Since I concentrate on full-time employees in my sample, this suggests that positive wage spillover effects only occur if the workers in the minimum wage sector are in a comparable employment contract to the workers in my sample.

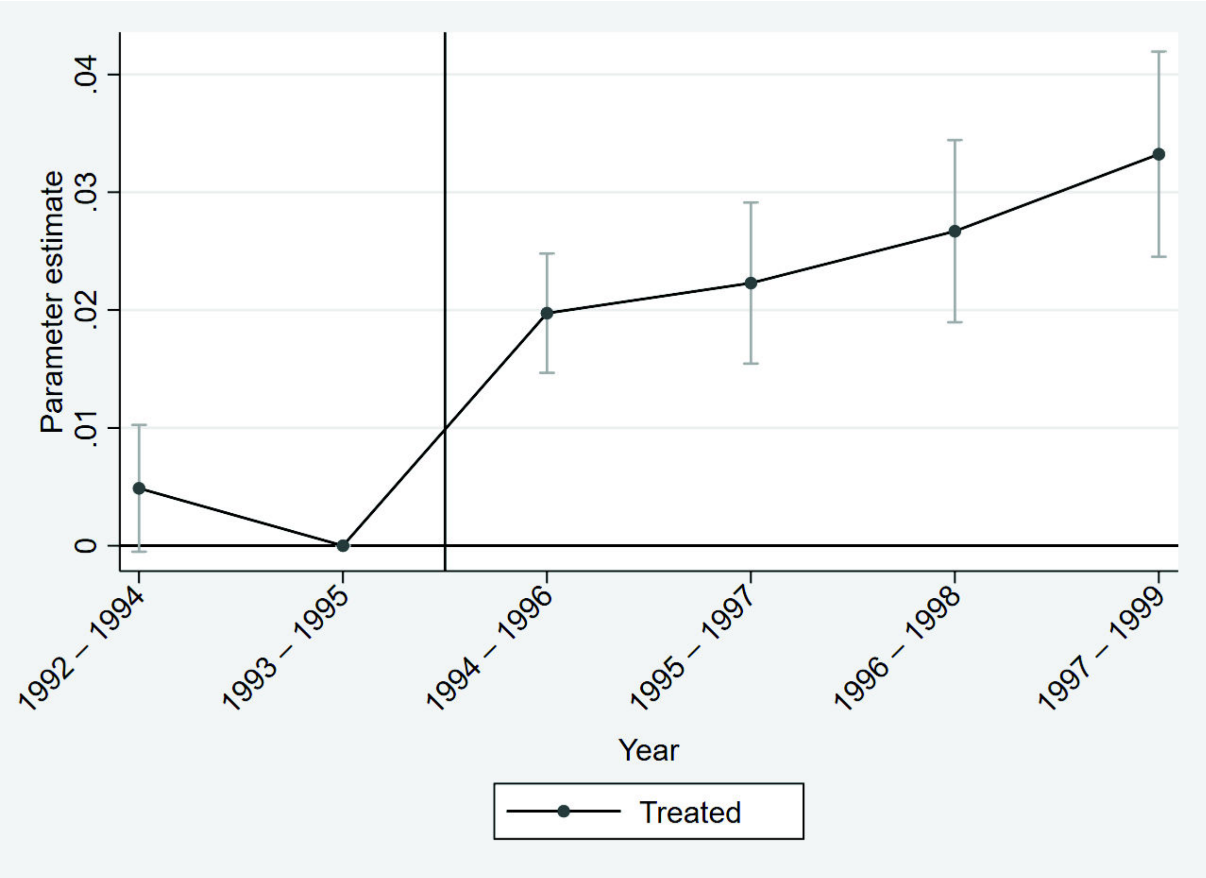
[24] I observe a similar spike in wage growth for the partially treated group. However, the coefficient is much smaller in magnitude. Because I use the partially treated group mainly to catch measurement errors that may arise from imputation of hours worked and minimum wage adjustments (see Section 3.3.3), I focus on the treated group.

[25] Specifically, I drop the 3-digit industries 451, 452, 454, and 455 from the list of outside option industries (see Appendix Table 3.A.3).

[26] To be precise, I estimate the following specification separately by outside option industries and non-outside option industries:

$$w_{i,t+2} - w_{i,t} = \alpha_i + \zeta_t + \sum_{t=1992, t\neq 1993}^{1997} \beta_t Treated_{i,t} \times Year_t + \sum_{t=1992, t\neq 1993}^{1997} \gamma_t Partial_{i,t} \times Year_t + \delta X_{i,t} + \epsilon_{i,t}.$$

Figure 3.3.: Triple Differences: Wage Spillover Effects of the Main Construction Sector Minimum Wage



Notes: This figure illustrates the results of the triple differences specification with the two-year change in log daily wages as the outcome (see Equation 3.3). I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. Column 4 of Appendix Table 3.A.5 illustrates this result in table form including the number of observations, standard errors, and partially treated group. **Source:** SIEED and BHP. Authors' calculations.

I observe a positive and statistically significant coefficient of the DiD estimate in the pre-period of 1992–94 for both, non-outside option and outside option industries. In other words, I observe a common shock to either all treated group or control group workers in 1992–94. The triple differences specification, illustrated in Figure 3.3, is able to partly filter this common group-specific time shock out. Assuming that the DiD in non-outside option industries represents the counterfactual wage growth change in outside option industries, I find that in the absence of the public discussion and announcement of the minimum wage (captured in 1994–96) and introduction of the minimum wage (captured in 1995–97) in the main construction sector, no change in wage growth would have been present. In 1996–98 and 1997–99, I observe a relatively small negative shock to the relative wage growth of workers in the treated group in the counterfactual scenario (non-outside option industries). Reassuringly, most of the action in the triple differences estimations in Figure 3.3 comes from higher wage growth of treated workers relative to control group workers in outside option industries.

Based on Appendix Table 3.A.6, I assume that the positive and statistically significant coefficient of the DiDiD in 1992–94 is a one-time common shock to all treated or control group workers. Moreover, to gain more pre-periods for the placebo check, in Appendix Figure 3.A.2, I estimate the triple differences specification with 1-year wage growth instead of 2-year wage growth as the outcome. I find that the common pre-period shock occurred mainly in 1993–94 and no significant pre-trend for 1992–93. Finally, in Appendix Figure 3.B.2, I use different bandwidths to define the control group. "Treated - Base" refers to the bandwidths of the baseline estimation defined in Section 3.3.3 and illustrated in Figure 3.3. I additionally define a control group with broader bandwidths ("Treated - Broad") with $MW + 60\% \leq h_{i,t} < MW + 120\%$ and tighter bandwidths ("Treated - Tight") with $MW + 20\% \leq h_{i,t} < MW + 40\%$, where MW refers to the minimum wage. The tradeoff in using narrower or wider bandwidths is that narrower bandwidths allow comparisons between treated and control group workers who are more similar to each other, while wider bandwidths make potential identification threats such as spillover effects to the control group or substitution between groups less likely (Stewart, 2004). Indeed, I find that using a narrow bandwidth for the control group completely eliminates the pre-trend in the 1992–94 period. The wider the bandwidth for the control group, the larger the coefficient in the 1992–94 pre-period. In all three cases, however, I find a sharp increase in the coefficients immediately upon the public discussion and announcement of the minimum wage in the main construction sector in 1994–96. Therefore, I interpret the sharply increasing and positive coefficients in the post-announcement period for treated workers in outside option industries as spillover effects from the sectoral minimum wage in the main construction sector.

In Table 3.3, I estimate the pooled pre- vs. post-period triple differences specification of Equation 3.4. On average, wage growth of treated workers in outside option industries increased by 2.1% in the post-period relative to the pre-period. To compare the effect size, I use a similar triple differences specification to estimate the wage growth effects *within* the main construction sector in Appendix Figure 3.A.3 and Appendix Table 3.A.7.[27] I find that the wage spillover

[27] Specifically, I use a sample including all workers in establishments within the main construction sector (see Appendix Table 3.A.1) and non-outside option industries. With this sample, I estimate a triple differences specification similar to Equation 3.3. The only change is that instead of comparing the DiD of treated vs. control group workers in outside option industries to the DiD in non-outside option industries, I compare the DiD in the main construction sector to the DiD in non-outside option industries.

effects are about one-third of the wage effect within the main construction sector.

Table 3.3.: Triple Differences: Pre- vs. Post-Period Specifications

	2-year wage growth	Job-to-job
Treated x Option x Post	0.021***	0.037***
	(0.003)	(0.006)
Partial x Option x Post	0.011***	0.037***
	(0.001)	(0.005)
No. of observations	761,276	796,763
No. of workers	177,647	194,574
Year fixed effects	yes	yes
1-digit industry fixed effects	yes	yes
Federal state fixed effects	yes	yes
Region type fixed effects	yes	yes
Worker fixed effects	yes	yes

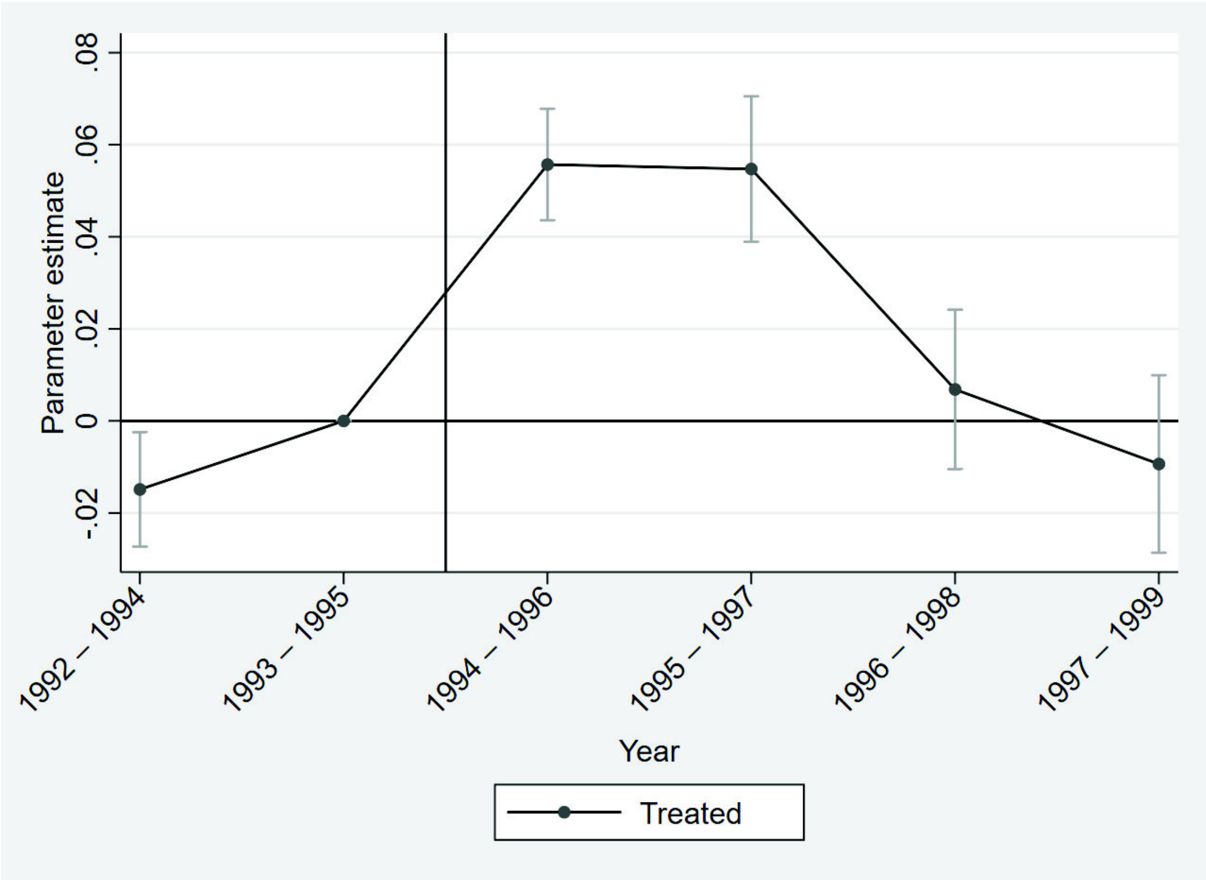
Notes: Standard errors in parentheses. The table shows specifications of Equation 3.4 with different outcome variables. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: SIEED and BHP. Authors' calculations.

In frictional labor markets, the publicly announced introduction of the main construction sector minimum wage should lead to an increase in reallocation of workers (e.g. Bhaskar et al., 2002; Jäger et al., 2022). I test this prediction by using the specification of Equation 3.3 with the change of jobs as the outcome variable. The outcome variable takes the value 0 if the worker did not change establishments from t to $t+2$ and 1 if the worker did change establishments from t to $t+2$. Figure 3.4 illustrates the results.[28] I find small statistically significant negative effects for the pre-period of 1992–94 for workers in the treated group. After the public discussion and announcement of the main construction sector minimum wage, I find a sharp increase in the probability of switching jobs for treated group workers in outside option industries. Specifically,

[28]Appendix Table 3.A.8 illustrates the results in table form and includes number of observations, standard errors, and the partially treated group.

treated workers in outside option industries had a 5.6 percentage points and 5.5 percentage points higher likelihood of switching jobs in 1994–96 and 1995–97 respectively, relative to the reference period 1993–95. For the subsequent periods, the DiDiD coefficient is insignificant in 1996–98 and 1997–99 for treated group workers in outside option industries. As I show in Appendix Figure 3.B.1, the results on the probability to switch establishments were not driven by the (ancillary) construction industries in the outside option industry classification. Overall, Table 3.3 illustrates that the probability that more exposed workers decided to leave their job to find a new employer increased by 3.7 percentage points in the post-period relative to the pre-period. The finding that the change in wage growth remained elevated during the years 1996–98 and 1997–99, concurrent with a decline in the probability of worker switching returning to baseline levels during these same years, suggests that treated workers switched to establishments that featured not only higher wage levels, but also had higher wage-tenure profiles.

Figure 3.4.: Triple Differences: Probability to Switch Establishments



Notes: This figure shows the result of a triple differences specifications using the probability to switch establishments as the outcome variable (see Equation 3.3). I use 95% confidence intervals. The variable takes the value 1 if the individual switched establishments from t to $t+2$ and 0 if she did not. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. Appendix Table 3.A.8 illustrates these results in table form including the number of observations, standard errors, and partially treated group. **Source:** SIEED and BHP. Author's calculations.

3.5.2. Robustness Checks

The triple differences specification of Equation 3.3 and estimated in Figures 3.3 and 3.4 is robust to macroeconomic shocks, mean reversion, worker-specific unobserved heterogeneity and group-specific time shocks, such as shocks to the low-wage labor market. However, around the time of the introduction of the main construction sector minimum wage, other potential shocks are not captured by my identification strategy and could therefore bias the results. Specifically, migration from East Germany and Eastern Europe, the integration of East Germany to the German economy, city and state specific policy changes, structural changes in the German labor market, international trade and technological change could potentially bias the estimations. I proceed in three steps to probe the robustness of my results to these kinds of shocks. First, I employ a time shifted placebo test. Second, I test the robustness of the results to region- and industry-specific shocks. In particular, I test whether international trade, which was very important during the analysis period, is the key driver of the results. Third, I use different definitions of the key independent variables.

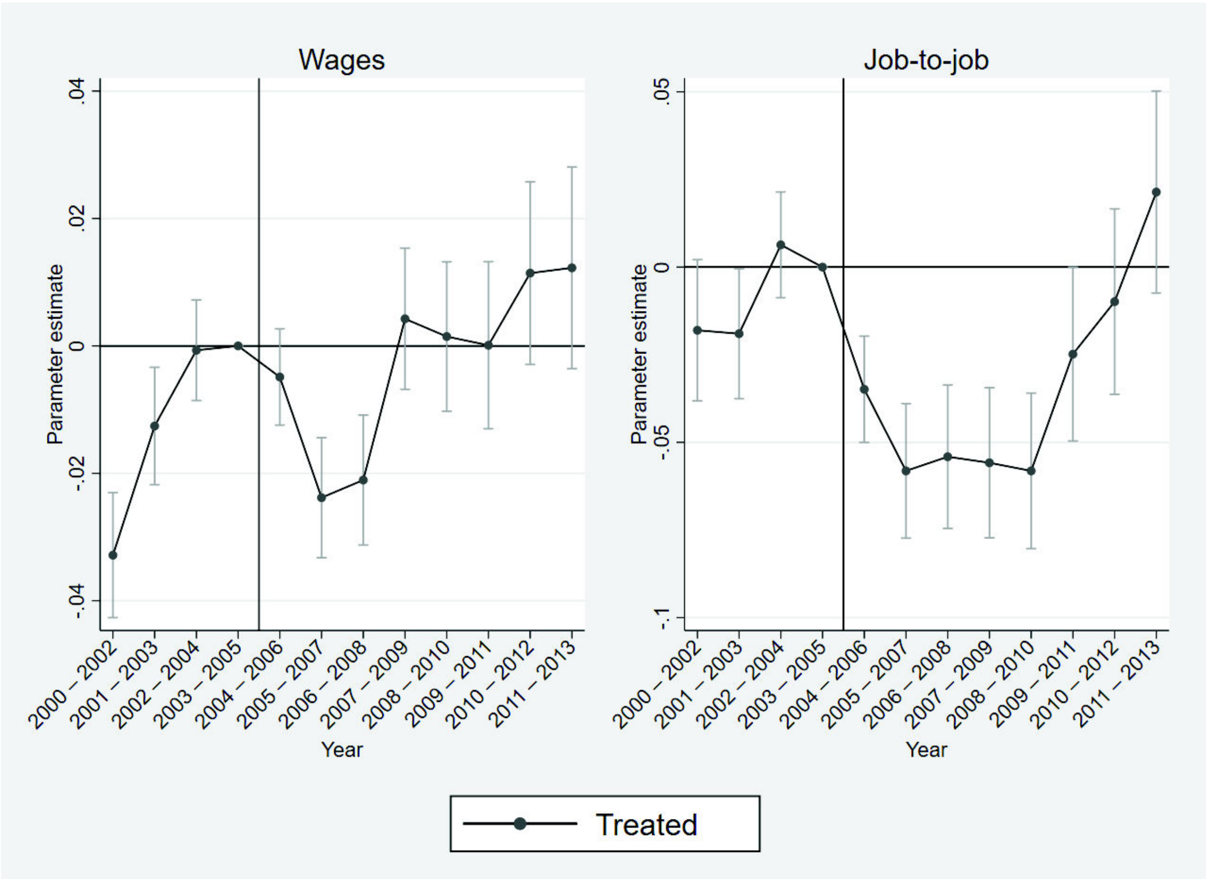
Time Shifted Placebo

In my main analysis, I only use one pre-period as a placebo test to determine whether the outcome variables between my treatment and control groups would have evolved similarly without the main construction sector minimum wage. It would be significantly more convincing to offer several pre-periods as a placebo test in this context. However, since this is not possible with the entire sample, I use an approach in which I run the exact same specification with a fake event at a later time. To do this, I use the entire period after my main analysis period from 2000 and up to 2013, to exclude any possible anticipatory effects of the nationwide minimum wage in 2015. Because in 2003–05 the main construction sector minimum wage did not change significantly (Popp, 2021), I choose this period as the reference period.

Figure 3.5 illustrates the results of the time shifted placebo test. The results clearly demonstrate the strengths and limitations of the empirical approach employed in this paper. Ideally, all coefficients should be insignificant to indicate that treated workers versus control group workers in outside option industries versus non-outside option industries would have evolved similarly in the absence of the main construction sector minimum wage. In Figure 3.5, I observe that 7 (5) out of 11 coefficients are statistically insignificant for wage growth (establishment switch) as the outcome. I observe negative and statistically significant coefficients at the time periods around the financial crises of 2001–03 ("dot-com bubble") and 2008–09 ("global financial crisis"). These events very likely affected workers in non-outside option industries more than in outside option industries.

While the empirical approach effectively controls for common shocks experienced by workers in both the treated and control groups across outside option and non-outside option industries, it may not adequately control for shocks that are specific to subgroups within the triple differencing framework, such as shocks experienced by treated workers in outside option industries or control group workers in non-outside option industries. Therefore, in the remainder of this section, I specifically test the robustness of the results to simultaneous shocks by, for example, using other types of variation with fixed effects or redefining the main independent variables to create new treatment and control groups.

Figure 3.5.: Triple Differences: Time Shifted Placebo



Notes: This figure shows the results of two triple differences specifications using the observation period after the main analysis period of 2000–13 (see Equation 3.3). In the first panel, I use the two-year change in log daily wages as the outcome. In the second panel, I use the probability of switching establishments as the outcome variable, which takes the value 1 if the individual switched establishments from t to $t + 2$ and 0 if she did not. I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 2003–05. **Source:** SIEED and BHP. Author's calculations.

Simultaneous Shocks

I include labor market region (LMR) times year fixed effects in the second column of Table 3.4.[29] These fixed effects exploit variation within labor market regions across differentially

[29]In Appendix Tables 3.B.1 and 3.B.2, I present the full table by including the partially treated group.

exposed individuals and therefore control for region-specific shocks such as migration shocks to specific labor market regions, city and state specific policy changes, and international trade shocks with different effects across regions. I find that the inclusion of these fixed effects does not change the results qualitatively. Thus, the positive wage spillover and reallocation effects were not driven by region-specific shocks.

Next, I include 1-digit industry times year fixed effects in the third column of Table 3.4. These fixed effects exploit variation within 1-digit industries across differentially exposed individuals and therefore control for industry-specific shocks, such as technological change or also international trade shocks and structural changes to the German economy, which affected some industries differently than others. I find that the inclusion of industry times year fixed effects does not change the results qualitatively.

Furthermore, I include both, labor market region times year and 1-digit industry times year fixed effects in the fourth column of Table 3.4. Again, the positive wage spillover effects and the increase in job-to-job changes are robust to the inclusion of these fixed effects.

In the fifth column of Table 3.4, I exclude all observations in establishments during their closing year.[30] Demand shocks during the observation period could bias my results. Excluding observations that are affected by establishment closure should capture these shocks on the demand side. I find virtually no change in the coefficients for the wage spillover and reallocation estimations.

The above robustness analyses account for international trade by keeping the variation at time t fixed for regions or industries. However, international trade could still drive the effects. For example, by making employees more aware of international trade through the discussions in 1996, individuals may have moved into or out of industries that were more affected by international trade or by the posting practices of other EU countries.[31] The manufacturing sector was particularly affected by international trade (Dauth et al., 2014, 2021). Therefore, to exclude international trade as the decisive driver of the effects, in Appendix Figure 3.B.3, I exclude manufacturing at time t and all switches to manufacturing at time $t + 2$. I find that my main results are robust to the exclusion of the manufacturing sector from the sample.

[30] To make sure that these are real establishment closures and not just an establishment takeover or ID change, I use the heuristic in Hethey and Schmieder (2010) and the variables created for it in the BHP.

[31] Because I view international trade in this context as a kind of omitted variable rather than a mechanism for spillover effects from the main construction sector minimum wage, I treat this aspect here.

Moreover, European worker postings to Germany have increased during the analysis period, as described in Section 3.2. To test whether there has been a change in the number of people moving into sectors that have been particularly affected by the posting practices, I define a dependent variable which takes the value 1 if a worker switched into a ”posted” sector and 0 if there was no change.[32] Note that posted sectors are already excluded in t. In Appendix Figure 3.B.4, I find some change of outflows to posted sectors. However, these coefficients are too small to be the driver of job-to-job switches.

[32]Posted sectors in this context are all sectors listed in Table 3.1.

Table 3.4.: Triple Differences: Robustness Checks

	Baseline	Region shocks	Industry shocks	Region + Industry shocks	No closing plants	Different Treated	Different Option
Panel A: Wages							
Treated x Option							
x 1992-94	0.005*	-0.008***	0.006*	-0.003	0.006**	-0.007*	0.006***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)
x 1994-96	0.020***	0.024***	0.014***	0.017***	0.019***	0.020***	0.025***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
x 1995-97	0.022***	0.029***	0.007*	0.013***	0.020***	0.022***	0.024***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.002)
x 1996-98	0.027***	0.037***	0.010**	0.019***	0.024***	0.011**	0.036***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
x 1997-99	0.033***	0.046***	0.016***	0.028***	0.031***	0.015***	0.044***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	(0.003)

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Table 3.4 – continued from previous page

	Baseline	Region shocks	Industry shocks	Region + Industry shocks	No closing plants	Different Treated	Different Option
No. of observations	761,276	752,408	761,276	752,408	754,698	761,276	2,117,788
No. of workers	177,647	175,700	177,647	175,700	176,157	177,647	481,939
Panel B: Job-to-job							
Treated x Option							
x 1992-94	-0.015**	0.001	0.008	0.018***	-0.014**	-0.028***	0.042***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.004)
x 1994-96	0.056***	0.069***	0.044***	0.057***	0.054***	0.078***	0.106***
	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)	(0.004)
x 1995-97	0.055***	0.058***	0.031***	0.040***	0.052***	0.079***	0.071***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)	(0.010)	(0.006)
x 1996-98	0.007	0.013	0.007	0.013	0.005	-0.016	0.044***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.006)
x 1997-99	-0.009	0.005	-0.024**	-0.009	-0.011	-0.044***	0.016**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.007)

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Table 3.4 – continued from previous page

	Baseline	Region shocks	Industry shocks	Region + Industry shocks	No closing plants	Different Treated	Different Option
No. of observations	796,763	787,452	796,763	787,452	789,906	796,763	2,207,206
No. of workers	194,574	192,416	194,574	192,416	192,959	194,574	524,356
LMR x year fixed effects	no	yes	no	yes	no	no	no
Industry x year fixed effects	no	no	yes	yes	no	no	no

Notes: This table shows several robustness checks on the triple differences estimation with the two-year change in log daily wages as the outcome variable in Panel A and the two-year change in job-to-job transition as the outcome variable in Panel B (see Equation 3.3). Standard errors (in parentheses) are clustered at the worker level. In the first column, I show the baseline specification. In the second column, I add labor market region times year fixed effects. In the third column, I add 1-digit industry times year fixed effects to the baseline specification. In the fourth column, I combine labor market region times year fixed effects and industry times year fixed effects and add them to the baseline specification. In the fifth column, I use the baseline specification and drop all observations in establishments that are in their closing year. In the sixth column, I use a time-constant treatment variable. In the seventh column, I change the $Option_{it}$ variable to be equal to 1 if an individual i is working in an occupation that had large outflows to the main construction sector at year t and equal to 0 if an individual i is working in an occupation that had low outflows to the main construction sector at year t. The reference period is 1993–95. All specifications include the baseline fixed effects: year, 1-digit industry, federal state, region type, and worker. Furthermore, in all specifications I also include the interaction of "Partial x Option", but report their coefficients only in Online Appendix Table 3.B.1. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Source: SIEED and BHP. Author's calculations.

Alternative Definitions of Variables

I also check the robustness of my results to different definitions of the key independent variables of interest in the last two columns of Table 3.4. First, I define a time-constant version of the $Treated_{i,t}$ and $Partial_{i,t}$ variable ($Treated_i$ and $Partial_i$) so that variation in these variables, with the inclusion of worker fixed effects, only comes from changes in the outcome variable for the same individuals over time and not from switchers from, for example, the treated group to the control group. To do so, I classify an individual as belonging to the (partially) treated group if one observation between 1992 and 1995 of the individual is classified as (partially) treated. I proceed similarly for control group workers. Intuitively, I relax the no-carryover assumption of my baseline estimation, where I implicitly assumed that potential outcomes depend only on current treatment status and not on the entire treatment history (Roth et al., 2022). I find qualitatively similar results for the wage spillover and reallocation effects with these time-constant versions of the $Treated_i$ and $Partial_i$ variables. However, as I analyze changes in wage growth from t to $t+2$ in the analysis, and have therefore already defined treated or control group workers over two-year windows (as in, for example, Dustmann et al. (2022)), I use the baseline $Treated_{i,t}$ variable rather than the more restrictive $Treated_i$ variable.

Second, one could argue that the relevant labor market definition of workers is based on occupations instead of industries. Therefore, I define the $Option_{i,t}$ variable based on employment flows within 3-digit occupations instead of employment flows within 3-digit industries (see Section 3.3.3). In the last columns of Table 3.4, I find that the patterns of spillover effects using occupation flows are similar to the baseline specification using industry flows.

Finally, I use the continuous flow measure $\pi_{k\rightarrow\text{main construction}}$ of Equation 3.2 instead of the binary outside option vs. non-outside option industries definition. The top and bottom 10% of flow-connected industries could be affected by simultaneous shocks that are specific to these industries. Because the flow measure uses all industries in the data, it should be more robust to these kinds of shocks.

I illustrate the results in Figure 3.6. Similar to the specification with the binary indicator variable, I find an increase in wage growth and establishment switches right at the year of public discussion and announcement of the minimum wage in 1996 for industries that had more worker transitions to the main construction sector in the past. However, in the following analysis, I maintain the use of the binary indicator variable as the primary method of estimation, as utilizing a continuous treatment variable brings its own set of assumptions, and necessitates a stronger parallel trend assumption that cannot be verified using pre-trends alone (Callaway et al., 2021).

Figure 3.6.: Triple Differences: Continuous Industry Flows

Notes: This figure shows the results of two triple differences specifications using the continuous industry flows variable of Equation 3.2, instead of $Option_{i,t}$ (see Equation 3.3). In the first panel, I use the two-year change in log daily wages as the outcome. In the second panel, I use the probability of switching establishments as the outcome variable, which takes the value 1 if the individual switched establishments from t to $t+2$ and 0 if she did not. I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Author's calculations.

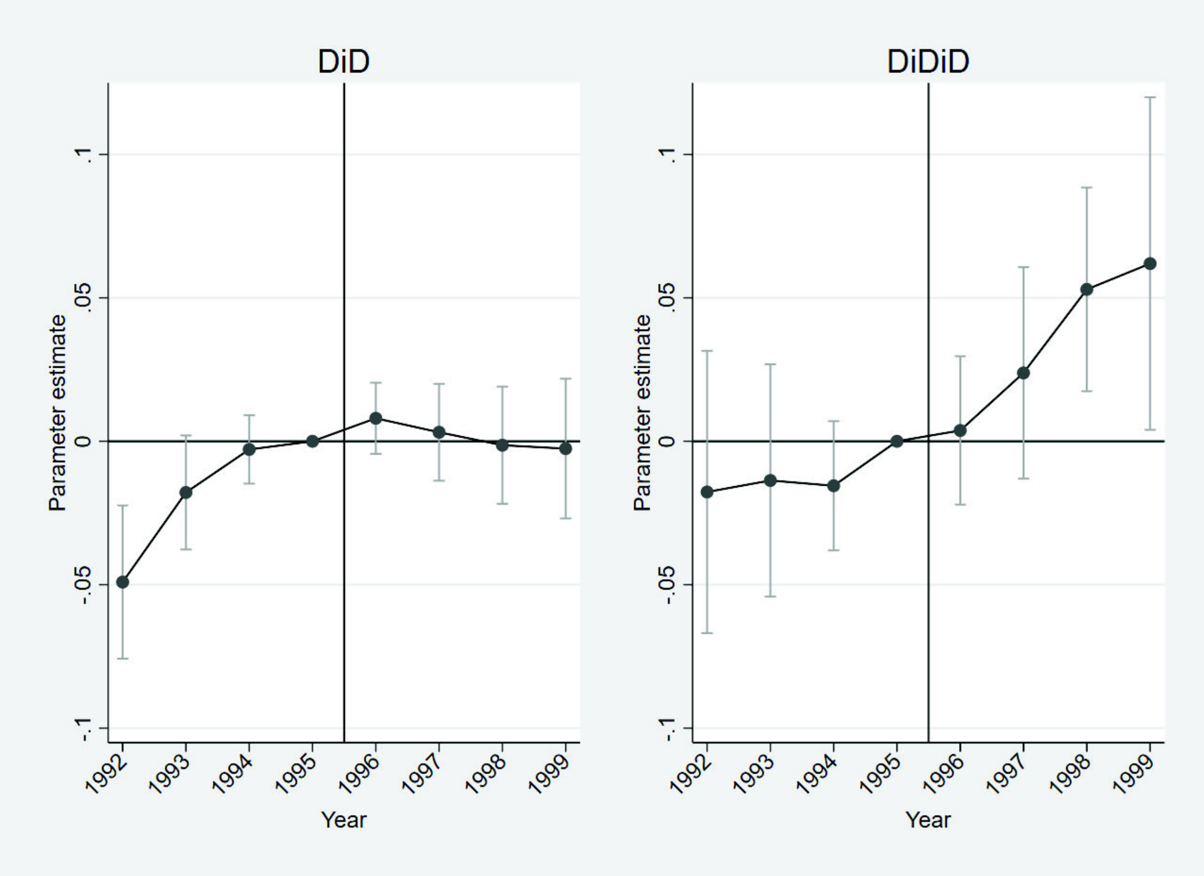
3.5.3. Establishments

To shed light on demand-side responses and to compare the results with the existing empirical evidence on cross-employer spillover effects (Bassier, 2021; Derenoncourt et al., 2021; Staiger et al., 2010), I analyze the spillover effects from the main construction sector minimum wage from the perspective of establishments.

Figure 3.7 plots the coefficient estimates for γ_t for the DiD specification from Equation 3.5 as well as the coefficient estimates for γ_t for the DiDiD specification from Equation 3.6. The outcome variable in these figures are log (daily) average wages of an establishment. I find no statistically significant effect on average wages on more exposed establishments using the DiD specification. In line with previous research on cross-employer wage spillovers, the DiDiD estimates in Figure 3.7 show that more exposed establishments increased average wages following the introduction of the main construction sector minimum wage. Wage growth evolved similarly for establishments with different levels of exposure in outside option and non-outside option industries in the years prior to the minimum wage introduction. However, after the introduction, establishments in outside option industries with higher levels of exposure increased their average wages relatively more, compared to establishments in non-outside option industries and establishments with lower levels of exposure. Specifically, the coefficient estimates from 1992–97 are statistically insignificant and increase only after the introduction of the main construction sector minimum wage to 5.3% in 1998 and 6.2% in 1999.[33]

[33] Appendix Tables 3.A.9 and 3.A.10 illustrate the results in table form including the number of observations and standard errors.

Figure 3.7.: Establishment Level: Wage Spillovers from the Main Construction Sector Minimum Wage

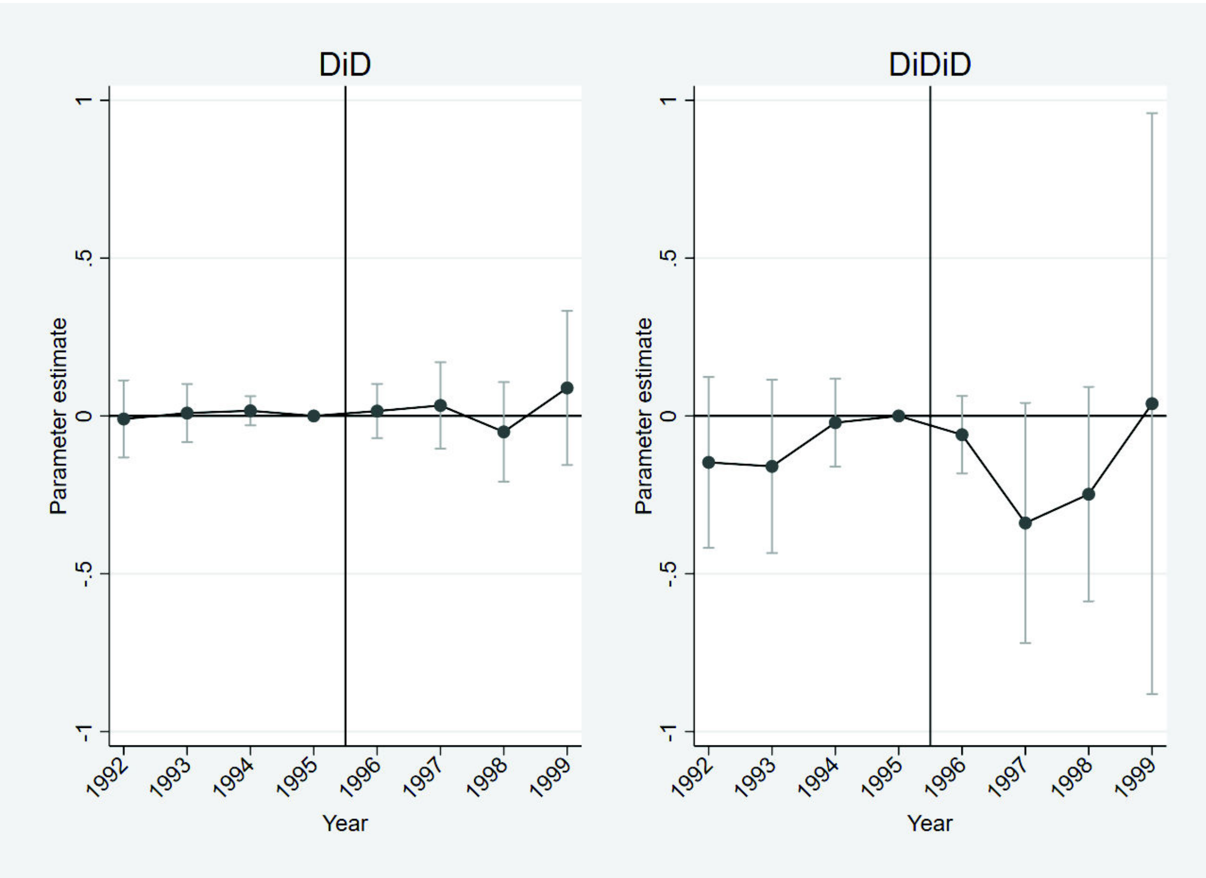


Notes: The outcome variable is the log (daily) average wage. In the panel DiD, I estimate Equation 3.5 and in the panel DiDiD, I estimate Equation 3.6. Both estimations are weighted by the average number of full-time employees within establishments in the 1992–95 pre-period. Appendix Tables 3.A.9 and 3.A.10 illustrate these results in table form including the number of observations and standard errors. **Source:** SIEED and BHP 1992–99. Author's calculations.

Note that while workers experienced higher wage growth already right at the public discussion and announcement of the main construction sector minimum wage (see Figure 3.3), establishments increased average wages only after the introduction in 1998. Thus, while employees reacted very quickly and strongly to the public announcement of the minimum wage, for example by changing jobs, establishments responded rather relatively late to the minimum wage. In addition, I show in Section 3.5.4 that the wage spillover effects can be explained mainly by establishment switches. This is consistent with the results here, as they show that it is primarily a change in worker behavior that drives the results in this study.

In Figure 3.8, I estimate the DiD and DiDiD specifications on the establishment level using the log number of full-time employed workers as the outcome variable. Again, using the DiD specification I do not find that more exposed establishments experienced a change in their number of full-time employees. However, using the DiDiD specification, I find that more exposed establishments in outside option industries experienced on average a loss of their full-time employment force. The negative employment effects for more exposed establishments in outside option industries amounted to 33.9% in 1997 and are relatively imprecise estimates. This result is in general consistent with labor market models which incorporate frictions, as these models predict a loss in employment for more exposed establishments.

Figure 3.8.: Establishment Level: Employment Effects from the Main Construction Sector Minimum Wage



Notes: The outcome variable is the log number of full-time employed workers (according to sample restrictions). In the panel DiD, I estimate Equation 3.5 and in the panel DiDiD, I estimate Equation 3.6. Both estimations are weighted by the average number of full-time employees within establishments in the 1992–95 pre-period. Appendix Tables 3.A.9 and 3.A.10 illustrate these results in table form including the number of observations and standard errors. **Source:** SIEED and BHP 1992–99. Author's calculations.

3.5.4. Mechanisms

The spillover effects from the main construction sector minimum wage are consistent with labor market models that include frictions. In Section 3.5.2, I excluded alternative hypotheses such as region- and industry-specific shocks and international trade as possible mechanisms. However, within a model world with labor market frictions, it remains unclear whether strategic complementarity or information frictions can explain the spillover effects. Based on theoretical considerations, I will explore the mechanisms for spillover effects in this section.

Strategic Complementarity

To understand whether strategic complementarity can explain the spillover effects, I use a simple version of the theoretical models in Bhaskar et al. (2002); Bhaskar and To (1999, 2003) which in turn build on the spatial model of Salop (1979). A version of this model is also applied in Staiger et al. (2010), who find evidence for strategic complementarity in their spillover effects.

In the spatial model of strategic complementarity, workers have heterogeneous preferences for employers due to transportation costs. I ignore other non-pecuniary job characteristics by which heterogeneous preferences of workers may arise and assume that all non-pecuniary job characteristics, except transportation costs, are similar for the main construction sector and outside option industries. Note that because I use employment flows to determine outside and non-outside option industries, outside option industries are already "close" to the main construction sector in terms of task similarity, transferability of skills and possibly other non-pecuniary characteristics by revealed preference. The model posits that sectors located at a greater geographic distance possess a greater degree of autonomy in determining their wages, in contrast to sectors that are situated in proximity to one another, which exhibit a diminished degree of independence in setting their wages. For modeling details, I refer the interested reader to Appendix 3.C.

I can use this model to derive testable predictions on wage spillover and reallocation effects from the main construction sector minimum wage. I assume that the share of the main construction sector in a labor market region (LMR) is negatively correlated with the distance to its competitors in the LMR. With respect to wages, the model predicts:

1. **Outside option industries increased wages more in LMRs with a higher share**

of the main construction sector.

I test this prediction in the second column of Table 3.5.[34] I use the terciles of the distribution of the share of the main construction sector among LMRs described in Section 3.3.2. LMRs in the lowest tercile have shares of the main construction sector that range from 0% to 4%, LMRs in the middle tercile have shares of the main construction sector that range from 4.1% to 7.2%, and LMRs in the highest tercile have shares of the main construction sector that range from 7.2% to 36.9%. I interact these terciles with the baseline triple interaction. In contrast to the prediction, I find that treated workers in outside option industries in LMRs with a higher share of the main construction sector experience a lower wage growth compared to similar workers in LMRs with a lower share of the main construction sector. I rationalize this result in Section 3.5.4.

[34]Appendix Table 3.A.11 illustrates the full table with the partially treated group.

Table 3.5.: Tests of Strategic Complementarity Model Predictions

	Baseline	Tercile share	Bite (West Germany)	Bite (East Germany)	Switcher 1	Switcher 2
Treated x Option x Post	0.021***	0.046***	0.055***	0.054***	0.006**	0.006**
	(0.003)	(0.005)	(0.003)	(0.006)	(0.003)	(0.003)
Treated x Option x Middle x Post		-0.037***				
		(0.007)				
Treated x Option x High x Post		-0.025***				
		(0.007)				
Treated x Option x Bite x Post			-0.004	0.018***		
			(0.003)	(0.006)		
Treated x Option x Switch x Post					0.021***	0.021***
					(0.005)	(0.005)

Continued on next page

Table 3.5 – continued from previous page

	Baseline	Tercile share	Bite (West Germany)	Bite (East Germany)	Switcher 1	Switcher 2
No. of observations	761,276	752,408	817,826	176,319	761,276	746,624
No. of workers	177,647	175,700	150,801	42,836	177,647	173,237
LMR fixed effects	no	yes	yes	yes	no	no
Excluding mcs switchers?	no	no	no	no	no	yes

Notes: Standard errors in parentheses. The table displays specifications of Equation 3.4 with 2-year change in log (daily) wages as the outcome. Column 2 shows the interactions with the main construction sector share terciles in LMRs. Columns 3 and 4 show interaction with the bite of the main construction sector minimum wage. Here, the sample is split between West and East Germany, with West Germany including years 1989–1991. All specifications include year, industry, federal state, region type, and worker fixed effects, and the interaction of "Partial x Option" (see Appendix Table 3.A.11). Significance levels: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Source: SIEED and BHP. Authors' calculations.

2. **Outside option industries increased wages more in LMRs with a higher bite of the main construction sector.**

Intuitively, a higher bite means that more establishments in the main construction sector have to adjust their wages upward, and therefore more establishments in outside option industries will have to increase their wages. Since, by definition, hardly any establishment in the main construction sector would have to adjust its wages in labor market regions with a low bite, no establishment in outside option industries would have to adjust wages either. To test this prediction, I use the bite of the main construction sector minimum wage, calculated for each LMR using the pre-period (see Section 3.3.2). Because the bite measure varies strongly between West and East Germany (see Appendix Table 3.A.2), I divide the sample to West and East Germany and standardize the bite measure across LMRs within these two samples, weighted by the number of employees in each LMR, to have mean 0 and standard deviation 1. The third and fourth columns of Table 3.5 illustrate the results.

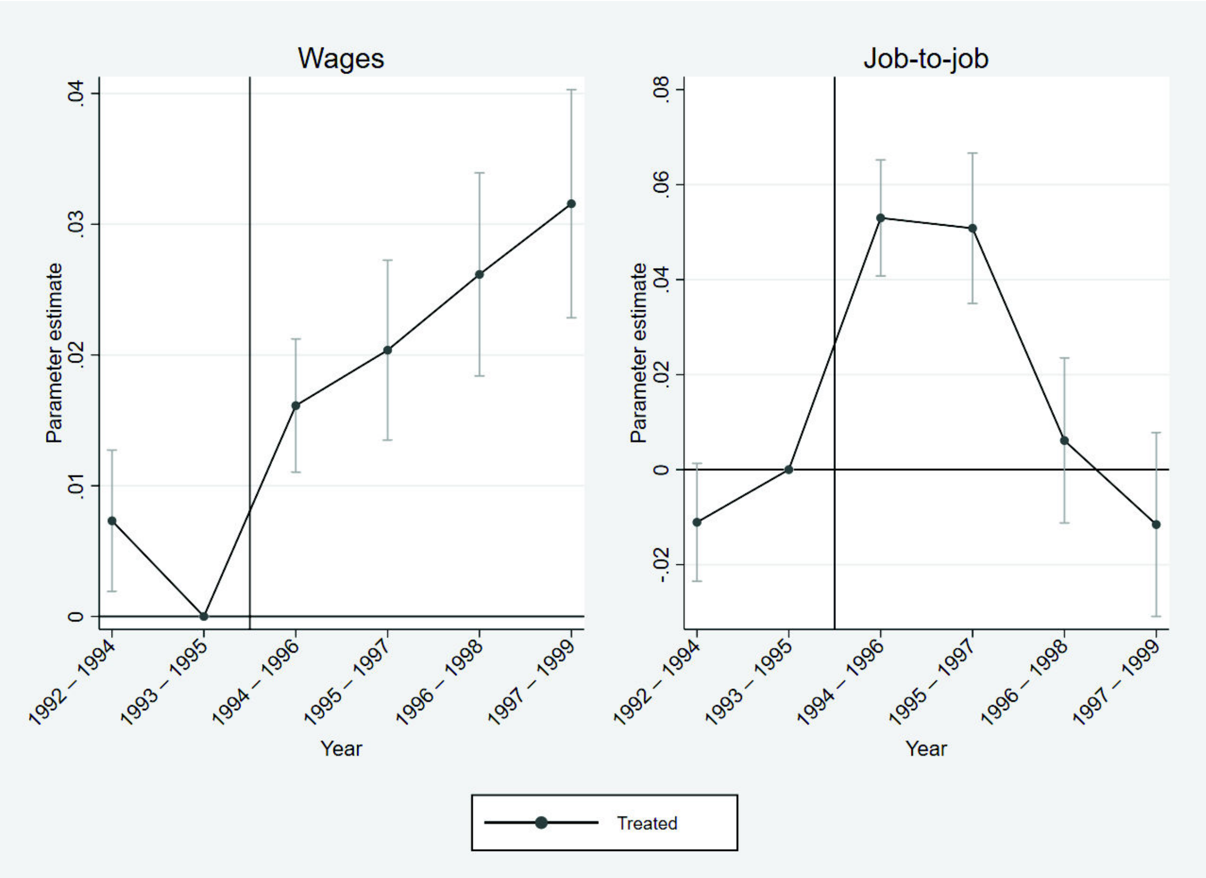
I find that West German treated workers in outside option industries within LMRs with a higher main construction sector minimum wage bite do not experience a different change in their wage growth compared to workers in LMRs with a lower bite. However, for East Germany, I do indeed find that treated workers in outside option experience a higher wage growth in LMRs that have a higher bite.

3. **The wage increase stemmed mostly from staying within the same establishment or switching to the main construction sector.**

Since every establishment outside the main construction sector would respond similarly to the minimum wage in the main construction sector, the net wage (wage minus transportation costs) of the current establishment would not change relative to all other establishments within the outside option industries. Therefore, in the simple strategic complementarity model presented above, it would only be rational for workers in the outside option industries to remain in the same establishment or increase reallocation to the main construction sector.

In Figure 3.9, I re-estimate the specification of triple differences for wage changes and job-to-job changes by excluding switchers to the main construction sector. In contrast to the model prediction, I find that the wage spillover and reallocation effects were not driven by switchers to the main construction sector.

Figure 3.9.: Triple Differences: Wage Spillover and Reallocation Excluding Switches to Main Construction



Notes: This figure shows the results of two triple differences specifications using different outcome variables (see Equation 3.3) and excluding switchers to the main construction sector from t to $t+2$. I use 95% confidence intervals. In the first panel, I use the two-year change in log daily wages as the outcome. In the second panel, I use the probability of switching establishments as the outcome variable, which takes the value 1 if the individual switched establishments from t to $t+2$ and 0 if she did not. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Author's calculations.

Furthermore, in the last two columns of Table 3.5, I compare workers who made at least one job-to-job transition to any establishment in the post-period (switcher) to workers who stayed in the same establishment during the post-period.[35] I find that switchers had higher wage growth during the post-period than stayers. Moreover, as the last column of Table 3.5 shows, the increase in wage growth stems mostly from switching to any establishment, not switching to the main construction sector.

In Figure 3.10, I use a slightly different approach by using sub-samples for stayers vs. switch-

[35] More specifically, I define a variable "Switch" which takes the value 1 if a worker changed establishments from t to $t+2$ in 1994–97 at least once, and 0 if a worker stayed at the same establishment in the 1994–97 period.

ers, i.e. comparing stayers to stayers and switchers to switchers over time. This approach should alleviate concerns that switchers generally have higher wage growth than stayers. Again, I find a higher change in wage growth after the public discussion and announcement of the main construction sector minimum wage for switchers compared to stayers. This finding is again not driven by switchers to the main construction sector, as excluding these switchers in Appendix Figure 3.B.5 shows. Thus, switchers to any establishment were driving the overall positive wage spillover effects for sub-minimum wage workers in outside option industries. Moreover, this analysis allows me to rule out the possibility that bargaining within the existing employment relationship drove the wage spillover effects. The increase in average wages at the establishment level, as depicted in Figure 3.7, in conjunction with the lack of change in wage growth for individual stayers, can be attributed to the movement of low-wage (treated) workers away from more exposed establishments. This leads to an increase in average wages for establishments that were more exposed, as a result of a composition of fewer low-wage workers within these establishments. As previously noted in Section 3.5.3, this supports the assertion that it is the actions and decisions of individual workers, rather than establishments, that are driving the spillover effects observed in this study.

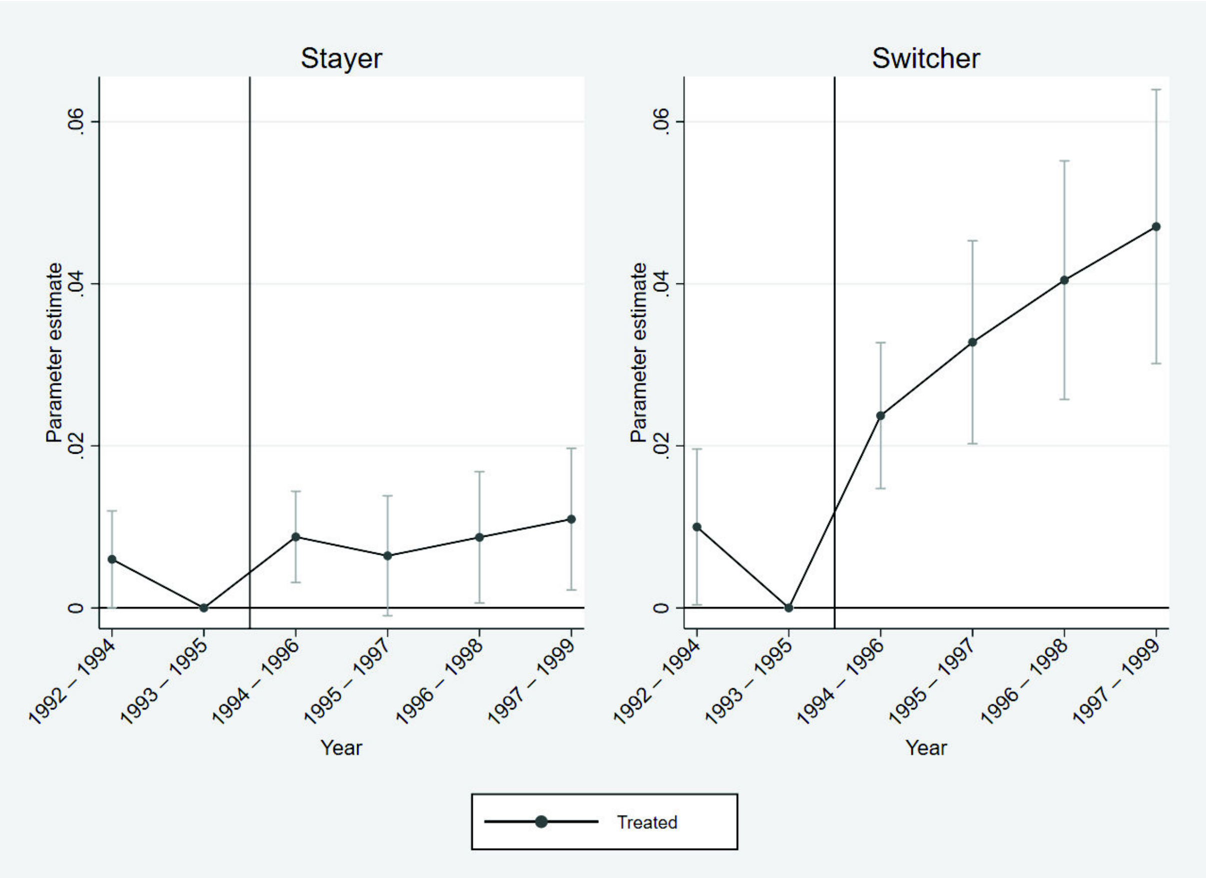
Taking stock, I have sketched a simple spatial model of strategic complementarity in this section. I tested the predictions of the model and found no or only weak evidence for strategic complementarity. Thus, strategic complementarity does not seem to explain the spillover effects from the main construction sector minimum wage. In the next chapter, I present a model that fits the patterns of the spillover effects better.

Biased Beliefs about Outside Options

In this section, I apply the theoretical model of Jäger et al. (2022) to my context and derive testable predictions. I present the main components of the model relevant to my context in Appendix 3.D and refer the interested reader to Jäger et al. (2022) for details. In the theoretical model of Jäger et al. (2022), workers form beliefs about their outside options in the labor market. Biased beliefs about outside options can cause workers to stay in lower-paying firms and receive marked down wages.

I derive testable predictions from this model for the context of this paper by modeling the public discussion, announcement, and introduction of the main construction sector minimum

Figure 3.10.: Triple Differences: Wage Spillover for Stayers vs. Switchers



Notes: This figure shows the results of two triple differences specifications using the two-year change in log daily wages as the outcome (see Equation 3.3). I define Stayers as workers who stayed within the same establishment during the 1994–97 period. Switchers are workers who changed establishments at least once from t to $t+2$ during 1994–97. For the left panel, I use a sub-sample of Stayers. For the right panel, I use a sub-sample of Switchers. I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Author's calculations.

wage as a reduction in information costs (c_A) and an update in beliefs about the highest wage attainable for a worker ($\widetilde{w}^{max}$). The public discussion, announcement, and introduction of the minimum wage informs workers on what they could potentially earn in the labor market. Given the high anchoring on current wages (Jäger et al., 2022), the public discussion, announcement, and introduction reduced biased beliefs about outside options in the labor market. This information shock should primarily affect workers who have similar job tasks as well as transferable skills (outside option industries) and earn a wage below the minimum wage (treated workers). Similarly, $\widetilde{w}^{max}$ (and w_j) can also be thought of as wage growth instead of a wage level. In the model, firms then could differ in their wage-tenure profiles which they offer to workers. The testable predictions are as follows:

A. **The reallocation of treated workers in outside option industries from low-wage to high-wage establishments increased.**

This prediction follows naturally from the Jäger et al. (2022) model. Through the publicly discussed and announced introduction of the main construction sector minimum wage, treated workers in outside option industries learn what wages they could earn in the labor market. They learn that they are working in a low-paying establishment that pays them a marked down wage with a lower wage-tenure profile, and as a result move to a better-paying establishment.

To test this prediction, I follow the approach in Dustmann et al. (2022). I define the change in the establishment j average wage or AKM establishment effect for worker i as $q^{l=t}_{j(i,t+2)} - q^{l=t}_{j(i,t)}$, where $q^{l}_{j(i,t+2)}$ denotes the time l characteristics of establishment j at which worker i is employed in year $t+2$. Thus, I measure the establishment average wage or AKM establishment effect in the baseline period t in both periods. For workers who remain employed at their baseline establishment from t to $t+2$, this measure of establishment quality is zero by construction. Using this approach, I make sure that any change in establishment average wage or AKM establishment effect reflects compositional changes only and not improvements in the quality of establishments over time.

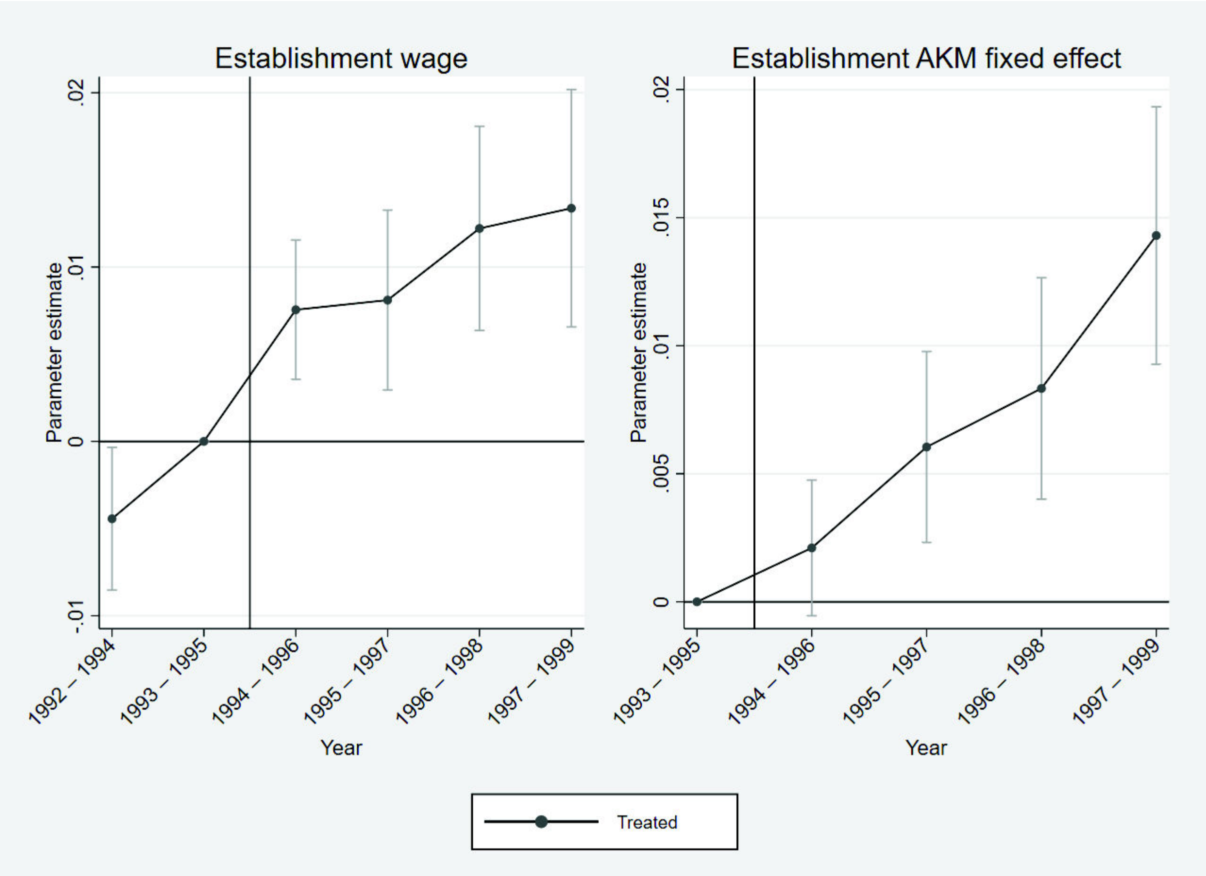
In the first panel of Figure 3.11, I show the results for the change in average establishment (daily) imputed wages.[36] I find that treated workers in outside option industries had a higher likelihood of switching to establishments which pay a higher average wage to their workforce in the post-period. Specifically, treated workers in outside option industries switched to establishments that on average have a 0.8% higher mean wage than their previous establishment in 1994–96 and up to 1.3% in 1997–99.

In the second panel of Figure 3.11, I show the triple differences results using the change in AKM establishment fixed effects as the outcome variable. While a negative coefficient would indicate that workers moved to establishments with a lower pay premium to the same worker type, a positive coefficient indicates that workers moved to establishments with a higher pay

[36] Specifically, I use the average imputed gross daily wage of an establishment's full-time employees provided by the IAB in the BHP and deflate this variable using the consumer price index of the Federal Statistical Office. In comparison to the censored wage variable, the imputed wage variable has the benefit that it can more accurately represent job-to-job transitions to establishments with better workforce composition. For details on the imputation procedure see Ganzer et al. (2022). Appendix Table 3.A.12 illustrates the results in table form and includes the number of observations and standard errors.

premium to the same worker type. Because the AKM effects for West and East Germany are only available from 1993 onward, I cannot estimate a pre-period placebo test for the baseline specification (see Section 3.3.2). The triple differences coefficient is statistically insignificant in 1994–96 and increases in size in the following years from 0.6% in 1995–97 to up to 1.4% in 1997–99.

Figure 3.11.: Triple Differences: Reallocation to Higher-Paying Establishments



Notes: This figure shows the results of two triple differences specifications using different outcome variables (see Equation 3.3). I use 95% confidence intervals. In the first panel, I use the change in log establishment average imputed wages as the outcome variable. Specifically, I use the average imputed gross daily wage of an establishment's full-time employees provided by the IAB in the BHP and deflate this variable using the consumer price index of the Federal Statistical Office. In the second panel, I use the change in establishment AKM fixed effects as the outcome variable. I measure establishment quality in both specifications in t. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. Appendix Table 3.A.12 illustrates these results in table form including the number of observations, standard errors, and partially treated group. **Source:** SIEED and BHP. Author's calculations.

In Appendix Table 3.B.3, I re-estimate the specifications in Figure 3.11 by excluding switches to the main construction sector from t to $t+2$ and by excluding establishments during their closing year from the sample. I find that the results presented here are not driven by switches to the main construction sector or by establishment closure. Rather, the results suggest that, consistent with the prediction of Jäger et al. (2022)'s model, more exposed low-wage workers switched to better-paying establishments after their biased beliefs about wages in the labor market were updated. Furthermore, the pre-trends of change in average establishment (daily) imputed wages become insignificant with the exclusion of main construction sector switchers from the specification, suggesting that the pre-trends were driven by switchers to the main construction sector in the past. Since the standard error remains the same in columns 1 and 2 of Appendix Table 3.B.3 for the period 1992–94, the statistical insignificance of the coefficient is not due to the lower number of observations and the resulting more imprecise estimation.

B. **The increase in wage growth was mainly due to switches in establishments, although not necessarily to switches to the main construction sector.**

The intuition for this prediction is similar to the intuition of prediction A. Workers in low-paying establishments learn about their establishment quality which pays them a marked down wage and reallocate to better paying establishments which pay them a higher wage.

In Table 3.5, Figure 3.10, and Appendix Figure 3.B.5, I showed that in contrast to the prediction of a spatial model with strategic complementarity, most of the wage growth stems from switching to any establishment and not from staying within the same establishment or switching to the main construction sector. The model in Jäger et al. (2022) can rationalize this result. Appendix 3.D Equation 3.D.11 models the job search decision of workers with biased beliefs and positive information costs. As workers update their biased beliefs about potential outside wages through the public discussion and announcement, they start searching for new jobs. Job search is not directed to the main construction sector in this case. Furthermore, the fact that the wage spillover effects and job-to-job transitions occurred precisely before the introduction of the minimum wage but after the public discussion and announcement in 1996, is also consistent with an information shock story.

C. **The spillover effects were heterogeneous by initial information cost level. Expert workers were not affected by the publicly announced introduction of the main construction sector minimum wage.**

The model in Jäger et al. (2022) distinguishes between employees with high information costs (amateurs) and employees with no information costs (experts). Only amateurs should be affected by the information shock. Experts were aware of the wages already above the minimum wage in establishments with collective bargaining agreements in the main construction sector (see Section 3.2). Consequently, the public discussion and announcement of the minimum wage, which is below the entry-level wage in establishments covered by collective agreements, should have had no effect on experts.

Since it is not possible to precisely identify amateurs and experts in my data set, I make two assumptions. First, non-German workers are more likely to have higher information frictions about their outside options in the labor market in general, compared to native workers. Second, I also expect workers with less labor market experience to have less information about possible outside options in the labor market than workers with more labor market experience.

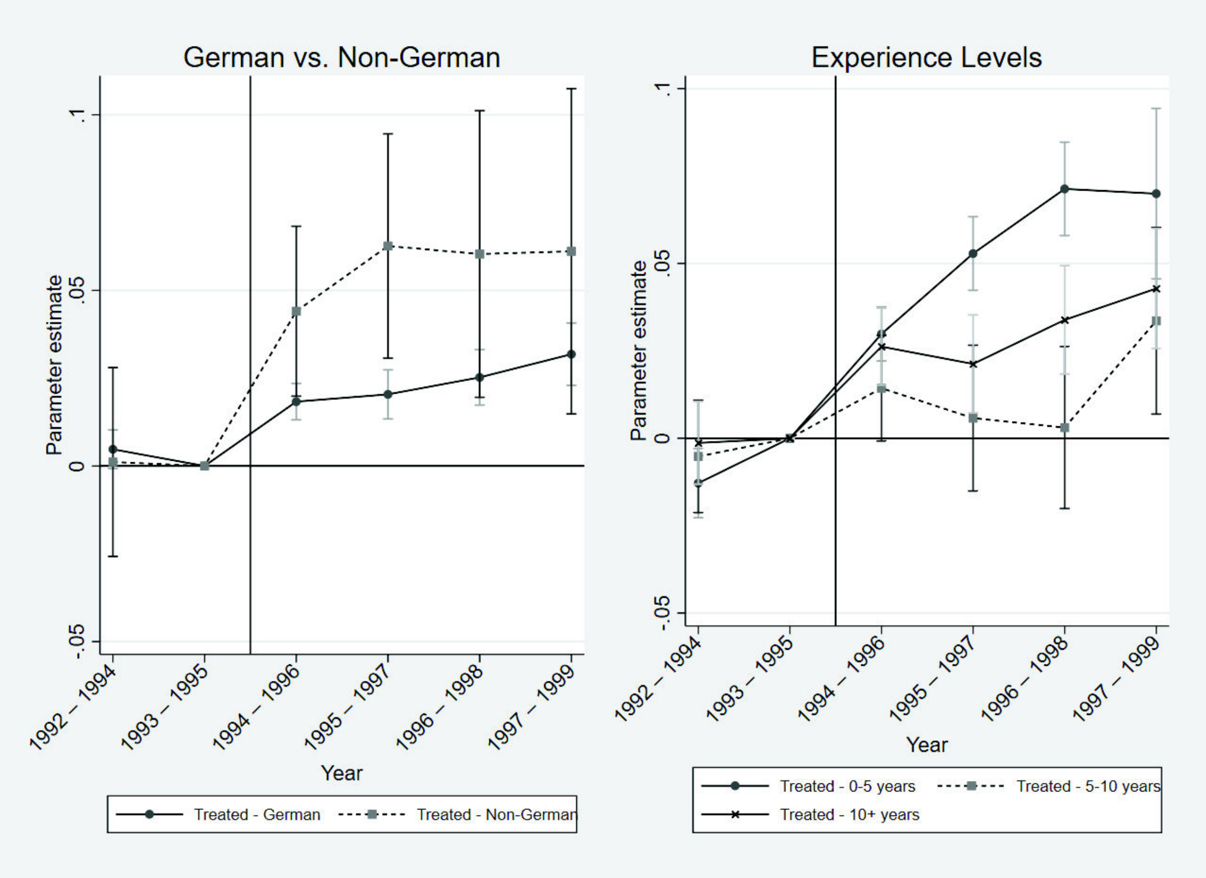
In Figure 3.12, I estimate the triple differences specification of Equation 3.3 separately for sub-samples of German, non-German, workers with 0 to 5 years, 5 to 10 years or more than 10 years of labor market experience.[37] In line with the model predictions, I find on average larger effects for non-German compared to German workers and larger effects for workers with only little labor market experience compared to workers with more labor market experience. Thus, the results suggest that workers who are more likely to have higher information frictions about their outside options also experienced on average higher wage spillover effects compared to their better informed counterparts, following the publicly discussed and announced introduction of the main construction sector minimum wage.

Prediction C can also rationalize the findings in the second column of Table 3.5. Namely, treated workers in LMRs with a lower share of the main construction sector experienced higher wage spillover effects compared to LMRs with a higher share of the main construction sector. Workers in LMRs with a higher share of the main construction sector were more likely to be informed about the already high entry-wages in the main construction sector. Consequently, the publicly discussed and announced introduction of the main construction sector minimum wage should only be an information shock for workers in LMRs with a low share of the main construction sector.

Taking stock, I presented the theoretical model of Jäger et al. (2022) and applied its insights

[37] Appendix Table 3.A.13 shows the results in table form including the partially treated group, the number of observations and standard errors.

Figure 3.12.: Triple Differences: Heterogeneity in Wage Spillover Effects



Notes: This figure illustrates the results of the triple differences specification with the two-year change in log daily wages as the outcome (see Equation 3.3). I use 95% confidence intervals. The figure illustrates the coefficients only for treated workers. In the first panel, I present the results separately for sub-samples of workers with German nationality and workers with non-German nationality. In the second panel, I present the results separately for sub-samples of workers with 0 to 5 years, 5 to 10 years, and 10+ years of labor market experience. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Authors' calculations.

to my context. The results suggest that as a result of the publicly discussed and announced introduction of the minimum wage treated workers in outside option industries updated their biased beliefs about the wages they could earn in the labor market. The information shock revealed information about workers' current establishment quality. Therefore, workers moved to better-paying establishments and experienced higher wage growth.

3.6. Conclusion

Firms differ in the wages they pay to equally skilled workers even if they are in similar jobs. Wage and information shocks related to potential outside options for workers currently in bad jobs could shed light on why workers stay in those bad jobs in the first place. In this paper, I investigate whether and why publicly discussed and announced sectoral minimum wages had spillover effects on sub-minimum wage workers outside the targeted sectors in similar jobs. I find that sub-minimum wage workers in outside option industries experienced an increase in their wage growth that was driven by switching to new jobs in establishments with better average pay and higher wage premium to the same type of worker. I find that the reduction of information frictions, due to the extensive public discussion and announcement of the main construction sector minimum wage in the media, seems to have been the most likely mechanism for the positive wage spillover effects. Thus, the public discussion and announcement of sectoral minimum wages had an unexpected benefit, informing workers with bad jobs of their possible outside options and encouraging them to look for new and better-paying jobs. The unsolicited public disclosure of the minimum wages, along with its prominent placement in the media, set them apart from other wage transparency laws and may account for their effectiveness (Brütt and Yuan, 2022).

Using the same data and identification strategy, I find that the spillover effects are about one-third of the wage effects within the main construction sector. A back-of-the-envelope calculation suggests that those exposed to the spillover effects earned on average 383 Euro more every year after the public discussion and announcement of the minimum wage than they would have earned without the public discussion and announcement.[38] If we take into account that sub-minimum wage workers earned an average of 19,188 Euro annually before the minimum wage was announced, this shows that the spillover effects have led to a substantial improvement in the income situation of low-wage employees. Moreover, because low-paying establishments are less productive than high-paying establishments (Abowd et al., 1999), the reallocation of employment from low-paying establishments to high-paying establishments may have increased the welfare of the economy as a whole.

[38]On average, sub-minimum wage workers in my sample earned 52.57 Euro daily before the public announcement of the minimum wage (Table 3.2). Two-year wage growth was 11% before the public announcement. Thus, daily wages grew by an average of 5.78 Euro every two years. After the public announcement, the daily wage grew by 13% every two years and thus by an average of 6.83 Euro every two years. For a continuously employed person this means on average (1.05 Euro $\times$ 365 days) 383 Euro more every year.

The current German government is again increasingly thinking in the direction of generally binding collective agreements in order to set sectoral minimum wages. Two of the three governing parties have announced in their government programs that they will facilitate the introduction of generally binding collective agreements (Greens, 2021; SPD, 2017, 2021). In the coalition agreement, the government parties agreed to tie public payments to compliance with a representative collective agreement for the respective sector (SPD et al., 2021). In this context, the current German government has already passed the *Gesundheitsversorgungsweiterentwicklungs-gesetz* (Health Care Advancement Act), which will restrict public payments to care facilities that pay their employees according to collective agreements. In this paper, I have shown that publicly disclosed sectoral collective agreements can have a significant signaling effect on the low-wage labor market and thus have positive wage and reallocation effects far beyond the boundaries of the sector actually affected.

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Appendix

3.A. Additional Tables and Figures

Table 3.A.1.: Classification of Sectoral Minimum Wages

Sector	**WZ73 (1975–2002)**	**WZ93 (1999–2003)**	**WZ03 (2003–2008)**	**WZ08 (from 2008)**	**First MW**
Main Construction	590/ 591/ 592/ 593/ 594/ 600/ 614	45.11.2/ 45.11.4/ 45.12.0/ 45.21.1-45.21.7/ 45.22.2/ 45.22.3/ 45.23.1/ 45.23.2-45.25.3/ 45.25.5/ 45.25.6/ 45.32.0/ 45.41.0/ 45.43.2/ 45.43.3/ 45.50.0	45.11.2/ 45.11.4/ 45.12.0/ 45.21.1-45.21.7/ 45.22.2/ 45.22.3/ 45.23.1-45.25.3/ 45.25.5/ 45.25.6/ 45.32.0/ 45.41.0/ 45.43.2/ 45.43.3/ 45.50.1/ 45.50.2	41.20.1-42.99.0/ 43.12.0/ 43.13.0/ 43.29.1/ 43.31.0/ 43.33.0/ 43.91.2-43.99.9	01/1997
Electrical Trade	611	45.31.0	45.31.0	43.21.0	06/1997
Roofing	601	45.22.1	45.22.1	43.91.1	10/1997
Painting & Varnishing	211/ 613	28.51.0/ 45.44.1	28.51.0/ 45.44.1	25.61.0/ 43.34.1	12/2003
Commercial Cleaning		74.70.1/ 74.70.3/ 74.70.4	74.70.1/ 74.70.3/ 74.70.4	81.21.0/ 81.22.9-81.29.9	04/2004
Waste Removal			37.10.1/ 37.10.2/ 37.20.1-37.20.5/ 90.02.1-90.02.5/ 90.03.0	38.11.0-39.00.0	01/2010
Nursing Care			85.31.5/ 85.31.7/ 85.32.6	87.10.0/ 88.10.1	08/2010
Security			74.60.2	80.10.0/ 80.20.0	06/2011
Temporary Work			74.50.2	78.20.0/ 78.30.0	01/2012

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Table 3.A.1 – continued from previous page

Sector	WZ73 (1975–2002)	WZ93 (1999–2003)	WZ03 (2003–2008)	WZ08 (from 2008)	First MW
Scaffolding				43.99.1	08/2013
Stonemasonry				23.70.0	10/2013
Hairdressing				96.02.1	11/2013
Chimney Sweeping				81.22.1	04/2014
Slaughtering & Meat Processing				10.11.0-10.13.0	08/2014
Textile & Clothing				13.10.0-14.39.0	01/2015
Agriculture, Forestry & Gardening				01.11.0-02.40.0/ 03.12.0-03.22.0	01/2015

Table 3.A.2.: Descriptives for Minimum Wage Sectors ($t-5$ to $t-1$)

	Main Construction	Electrical Trade	Roofing	Painting & Varnishing	Commercial Cleaning	Waste Removal	Nursing Care	Security	Temporary Work	Scaffolding	Stonemasonry	Hairdressing	Chimney Sweeping	Slaughtering & Meat Processing
Panel A: West Germany														
Bite (for main sample restrictions)	5.82	9.38	5.73	6.89	26.81	2.18	15.24	13.86	28.55	39.35	10.27	46.22	5.27	11.78
Share in the economy	5.59	0.78	0.42	0.79	0.69	0.54	1.48	0.33	4.55	0.14	0.08	0.42	0.04	0.51
Share of full-time workers	93.35	79.78	89.31	76.00	19.05	82.79	38.98	55.93	72.38	74.62	62.15	39.97	50.00	58.78
Share of part-time workers	2.30	3.83	2.72	4.87	22.78	4.53	34.86	5.57	14.22	6.10	5.69	13.73	7.14	13.84
Share of women	9.11	16.29	8.07	21.45	69.05	16.81	80.57	19.03	43.62	8.87	19.85	91.87	36.67	56.33
Share of full-time women (full-time)	7.22	14.42	6.12	13.26	34.46	11.17	71.90	13.65	29.71	4.40	8.46	90.76	8.96	41.06
Share of full-time entrants	88.39	71.18	85.09	59.52	15.90	73.60	31.68	48.45	69.99	71.23	54.69	34.57	37.17	52.72
Share low-skill (full-time)	13.44	4.52	14.00	12.26	33.20	16.27	7.84	10.38	17.88	27.67	6.47	4.63	2.56	12.67
Share middle-skill (full-time)	79.33	93.16	83.32	84.51	57.90	77.10	81.50	84.53	70.82	63.98	88.56	93.51	96.38	80.39
Share high-skill (full-time)	5.88	1.77	2.44	2.55	4.62	5.43	10.00	3.80	10.23	4.12	2.89	1.11	0.64	5.94
Share non-German nationality (full-time)	15.32	8.70	10.46	10.90	32.12	5.40	3.92	8.89	16.67	34.76	26.07	9.33	0.43	10.04
Panel B: East Germany														
Bite (for main sample restrictions)	25.15	14.84	20.05	9.17	56.81	12.99	20.30	61.52	43.52	39.93	59.22	73.28	12.82	51.03
Share in the economy	10.04	1.48	0.39	1.06	1.47	1.32	2.57	0.63	3.82	0.11	0.09	0.67	0.03	0.64
Share of full-time workers	92.96	89.14	88.53	81.78	27.94	80.15	37.98	69.27	78.89	79.84	81.03	47.32	42.39	70.39
Share of part-time workers	1.24	2.53	1.33	1.83	33.94	3.76	48.51	4.15	7.97	5.94	6.32	31.31	16.85	11.74
Share of women	8.28	11.24	6.86	11.15	68.04	19.56	82.82	19.03	28.42	10.08	26.88	93.65	42.39	54.48
Share of full-time women (full-time)	7.68	9.72	6.45	8.76	59.93	14.86	77.84	15.48	17.90	8.09	22.44	93.13	3.85	54.27
Share of full-time entries	91.49	87.00	87.38	74.04	21.49	63.11	29.41	53.11	77.45	72.83	77.92	36.41	37.50	47.61
Share low-skill (full-time)	3.80	2.18	4.75	3.29	17.56	6.40	3.27	1.86	4.04	2.59	2.93	1.85	6.41	9.61
Share middle-skill (full-time)	89.66	92.12	92.90	94.17	75.34	85.34	82.36	91.39	92.14	93.20	88.78	97.16	88.46	75.41
Share high-skill (full-time)	5.70	4.56	1.11	2.15	3.82	7.78	13.52	6.52	3.61	3.56	5.37	0.87	0.00	4.52
Share non-German nationality (full-time)	3.32	1.28	1.24	1.15	9.89	0.51	1.48	0.47	0.78	0.65	4.88	0.38	0.00	12.72

Notes: This table shows descriptive statistics for the minimum wage sectors. The bite is calculated for the sample restrictions mentioned in Section 3.3.2. All other descriptives are calculated in each case in t-5 to t-1 before the introduction of the respective minimum wage using the full SIEED and BHP data. For example, the descriptives in column "Main Construction" are calculated from 1992 to 1996. All rows followed by the parentheses "(full-time)" are calculated by using the number of all full-time workers in the respective minimum wage sector as the denominator.
Source: SIEED and BHP. Authors' calculations.

Table 3.A.3.: List of Outside Option Industries (Main Construction Sector)

No.	Description
11	Growing of crops; market gardening; horticulture
12	Farming of animals
13	Growing of crops combined with farming of animals (mixed farming)
14	Agricultural and animal husbandry service activities, except veterinary activities
20	Forestry, logging and related service activities
102	Mining and agglomeration of lignite
103	Extraction and agglomeration of peat
111	Extraction of crude petroleum and natural gas
112	Service activities incidental to oil and gas extraction, excluding surveying
131	Mining of iron ores
141	Quarrying of stone
142	Quarrying of sand and clay
143	Mining of chemical and fertilizer minerals
144	Production of salt
145	Other mining and quarrying n.e.c.
201	Sawmilling and planing of wood; impregnation of wood
202	Manufacture of veneer sheets;
	manufacture of plywood, laminboard, particle board, fibre board and other panels and boards
203	Manufacture of builders' carpentry and joinery
204	Manufacture of wooden containers
261	Manufacture of glass and glass products
264	Manufacture of bricks, tiles and construction products, in baked clay
265	Manufacture of cement, lime and plaster
266	Manufacture of articles of concrete, plaster and cement
267	Cutting, shaping and finishing of stone
281	Manufacture of structural metal products
282	Manufacture of tanks, reservoirs and containers of metal;

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Table 3.A.3 – continued from previous page

No.	Description
	manufacture of central heating radiators and boilers
283	Manufacture of steam generators, except central heating hot water boilers
285	Treatment and coating of metals; general mechanical engineering
355	Manufacture of other transport equipment n.e.c.
361	Manufacture of furniture
364	Manufacture of sports goods
371	Recycling of metal waste and scrap
372	Recycling of non-metal waste and scrap
451	Site preparation
452	Building of complete constructions or parts thereof; civil engineering
454	Building completion
455	Renting of construction or demolition equipment with operator
701	Real estate activities with own property
703	Real estate activities on a fee or contract basis
713	Renting of other machinery and equipment
742	Architectural and engineering activities and related technical consultancy
900	Sewage and refuse disposal, sanitation and similar activities

Table 3.A.4.: List of Non-Outside Option Industries (Main Construction Sector)

Industry	
No.	Description
15	Hunting, trapping and game propagation, including related service activities
233	Processing of nuclear fuel
242	Manufacture of pesticides and other agro-chemical products
403	Steam and hot water supply
523	Retail sale of pharmaceutical and medical goods, cosmetic and toilet articles
603	Transport via pipelines
621	Scheduled air transport
623	Space transport
642	Telecommunications
651	Monetary intermediation
724	Database activities
726	Other computer related activities
732	Research and experimental development on social sciences and humanities
801	Primary education
851	Human health activities
912	Activities of trade unions
924	News agency activities
930	Other service activities

Table 3.A.5.: Triple Differences: Wage Spillover Effects of the Main Construction Sector Minimum Wage

	(1)	(2)	(3)	(4)	(5)
Treated x Option					
x 1992-94	-0.004	0.000	0.005*	0.005*	0.006**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
x 1994-96	0.012***	0.010***	0.020***	0.020***	0.019***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
x 1995-97	0.010***	0.007*	0.022***	0.022***	0.022***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
x 1996-98	0.011***	0.007*	0.026***	0.027***	0.025***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
x 1997-99	0.012***	0.007*	0.033***	0.033***	0.032***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Partial x Option					
x 1992-94	-0.009***	-0.008***	-0.006***	-0.006***	-0.005***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
x 1994-96	0.012***	0.009***	0.009***	0.009***	0.009***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
x 1995-97	0.013***	0.006***	0.008***	0.007***	0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
x 1996-98	0.015***	0.007***	0.010***	0.009***	0.009***

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Table 3.A.5 – continued from previous page

	(1)	(2)	(3)	(4)	(5)
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
x 1997-99	0.012***	0.002	0.008***	0.008***	0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
No. of observations	796,968	796,968	761,276	761,276	757,763
No. of workers	213,339	213,339	177,647	177,647	176,786
Year fixed effects	yes	yes	yes	yes	yes
Demographic controls	no	yes	no	no	no
1-digit industry fixed effects	no	yes	no	yes	no
3-digit industry fixed effects	no	no	no	no	yes
Federal state fixed effects	no	yes	no	yes	yes
Region type fixed effects	no	yes	no	yes	yes
Worker fixed effects	no	no	yes	yes	yes

Notes: This table shows the results of different triple differences specifications with the two-year change in log daily wages as the outcome using different controls (see Equation 3.3). Intuitively, the estimator compares the DiD of workers in industries listed in Table 3.A.3 with workers in industries listed in Table 3.A.4. Standard errors (in parentheses) are clustered at the worker level. In column (1), I only use year fixed effects. In column (2), I add demographic controls, 1-digit industry, federal state and region type fixed effects. In column (3), I use worker fixed effects with only the year fixed effects. In column (4), I present my baseline specification by using worker fixed effects and all controls, excluding demographic controls. In column (5), I use a similar specification as column (4) but with 3-digit industry fixed effects instead of 1-digit industry fixed effects. The reference period is 1993–95. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Source: SIEED and BHP. Author's calculations.

Table 3.A.6.: Difference-in-Differences: Spillover Effects of the Main Construction Sector Minimum Wage, Separately by Non-Outside vs. Outside Option Industries

	Non-outside option	Outside option
Treated		
x 1992-94	0.010***	0.016***
	(0.002)	(0.002)
x 1994-96	0.000	0.020***
	(0.002)	(0.002)
x 1995-97	0.001	0.024***
	(0.002)	(0.003)
x 1996-98	-0.004*	0.022***
	(0.002)	(0.003)
x 1997-99	-0.010***	0.023***
	(0.003)	(0.003)
Partial		
x 1992-94	0.004***	-0.001
	(0.001)	(0.001)
x 1994-96	0.002*	0.010***
	(0.001)	(0.001)
x 1995-97	0.003**	0.010***
	(0.001)	(0.002)
x 1996-98	-0.001	0.008***

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Table 3.A.6 – continued from previous page

	Non-outside option	Outside option
	(0.001)	(0.002)
x 1997-99	-0.001	0.007***
	(0.002)	(0.002)
No. of observations	394,299	364,929
No. of workers	88,739	88,947
Year fixed effects	yes	yes
1-digit Industry fixed effects	yes	yes
Federal state fixed effects	yes	yes
Region type fixed effects	yes	yes
Worker fixed effects	yes	yes

Notes: This table shows the results of two difference-in-differences specifications. In the column "non-outside option" the table shows the DiD estimates for the industries listed in Table 3.A.4 and in column "outside option" the estimator shows the DiD estimates for the industries listed in Table 3.A.3. Standard errors (in parentheses) are clustered at the worker level. In both columns I use year fixed effects, 1-digit industry fixed effects, federal state fixed effects, region type fixed effects and worker fixed effects. The reference period is 1993–95. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Source: SIEED and BHP. Authors' calculations.

Table 3.A.7.: Triple Differences: Change in Wage Growth within the Main Construction Sector

	Within main construction	Spillover outside main construction
Treated x Option x Post	0.066***	0.021***
	(0.003)	(0.003)
Partial x Option x Post	0.011***	0.011***
	(0.001)	(0.001)
No. of observations	738,117	761,276
No. of workers	163,189	177,647
Year fixed effects	yes	yes
1-digit industry fixed effects	yes	yes
Federal state fixed effects	yes	yes
Region type fixed effects	yes	yes
Worker fixed effects	yes	yes

Notes: Standard errors in parentheses. The table shows specifications using different versions of Equation 3.4. In the first column, I compare treated workers to control group workers in the main construction sector with the same comparison in non-outside option industries. For comparison, I show the pre-post spillover specification for outside option industries vs. non-outside option industries in the second column. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.
Source: SIEED and BHP. Authors' calculations.

Table 3.A.8.: Triple Differences: Probability to Switch Establishments

	Job-to-job
Treated x Option	
x 1992-94	-0.015**
	(0.006)
x 1994-96	0.056***
	(0.006)
x 1995-97	0.055***
	(0.008)
x 1996-98	0.007
	(0.009)
x 1997-99	-0.009
	(0.010)
Partial x Option	
x 1992-94	-0.004
	(0.006)
x 1994-96	0.059***
	(0.005)
x 1995-97	0.064***
	(0.006)
x 1996-98	0.015**

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Table 3.A.8 – continued from previous page

	Job-to-job
	(0.007)
x 1997-99	0.009
	(0.007)
No. of observations	796,763
No. of workers	194,574
Year fixed effects	yes
1-digit Industry fixed effects	yes
Federal state fixed effects	yes
Region type fixed effects	yes
Worker fixed effects	yes

Notes: This table shows the results of a triple differences specifications using the probability of switching establishments as the outcome variable (see Equation 3.3). The variable takes the value 1 if the individual switched establishments from t to $t+2$ and 0 if she did not. Standard errors (in parentheses) are clustered at the worker level. The reference period is 1992–94. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Source: SIEED and BHP. Author's calculations.

Table 3.A.9.: Establishment Level: Difference-in-Differences Estimations on Wages and Employment

	Log average wage	Log number of employment
Exposure		
x 1992	-0.049***	-0.010
	(0.014)	(0.062)
x 1993	-0.018*	0.009
	(0.010)	(0.047)
x 1994	-0.003	0.016
	(0.006)	(0.024)
x 1996	0.008	0.015
	(0.006)	(0.044)
x 1997	0.003	0.033
	(0.009)	(0.070)
x 1998	-0.001	-0.051
	(0.010)	(0.081)
x 1999	-0.003	0.089
	(0.012)	(0.125)
No. of observations	146,826	146,826
No. of establishments	21,649	21,649

Notes: This table shows the results of two difference-in-differences estimations on the establishment level using Equation 3.5. The outcome variable in the first column is the log average wage in an establishment. The outcome variable in the second column is the log number of full-time employees in an establishment. Standard errors (in parentheses) are clustered at the establishment level. The reference period is 1995. All estimations are weighted by the average number of full-time employees within establishments in the 1992–95 pre-period. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Source: SIEED and BHP. Author's calculations.

Table 3.A.10.: Establishment Level: Triple Differences Estimations on Wages and Employment

	Log average wage	Log number of employment
Exposure x Option		
x 1992	-0.018	-0.147
	(0.025)	(0.138)
x 1993	-0.014	-0.160
	(0.021)	(0.140)
x 1994	-0.016	-0.021
	(0.011)	(0.071)
x 1996	0.004	-0.059
	(0.013)	(0.063)
x 1997	0.024	-0.339*
	(0.019)	(0.194)
x 1998	0.053***	-0.248
	(0.018)	(0.173)
x 1999	0.062**	0.039
	(0.030)	(0.469)
No. of observations	43,237	43,237
No. of establishments	6,303	6,303

Notes: This table shows the results of two triple estimations on the establishment level using Equation 3.6. Intuitively, the estimator compares the DiD of establishments in industries listed in Table 3.A.3 with the DiD of establishment in industries listed in Table 3.A.4. The outcome variable in the first column is the log average wage in an establishment. The outcome variable in the second column is the log number of full-time employees in an establishment. Standard errors (in parentheses) are clustered at the establishment level. The reference period is 1995. All estimations are weighted by the average number of full-time employees within establishments in the 1992–95 pre-period. Significance: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Source: SIEED and BHP. Author's calculations.

Table 3.A.11.: Tests of Strategic Complementarity Model Predictions. Full table

	Baseline	Tercile share	Bite (West Germany)	Bite (East Germany)	Switcher 1	Switcher 2
Treated x Option x Post	0.021***	0.046***	0.055***	0.054***	0.006**	0.006**
	(0.003)	(0.005)	(0.003)	(0.006)	(0.003)	(0.003)
Partial x Option x Post	0.011***	0.018***	0.011***	0.032***	0.005***	0.004***
	(0.001)	(0.003)	(0.001)	(0.005)	(0.001)	(0.001)
Treated x Option x Middle x Post		-0.037***				
		(0.007)				
Treated x Option x High x Post		-0.025***				
		(0.007)				
Partial x Option x Middle x Post		-0.011***				
		(0.004)				

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Table 3.A.11 – continued from previous page

	Baseline	Tercile share	Bite (West Germany)	Bite (East Germany)	Switcher 1	Switcher 2
Partial x Option x High x Post		-0.010***				
		(0.004)				
Treated x Option x Bite x Post			-0.004	0.018***		
			(0.003)	(0.006)		
Partial x Option x Bite x Post			-0.001	0.013***		
			(0.001)	(0.005)		
Treated x Option x Switch x Post					0.021***	0.021***
					(0.005)	(0.005)
Partial x Option x Switch x Post					0.012***	0.016***
					(0.003)	(0.003)

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Table 3.A.11 – continued from previous page

	Baseline	Tercile share	Bite (West Germany)	Bite (East Germany)	Switcher 1	Switcher 2
No. of observations	761,276	752,408	817,826	176,319	761,276	746,624
No. of workers	177,647	175,700	150,801	42,836	177,647	173,237
Year fixed effects	yes	yes	yes	yes	yes	yes
1-digit industry fixed effects	yes	yes	yes	yes	yes	yes
Federal state fixed effects	yes	yes	yes	yes	yes	yes
Region type fixed effects	yes	yes	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes	yes	yes
LMR fixed effects	no	yes	yes	yes	no	no
Excluding mcs switchers?	no	no	no	no	no	yes

Notes: Standard errors in parentheses. The table displays specifications of Equation 3.4 with 2-year change in log (daily) wages as the outcome. Column 2 shows the interactions with the main construction sector share terciles in LMRs. Columns 3 and 4 show interaction with the bite of the main construction sector minimum wage. Here, the sample is split between West and East Germany, with West Germany including years 1989–1991. All specifications include year, industry, federal state, region type, and worker fixed effects. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Source: SIEED and BHP. Authors' calculations.

Table 3.A.12.: Triple Differences: Reallocation to Higher-Paying Establishments

	Establishment mean wage	Establishment AKM fixed effect
Treated x Option		
x 1992-94	-0.004**	
	(0.002)	
x 1994-96	0.008***	0.002
	(0.002)	(0.001)
x 1995-97	0.008***	0.006***
	(0.003)	(0.002)
x 1996-98	0.012***	0.008***
	(0.003)	(0.002)
x 1997-99	0.013***	0.014***
	(0.003)	(0.003)
Partial x Option		
x 1992-94	-0.006***	
	(0.001)	
x 1994-96	0.004***	0.002***
	(0.001)	(0.001)
x 1995-97	0.007***	0.005***
	(0.002)	(0.001)
x 1996-98	0.008***	0.007***

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Table 3.A.12 – continued from previous page

	Establishment mean wage	Establishment AKM fixed effect
	(0.002)	(0.001)
x 1997-99	0.005***	0.006***
	(0.002)	(0.001)
No. of observations	693,303	509,298
No. of workers	174,569	140,934
Year fixed effects	yes	yes
1-digit Industry fixed effects	yes	yes
Federal state fixed effects	yes	yes
Region type fixed effects	yes	yes
Worker fixed effects	yes	yes

Notes: This table shows the results of two triple differences specifications using different outcome variables (see Equation 3.3). Intuitively, the estimator compares the DiD of workers in industries listed in Table 3.A.3 with workers in industries listed in Table 3.A.4. In the first column, I use the change in log establishment average wages as the outcome variable. Specifically, I use the average imputed gross daily wage of an establishment's full-time employees provided by the IAB in the BHP and deflate this variable using the consumer price index of the Federal Statistical Office. In the second column, I use the change in establishment AKM fixed effects as the outcome variable. I measure establishment quality in both specifications in t. Standard errors (in parentheses) are clustered at the worker level. The reference period is 1993–95. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Source: SIEED and BHP. Author's calculations.

Table 3.A.13.: Triple Differences: Wage Spillover Effects by Socio-Demographic Characteristics

	German	**Foreign**	**0 - 5 years exp.**	**5 - 10 years exp.**	**10+ years exp.**
Treated x Option					
x 1992-94	0.005*	0.001	-0.013**	-0.005	-0.001
	(0.003)	(0.014)	(0.005)	(0.008)	(0.006)
x 1994-96	0.018***	0.044***	0.030***	0.014*	0.026***
	(0.003)	(0.012)	(0.004)	(0.008)	(0.006)
x 1995-97	0.020***	0.063***	0.053***	0.006	0.021***
	(0.004)	(0.016)	(0.005)	(0.011)	(0.007)
x 1996-98	0.025***	0.060***	0.071***	0.003	0.034***
	(0.004)	(0.021)	(0.007)	(0.012)	(0.008)
x 1997-99	0.032***	0.061**	0.070***	0.034**	0.043***
	(0.005)	(0.024)	(0.012)	(0.014)	(0.009)

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Table 3.A.13 – continued from previous page

	German	Foreign	0 - 5 years exp.	5 - 10 years exp.	10+ years exp.
Partial x Option					
x 1992-94	-0.006***	-0.005	-0.015***	-0.006	0.003
	(0.002)	(0.007)	(0.005)	(0.004)	(0.002)
x 1994-96	0.008***	0.013**	0.009***	0.007*	0.012***
	(0.002)	(0.006)	(0.003)	(0.004)	(0.002)
x 1995-97	0.006***	0.014*	0.012***	0.007	0.017***
	(0.002)	(0.007)	(0.004)	(0.005)	(0.003)
x 1996-98	0.009***	0.014*	0.026***	0.016***	0.019***
	(0.002)	(0.008)	(0.005)	(0.005)	(0.003)
x 1997-99	0.007***	0.014*	0.007	0.026***	0.025***
	(0.002)	(0.009)	(0.009)	(0.006)	(0.003)

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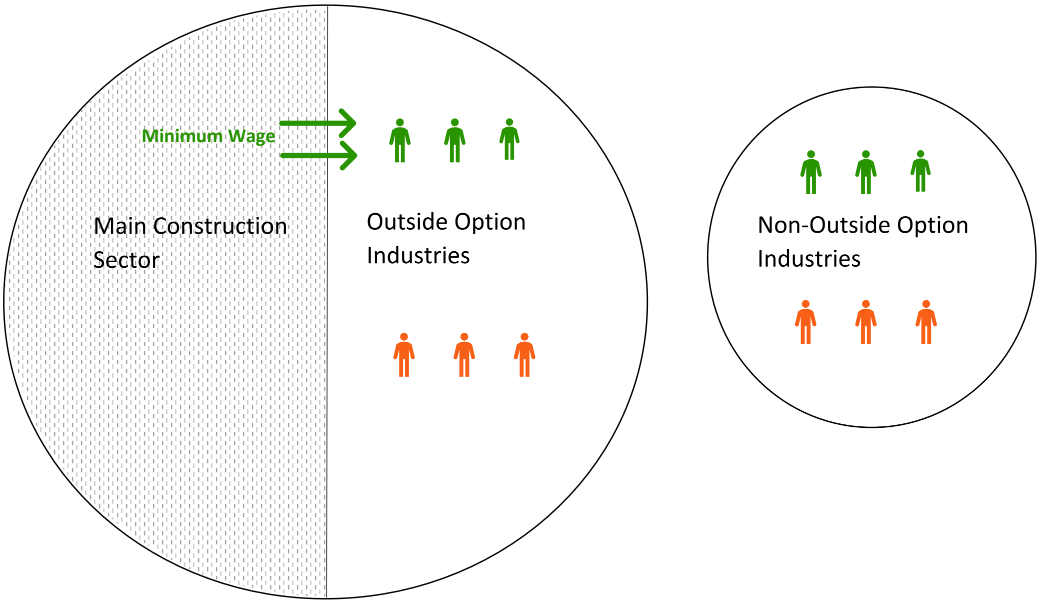
Table 3.A.13 – continued from previous page

	German	**Foreign**	**0 - 5 years exp.**	**5 - 10 years exp.**	**10+ years exp.**
No. of observations	713,851	46,503	285,336	164,313	261,290
No. of workers	166,763	11,014	84,866	55,516	62,428
Year fixed effects	yes	yes	yes	yes	yes
1-digit Industry fixed effects	yes	yes	yes	yes	yes
Federal state fixed effects	yes	yes	yes	yes	yes
Region type fixed effects	yes	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes	yes

Notes: This table shows the results of multiple triple differences specifications with the two-year change in log daily wages as the outcome separately for workers with different nationality and workers with different levels of labor market experience (see Equation 3.3). Intuitively, the estimator compares the DiD of workers in industries listed in Table 3.A.3 with workers in industries listed in Table 3.A.4. Standard errors (in parentheses) are clustered at the worker level. The reference period is 1993–95. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

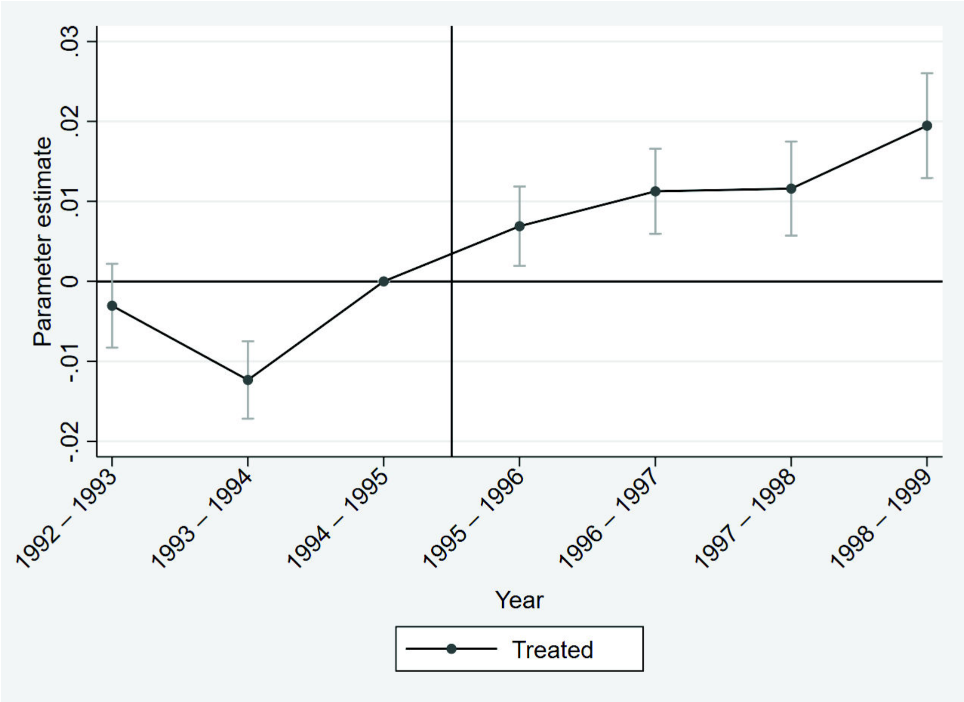
Source: SIEED and BHP. Author's calculations.

Figure 3.A.1.: Illustration of the Triple Differences Identification Strategy



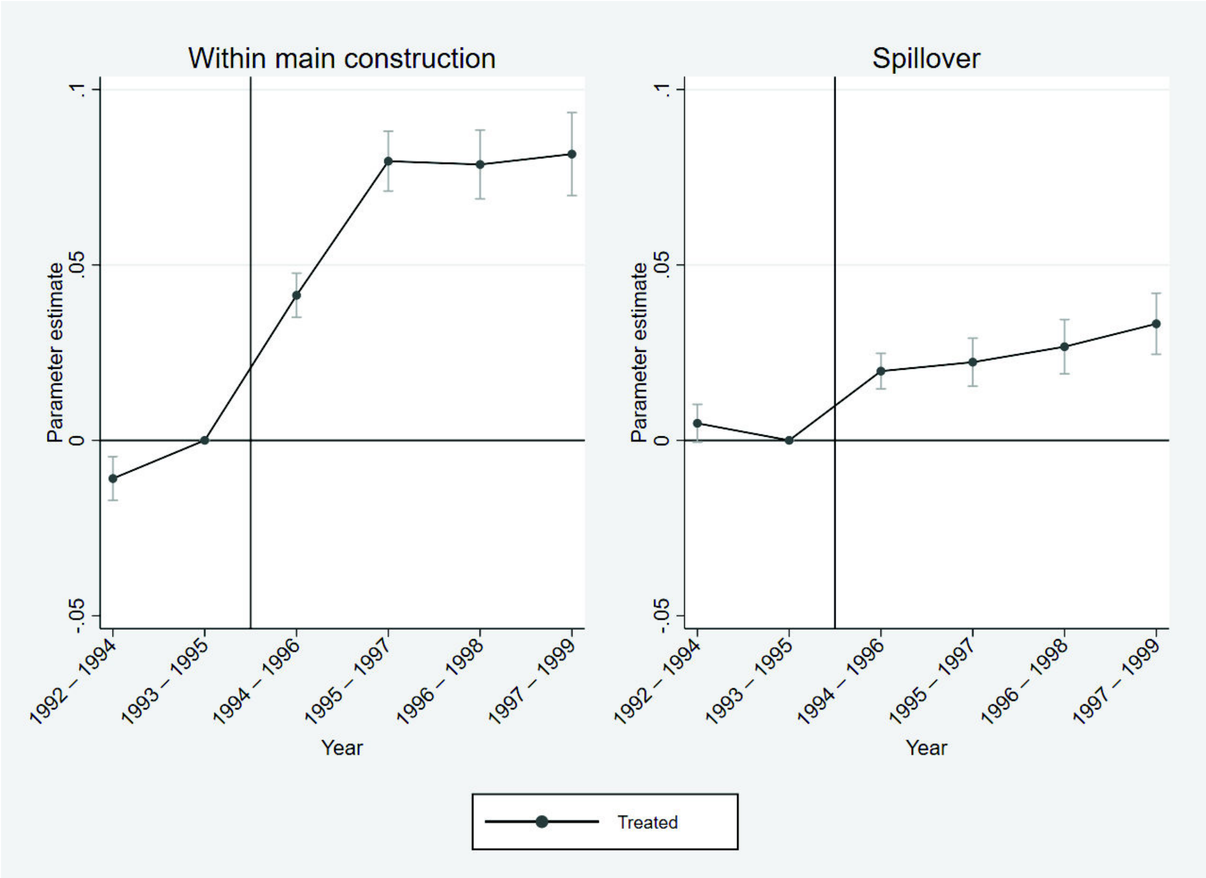
Notes: This figure illustrates the triple differences identification strategy from Equation 3.3. The green individuals in the top half of the figure represent the treated workers, while the orange individuals in the bottom half of the image represent the control group. The main construction sector and outside option industries share one common labor market. However, because the minimum wage was only implemented in the main construction sector, there is a dividing line between these two sectors. The area for the main construction sector is dot-filled gray because I concentrate on the spillover effects on the outside option industries in this paper rather than the within-sector effects. I expect this minimum wage to have spillover effects on treated workers in outside option industries. Non-outside option industries are outside this common labor market and serve as an additional control group.

Figure 3.A.2.: Triple Differences: Wage Spillover Effects of the Main Construction Sector Minimum Wage. 1-Year Wage Growth Changes



Notes: This figure illustrates the results of the triple differences specification with the one-year change in log daily wages as the outcome (see Equation 3.3). I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1994–95. **Source:** SIEED and BHP. Authors' calculations.

Figure 3.A.3.: Triple Differences: Wage Growth Effects within the Main Construction Sector



Notes: In the first panel of this figure, I estimate the within-effects of the minimum wage in the main construction sector by using a similar triple differences specification as in Equation 3.3. The only difference is that I compare the DiD in the main construction sector itself with the non-outside option industries. For comparison, the second panel shows the baseline specification with triple differences to estimate spillover effects. I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Authors' calculations.

3.B. Additional Robustness Checks

Table 3.B.1.: Triple Differences: Robustness Checks on Wage Spillovers. Full table

	Baseline	Region shocks	Industry shocks	Region + Industry shocks	No closing plants	Different Treated	Different Option
Treated x Option							
x 1992-94	0.005*	-0.008***	0.006*	-0.003	0.006**	-0.007*	0.006***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)
x 1994-96	0.020***	0.024***	0.014***	0.017***	0.019***	0.020***	0.025***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
x 1995-97	0.022***	0.029***	0.007*	0.013***	0.020***	0.022***	0.024***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.002)
x 1996-98	0.027***	0.037***	0.010**	0.019***	0.024***	0.011**	0.036***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
x 1997-99	0.033***	0.046***	0.016***	0.028***	0.031***	0.015***	0.044***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	(0.003)
Partial x Option							
x 1992-94	-0.006***	-0.006***	-0.009***	-0.007***	-0.006***	-0.005***	0.006***

Continued on next page

Table 3.B.1 – continued from previous page

	Baseline	Region shocks	Industry shocks	Region + Industry shocks	No closing plants	Different Treated	Different Option
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
x 1994-96	0.009***	0.009***	0.007***	0.008***	0.008***	0.003**	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
x 1995-97	0.007***	0.010***	0.001	0.003	0.006***	0.004*	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
x 1996-98	0.009***	0.015***	0.002	0.006***	0.009***	0.000	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
x 1997-99	0.008***	0.015***	0.000	0.006**	0.007***	-0.001	-0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
No. of observations	761,276	752,408	761,276	752,408	754,698	761,276	2,117,788
No. of workers	177,647	175,700	177,647	175,700	176,157	177,647	481,939
Year fixed effects	yes	yes	yes	yes	yes	yes	yes
1-digit Industry fixed effects	yes	yes	yes	yes	yes	yes	yes

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Table 3.B.1 – continued from previous page

	Baseline	Region shocks	Industry shocks	Region + Industry shocks	No closing plants	Different Treated	Different Option
Federal state fixed effects	yes	yes	yes	yes	yes	yes	yes
Region type fixed effects	yes	yes	yes	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes	yes	yes	yes
LMR x year fixed effects	no	yes	no	yes	no	no	no
Industry x year fixed effects	no	no	yes	yes	no	no	no

Notes: This table shows several robustness checks on the triple differences estimation with the two-year change in log daily wages as the outcome variable (see Equation 3.3). Standard errors (in parentheses) are clustered at the worker level. In the first column, I show the baseline specification of Figure 3.3 and Table 3.A.5. In the second column, I add labor market region times year fixed effects. In the third column, I add 1-digit industry times year fixed effects to the baseline specification. In the fourth column, I combine labor market region times year fixed effects and industry times year fixed effects and add them to the baseline specification. In the fifth column, I use the baseline specification and drop all observations in establishments that are in their closing year. In the sixth column, I use a time-constant treatment variable. In the seventh column, I change the $Option_{it}$ variable to be equal to 1 if an individual i is working in an occupation that had large outflows to the main construction sector at year t and equal to 0 if an individual i is working in an occupation that had low outflows to the main construction sector at year t. The reference period is 1993–95. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Source: SIEED and BHP. Author's calculations.

Table 3.B.2.: Triple Differences: Robustness Checks on Job-to-Job Probability. Full table

	Baseline	Region shocks	Industry shocks	Region + Industry shocks	No closing plants	Different Treated	Different Option
Treated x Option							
x 1992-94	-0.015**	0.001	0.008	0.018***	-0.014**	-0.028***	0.042***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.004)
x 1994-96	0.056***	0.069***	0.044***	0.057***	0.054***	0.078***	0.106***
	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)	(0.004)
x 1995-97	0.055***	0.058***	0.031***	0.040***	0.052***	0.079***	0.071***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)	(0.010)	(0.006)
x 1996-98	0.007	0.013	0.007	0.013	0.005	-0.016	0.044***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.006)
x 1997-99	-0.009	0.005	-0.024**	-0.009	-0.011	-0.044***	0.016**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.007)
Partial x Option							
x 1992-94	-0.004	0.002	0.013**	0.017***	-0.004	-0.011**	0.038***

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Table 3.B.2 – continued from previous page

	Baseline	Region shocks	Industry shocks	Region + Industry shocks	No closing plants	Different Treated	Different Option
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.004)
x 1994-96	0.059***	0.062***	0.052***	0.055***	0.058***	0.073***	0.029***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
x 1995-97	0.064***	0.064***	0.049***	0.052***	0.064***	0.088***	0.011**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.005)
x 1996-98	0.015**	0.018***	0.014**	0.017**	0.016**	0.005	-0.002
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.005)
x 1997-99	0.009	0.019**	-0.003	0.007	0.010	-0.022***	-0.015***
	(0.007)	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.005)
No. of observations	796,763	787,452	796,763	787,452	789,906	796,763	2,207,206
No. of workers	194,574	192,416	194,574	192,416	192,959	194,574	524,356
Year fixed effects	yes	yes	yes	yes	yes	yes	yes
1-digit Industry fixed effects	yes	yes	yes	yes	yes	yes	yes

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Table 3.B.2 – continued from previous page

	Baseline	Region shocks	Industry shocks	Region + Industry shocks	No closing plants	Different Treated	Different Option
Federal state fixed effects	yes	yes	yes	yes	yes	yes	yes
Region type fixed effects	yes	yes	yes	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes	yes	yes	yes
LMR x year fixed effects	no	yes	no	yes	no	no	no
Industry x year fixed effects	no	no	yes	yes	no	no	no

Notes: This table shows several robustness checks on the triple differences estimation with the two-year change in job-to-job transition as the outcome variable (see Equation 3.3). Standard errors (in parentheses) are clustered at the worker level. In the first column, I show the baseline specification of Figure 3.4 and Table 3.A.8. In the second column, I add labor market region times year fixed effects. In the third column, I add 1-digit industry times year fixed effects to the baseline specification. In the fourth column, I combine labor market region times year fixed effects and industry times year fixed effects and add them to the baseline specification. In the fifth column, I use the baseline specification and drop all observations in establishments that are in their closing year. In the sixth column, I use a time-constant treatment variable. In the seventh column, I change the $Option_{it}$ variable to be equal to 1 if an individual i is working in an occupation that had large outflows to the main construction sector at year t and equal to 0 if an individual i is working in an occupation that had low outflows to the main construction sector at year t. The reference period is 1993–95. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Source: SIEED and BHP. Author's calculations.

Table 3.B.3.: Triple Differences: Reallocation to Higher-Paying Establishments

	Establishment mean wage			**Establishment AKM fixed effect**		
	Baseline	Excluding main construction switchers	No closing plants	Baseline	Excluding main construction switchers	No closing plants
Treated x Option						
x 1992-94	-0.004**	-0.003	-0.004**			
	(0.002)	(0.002)	(0.002)			
x 1994-96	0.008***	0.005**	0.006***	0.002	0.001	0.002
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
x 1995-97	0.008***	0.006**	0.007***	0.006***	0.004**	0.005***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
x 1996-98	0.012***	0.010***	0.010***	0.008***	0.006**	0.007***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
x 1997-99	0.013***	0.009***	0.011***	0.014***	0.011***	0.014***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Partial x Option						
x 1992-94	-0.006***	-0.005***	-0.006***			

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Table 3.B.3 – continued from previous page

	Establishment mean wage			Establishment AKM fixed effect		
	Baseline	Excluding main construction switchers	No closing plants	Baseline	Excluding main construction switchers	No closing plants
	(0.001)	(0.001)	(0.001)			
x 1994-96	0.004***	0.003**	0.004***	0.002***	0.002***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
x 1995-97	0.007***	0.006***	0.007***	0.005***	0.004***	0.005***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
x 1996-98	0.008***	0.007***	0.007***	0.007***	0.006***	0.007***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
x 1997-99	0.005***	0.004**	0.005***	0.006***	0.005***	0.006***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
No. of observations	693,303	686,013	690,064	509,298	504,386	505,617
No. of workers	174,569	172,173	173,619	140,934	139,259	139,759
Year fixed effects	yes	yes	yes	yes	yes	yes
1-digit Industry fixed effects	yes	yes	yes	yes	yes	yes

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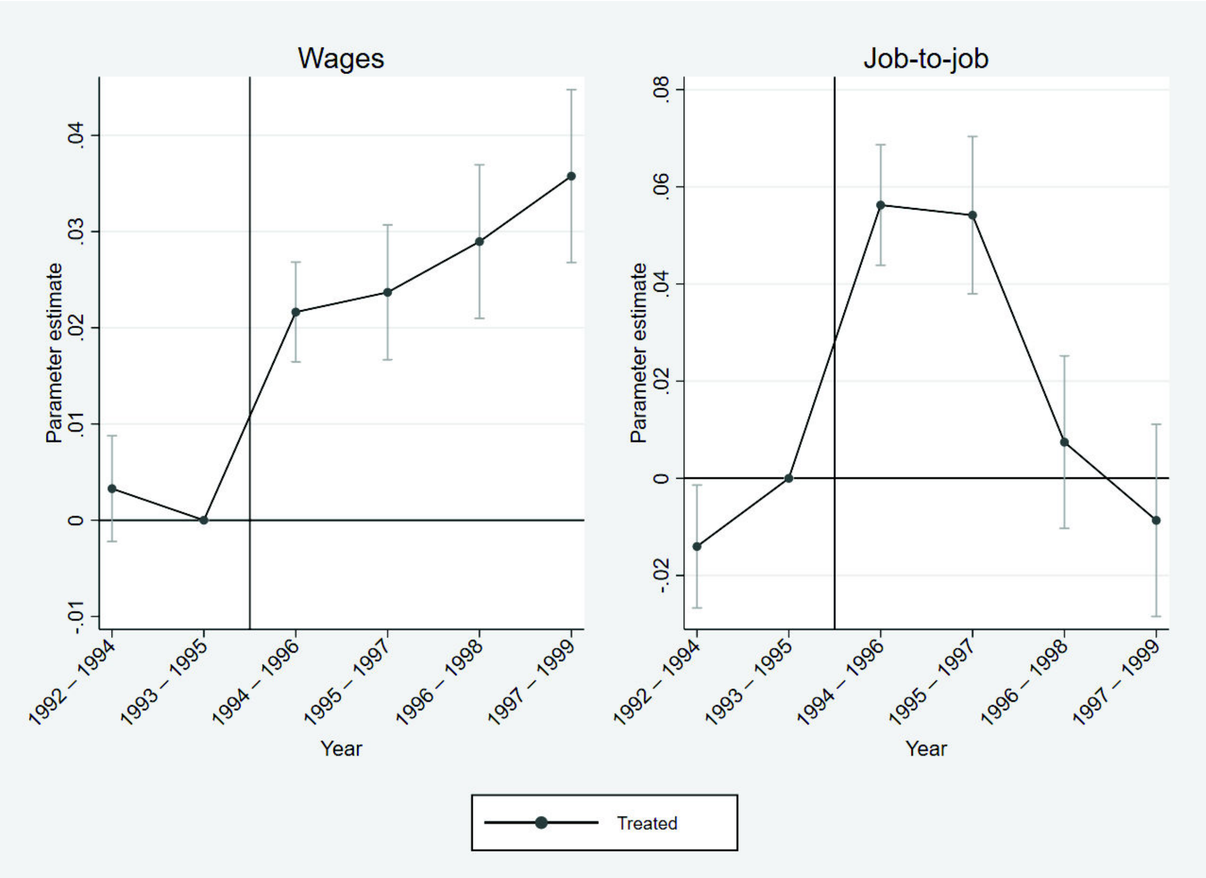
Table 3.B.3 – continued from previous page

	Establishment mean wage			Establishment AKM fixed effect		
	Baseline	Excluding main construction switchers	No closing plants	Baseline	Excluding main construction switchers	No closing plants
Federal state fixed effects	yes	yes	yes	yes	yes	yes
Region type fixed effects	yes	yes	yes	yes	yes	yes
Worker fixed effects	yes	yes	yes	yes	yes	yes

Notes: This table shows the results of several triple differences specifications (see Equation 3.3). Intuitively, the estimator compares the DiD of workers in industries listed in Table 3.A.3 with workers in industries listed in Table 3.A.4. In the first three columns, I use the change in log establishment average wages as the outcome variable. In the last three columns, I use the change in establishment AKM fixed effects as the outcome variable. I measure establishment quality in both specifications in t. I present the baseline results for each outcome, change in establishment average wages and change in establishment AKM fixed effects, without switchers to the main construction sector and excluding workers in establishments during their closing year (from the baseline). Standard errors (in parentheses) are clustered at the worker level. The reference period is 1993–95. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

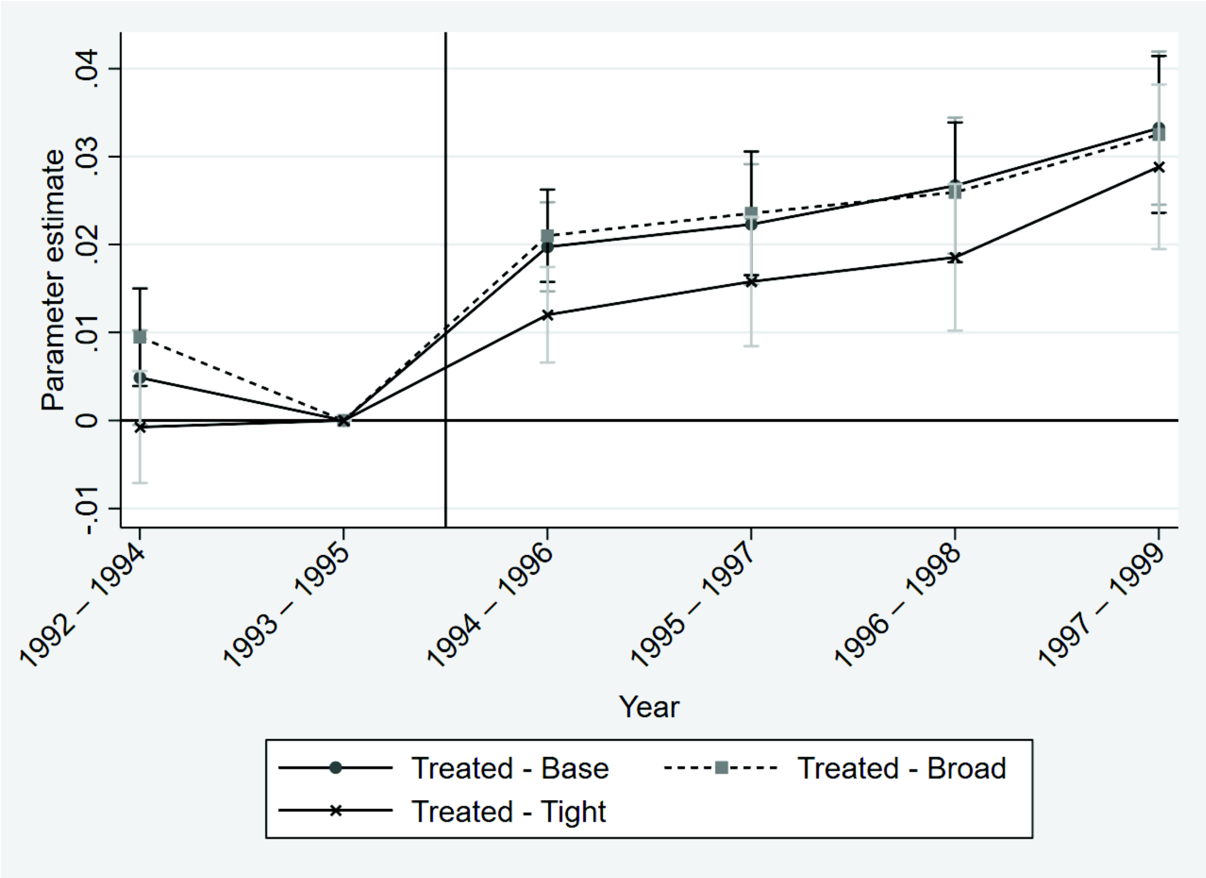
Source: SIEED and BHP. Author's calculations.

Figure 3.B.1.: Triple Differences: Excluding other Construction Industries from Outside Option Industries Classification



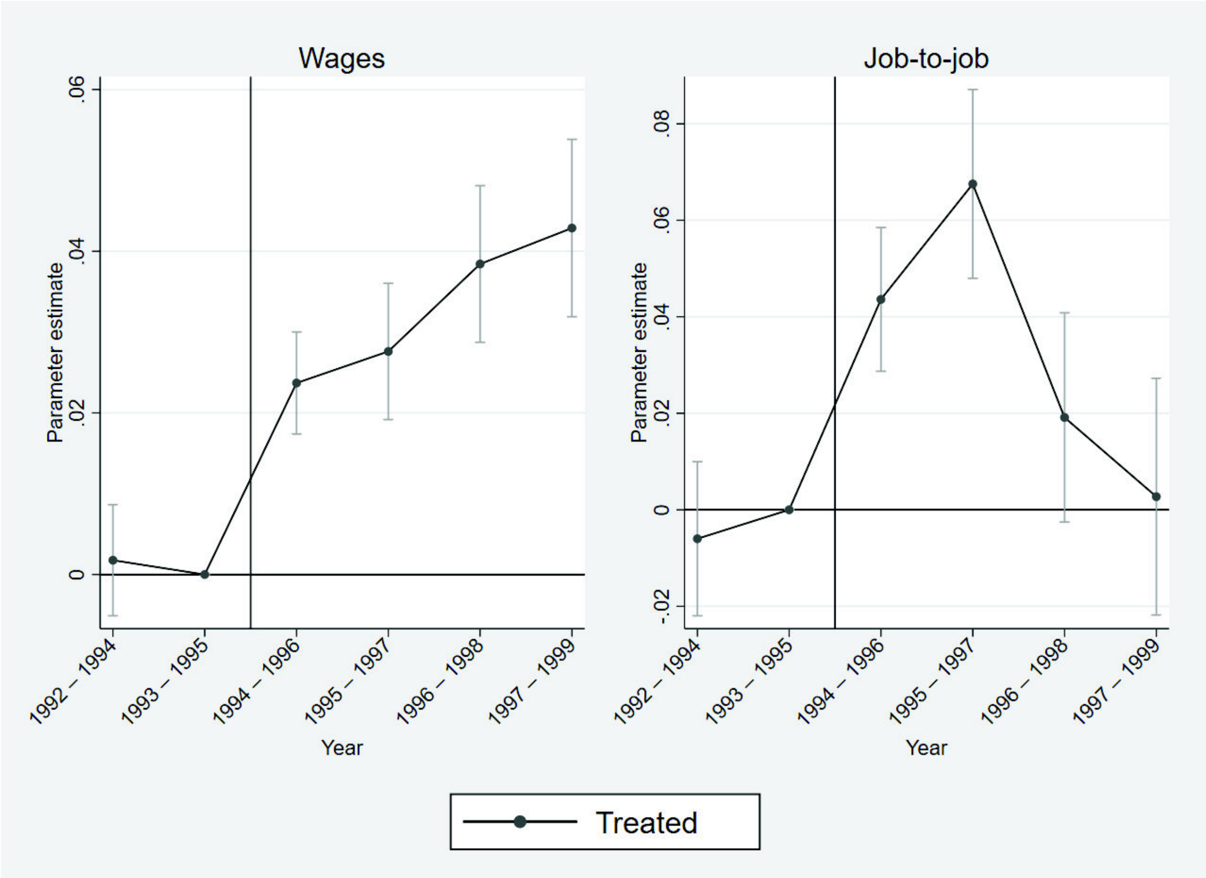
Notes: This figure shows the results of two triple differences specifications using different outcome variables (see Equation 3.3) and excluding other construction industries from the outside option industries classification in Table 3.A.3. Specifically, I drop the 3-digit industries 451, 452, 454, and 455. I use 95% confidence intervals. In the first panel, I use the two-year change in log daily wages as the outcome. In the second panel, I use the probability of switching establishments as the outcome variable. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Author's calculations.

Figure 3.B.2.: Triple Differences: Different Bandwidths on Control Group



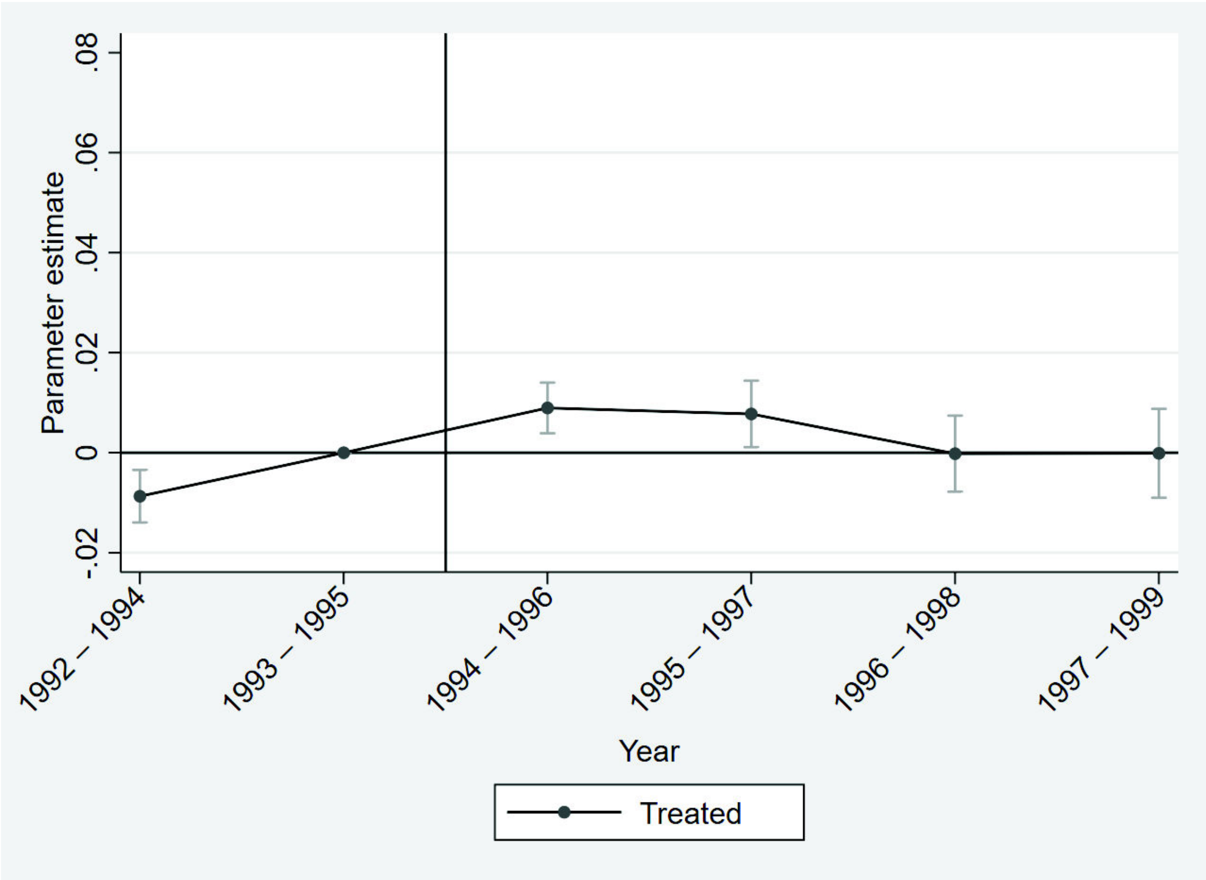
Notes: This figure illustrates the results of the triple differences specification with the two-year change in log daily wages as the outcome (see Equation 3.3). I use 95% confidence intervals. "Treated - Base" refers to the baseline approach in which the control group is defined with $MW + 40\% \leq h_{i,t} < MW + 80\%$, where MW refers to the minimum wage. In "Treated - Broad" I use $MW + 60\% \leq h_{i,t} < MW + 120\%$ to define the control group and in "Treated - Tight" I use $MW + 20\% \leq h_{i,t} < MW + 40\%$. In all three cases, I use the outside option industries and non-outside option industries in Tables 3.A.3 and 3.A.4. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Authors' calculations.

Figure 3.B.3.: Triple Differences: Excluding the Manufacturing Sector



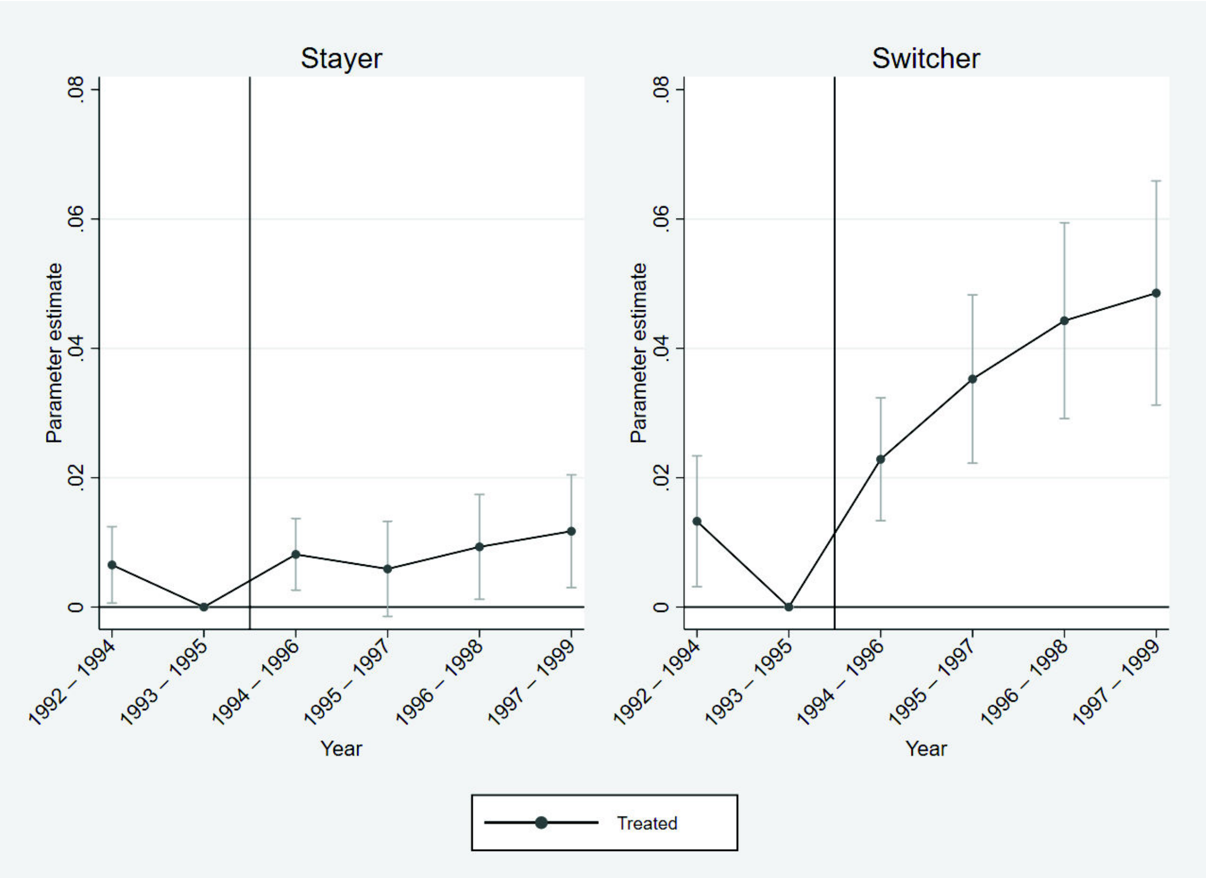
Notes: This figure shows the result of two triple differences specifications excluding the manufacturing sector in t and $t+2$ (see Equation 3.3). In the first panel, I use the two-year change in log daily wages as the outcome. In the second panel, I use the probability of switching establishments as the outcome variable. I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Author's calculations.

Figure 3.B.4.: Triple Differences: Probability to Switch to Posted Sectors



Notes: This figure shows the result of a triple differences specifications using the probability to switch into the posted sectors as the outcome variable (see Equation 3.3). I use 95% confidence intervals. The variable takes the value 1 if the individual switched to a posted sector (see Table 3.1) from t to $t + 2$ and 0 if she did not. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Author's calculations.

Figure 3.B.5.: Triple Differences: Wage Spillover for Stayers vs. Switchers. Excluding Switchers to the Main Construction Sector.



Notes: This figure shows the results of two triple differences specifications using the two-year change in log daily wages as the outcome (see Equation 3.3). I define Stayers as workers who stayed within the same establishment during the 1994–97 period. Switchers are workers who changed establishments at least once from t to $t+2$ during 1994–97. For the left panel, I use a sub-sample of Stayers. For the right panel, I use a sub-sample of Switchers. In both panels, I exclude switchers to the main construction sector in any period during the observation window. I use 95% confidence intervals. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. The reference period is 1993–95. **Source:** SIEED and BHP. Author's calculations.

3.C. Theoretical Model: Strategic Complementarity

Suppose that workers are uniformly distributed along a straight line. Two sectors, A and B, are located at distance d_r from each other at the straight line. The distance d_r between the two sectors can vary by local labor market region (LMR) r. I assume that each LMR is a closed labor market. Workers have to pay transportation costs τ for each distance unit traveled. An individual located at x_r^* distance units from sector A is indifferent between working for sector A or sector B if:

$$w_r^A - \tau x_r^* = w_r^B - \tau(d_r - x_r^*), \tag{3.C.7}$$

where sector A pays wage w_r^A in LMR r and sector B pays w_r^B. Solving for x_r^* gives:

$$x_r^* = \frac{w_r^A - w_r^B + d_r\tau}{2\tau}. \tag{3.C.8}$$

This point of indifference, x_r^*, is sector A's labor supply L_r^A.

Each firm in the respective sectors maximizes profits given β, the marginal benefit of employing a worker. Substituting labor supply into the profit maximization problem and then solving for the optimal wage using the first-order condition provides the wage-setting equation in this model:

$$w_r^A = \frac{\beta + w_r^B - d_r\tau}{2}. \tag{3.C.9}$$

Wages increase with β and the wage of competitor B. However, whenever the distance d_r between sectors A and B is larger, the wage response of sector A to an increase in sector B wages will not be as high. In other words, sector A can set its wages more independently from sector B's wages (and vice versa) whenever the distance between these two sectors is larger. The optimal labor demand given labor supply is:

$$L_r^A = \frac{\beta + d_r\tau - w_r^B}{4\tau}. \tag{3.C.10}$$

Labor in sector A increases with β and decreases with the wage in sector B. However, the decreasing effect of w_r^B on L_r^A is lower whenever the distance to the competitor is larger.

3.D. Theoretical Model: Biased Beliefs about Outside Options

In this section, I sketch the theoretical model in Jäger et al. (2022).

In the model, first N homogenous firms enter the labor market. Then, L workers are randomly assigned to firms and supply labor inelastically. Workers learn their wages and potentially update their beliefs about the external wage distribution. Assume the existence of two types of workers who differ in their cost to gather complete information about the labor market. A share α of workers are experts who face no information costs $c_E = 0$ and are always perfectly informed about their outside options in the labor market. The remaining share $1-\alpha$ are amateur workers who face information costs $c_A > 0$ and can therefore form biased beliefs about their outside options. Amateur's job search decision depends on their beliefs about the benefits of job search

$$\widetilde{w}^{max}(w_j, w_{j-1}) - w_j > c_A, \tag{3.D.11}$$

where w_j is the wage of a worker in her current firm j. $\widetilde{w}^{max}(w_j, w_{j-1})$ is the belief about the highest wage. Thus, workers search for new jobs if they believe that the wage they could potentially earn is higher than their current wage plus search costs. The belief about the highest potential wage is a weighted average of the actual highest wage and worker's current wage:

$$\widetilde{w}^{max} = \gamma w_j + (1-\gamma) w^{max}. \tag{3.D.12}$$

The variable $\gamma \in [0, 1]$ captures the degree of anchoring on the current wage. If, e.g., $\gamma = 1$ then workers fully anchor their belief about potential outside options on their current wage. With $\gamma = 0$, workers have accurate beliefs. Empirically, Jäger et al. (2022) show that especially low-wage workers anchor their beliefs about outside options on their current wage and therefore underestimate wages elsewhere.

In the theoretical model, firms maximize their profits given the labor costs per worker. The competitive wage is w^* and equals the marginal product of labor. Jäger et al. (2022) also model how a segmented labor market of firms paying the competitive wage (high-wage firms) and firms paying a marked down wage (low-wage firms) can emerge. For such a segmented labor market to emerge, the only profitable departure from the competitive wage w^* is to pay a wage below w^*, but still large enough to retain a firm's stock of amateur workers. Any downward deviation from the competitive wage will result in an immediate loss of a firm's stock of expert workers.

The reservation wage of amateur workers to not become informed is given by Equation 3.D.11. The most profitable deviation is to exactly pay the reservation wage. Considering the formation of biased beliefs in Equation 3.D.12 and using it in Equation 3.D.11 gives:

$$w' = w^* - \frac{c_A}{1-\gamma}. \tag{3.D.13}$$

w' is the most profitable deviation and represents a markdown of the competitive wage w^*. The markdown from the competitive wage is higher with higher information costs c_A and higher anchoring γ. Deviant firms only retain their amateur workforce and therefore employment in these firms is

$$l(w') = (1-\alpha)\frac{L}{N}. \tag{3.D.14}$$

The deviant wage w' and employment $l(w')$ describe the behavior of low-wage firms in the labor market. For completeness, high-wage firms pay the competitive wage and employ all expert workers in the labor market (plus a share of amateur workers who initially sorted into those firms).

3.E. Other Sectoral Minimum Wages

In this Appendix, I zoom out and analyze the spillover effects of other sectoral minimum wages. The goal is to understand which economic contexts favor positive spillover effects and which are more likely to lead to no or negative spillover effects.[39]

By using the same identification strategy on the worker level as for the analysis of the main construction sector minimum wage, I can analyze the wage spillover effects of other sectoral minimum wages on exposed workers in outside option industries. The electrical trade and roofing sector minimum wages were introduced at the same time as the main construction sector minimum wage. Therefore, I use Equation 3.3 to estimate the spillover effects from these sectors. However, the 3-digit industries that fall into the outside option and non-outside option classification differ from the industries that fall into these categories in the main construction sector. To estimate the spillover effects from all other sectoral minimum wages, I use a generalized version of Equation 3.3 in which I use three pre-periods:

$$
\begin{aligned}
w_{i,t+2} - w_{i,t} = \alpha_i + \zeta_t + \sum_{\tau=-3}^{3} \beta_\tau Treated_{i,t} \times Option_{i,t} \times \mathbb{1}_{[t=\tau]} \\
+ \sum_{\tau=-3}^{3} \gamma_\tau Partial_{i,t} \times Option_{i,t} \times \mathbb{1}_{[t=\tau]} + \delta X_{i,t} + \epsilon_{i,t},
\end{aligned}
\tag{3.E.15}
$$

where $\tau = -3$ are 3 periods prior to the announcement of the sectoral minimum wage and $\tau = 3$ is the period in which the sectoral minimum wage was introduced. The reference period is $\tau = -1$. I define treated (sub-minimum wage) workers as workers with an hourly wage below the respective minimum wage, and use the same thresholds as in Section 3.3.3 to define the partially treated and control group. I define outside option and non-outside option industries by using the procedure outlined in Section 3.3.3 and use the same control variables as in Equation 3.3.

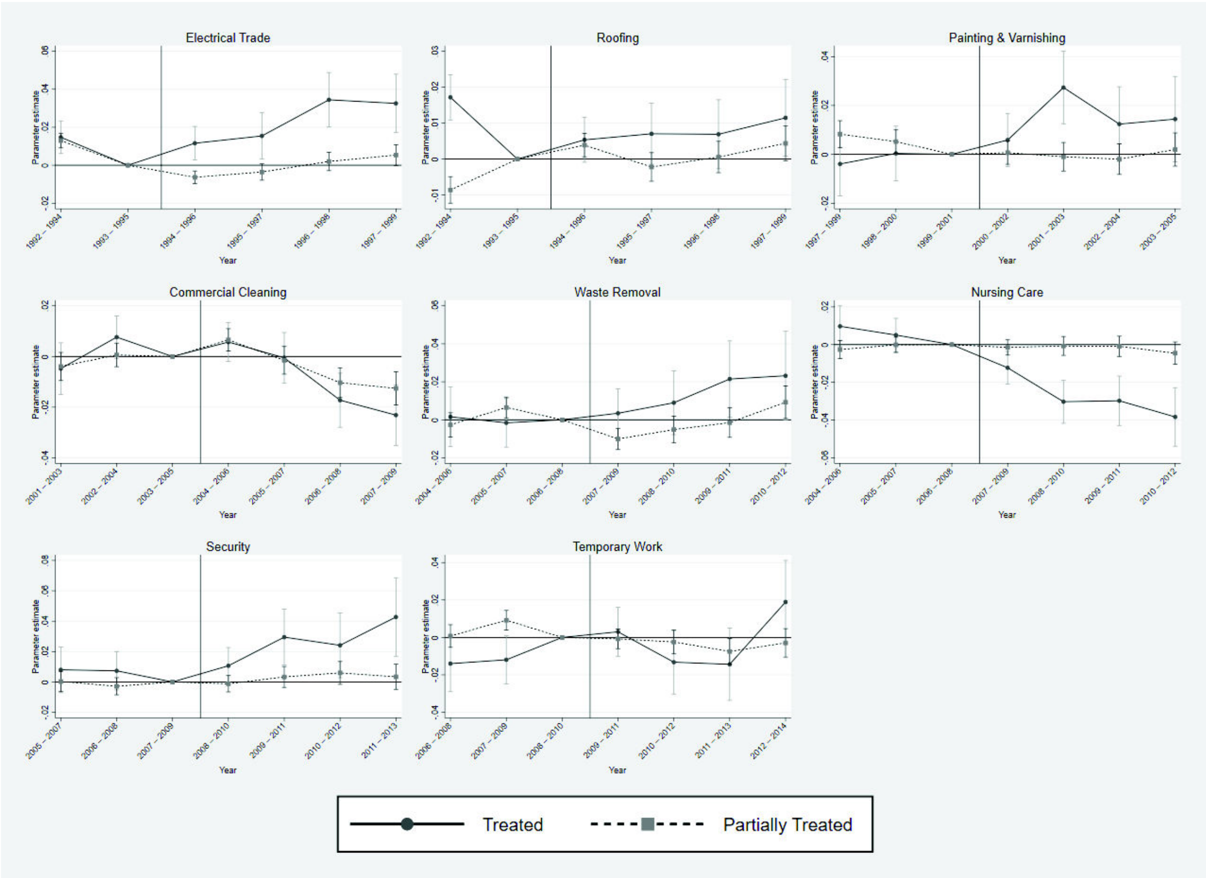
Figure 3.E.1 illustrates the results. The y-axis displays the coefficient estimates of the triple interaction and the x-axis indicates the time period. As information treatments could be im-

[39] Because I do not want to capture possible effects of the federal minimum wage, I do not analyze the spillover effects from the scaffolding sector, stonemasonry sector, hairdressing sector, textile & clothing sector, chimney sweeping sector, slaughtering & meat processing sector, and the agriculture, forestry & gardening sector. These sectoral minimum wages were either introduced shortly before or right at the federal minimum wage was introduced which makes it difficult to distinguish possible anticipation or direct effects from the federal minimum wage introduction (see Table 3.1).

portant in the context of spillover effects (Section 3.5.4), I expect to find spillover effects one year prior to the introduction of each minimum wage (e.g., I expect spillover effects from the painting & varnishing sector minimum wage in 2000-02). I find positive wage spillover effects on sub-minimum wage workers in outside option industries from the electrical trade minimum wage, the roofing minimum wage, the painting & varnishing minimum wage, the waste removal minimum wage, the security minimum wage, and the temporary work minimum wage. I find negative wage spillover effects on sub-minimum wage workers in outside option industries from the commercial cleaning minimum wage and the nursing care minimum wage. Positive wage spillover effects range from 1.1% from the minimum wage in the roofing sector to 4.3% from the minimum wage in the security sector. Negative wage spillover effects range from 2.3% from the minimum wage in the commercial cleaning sector to 3.8% from the minimum wage in the nursing care sector. Note that, even though the waste removal sector and nursing care sector minimum wages were introduced in the same year, their spillover effects on the respective outside option industries differ greatly. This provides additional evidence that my identification strategy does not capture year-specific common shocks to low-wage earners, but rather spillover shocks that affect only low-wage earners in specific industries.

The sectors with minimum wages that had negative spillover effects clearly differ from the other minimum wage sectors in that they employ a high proportion of women in part-time or mini-jobs (see Appendix Table 3.A.2). Since my sample only includes full-time workers, I interpret the different signs of the spillover effects for the commercial cleaning and nursing care sector as an indication that positive wage spillover effects can only occur when workers in the minimum wage sector are in a similar employment relationship. For example, because switching from full-time to part-time is associated with substantial earnings declines (for workers with similar hourly wages), full-time workers might compare their wages only with other full-time jobs or switch to the minimum wage sector only if it also offers sufficient full-time jobs.

Figure 3.E.1.: Triple Differences: Wage Spillover Effects from Other Sectoral Minimum Wages



Notes: This figure illustrates the results of the triple differences specification with the two-year change in log daily wages as the outcome (see Equation 3.E.15). I use 95% confidence intervals. In each panel, I present the wage spillover effects from the minimum wages of different sectors. Thus, I compare the wage growth of (partially) treated versus control group workers in outside option versus non-outside option industries. The definition of (partially) treated, control group, outside option and non-outside option industries changes for the analysis of spillover effects from each minimum wage sector. Control variables include: year fixed effects, 1-digit industry fixed effects, federal state as well as region type fixed effects and worker fixed effects. **Source:** SIEED and BHP. Authors' calculations.

4. Students' Coworker Networks and Labor Market Entry*

Abstract: This paper analyzes whether and to what extent college students' coworker networks from student jobs affect their labor market transition after graduation. The empirical analysis is based on administrative data, which includes all pre- and post-graduation job-related networks of college students who graduated from a large German university between 1995 and 2016. Our identification strategy overcomes potential bias due to non-random selection into networks by controlling for coherent sets of individual, network, and firm characteristics, as well as firm fixed effects, and by distinguishing between close and less close colleagues in the same firm. Our results suggest that college graduates benefit from the quality of their coworkers in student jobs by speeding up their transition to the labor market and earning higher wages in their first job after graduation. Our results are important for understanding the relevant ingredients for a successful transition from higher education to the labor market.

*This chapter is co-authored by Friederike Hertweck, Malte Sandner, and Ipek Yükselen. We are grateful for comments from David Card, Thomas Cornelissen, Simon Janssen, Philipp Lergetporer, and participants at various RWI seminars for helpful comments. This paper uses confidential data from the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB).

4.1. Introduction

A large body of theoretical and empirical research has emphasized that coworker networks play an important role in accessing job opportunities and enhancing career advancement. Studies analyzing the role of networks in labor market success have focused on the role of family (Kramarz and Skans, 2014), neighborhood (Ioannides and Loury, 2004), student peers (Marmaros and Sacerdote, 2002), ethnic networks (Dustmann et al., 2016), close friends (Cappellari and Tatsiramos, 2015), or former coworkers in regular employment (e.g. Cingano and Rosolia, 2012; Eliason et al., 2022; Glitz, 2017; Saygin et al., 2021).

However, little is known about the role of coworker networks from student jobs in accessing job opportunities and enhancing career advancement. This lack is surprising for at least three reasons. First, student employment is a common phenomenon that has increased in recent years, reaching 40 % in the US (Irwin et al., 2022) and over 60 % in Germany (Staneva, 2015). Second, information frictions between employers and employees may be particularly strong during the transition from higher education to employment – and students' coworkers may be helpful in reducing these frictions. Third, the transition from higher education to employment is a crucial career stage with lasting consequences for later careers (e.g. Oreopoulos et al., 2012; Oyer, 2006; Wachter, 2020). Therefore, additional knowledge about the mechanisms and important factors of the transition process is important for designing effective policies to smooth transitions.

In this paper, we go beyond the existing literature and analyze whether college students benefit from the quality of their coworker networks during their transition from college to the labor market. Conducting this research is challenging, first, because the data must include information about a student's labor market history, including college and post-graduate employment, about all former coworkers, and about the student's university studies. Second, it is challenging because students are not randomly selected into student jobs and thus into coworker networks.

To solve the data challenge, our study uses data which links the administrative university records of students from a large German university with their social security records. This data provide detailed information on university enrollment, fields of study, and grades, on student's college and graduate employment, and on the employment of all coworkers who work with the student in the same firm. The data also allow us to identify those coworkers who work with the student in the same firm and in the same (or another) occupation as the student, which we use to indicate close or less close coworkers. Finally, the data provide the wages of the former coworkers at the student's labor market entry, which we use to operationalize our measure of network quality.

To address the endogeneity issue that students with better coworkers also work in more productive firms, which may have positive effects on the transition from college to employment, we control for detailed student job characteristics, industry characteristics, and a proxy for firm productivity suggested by Abowd et al. (1999). In addition, we focus on the close coworkers and control for the less close coworkers. If we expect unobserved firm shocks or policies, such as firm training policies, to positively bias our network quality indicator, we should expect the same bias for coworkers who work in the same firm but not closely with the student. Finally, we run an estimation including firm fixed effects, estimating the coworker effect for students with different coworker networks within the same firm, which prevents specific firm characteristics from biasing our results. Another potential endogeneity issue is the sorting of high-ability students to better coworkers. To account for this issue, we cannot use student fixed effect because we are interested in the coworker network during studying on wages after graduation. However, we control for a wide range of individual pre-college characteristics and a reliable measure of pre-college ability, students' high school GPA.

Our results show that graduates benefit from their embedded coworker networks by speeding up the labor market transition and by receiving higher wages after graduating from college. For instance, a 10 % increase in the average wage of former coworkers at the time of a student's graduation is associated with a c.p. 0.78 % higher entry wage at the graduate's first full-time job and a 1.5 % reduction in the time between graduation and first job. Our results also indicate that the network quality has no effect on the probability that the graduate separates from the first employer after 6, 12, or 24 months. These results are robust to the different specifications and robustness checks we apply.

In a series of heterogeneity analyses, we distinguish between jobs that students typically take to support themselves (e.g., bartending or cashiering) and jobs that are more related to their studies, including paid internships. We show that coworkers in more related jobs drive the effects on wages, while coworkers in unrelated jobs drive the effects on the time between graduation and first job. These results suggest that the quality of coworkers in more related jobs improves the quality of the first job, while the quality of coworkers in unrelated jobs leads to faster employment. One might be concerned that the faster entry into the labor market leads to a worse match. However, this negative effect does not appear as the coefficient on wages for the unrelated job is not negative. When we look at potential heterogeneity across gender and student ability, measured by college GPA, our results do not show much difference across these groups. The gender result is surprising, as previous research shows that male college graduates benefit more from their employee networks than female graduates (Mengel, 2020).

One possible channel explaining the coworker effects on transition is an increase in students' effort at university. However, we can rule out this channel as we do not find any effects of coworker quality on graduation grades. Another channel is that students benefit from good coworkers if they start their career in the same firm where they worked as a student. If only this channel explains the effects, we should see no effects after excluding students who start in their student job firm after graduation, which is not the case. Excluding these two channels, a likely remaining explanation for why student job coworkers may improve graduates' labor market entry is information frictions on potential outside options, which are high during labor market entry: Jäger et al. (2022) shows that these frictions partly explain wage differentials between similar workers, and Belot et al. (2019); Carranza et al. (2022); Demir (2022) provide evidence that reducing these frictions can induce workers to switch to better-paying jobs. A recent paper by Caldwell and Harmon (2019) shows that networks of coworkers can help reduce information frictions for individual workers. However, we can not state with certainty that information frictions are the underlying mechanism.

Because our study is the first to examine how peers from student jobs affect the labor market entry of college graduates, it makes several novel contributions to the literature. First, we add to the literature on peer effects in college that exploits random variation in the assignment of students to dorms, classes, or introductory courses. However, this literature examines the effects of peers on student achievement or behavioral outcomes (e.g. Feld and Zölitz, 2017; Sacerdote, 2001) and does not examine whether networks support later career success. The small literature that has examined how networks during education relate to labor market entry has focused on classmate networks. For example, Zhu (2022) examines how classmate networks at community colleges in Arkansas affect job search. Zimmerman (2019) focuses on elite colleges in Chile and shows that peer ties formed between classmates at elite colleges can affect labor market outcomes later in life. Finally, Marmaros and Sacerdote (2002) examine how randomly assigned roommates at Darmouth College affect each other's labor market entry. However, this literature neglects peer effects from student employment networks, which are very likely to affect students given the high employment rates and many hours students spend working while studying.

This paper also contributes to a growing literature that examines the determinants of the transition from college to employment. This literature has shown that lower early career wages have long-lasting effects on the careers of college graduates (e.g. Oreopoulos et al., 2012; Oyer, 2006; Wachter, 2020) and has identified factors that influence the transition from college to the labor market. For example, Oreopoulos et al. (2012) show that graduates who enter the labor market during a recession have lower earnings on average than graduates who start their careers in better labor market conditions, and that this earnings decline persists for 10 years. We show that the network of students' jobs is also important for this important transition. This knowledge can help improve policies to smooth the transition, such as career counseling.

Finally, this paper contributes to the literature on the effect of working while studying. While some studies show that working during studies can have positive effects on later wages (e.g. Hotz et al., 2002; Le Barbanchon et al., 2023), this literature rarely identifies channels why working during studies has positive effects on wages. With our results, we show that the quality of coworkers in student jobs is an important channel mediating the returns to working while studying.

The rest of the paper is organized as follows: Section 4.2 describes the data and sample selection, and presents descriptive statistics. Section 4.3 describes our empirical strategy. Section 4.4 presents and discusses our results. Finally, Section 4.5 concludes.

4.2. Data and Descriptives

Our data include detailed labor market and college data for each student, social security records for their coworkers, and information on the firms where the students worked during their studies. All datasets and links are described below.

Student-level data

The core of our dataset is the detailed social security records of all students who graduated from a large German university between 1995 and 2016. These records come from the Integrated

Employment Biographies (IEB) of the Institute for Employment Research (IAB) and the administrative records of the university. The IEB covers the universe of employees in Germany[1] and contain detailed daily information on employment, benefit receipt, and job search. Since the IEB does not include educational trajectories, the university administrative records are matched to the IEB based on a student's name, date of birth, and gender (Möller and Rust, 2018). This matching allows us to uniquely identify students in the data who worked in student jobs while studying.

For each student in the dataset, we have detailed information on individual characteristics (e.g., gender, year of birth), pre-college and college education (e.g., field of study, high school and college GPA, time of enrollment and graduation), and each student's entire labor market history, including student and graduate employment (e.g., start and end dates, occupation, employment type, wage).

Coworker networks

For each student job, we know the firm and the exact start and end dates. Since we have access to the social security records of the entire workforce in Germany, we can then create a list of employees who worked in the same firm at the same time as the student. We define these individuals as potential coworkers of student i in student job k and all potential coworkers as the student coworker network. In a final step of data preparation, we then extract socio-demographic characteristics (gender, age, nationality, educational attainment) and labor market history (employment status, deflated (daily) wage) of each of these potential coworkers.

AKM data

We also add AKM fixed effects, provided by Bellmann et al. (2020), to our data. The establishment AKM fixed effect measures the proportional wage premium to all workers in an establishment, net of worker composition (Abowd et al., 1999; Card et al., 2013). Abowd et al. (1999) show that establishments with a high establishment fixed effect are more productive and

[1]The IEB allows the employment status of an individual to be tracked to the day. Individuals are included in the IEB if they have (or had) at least one of the following employment statuses: employment subject to social security contributions (in the data since 1975), marginal part-time employment (in the data since 1999), receipt of benefits according to SGB III or II (SGB III since 1975, SGB II since 2005), officially registered as job-seekers with the Federal Employment Agency, or (planned) participation in active labor market policy programs (in the data since 2000).

profitable. In addition, Card et al. (2013) show systematic selection of highly skilled workers into establishments with a higher AKM fixed effect. We use the establishment AKM fixed effect as a proxy for the productivity of an establishment, thereby accounting for the non-random selection of workers into establishments.

4.2.1. Sample selection

The relationship of interest is whether students' networks of coworkers affect their labor market transitions after graduation. Therefore, we include in our sample only those students who are likely to work in the social security system after graduation and who had a student job while studying.[2] We consider any job up to 5 years before graduation as a student job (Figure 4.1).

To ensure that students and their coworkers have sufficient contacts and interactions, we drop student jobs (and thus coworker networks) that last less than three months, as well as student jobs in firms with more than 250 employees.[3] In addition, we distinguish between close coworkers, those coworkers in the same 3-digit occupation and firm, and less close co-workers, all other coworkers in the same firm.

Our outcomes of interest relate to a graduate's transition to the labor market. We restrict our analysis to the first full-time job, dropping all graduates who did not find a full-time job within three years of graduation and dropping some implausible cases (i.e., graduates who earn less than 10 Euro per day in a full-time job).

We select the daily wage of the graduate and compute the deflated log daily wage of the graduate using the Consumer Price Index from the Federal Statistical Office and the number of days until the graduate starts full-time employment.[4]

For coworkers, we assign a missing value to observations with a wage below the first percentile of the wage distribution for coworkers. We convert gross daily wages to real daily wages using the Consumer Price Index from the Federal Statistical Office. We measure coworker characteristics at the exact time the student graduates from college ($t = 0$ in Figure 4.1). If employees have

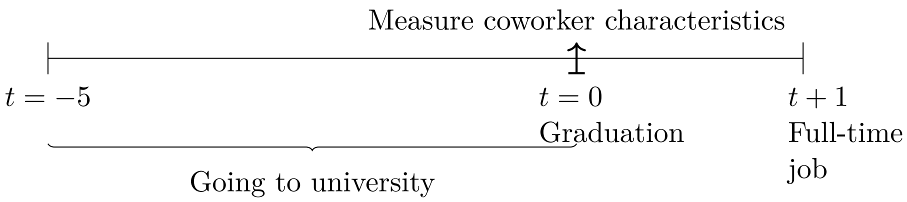
[2]This means that we exclude all students enrolled in teacher training programs, as they often become civil servants shortly after entering the labor market and thus do not work in the social security system. We also exclude bachelor students because they may enroll in a master's program after completing their undergraduate studies and do not enter the labor market directly.

[3]Figures 4.A.5 and 4.A.6 show that students tend to work in smaller firms during their studies anyway.

[4]The daily wage variable is top-coded at the annually varying ceiling on social security contributions in the IEB data. Because we focus on the first job after graduation, only 1.20% of the graduates' wages are censored. Thus, censored wages are unlikely to affect our results.

multiple employment spells at this time, we keep the spell with the longest tenure.

Figure 4.1.: Measurement of coworker characteristics



We then create a comprehensive set of variables that describe the quality of the network. These include the average daily wage, the employment rate, the network size, the average age (and its square), the share of coworkers with vocational training, the share of coworkers with a college degree, the share of female coworkers, and the share of non-German coworkers. In addition, we calculate the average AKM establishment fixed effects across student jobs, i.e. weighted by the duration of the student job in the establishment of interest.

4.2.2. Descriptive statistics

Our sample restrictions leave us with 3,285 individual graduates who worked in student jobs and started their first full-time job within three years of graduation between 2000 and 2016. Table 4.1 presents descriptive statistics on the graduates, their coworker networks, and their first full-time job.

58 % of the graduates are female. 2 % of the graduates have a non-German citizenship. The average age at first full-time employment is 28. Average high school GPA is 2.33 – it ranges from 1 (best) to 4 (passed). Most graduates in our sample studied either Humanities and Social Sciences (43 %) or Economics and Business (29 %). 17 % of the graduates studied a medical subject, 11 % studied a program in Mathematics and Natural Sciences. Table 4.1 also displays the top last industry in which students worked besides their studies: The industries ”Wholesale and retail trade; repair of motor vehicles and motorcycles” and ”Accommodation and food service activities” have the highest share of graduates with 20% and 22 % respectively. Furthermore, Table 4.1 displays the top last occupations of students. Students in our sample are most likely to work as waiters or office specialists.

In the five years prior to graduation, students worked on average 3.7 different student jobs in rather small establishments with below-average productivity, as indicated by the negative

AKM fixed effect. Figure 4.A.2 in the Appendix shows the distribution of the network size of graduates. The average coworker network can be described as female-dominated (64 %), mostly employed (61%), of German citizenship (94 %), and lower educated (71 %).

The average daily wage of graduates in their first full-time job after graduation is about 76 Euro, which is about 2,280 Euro per month. Figure 4.A.3 in the Appendix shows the distribution of daily wages of graduates in their first full-time job. The average time between graduation and the first full-time job is about seven months. Because we focus on the first full-time job after graduation, if students work in other types of jobs before their first full-time job, that time is also included in the number of days to the first job. Figure 4.A.4 in the Appendix shows the distribution of days to first full-time job. The distribution is left skewed with a median of 112 days (about 3-4 months).

Table 4.1.: Descriptive Statistics

	Mean	SD
First Job after Graduation Characteristics		
Log Daily Wage at the First Job After Graduation	4.33	0.63
Log Days to Start First Job After Graduation	4.66	1.36
Network Quality at Graduation		
Average Log Daily Wage of Close Coworkers	3.90	0.66
Graduate Characteristics		
Female	0.58	0.49
Non-German	0.02	0.14
Age at the First Job After Graduation	27.45	2.57
Final High School GPA	2.33	0.60
Number of Student Jobs	3.65	3.30
Log Average Wage in Student Jobs	2.41	0.87
Field of Study		
Economics and Business	0.29	0.45
Mathematics and Natural Sciences	0.11	0.31

Continued on next page

Table 4.1 – continued from previous page

	Mean	SD
Humanities and Social Sciences	0.43	0.50
Medical Studies	0.17	0.38
Student Jobs Characteristics		
Average AKM Establishment FE	-0.14	0.44
Industry of Student Jobs		
Acommodation and Food Service Activities	0.22	0.41
Wholesale and Retail Trade; Repair of Motor Vehicles and Motorcyle	0.20	0.40
Professional, Scientific and Technical Activities	0.14	0.34
Human Health and Social Work Activities	0.10	0.30
Information and Communication	0.08	0.26
Manufacturing	0.05	0.21
Administrative and Support Service Activities	0.05	0.21
Occupation in Student Job		
Waiters, Stewards	0.16	0.37
Office Specialists	0.14	0.35
Salespersons	0.11	0.31
Office Auxiliary Workers	0.07	0.25
Others Attending on Guests	0.05	0.22
Network Characteristics		
Log Network Size	3.20	1.33
Employment Rate of Coworkers	0.61	0.20
Share of Female	0.64	0.29
Share of Non-German	0.06	0.11
Mean Age of employes	34.23	7.44
Share of Middle Educated	0.20	0.27
Share of Highly Educated	0.09	0.19
Individuals	3,285	

Continued on next page

Table 4.1 – continued from previous page

	Mean	SD

Notes: This table reports the means and standard deviations of the selected characteristics. Graduate characteristics include the individual characteristics of students who graduated between 2000 and 2016, as well as the characteristics of jobs where students worked for at least three months over five years prior to graduation (student jobs). We include the industry and occupation of the last student job. 12 industries are not displayed here because less than 5 % of the students in the sample worked in these industries. Network characteristics include the characteristics of close coworkers (same firm and occupation) of college students from their student jobs. We present descriptive statistics on the network characteristics of less close coworkers (same firm but other occupation) in Table 4.A.1. Network coworkers characteristics are measured at the time of graduation. First job characteristics are based on the first full-time job after graduation.

4.3. Empirical Strategy

The relationship of interest is whether the network of coworkers a student builds in student jobs affects the student's labor market outcomes after graduation. Our empirical analysis must account for the non-random allocation of students to their student jobs and the underlying unobserved motivation for choosing one job over another.

Our empirical analysis relies on the fact that student jobs are typically fixed in time and associated with unidirectional knowledge spillovers from coworkers to students. We observe the local labor market from 1995 to 2016 and can exploit the variation in coworker networks induced by individual students having multiple student jobs, and by different students working in the same firm but at different times (and thus having different coworkers).

We estimate the following baseline wage Equation:

$$
\begin{aligned}
log\, w_{i,t^G} =& \beta_1 log\, \overline{w}_{\sim i,jot^G} + \beta_2 log\, \overline{w}_{\sim o,ijt^G} + \\
& \gamma \mathbf{x'}_{i,t^G} + \delta_1 \mathbf{p'}_{\sim i,jot^G} + \delta_2 \mathbf{p'}_{\sim o,ijt^G} + \\
& s_{jt^C} + \theta_{ot^C} + \mu_{jt^C} + \eta_{t^G} + \epsilon_{i,jot^C}
\end{aligned} \tag{4.1}
$$

Our main outcome is $log\, w_{i,t^G}$, the log wage of student i after graduation, i.e. at time t^G. We regress the log wage of the graduate on the average quality of all former coworkers from student jobs. Coworkers are defined as working in the same firm j in the same (three-digit) occupation o at the same time t^C as the student. We proxy the quality of coworkers by their wages at the time of the student's graduation, i.e., $log\, \overline{w}_{\sim i,jot^G}$. While students sort themselves into occupations and firms during their student jobs (i.e., at time t^C), the actual wage of the coworkers at the time the student graduates from college (i.e., at time t^G) is unrelated to the students' non-random allocation to student jobs. The corresponding β_1 is our main coefficient of interest.

We also include the average wage at time t^G of all workers who worked in the same firm at the same time as the student but in different occupations ($\sim o$) than the student: $log\, \overline{w}_{\sim o,ijt^G}$ thereby controlling for shocks common to all workers who worked at the same time and in the same firms as the student. An example of such a shock is a common training for all workers.

To control for high ability students sorting into jobs with high quality coworkers, we include a large set of individual, firm, occupation, and network characteristics. First, we include individual characteristics x'_{i,t^G} that include time-invariant characteristics (gender, nationality, high school GPA) as well as characteristics at the time of graduation (number of student jobs, log average wage in student jobs, field of study).

Second, we include characteristics of the student's job: We control for the industry of the firm of the student job, s_{jt^C}, the occupation of the student, θ_{ot^C}, and the characteristics of the firm, μ_{jt^C}. The firm and occupation characteristics of the student's job were observable to the student. Students may have chosen certain firms or occupations in order to build a network of high quality colleagues. By including θ_{ot^C} and μ_{jt^C}, we account for self-selection into student jobs. To reduce the dimensions in our estimation, we operationalize μ_{jt^C} with the AKM establishment effects developed by Abowd et al. (1999) and provided for the universe of German employees by Bellmann et al. (2020). In addition, we include s_{jt^C}, θ_{ot^C}, and μ_{jt^C} only for the last student job before graduation. While we include these restrictions for practical reasons, we believe that the student job before graduation is the job with the highest degree of selection into favorable coworker networks.

Third, we include a comprehensive set of network characteristics. Again, we distinguish between networks of direct coworkers, i.e., employees working in the same occupation as the student, $p'_{\sim i,jot^G}$, and networks of other employees from the same firm, $p'_{\sim o,ijt^G}$. These two vectors of network characteristics p' include the log network size of a student, the employment rate of the coworkers, the share of female and non-German coworkers, the average age of the coworkers, and their education. We measure these characteristics at the time of graduation t^G to account for possible changes in the network since the student left the student job.[5]

We also include fixed effects for graduation cohort η_{t^G}. This is relevant because of differences in the first wage after graduation caused by different labor market conditions at the time of graduation (e.g. Schwandt and Von Wachter, 2019; Wachter, 2020).

[5]This strategy accounts for the fact that former coworkers may have been promoted, taken parental leave, or changed employers since the student left the firm. Of course, in an alternative specification, we could also include these network characteristics at the time of the student job. Including all $p'_{\cdot,t^C}$ would then account for the fact that students have preferences regarding their network prior to starting a student job. While in most cases the characteristics of future coworkers are unobserved, students may have some knowledge about potential coworkers from interviews for the student job, referrals from student peers who previously worked at the firm, or career counselors who have close ties to some firms. We believe that these cases are rare and are already captured by including occupation and firm effects.

ϵ_{ijot^C} is the residual error term. After controlling for individual characteristics, student sorting, and labor market conditions at graduation, we argue that the error term is uncorrelated with both our dependent variable and all covariates. However, there are more hypothetical scenarios that could lead to bias: First, workers could choose a particular firm and occupation *after* a student joins the firm, leading to the reflection problem provided by Manski (1993). We believe that – if present at all – these cases are so rare that they hardly affect our results. Second, we cannot observe the occupation-specific knowledge of the student. Suppose that a new technology is adopted by various firms shortly before students graduate from college, and the network of coworkers is already benefiting from the new technology. If students lack knowledge about the technology, the higher quality of workers may not be reflected in the wage of the graduate. This would lead to a downward bias, underestimating the true effect of the network on the graduate's wage.

4.4. Results

4.4.1. Main Results

Table 4.2 shows our main results from estimating Equation 4.1. Column (1) reports the regression results for our main outcome - a graduate's log wage in the first full-time job after graduation. In columns (2) to (4c), we report the coefficients on a set of additional outcomes regressed on coworker quality and respective controls.

Table 4.2 shows a positive and statistically significant relationship between coworker quality and the log wage of the graduate's first full-time job. Former coworkers positively affect the first wage after graduation (column (1)) and increase the speed of starting the first full-time job (column (2)). More specifically, we find that a 10% increase in the average wage of coworkers is associated with a 0.78% higher wage of graduates' first full-time job and a 1.45% reduction in the number of days to start a full-time job.

In column (3) of Table 4.2, we use college GPA as the outcome variable. Students with higher coworker quality might increase their study effort, for example, because they are more motivated or because they receive information that higher grades increase the likelihood of getting a higher-paying job. Conversely, students could also reduce their study effort if job-specific human capital (student job) and general human capital (university study) are substitutes. We find no effect of coworker quality on college GPA. Finally, coworker quality may also affect the match stability of the graduate's first full-time job, as coworker quality may affect employer and employee screening, and coworker quality leads to faster matches, which may reduce match stability. However, in columns (4a) to (4c), we do not find that coworker quality affects the probability of separation within the first 24 months of full-time employment.

In our subsequent analysis, we focus on the first two outcomes of Table 4.2 as these are the most relevant indicators for transition quality.

Table 4.2.: Effects of Student Job Coworker Networks

	Log daily wage at first job	Log days to start first job	College GPA	Separation within 6 months	Separation within 12 months	Separation within 24 months
	(1)	(2)	(3)	(4a)	(4b)	(4c)
Log avg. coworker wage	0.078***	-0.145***	0.011	0.007	0.006	0.014
– Same occupation	(0.022)	(0.047)	(0.015)	(0.017)	(0.018)	(0.019)
Log avg. coworker wage	0.024	0.002	0.026	0.032*	0.029	-0.019
– Other occupation	(0.024)	(0.051)	(0.017)	(0.019)	(0.020)	(0.025)
Adjusted R-squared	0.241	0.137	0.027	0.054	0.130	0.355
Individuals	3,285	3,285	3,285	3,285	3,285	2,665
Graduate controls	yes	yes	yes	yes	yes	yes
Coworker network controls	yes	yes	yes	yes	yes	yes
Other employee controls	yes	yes	yes	yes	yes	yes
Industry fixed effects	yes	yes	yes	yes	yes	yes
Firm effects	yes	yes	yes	yes	yes	yes
Occupation fixed effects	yes	yes	yes	yes	yes	yes
Graduation cohort fixed effects	yes	yes	yes	yes	yes	yes

Notes: The table shows OLS estimation results from the regression specified in Equation 4.1. The unit of observation is an individual graduate. We consider only the first full-time job after graduation as the first job. Graduate characteristics include gender, nationality, high school GPA, number of student jobs, log average wage in student jobs, field of study, and age. Coworker network and other employee controls include student's log network size, the employment rate of coworkers, the share of female and non-German coworkers, the coworkers' mean age and their education. Firm effects are the average AKM establishment effects across student jobs. Industry fixed effects and occupation fixed effects are included for the last student job prior to graduation. Heteroskedasticity-robust standard errors in parentheses. Significance levels: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

4.4.2. Heterogeneity

Table 4.3 explores two different types of heterogeneity: In Panel A, we split the sample by college GPA because college GPA may be a proxy for socioeconomic status (SES) and network quality may be especially helpful for low SES students to compensate for missing family networks. Specifically, we split the sample at the median GPA and classify all graduates above the median as having a ”high grade” and those below the median as having a ”low grade”. The coefficients are slightly larger for students with a GPA below the median, but due to the small differences in the coefficients, we argue that college GPA is not driving our results.

Table 4.3.: Wage and Job Finding Effects of Student Job Coworker Networks: Heterogeneity Analysis

	Log (Daily) Wage at the First Job			**Log Days to Find First Job**		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: By Graduation Grade						
	All Students	**High Grade**	**Low Grade**	**All Students**	**High Grade**	**Low Grade**
Log avg. coworker wage	0.078***	0.088**	0.099***	-0.145***	-0.137*	-0.174**
– Same occupation	(0.022)	(0.035)	(0.033)	(0.047)	(0.077)	(0.078)
Log avg. coworker wage	0.024	-0.020	0.025	0.002	0.006	-0.068
– Other occupation	(0.024)	(0.043)	(0.032)	(0.051)	(0.096)	(0.082)
Adjusted R-squared	0.241	0.221	0.173	0.137	0.134	0.117
Observations	3,285	1,333	1,332	3,285	1,333	1,332
Panel B: By Gender						
	All Students	**Female**	**Male**	**All Students**	**Female**	**Male**
Log avg. coworker wage	0.078***	0.088***	0.075**	-0.145***	-0.151**	-0.108
– Same occupation	(0.022)	(0.032)	(0.033)	(0.047)	(0.059)	(0.078)
Log avg. coworker wage	0.024	0.006	0.057	0.002	-0.001	0.043
– Other occupation	(0.024)	(0.033)	(0.036)	(0.051)	(0.065)	(0.095)
Adjusted R-squared	0.241	0.203	0.225	0.137	0.149	0.107
Observations	3,285	1,898	1,387	3,285	1,898	1,387

Notes: The table shows OLS estimation results from the regression specified in Equation 4.1 and separately by college GPA and gender. We split the sample by median GPA and classify those students with a college GPA above the median as ”High Grade” and those below the median as ”Low Grade”. The unit of observation is an individual graduate. We consider only the first full-time job after graduation as the first job. We include all variables as in Table 4.2. Heteroskedasticity-robust standard errors in parentheses. Significance levels: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

In Panel B, we split our sample by gender and distinguish between female and male graduates.

The relationship between coworker quality and a graduate's wage at labor market entry remains positive and statistically significant for both female and male graduates. Regarding the speed of entry, we find a stronger effect for female graduates: Columns (5) and (6) in Panel B of Table 4.3 show that female graduates benefit more from a high quality network than male graduates.

In Table 4.4, we distinguish between student jobs that were most likely chosen by students simply to earn money and student jobs that were more likely chosen in expectation of better future labor market outcomes. Specifically, we define "unrelated" student jobs as student jobs that are in "Wholesale and retail trade; repair of motor vehicles and motorcycles" or "Accommodation and food service activities" and are not internships or student worker jobs (*Werkstudent*). Any other student job is classified as a "related" student job. We assume that student jobs in unrelated industries are more likely to be typical student jobs to earn extra money, such as working in a bar, restaurant, or supermarket. Consistent with the assumption that jobs in unrelated industries are typical student jobs to earn money, Table 4.1 shows that students disproportionately choose these industries.

Table 4.4 shows the results of our estimations separately for students who worked in unrelated student jobs in columns (2) and (5) and those who worked in related student jobs in columns (3) and (6). Because some students worked in both unrelated and related student jobs, the number of observations does not add up to our baseline specifications in columns (1) and (3). The results in Table 4.4 show that our effects for log wages as an outcome are driven by coworker networks from related jobs, while the effects for the time to find a first full-time job are driven by coworker networks from unrelated jobs. In other words, these results suggest that higher coworker quality in related jobs improves the quality of the initial job, while higher coworker quality in unrelated jobs accelerates the transition to employment.

Table 4.4.: Wage and Job Finding Effects of Student Job Coworker Networks - Unrelated vs. Related Jobs

	Log (Daily) Wage at the First Job			Log Days to Start First Job		
	All student jobs	**Only unrelated jobs**	**Only related jobs**	**All student jobs**	**Only unrelated jobs**	**Only related jobs**
	(1)	(2)	(3)	(4)	(5)	(6)
Log avg. coworker wage	0.078***	0.002	0.083***	-0.145***	-0.188***	-0.083
– Same occupation	(0.022)	(0.034)	(0.025)	(0.047)	(0.067)	(0.057)
Log avg. coworker wage	0.024	0.051	0.004	0.002	-0.004	-0.006
– Other occupation	(0.024)	(0.034)	(0.031)	(0.051)	(0.066)	(0.071)
Adjusted R-squared	0.241	0.219	0.236	0.137	0.194	0.122
Observations	3,285	1,451	2,265	3,285	1,451	2,265

Notes: The table shows OLS estimation results from the regression specified in Equation 4.1 and separately estimated by unrelated and related jobs. Unrelated jobs are in sectors which are not related to the graduates' field of studies and are not internships or student worker jobs. These sectors are whole sale and retail trade; repair of motor vehicles and motorcycles, and accommodation and food service activities. Related jobs are all other student jobs. The unit of observation is an individual graduate. We consider only the first full-time job after graduation as the first job. We include all variables as in Table 4.2. Heteroskedasticity-robust standard errors in parentheses. Significance levels: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

4.4.3. Channels

A relatively high share of college graduates start their first full-time job at the same establishment where they previously worked as a student (about 20% in our estimation sample). Student jobs may act as a screening device for both students and employers. Higher average coworkers' wages in an establishment may increase the likelihood that the student stays in the establishment and earns a higher wage in her first full-time job compared to other students who selected to lower-paying establishments. We account for this feature of student jobs by excluding graduates who started their first full-time job in any of the establishments where they worked as a student in columns (2) and (5) of Table 4.5. In columns (3) and (6), we only exclude graduates who started their first full-time job in the last establishment where they worked as a student. We find that our main results, namely a positive and statistically significant association between log average coworker wages and the log wage of the first full-time job, remain even after excluding these graduates. Thus, although starting as a full-time employee in an establishment where the student previously worked is an important feature of student jobs, it cannot fully explain why having quality coworkers helps graduates earn higher wages in their first job.

Table 4.5.: Wage and Job Finding Effects: With and without Students who Started in Student Job Establishment

	Log (Daily) Wage at the First Job			**Log Days to Start First Job**		
	All Student Jobs	**Excluding Job in Same Establishment**	**Excluding Job in Last Establishment**	**All Student Jobs**	**Excluding Job in Same Establishment**	**Excluding Job in Last Establishment**
	(1)	(2)	(3)	(4)	(5)	(6)
Log avg. coworker wage	0.078***	0.049**	0.055**	-0.145***	-0.137***	-0.145***
– Same occupation	(0.022)	(0.025)	(0.024)	(0.047)	(0.051)	(0.050)
Log avg. coworker wage	0.024	0.031	0.036	0.002	0.024	0.030
– Other occupation	(0.024)	(0.026)	(0.026)	(0.051)	(0.054)	(0.054)
Adjusted R-squared	0.241	0.266	0.260	0.137	0.171	0.172
Individuals	3,285	2,628	2,730	3,285	2,628	2,730

Notes: The table shows OLS estimation results from the regression specified in Equation 4.1. In columns (2) and (5), we exclude any establishment in which the student has worked before. In columns (3) and (6), we exclude the last establishment in which the student worked before graduating. The unit of observation is an individual graduate. We consider only the first full-time job after graduation as the first job. We include all variables as in Table 4.2. Heteroskedasticity-robust standard errors in parentheses. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

In Table 4.6, we present the results of regressions in which we measure network characteristics separately for full-time and part-time coworkers at the time of the student's graduation. Consistent with an advice, inspiration, or anchoring mechanism, we find that only *full-time* coworkers affect the wage of the graduate's first full-time job. However, we find no effect on the speed of entry into the first full-time job. A 10% increase in the average wage of coworkers working full-time at the time of graduation is associated with a 1.39% higher wage of the graduate.

If better-quality coworkers generally have more information about, say, potential job openings or better-paying establishments, then all coworkers should affect the entry wage of the graduate's first full-time job. On the other hand, if advice, inspiration, or anchoring by full-time coworkers is important, then we would expect only the characteristics of full-time coworkers to matter in our estimates and no effect for part-time coworkers, since we focus only on graduates' first full-time job. However, it could also be that full-time coworkers simply had more opportunities to interact with the student.

Table 4.6.: Wage and Job Finding Effects: Full-time vs. Part-time Network Members

	Log (Daily) Wage at the First Job	**Log Days to Find First Job**
	(1)	(2)
Log avg. coworker wage - Full-time - Same occupation	0.139***	0.073
	(0.036)	(0.080)
Log avg. coworker wage - Full-time - Other occupation	0.045	-0.007
	(0.044)	(0.104)
Log avg. coworker wage - Part-time - Same occupation	0.005	-0.035
	(0.023)	(0.056)
Log avg. coworker wage - Part-time - Other occupation	0.021	0.004
	(0.022)	(0.055)
Adjusted R-squared	0.248	0.147
Observations	2,413	2,413
Graduate characteristics	yes	yes
Network characteristics	yes	yes
Average AKM establishment effect	yes	yes
Industry and occupation in student job	yes	yes

Notes: The table shows the OLS estimation results from a regression specified similar to Equation 4.1. We further distinguish between network characteristics by full-time and part-time employment at time of graduation. Thus, e.g., we measure the share of female former coworkers in the same (and other) occupation who are either in full-time employment or part-time employment at time of graduation. The unit of observation is an individual graduate. We consider only the first full-time job after graduation as the first job. We include all other variables, besides the network characteristics, as in Table 4.2. Heteroskedasticity-robust standard errors in parentheses. Significance levels: $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

4.4.4. Robustness

Our main results in Section 4.4.1 and illustrated in Table 4.2 use an extensive set of covariates that control for the selection of observably better students into better student jobs. We believe that our controls are good measures of student ability and the quality of potential peers. However, we cannot rule out the possibility that unobservable factors jointly affect our key independent variable and the outcomes. Therefore, to test the robustness of our results, we use another empirical strategy to assess the effect of higher earning student job coworkers on labor market entry.

We perform two robustness checks to validate our results. First, we use establishment fixed effects rather than relying only on the AKM establishment effect. As a result, we restrict the variation in our variables to only coming from at least two different students employed at the same establishment. The timing of the student job – students work in the same place but at different times –, the date of graduation – students may work in the same place at the same time but graduate at different times –, or students working in the same establishment but in different occupations are the main sources of variation in average coworker quality in this case. In Panel B of Table 4.7, we use establishment fixed effects and show that the coefficient is similar in magnitude to the baseline, but lacks statistical power because the variation comes from only a small subset of our sample.

Second, one might argue that average wages are not an adequate proxy for quality. Despite our confidence in the ability of our control variables to reduce bias in our measure of coworker quality due to non-random sorting, we perform a robustness check by using an alternative measure of coworker quality. Specifically, we use the person AKM effect provided by Bellmann et al. (2020). The person AKM effect is estimated through a regression with worker and establishment fixed effects, and can be interpreted as a combination of skills and other factors that are equally valued across employers (Card et al., 2013). This eliminates the need for additional conditioning on other network control variables or the establishment AKM effect. The person AKM effect is estimated only for full-time workers aged 20 to 60 (Bellmann et al., 2020). The results using the average person AKM effect of a college graduate's coworkers are presented in Panel C of Table 4.7. Our results indicate that the coefficient size for log wages as the outcome is comparable to our baseline estimate, although statistically insignificant. This statistical insignificance is likely due to the smaller number of observations and reduced variation as the person AKM is only computed for full-time workers. Similar to Table 4.6, we find that the quality of full-time coworkers has no effect on the time it takes for a graduate to find his or her first full-time job.

Table 4.7.: Wage and Job Finding Effects of Student Job Coworker Networks

	Log (Daily) Wage at the First Job	Log Days to Start First Job
Panel A: Baseline		
Log avg. coworker wage - Same occupation	0.078***	-0.145***
	(0.022)	(0.047)
Adjusted R-squared	0.241	0.137
Individuals	3,285	3,285
Panel B: Establishment Fixed Effects		
Log avg. coworker wage - Same occupation	0.066	-0.163
	(0.048)	(0.109)
Adjusted R-squared	0.268	0.229
Individuals	3,285	3,285
Panel C: Worker AKM Effects		
Avg. coworker AKM - Same occupation	0.077	-0.017
	(0.050)	(0.098)
Adjusted R-squared	0.240	0.135
Individuals	3,040	3,040

Notes: The table shows robustness checks of our baseline estimation in Equation 4.1. In panel A, we present the baseline specification. In panel B, we add establishment fixed effects and include the same control variables as in our baseline specification. In panel C, we use the average person AKM of coworkers in the same and other occupation instead of log average wages. Therefore, we exclude all network characteristics and establishment AKM from the estimation in panel C. The unit of observation is an individual graduate. We consider only the first full-time job after graduation as the first job. We include all other variables, besides the network characteristics, as in Table 4.2. Heteroskedasticity-robust standard errors in parentheses. Significance levels: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

4.5. Conclusion

This paper provides new insights into the role of coworker networks from student jobs in enhancing career advancement and access to job opportunities. While previous studies have focused on more institutionalized networks such as classmates or roommates, we show that more informal networks from student jobs are also relevant. These networks of coworkers can help reduce information frictions, which are likely to be highest at the beginning of a career.

Our findings indicate that graduates benefit from the quality of their coworker networks in the form of faster labor market transitions and higher entry wages. Although we do not have exogenous variation in network quality, the strength of our data, in particular the large set of control variables, allows us to come close to a causal effect. Moreover, our results are robust to different specifications and robustness checks. Interestingly, we do not find much heterogeneity across gender or student ability. However, the results show that the type of student job matters, with the quality of coworkers in related jobs having a positive effect on the quality of the first job, while better-quality coworkers in unrelated jobs lead to faster employment.

The size of our effects are remarkable. A 10 % increase in the average wage of former coworkers is associated with a 0.78 % higher wage in the first full-time job. This effect is about twice as large compared to a 10 percentage point increase in the share of workers from the same minority in the same firm (Dustmann et al., 2016) and about 7 times larger than the spillover effects of working with productive coworkers (Cornelissen et al., 2017), both in the German context. Note, however, that while our paper estimates the effect of having better quality coworkers in student jobs on wages at a later point in time, the paper by Cornelissen et al. (2017) estimates the immediate spillover effects of having better quality coworkers in the same firm and occupation, which can explain much of the difference in the size of our and their results. Moreover, a back-of-the-envelope calculation suggests that a 100 % increase in coworker quality would reduce the number of days to find a first full-time job by approximately 16 days from the median. These results point into the same direction as the study by Kramarz and Skans (2014), who analyze the effect of having a parent working in the same plant. Given that a parent is much closer to the student than a former coworker, it is plausible that their result is about eleven times higher in magnitude.[6]

Overall, we show that student jobs matter beyond their purpose of providing a living. Our results clearly suggest that networks of better quality coworkers built during student jobs improve the transition from college to the labor market, most likely by reducing information frictions very early in a person's career. This study highlights the importance of considering coworker networks in policies aimed at smoothing the transition from higher education to employment and provides valuable insights for future research on the topic.

[6]Note also, that Kramarz and Skans (2014) employ a different definition of the first job than we do.

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Appendix

4.A. Additional Tables and Figures

Table 4.A.1.: Network Characteristics in Other Occupations

	Mean	SD
Student Jobs Network Characteristics - Other Occupations		
Log Average Coworker Wage	4.12	0.61
Log Network Size	3.27	1.29
Employment Rate of Coworkers	0.65	0.20
Share of Female Coworkers	0.58	0.27
Share of Non-German Coworkers	0.09	0.15
Mean Age of Coworkers	38.76	6.85
Share of Middle Educated Coworkers	0.26	0.27
Share of Highly Educated Coworkers	0.10	0.17
Individuals	3,285	

Notes: This table reports the means and standard deviations of the network characteristics of less close coworkers. Less close coworkers work in the same firm but in another occupation as college students in their student jobs. Network coworkers characteristics are measured at the time of graduation.

Figure 4.A.1.: Mean Daily Wage of Coworkers per Student

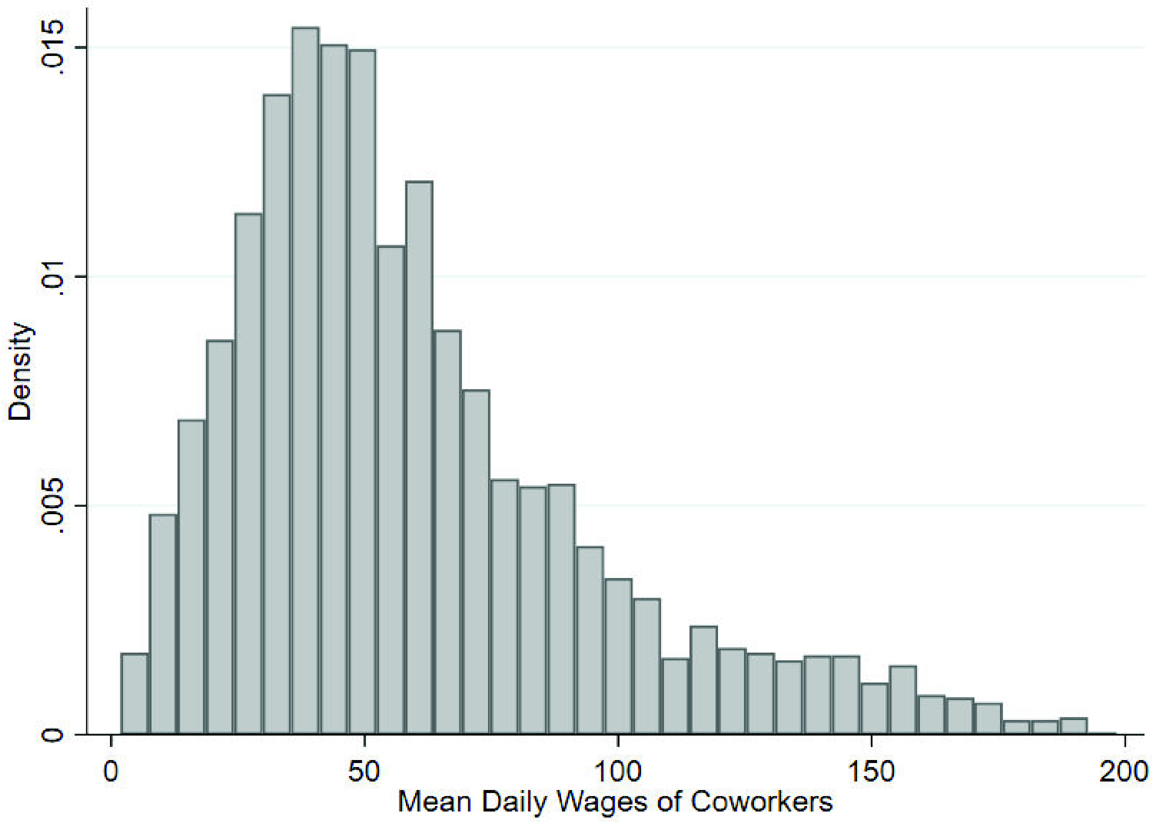
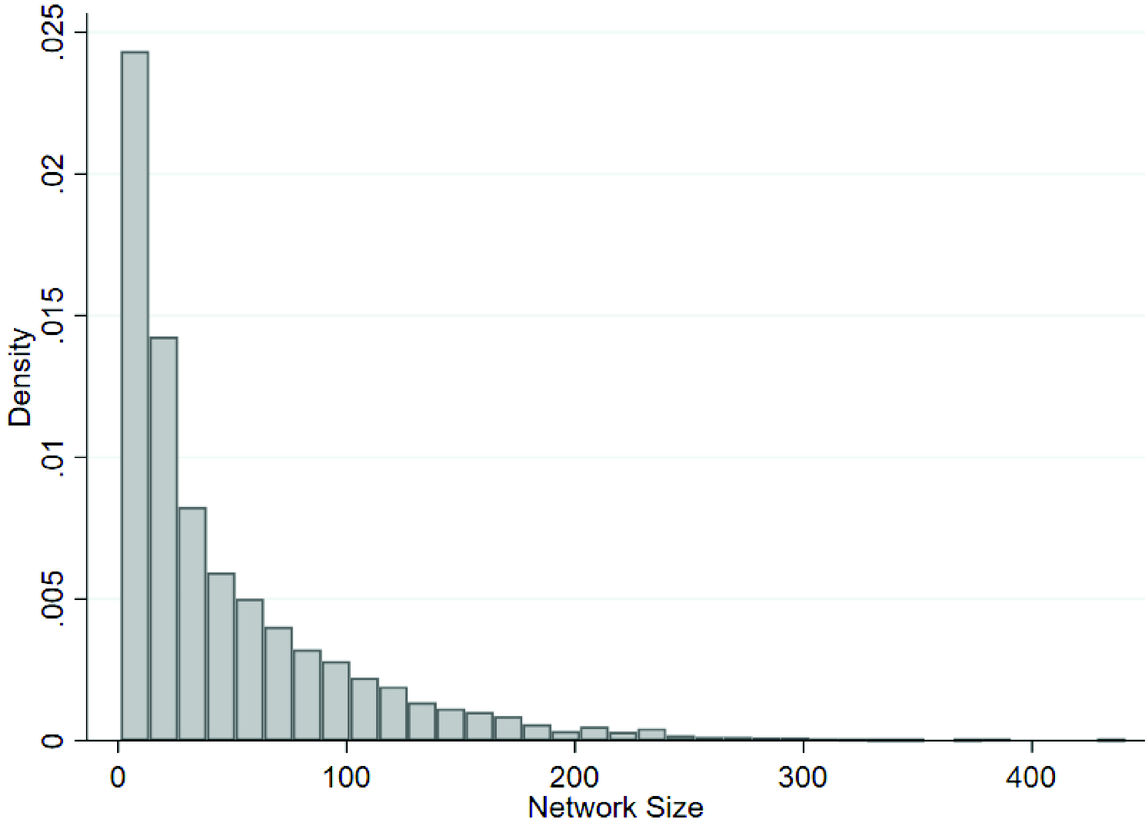


Figure 4.A.2.: Distribution of Network Size per Student



Note: This figure shows the distribution of network size for each student. Network size measures the number of coworkers a student has over 5 year prior to the graduation. Students with jobs longer than three months and a network size of 250 coworkers per job are excluded. The reason for having a network size greater than 250 is that students can work in several student jobs during their study.

Figure 4.A.3.: Daily Wage at the first Full-Time Job after Graduation

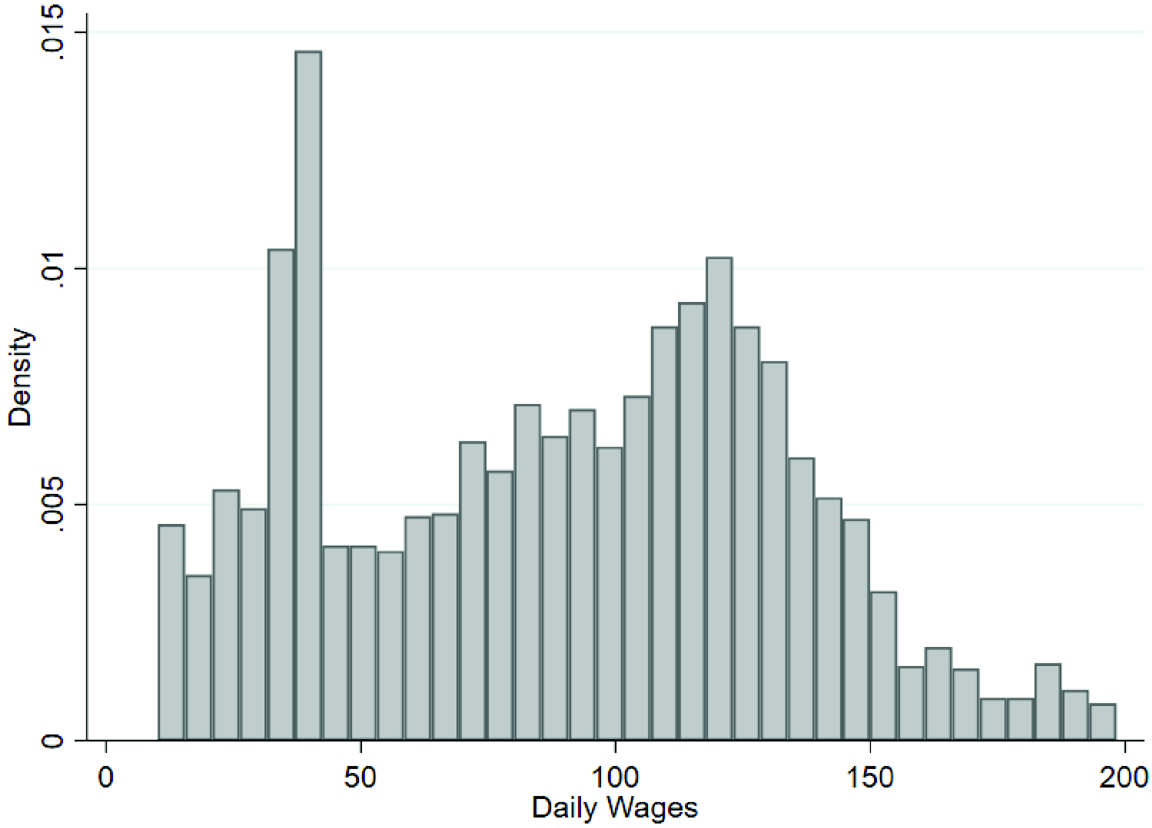


Figure 4.A.4.: Days to Find First Full-time Job After Graduation

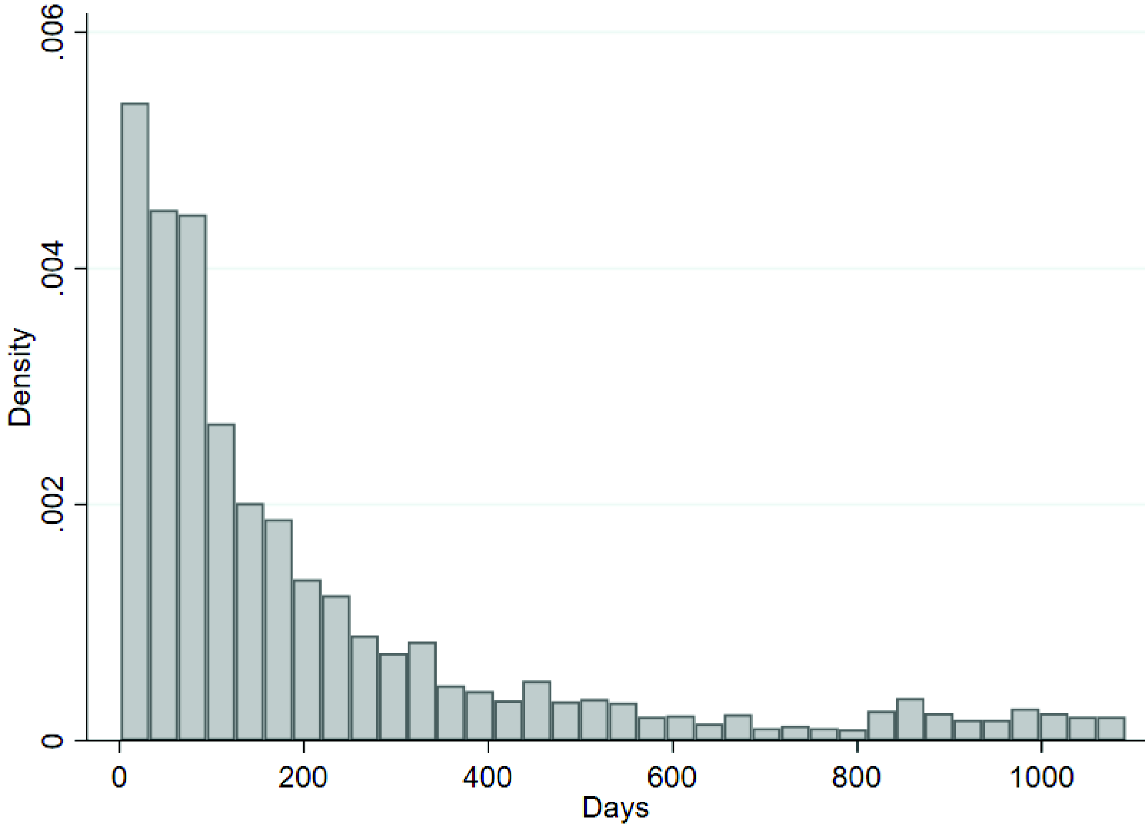


Figure 4.A.5.: Establishment Size of Student Jobs

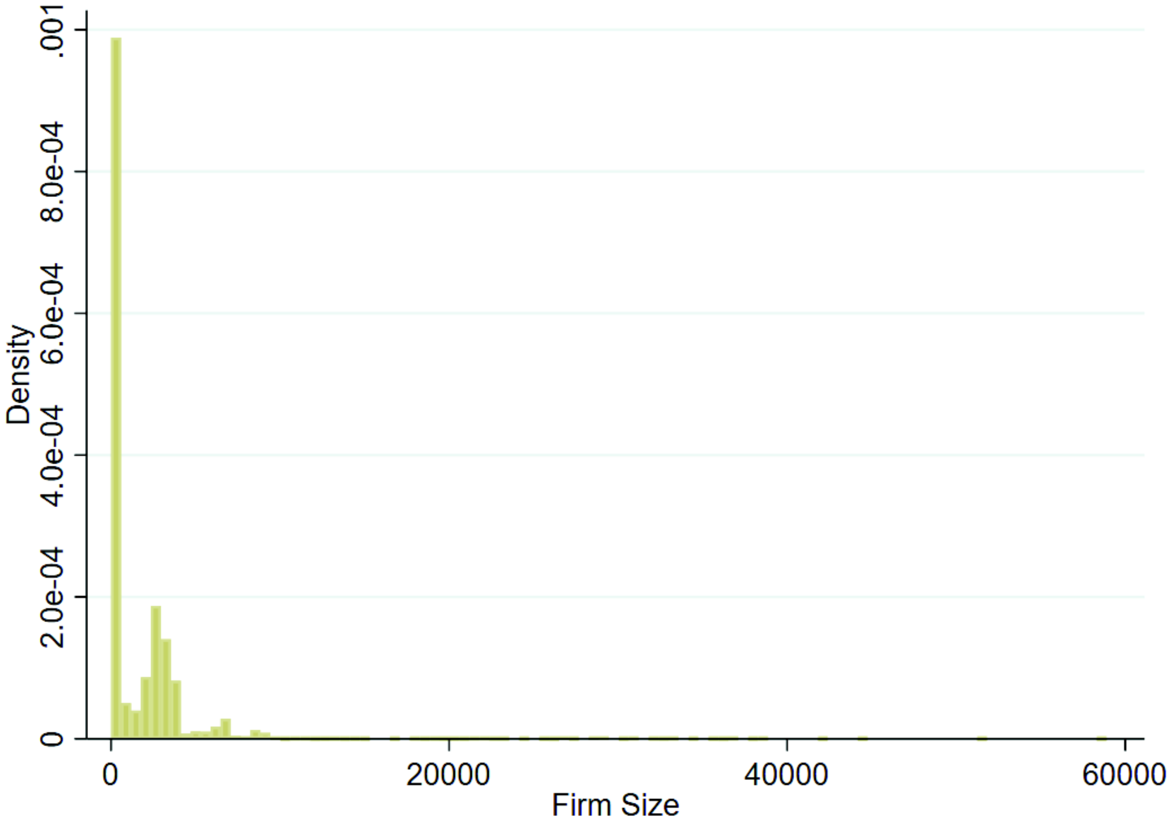
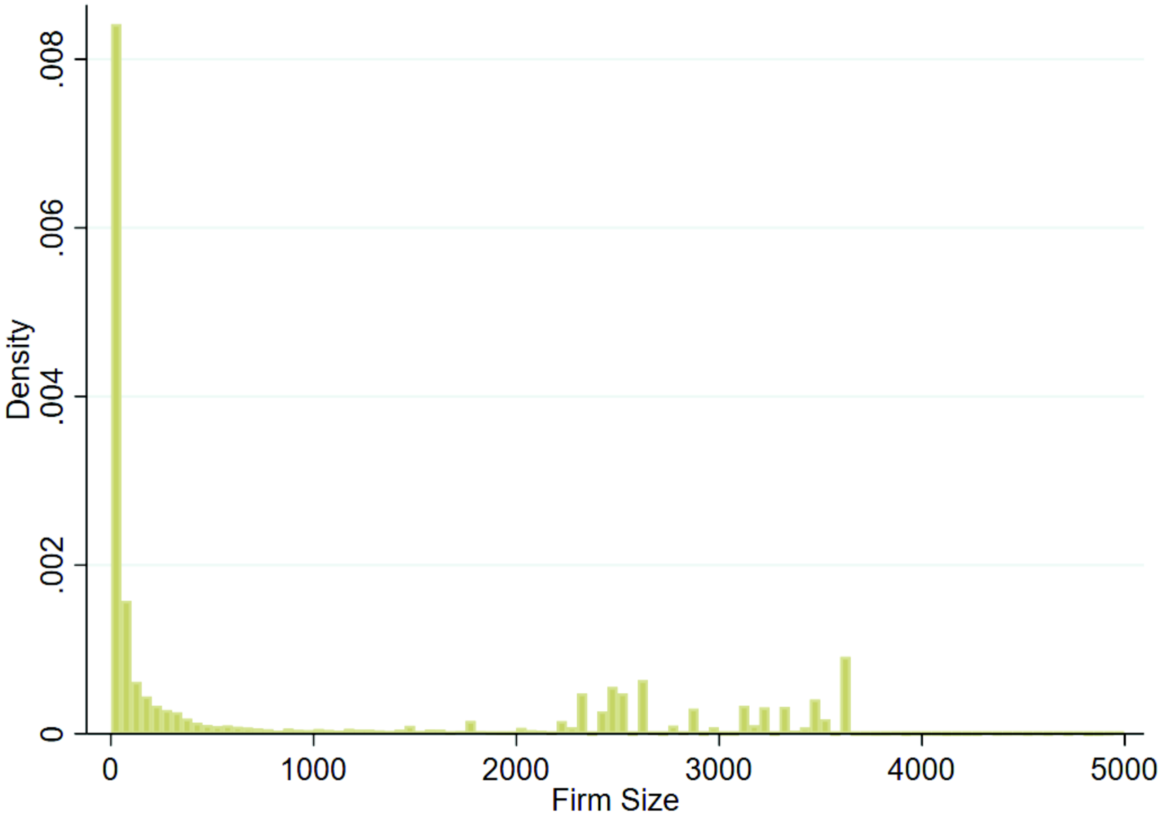


Figure 4.A.6.: Establishment Size of Student Jobs- Less than 5000 Employees



Declaration of Contribution

Hereby I, Gökay Demir, declare that the Chapter "Students' Coworker Networks and Labor Market Entry" is co-authored by Friederike Hertweck, Malte Sandner, and Ipek Yükselen. All authors contributed equally to the chapter.

Signatures of the co-authors:

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Eidesstattliche Versicherung

Ich, Herr M.Sc. Gökay Demir, versichere an Eides statt, dass die vorliegende Dissertation von mir selbstständig und ohne unzulässige fremde Hilfe unter Beachtung der „Grundsätze zur Sicherung guter wissenschaftlicher Praxis an der Heinrich-Heine-Universität Düsseldorf“ erstellt worden ist.

Essen, der 10. Februar 2023

Unterschrift