Three Essays on the Impact of International Trade on the German Labor Market

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Introduction

Political and economic developments have stimulated international trade over the last decades. In particular, the fall of the Iron Curtain in 1989 and subsequent transformation of the former socialist states in Eastern Europe, the Eastern enlargement of the European Union (EU) since 2004, and the economic rise of China and its accession to the World Trade Organization (WTO) in 2001 mark important events for the German economy. The liberalization of trade and factor flows that came with these events has led to a massive increase in German exports to and imports from China and Eastern Europe (the East) (see, e.g., Dauth *et al.*, 2014). Today, the global embeddedness and openness of the German economy is one of its success factors—hardly any other large Western country benefits from international trade as much as Germany (see, e.g., Felbermayr *et al.*, 2017).

Notwithstanding the gains from international trade for the German economy as a whole, effects at the regional, industry, firm, and individual level can be very heterogeneous. As a result, analyzing the distributional effects of international trade has emerged as an important field of economic research. After all, identifying the winners and losers of international trade integration is of central interest for policy makers.

This thesis provides new empirical evidence on the impact of international trade on the German labor market. In three chapters, I focus on the distributional effects associated with various forms of trade between Germany and the countries in the East, and the Czech Republic in particular. Exploiting highly reliable administrative data on German individual workers, I study effects of international trade integration at highly disaggregated levels.

The first chapter, which is joint work with Florian Knauth, addresses the question whether the increase in trade with China and Eastern Europe contributes to the increase in wage inequality in Germany (see, e.g, Dustmann *et al.*, 2009; Antonczyk *et al.*, 2018). In particular, we address through which channels trade affects wage inequality within German manufacturing industries. Does it affect inequality through changes in firm-specific pay premia, worker-specific wages, or assortative matching between high-wage workers and high-wage firms? By answering these questions, we contribute to the literature along the lines of Autor *et al.* (2013, 2014) that studies the labor market effects of China's integration into world trade (see Autor *et al.* 2016 and Muendler 2017 for an overview). For the empirical analyses, we use a large sample of administrative data on German workers in the manufacturing sector and decompose their wages into firm and worker components applying the method of Abowd *et al.* (1999). Our results show that the rising market access and competitiveness of the East plays a significant role in the rising wage inequality within German manufacturing industries. The rise in wage inequality is attributable to a more dispersed worker-wage component and partly to more assortative matching. By contrast, trade does not explain changes in the firm-wage premium. The results show that trade with the East explains about 19% of the recent increase in wage inequality. In additional analyses, we account for technological change, which contributes to the dispersion of worker-specific wages and explains an approximate 15% of the increase in wage inequality. Controlling for technological change, we find that trade with the East can still explain about 10% of the recent increase in wage inequality.

In the second chapter, which is joint work with Johann Eppelsheimer, we narrow the research focus down to effects of trade between Germany and the Czech Republic. More specifically, we examine the impact of German firms' foreign direct investment (FDI) in the Czech Republic on their workers' job stability. With this study, we contribute to the literature on the effects of FDI and offshoring on individual labor market outcomes (see Crinò 2009 and Hummels *et al.* 2018 for an overview). We argue that firm-internal job transitions are an important channel for firms to adjust their workforce to changes in the labor demand following FDI. For the empirical analyses, we use an administrative linked employer-employee data set on German firms with affiliates in the Czech Republic between 1990 and 2010 as well as an extensive set of domestic control firms that never invested abroad. After matching the investing firms to comparable domestic firms with equal probability to invest, we estimate the effects of FDI on individual job stability using proportional hazard models. In particular, we estimate the impact of FDI on the likelihood that workers experience firm-internal up- or downgrades, i.e., transitions into jobs with more or less analytical and interactive tasks. We find that FDI increases the likelihood of up- and downgrades by 24% and 34%, respectively. Moreover, our results show that job transitions become significantly more likely two years after the investment and are more likely for workers with a larger share of non-routine and interactive tasks. We additionally show that on average, FDI does not increase the hazard that workers and firms separate. Rather, FDI has a temporal lock-in effect after the investment. On the whole, the results in this chapter highlight that internal restructuring is an important channel through which firms adjust to FDI-induced changes in labor demand.

In the third chapter, I expand on the insights from Chapter 2 and estimate the impact of FDI on workers' labor market outcomes over their careers. To provide comprehensive insights into the longer-run effects of FDI, I study FDI-induced changes in workers' annual earnings and trace them back to changes in their daily wages on the one hand and their number of days in employment on the other. Using the matched samples of investing and domestic control firms from Chapter 2, I follow a fixed cohort of workers and estimate effects of FDI in an event study difference-in-difference (DiD) design. The DiD estimations yield no negative effects of FDI on earnings, nor on daily wages or the number of days in employment. For medium-skilled workers, FDI even has a positive effect on annual earnings, mainly because it increases their days in employment relative to the control group of workers starting out in domestic firms. For low- and high-skilled workers, I do not find any effect of FDI on annual earnings. Among workers who stay with their employer after FDI, I find benefits from FDI for low- and medium-skilled workers, who have higher average daily wages, and high-skilled workers, who have more days in employment compared to the control group. To sum up, this chapter's results show that independent of whether workers stay with their employer, effects of FDI on their labor market outcomes are minor—at best, they are positive.

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Chapter 1

Trade, Technology, and the Channels of Wage Inequality

Co-authored with Florian Knauth

1.1 Introduction

Over the last 30 years, wage inequality has grown significantly in many industrialized economies, such as Germany and the US (see, e.g., Dustmann *et al.*, 2009; Autor, 2014; Antonczyk *et al.*, 2018). Recent research has shown that a large part of this increase in wage inequality is attributable to technological change and globalization, in particular to offshoring and low-wage competition (e.g., Katz & Autor, 1999; Autor *et al.*, 2008; Krugman, 2008; Autor *et al.*, 2014; Hummels *et al.*, 2014).[1] An emerging strand of literature has exploited the rise of China as a trading economy to study how labor markets adjust to globalization (e.g., Autor *et al.* 2013, 2014, for the US, and Dauth *et al.* 2014, 2018, for Germany).[2]

Another prominent strand of literature has focused on how heterogeneity in the wage setting of firms on the one hand and the heterogeneity of workers on the other contribute to wage inequality (e.g., Card *et al.*, 2013, 2018, for Germany). These studies decompose individual wages into worker fixed effects, which capture all time-invariant worker characteristics that are equally valuable across firms, and firm fixed effects, which capture firm-specific pay premiums that are proportionally paid to all employees independent of their characteristics. Using high-dimensional fixed effects estimators introduced by Abowd *et al.* (1999, hereafter, AKM), they show that rising wage inequality is driven by the growing heterogeneity of workers, by increasing differences in firm-specific pay premiums, and by more assortative matching of high(low)-wage workers to high(low)-wage firms.

In this paper, we bridge these two strands of literature to determine the channels through which trade and technology affect wage inequality in industrialized countries. In particular, we provide evidence of whether technological progress and the increase of trade with China and Eastern Europe (hereafter, the East) affect assortative matching and the distribution of the worker- and firm-specific pay component in German manufacturing industries. In this way, the paper provides evidence showing which of the following theoretical channels explain how international trade and technology affect the wage structure.

First, theory suggests that changes in trade and technology will affect skill demand and returns on skills and thus the distribution of the worker-wage component. In-

[1] See Helpman (2016) for a comprehensive literature review on globalization and wage inequality.

[2] See Autor *et al.* (2016) and Muendler (2017) for a comprehensive overview.

tensified international trade raises the relative demand for skilled labor, and thereby increases the skill premium (e.g., Feenstra & Hanson, 1996, 2003; Monte, 2011; Epifani & Gancia, 2008). Similarly, technological change will lead to increasing skill demand, because skills are complementary to new technologies (Autor *et al.*, 2008; Dustmann *et al.*, 2009; Autor & Dorn, 2013).

Second, changes in trade intensity will affect inequality in the firm-pay premium. Theories that combine heterogeneous firms with labor market frictions (e.g., Egger & Kreickemeier, 2012; Amiti & Davis, 2012) show that exporters can share their additional foreign profits with their employees. Thus, high export orientation is assumed to pay an additional firm-wage premium. If only the most productive firms engage in international trade (Melitz, 2003), better market access should raise inequality in the firm-wage component within industries. Moreover, it must be considered that increasing trade with the East is largely due to Eastern firms' strong increase in competitiveness. German firms that cannot withstand this import competition will be crowded out. Therefore, it is also possible that inequality in the firm fixed effect will decrease. However, if firms do not withdraw from competition completely but reduce their firm-pay premium to withstand competition, inequality in the firm-wage premium may increase. On the whole, the effect of trade on the firm-pay premium is theoretically ambiguous and may point in either direction.

Third, according to theoretical models with two-sided heterogeneity and supermodular production functions, increased international trade will affect the matching between firms and workers (e.g., Helpman *et al.*, 2010; Davidson *et al.*, 2008; Davidson & Matusz, 2012; Sampson, 2014). Generally, given the complementarity between workers' ability and firms' productivity, high-ability workers will be more valuable to exporters, which are more productive (Melitz, 2003). More openness can change firms' production processes, so that some firms start exporting to larger markets or specializing in the production of high-tech goods. These firms' revenue and productivity increases and thus, it is more profitable for them to employ workers with higher abilities. Consequently, better workers will be matched to better firms, and this assortative matching will increase wage inequality within industries. In contrast, the effects of increased import competition on assortative matching are theoretically unclear (see Davidson & Matusz, 2012). Empirical findings support the prediction that more openness leads to more assortative matching (e.g., Davidson *et al.*, 2012, 2014; Bombardini *et al.*, 2018). In line with our approach, some of these papers also apply the AKM wage decomposition method to estimate assortative matching by the covariance between worker and firm fixed effects. Among these, Baziki *et al.* (2016) find that low-wage competition from China increases sorting in industries that intensively use information and communication technologies (ICT).

With respect to technological change, the task-based approach suggests that new technologies substitute routine tasks and complement nonroutine tasks (Autor *et al.*, 2003). The task-based approach predicts that technological progress comes with job polarization, as many routine jobs are performed by medium-qualified workers (e.g., Acemoglu & Autor, 2011; Spitz-Oener, 2006). These workers are typically found in the middle of the wage distribution. Thus, the task-based approach can also explain the polarization of wages in advanced economies (e.g., Dustmann *et al.*, 2009, for Germany).

In this paper, we take a series of steps to analyze through which of these channels trade and technology affect wage inequality within industries. In a first step, we decompose wages by applying the fixed effects estimator introduced by AKM. The decomposition reveals how much of the wage is firm- and how much of it is worker-specific. In a second step, we analyze how the distribution of these wage components responds to changes in the industry's exposure to trade and technology.

In our analysis, we isolate the effects of trade and technology on wage inequality. First, to estimate the causal effect of trade on wage inequality, we account for potential endogeneity regarding demand shocks. Unobserved product demand shocks could simultaneously affect imports and wages. Therefore, we apply a gravity residuals approach based on general equilibrium trade theory (see, e.g., Anderson & Van Wincoop, 2003). This approach was previously used by Autor *et al.* (2013, hereafter ADH) and Dauth *et al.* (2014, hereafter DFS), for example. Gravity residuals measure the increase in trade exposure that goes back to changes in productivity and transport costs. Thus, they reflect increases in the market access and competitiveness of Chinese and Eastern European industries relative to German industries. Alternatively to the gravity approach, we also apply the widely used instrumental variables (IV) (e.g., ADH).

Second, we control for concurrent developments in the automation of tasks and technological change by applying widespread task-based measures. Because automation technologies are supposed to substitute routine tasks, we exploit changes in an industry's share of routine jobs as a proxy for technological progress within industries.

Moreover, changes in wage inequality are also attributable to institutional changes and labor market reforms (see, e.g., Dustmann *et al.*, 2009). For instance, Felbermayr *et al.* (2014) find an interdependence between unionization and the exporter-wage premium for Germany. On that account, we also consider different effects of trade with regard to changes in the union coverage rate of industries.

The empirical implementation is based on a large sample of workers and firms. The AKM-method requires a sufficient set of firms connected by workers switching between them in order to identify high-dimensional fixed effects for both workers and firms. We therefore use a 50% sample of administrative data of all full-time working men in West Germany between 1985 and 2010. Information on trade volumes comes from the United Nations Commodity Trade Statistics Database (Comtrade). By linking the two data sets, we are able to measure the worker and firm contribution to wage inequality within industries which are heterogeneously exposed to trade.[3]

We find strong evidence that the rising competitiveness of the East leads to an increase in the dispersion of the worker-wage component and assortative matching between better firms and better workers, particularly in high-tech industries. We find no effect on the dispersion of the firm-wage component. Looking at the within skill-group distribution, results show that trade affects the wage dispersion of medium-skilled workers through the individual-wage component. Among the low-skilled and high-skilled workers, we also see a large increase in wage dispersion. However, this increase in inequality is not attributable to trade exposure. In general, our findings favor models of heterogeneous workers with assortative matching (e.g., Yeaple, 2005; Davidson *et al.*, 2008; Helpman *et al.*, 2010; Sampson, 2014; Grossman *et al.*, 2017) and models that are able to explain the positive skill premium by higher returns to scale in larger markets (e.g., Epifani & Gancia, 2008; Monte, 2011)[4] over models emphasizing the role of firm-wage premiums in determining wage inequality (e.g., Egger & Kreickemeier, 2012; Amiti & Davis, 2012).

Our paper complements the literature on the distributional effects of trade and technological change. ADH find that increased import exposure from China leads

[3]We use the terms firm, establishment, and plant interchangeably in this paper. In fact, the data at hand are at the establishment level.

[4]These theories, which assume a monotonic effect on skill, cannot explain more complex changes of the wage distribution, e.g., a polarization of wages that is mainly driven by a decrease in medium-skilled occupations (see, e.g., Autor *et al.*, 2008; Acemoglu, 2003; Acemoglu & Autor, 2011) or by an increase in wage inequality at both ends of the wage distribution.

to lower manufacturing employment in the US. They do not find a wage effect in the manufacturing sector. For Germany, DFS show that an increase in the export exposure of a region is followed by a small increase in the regional wage level. They do not find any impact of the regional import exposure on wages. In contrast to ADH and DFS, we focus on industry and not the regional effects of trade and on the distribution of wages rather than average wages. Ebenstein *et al.* (2014) do not find any effect of increased import exposure on the industry level for the US. However, they do find that workers in exposed occupations are pushed out of the manufacturing sector to find themselves in lower-paying sectors and occupations. In another study, Bloom *et al.* (2016) link trade to technological change and find that Chinese import competition leads to technology upgrading. Moreover, they show that leading high-tech firms react via increased sorting to low-wage competition.

Similar to our approach, previous papers have used results of the AKM decomposition to analyze the impact of international trade on wages. Frias *et al.* (2018) and Macis & Schivardi (2016) find evidence of a positive exporter wage premium by examining the relationship between the export status of a firm and the firm fixed effect. Moreover, the paper by Baziki *et al.* (2016), which is closely related to our approach, provides evidence that increased assortative matching occurs in industries with high Chinese trade exposure and intensive use of information and communication technologies. We expand their focus on the worker-to-firm sorting process by looking at the effects of international trade and technological change on all decomposed wage components separately.

The paper is structured as follows. In section 1.2, we present the data sets used for our empirical analysis, describe the wage decomposition, and provide some descriptive results and stylized facts about the inequality in wage components. In section 1.3, we introduce our estimation strategy and explain the construction of the independent variables. The estimation results on the impact of trade on wage components are presented in section 1.4. Section 1.5 concludes.

1.2 Data and wage decomposition

1.2.1 Data

Our main data source is the Employee History data (BeH, V.09.05.00) of the Institute for Employment Research (IAB), from which we draw wages and all relevant worker-level information. The BeH are comprehensive administrative data that contain all employees subject to social security in Germany. We use a 50% random sample of the BeH between 1985 and 2010 of all full-time working men aged 20 to 60 in West Germany.[5] For the wage decomposition we need person-year observations. Therefore, we only consider a worker's highest paid job in every year. As the administrative data is originally used to calculate social security contributions, it is highly reliable and complete. We correct missing and inconsistent education data by using the routine described in Fitzenberger *et al.* (2005). Apart from that, wages above the threshold level for social security notifications are not recorded and need to be imputed. The imputation procedure follows the method of Card *et al.* (2013). For information on the firm level, we use aggregated data of the Establishment History Panel (BHP, 75_10_v1).[6]

To calculate an industry's exposure to trade, we use the UN Comtrade database from 1985 to 2010. Following DFS, we look at Germany, China, various Eastern European countries, and their bilateral trading partners. We restrict our analysis to manufacturing industries. We match the data along four-digit product codes to the German Classification of Economic Activities 1993 by using the correspondence tables of the UN Statistics Division and correct for inflation. For our final analyses we only kept industries with at least 500 employees and 20 firms.

From the BIBB-IAB Employment Surveys 1979 to 1999 and the BIBB/BAuA Employment Survey 2006, we draw information on tasks to construct our measure of technological change.[7] Additionally, we use the IAB Establishment Panel (9313_v1) for industry-level information on collective wage agreements.[8]

[5] We restrict our analysis to full-time jobs and exclude trainees. The reason is that non-standard work, like part-time jobs, are different sources of wage inequality that we do not want to measure. Thus, we avoid letting changes in the use of non-standard work drive our results. Moreover, the data set does not provide exact information on working hours to make full- and part-time daily wages comparable.

[6] For more information on the Establishment History Panel see Gruhl *et al.* (2012).

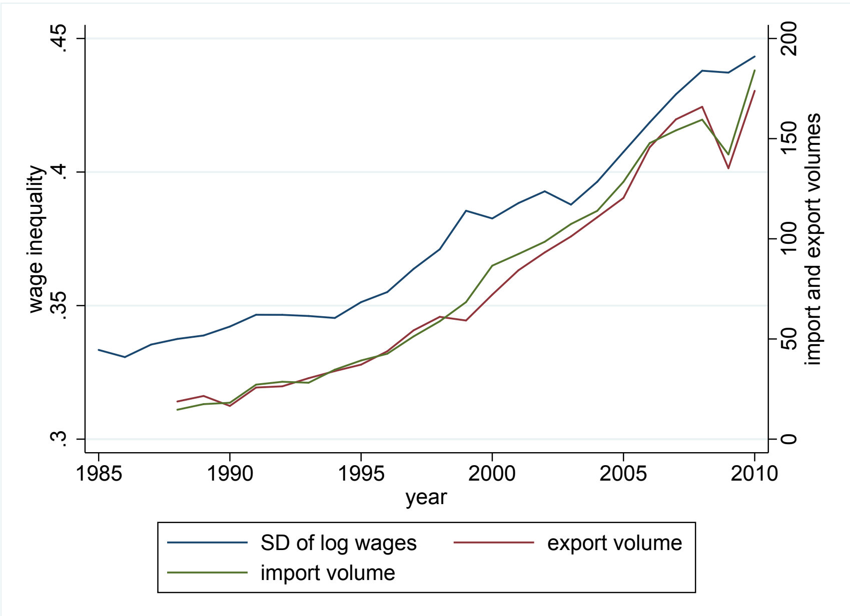
[7] For more information on the Employment Surveys see Hall & Tiemann (2006).

[8] For more information on the Establishment Panel see Ellguth *et al.* (2014).

1.2.2 Stylized facts about wage inequality and rising trade exposure

In the public perception, there is a strong connection between globalization and rising income inequality. Indeed, Germany has experienced a strong increase in wage dispersion, especially from the 2000s onwards. Over the same period several trade liberalizations took place that led world trade volumes to increase quickly, e.g., the fall of the Iron Curtain in 1989, China's accession to the World Trade Organization (WTO) in 2001, and the Eastern Enlargement of the European Union (EU) in 2004. In this paper, we use these trade shocks to analyze the impact of increased import competition from the East on the distributional changes in wage components within manufacturing industries in Germany. Figure 1.1 depicts the parallel rise of wage dispersion in Germany and in import and export values of Germany and the East.

Figure 1.1: Wage inequality and trade volumes in Germany, 1985–2010

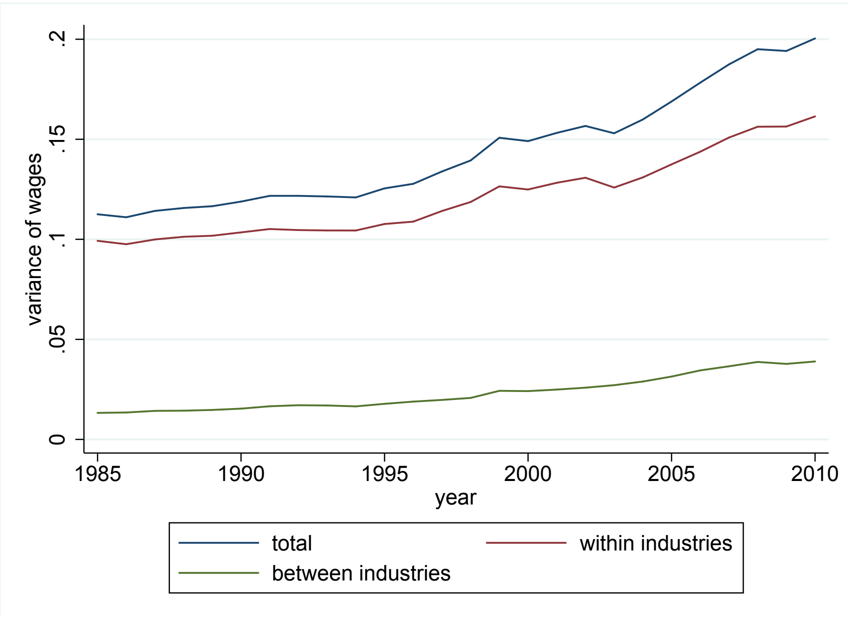


Notes: The left axis depicts the standard deviation of log wages of full-time working men aged between 20 to 60 in West Germany between 1985 and 2010. The right axis depicts import and export volumes in billion Euros between Germany and China as well as Germany and Eastern Europe between 1988 and 2010.
Source: Own calculations, BeH and Comtrade.

In our empirical model, described in detail in section 1.3, we analyze whether industry-specific shocks in trade and technology can explain the increase in wage dispersion within sectors. Sectors are differently exposed to the import competition and export opportunities of the East. Thus, we expect to see different effects on wage inequality within industries.

The question arises of how much of the overall wage variation in Germany is actually explained by the dispersion within and across sectors. Figure 1.2 shows that although the between-share is on the rise, the within-industry part explains by far the largest share, namely between 81% and 88% of wage inequality in Germany. This result is in line with the literature (e.g., Baumgarten, 2013; Helpman, 2014). Thus, we are convinced that by considering within-sector inequality, we can explain a major part of wage dispersion in this paper.

Figure 1.2: Within- and between-industry variance of log wages, 1985–2010



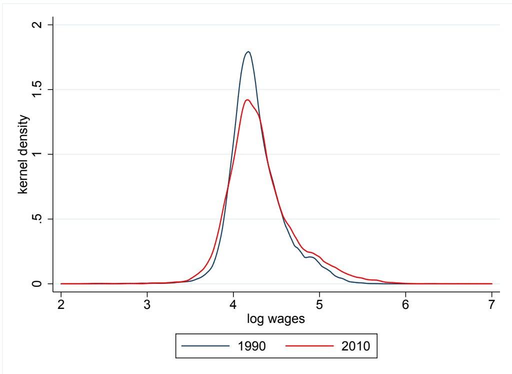
Notes: The graph depicts the variance of log wages (total) and the variance within and between three-digit manufacturing industries. The sample includes full-time working men aged between 20 and 60 in the manufacturing sector in West Germany between 1985 and 2010.
Source: Own calculations, BeH.

Figure 1.3 shows that wage inequality develops differently between industries.[9] The graphs present shifts in the wage distribution for selected industries between the first interval, 1990 to 1995, and the last, 2005 to 2010, in our data, i.e., a while before and after China entered the WTO in 2001 and the 2004 Eastern Enlargement of the

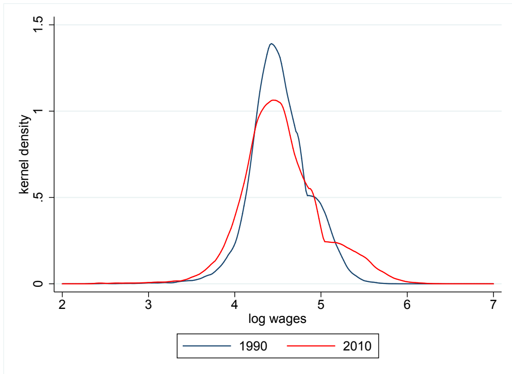
[9]For more information on all manufacturing sectors in our sample see Table 1.1 in section 1.3.

EU. Panel A depicts the German textile sector, a typical import sector. The wage distribution widens over time. At the end of our observational period, there are more workers at the lower and the upper end of the distribution, whereas less people are in the middle. Interestingly, the median wage does not change—the median employer earns approximately the same in the first and last interval.

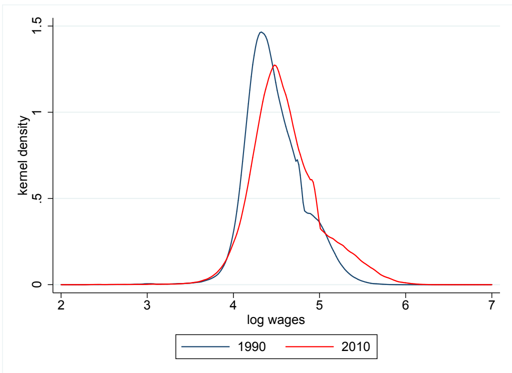
Figure 1.3: Distribution of log wages in selected industries, 1990–1995 and 2005–2010



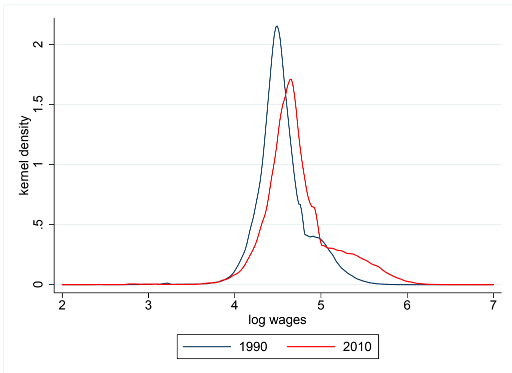
PANEL A: Textiles



PANEL B: Publishing



PANEL C: Machinery



PANEL D: Automobile

Notes: The graphs depict distributions of log wages within four major two-digit industries in Germany in Interval 2 (1990-1995) and Interval 5 (2005-2010). The sample includes full-time working men aged between 20 and 60 in the manufacturing sector in West Germany between 1985 and 2010.
Source: Own calculations, BeH.

Panel B shows that inequality also increases within the publishing, printing, and reproduction of the recorded media sector, which is among the sectors with the highest increase in both wage inequality and import exposure (see Table 1.1). Moreover, we find increasing wage inequality in export-intensive industries in Germany—the machinery industry, Panel C, and the automobile sector, Panel D. Compared with

Panel A and Panel B, the distributions of the export-intensive industries shift more to the right, indicating that most employees in these sectors experience a wage gain. The automobile industry has the most equal distribution of wages and is also closest to a pattern of first-order stochastic dominance among the four sectors presented here. In general, Figure 1.3 shows an increase in wage inequality with considerably less mass in the middle of the distribution in the later period for all industries.

1.2.3 Wage decomposition

The aim of this paper is to explore whether trade and technology influence wages in Germany through changes in either the firm- or worker-wage component. In a first step, we therefore decompose wages by using the two-way fixed effects estimator introduced by AKM, which aims to determine how much of the wage is worker- and how much of it is firm-specific. According to AKM, the individual log wage, y_{it}, can be fully described as an additive separable system of worker and firm fixed effects:

$$y_{it} = \alpha_i + \psi_{\mathbf{J}_{(it)}} + x'_{it}\beta + r_{it} \quad with \quad r_{it} = \eta_{i\mathbf{J}(it)} + \zeta_{it} + \varepsilon_{it}. \tag{1.1}$$

Here, the worker fixed effect ,α_i, can be interpreted as the worker-specific wage component. It comprises all characteristics of a worker that are equally valuable across firms, i.e., independent of the job a worker holds. The worker fixed effect captures time-invariant observable characteristics, like formal education, as well as unobservable traits, such as motivation and specific (e.g., interpersonal) skills. $\psi_{\mathbf{J}_{(it)}}$ is the establishment component. It comprises the wage that is proportionally paid by a firm to all of its employees independent of their characteristics. The firm effect also covers region- and industry-specific fixed effects, because firms do not change the region or industry in our sample.[10] x'_{it} is a vector of observable worker characteristics. Following Card *et al.* (2013), the vector includes year dummies as well as quadratic and cubic terms of age fully interacted with education dummies.[11] By construction, x'_{it} captures education-specific tenure. The impact of formal education is mainly included in the worker fixed effect because most workers in our sample have already reached their final degree. Typically, people within the age group of our sample

[10]Typically, firms would get a new identifier in the BeH if they change the industry or region. As this rarely happens, we do not believe that it alters our results.

[11]Our AKM estimations are based on the code of Card *et al.* (2013).

(20 to 60) have already completed their education when they start regular full-time work.

Last, r_{it} is the error term. As described in Card *et al.* (2013), it includes three independent random effects: $\eta_{i\mathbf{J}(it)}$ is the match component, i.e., an individual wage a worker i receives only at firm j. ζ_{it} is a unit root component of the error term. It captures a potential drift in employees' wages, e.g., any form of human capital accumulation or job mobility within the firm. ε_{it} is the transitory error term and includes, for example bonuses. We need to assume that all error components are orthogonal to the wage components and have a mean of zero conditional on the controls. This assumption requires exogenous mobility, meaning workers should not sort into firms depending on how good they match with the firm. If workers receive different wages depending on the match quality of their characteristics with the ones of the firm, the firm effect will be estimated with bias.

Some work has been done on the relationship between endogenous mobility and globalization. According to Helpman *et al.* (2010), more productive firms screen potential employees more intensively because workers with high abilities are complementary to these firms' high productivity. Thus, successful screening leads to worker-firm matches of higher quality. Krishna *et al.* (2014) conclude that the matching of employees in more productive exporting firms (in comparison to less productive non-exporters) is not random and, consequently, worker and firm effects would be estimated with bias. Ashournia *et al.* (2014) argue that import penetration might change workers' mobility following an unobservable match effect with the firm.

In contrast to the considerations above, Card *et al.* (2013) show that there are no sizable match effects in Germany by providing evidence that the match-specific wage premium is not considered by workers who switch employers. Moreover, they show that the residuals from the additive worker and firm effects estimation are remarkably homogeneous across deciles of worker and firm components, giving suggestive evidence against the concerns above. They also show that the estimation of interactive worker-firm fixed effect increases the model fit only marginally, limiting potential scope for bias. These tests imply that the exogenous mobility assumption holds for the German labor market.[12]

[12] See also Card *et al.* (2018) for a recent discussion of the identification of additive fixed effects and a link between the rent-sharing and AKM literature.

1.2.4 Descriptive results

In this subsection we replicate the results of Card *et al.* (2013), with some adjustments. For computational reasons, we use a 50% sample instead of the complete sample. Moreover, we change the intervals and use more, yet shorter periods (1985-1990, 1990-1995, 1995-2000, 2000-2005, and 2005-2010), which allow us to account for changes in trade more consistently over time. As expected, our results are very similar to those of Card *et al.* (2013) (see Table 1.A.1 in the Appendix).

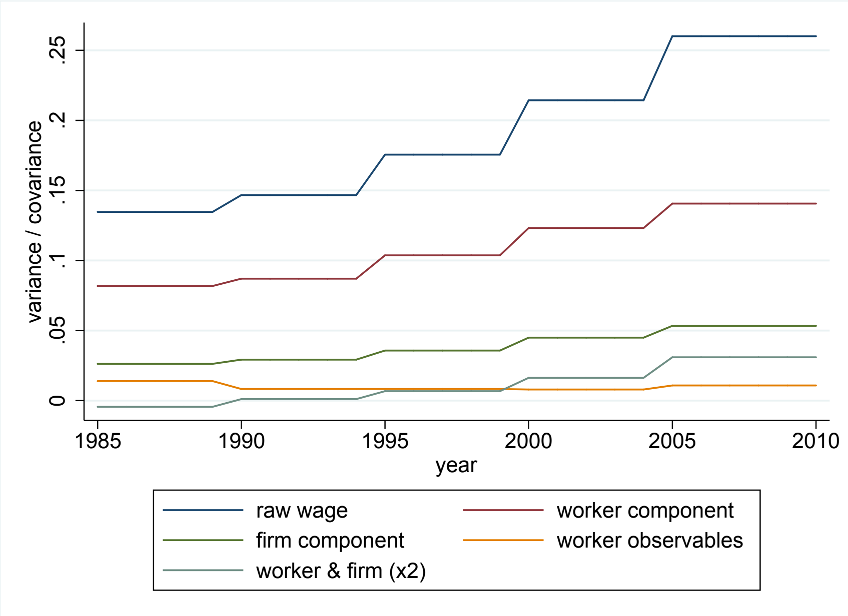
In Figure 1.4 we report the results of the AKM model and the variance decomposition. The decomposition of the variance of log raw wages, $Var(y_{it})$, described in Equation 1.2, allows us to assess how much of the increase in overall wage inequality can be explained by changes in the variation of the wage components separately. Because the worker and firm component are fixed effects, they cannot vary over time. To observe changes in these components, we estimate Equation 1.2 separately for five overlapping six-year intervals.

$$\begin{aligned} Var(y_{it}) = Var(\alpha_i) + Var(\psi_{\mathbf{J}_{(it)}}) + Var(x'_{it}\beta) + 2Cov(\alpha_i, \psi_{\mathbf{J}_{(it)}}) \\ +2Cov(\psi_{\mathbf{J}_{(it)}}, x'_{it}\beta) + 2Cov(\alpha_i, x'_{it}\beta) + Var(r_{it}). \end{aligned} \tag{1.2}$$

Again, we see that our results are very close to the findings by Card *et al.* (2013), despite our smaller sample and adjusted intervals. Figure 1.4 illustrates the increasing dispersion of the person and firm component of wages. The variance of the person effect rises from 0.082 to 0.141 over the observation period, representing 47% of the increase in overall wage inequality. The variance of firm effects increases from 0.026 to 0.053, explaining an additional 22%. The variance of time-varying individual characteristics is much lower and has a decreasing pattern. We also see that the correlation of person and firm effects rises from -0.004 to 0.031. This indicates that higher assortativeness in the assignment of workers to firms contributes another 28% to the rising dispersion of wages.[13]

[13]Postel-Vinay & Robin (2006) argue that as the firm effect is the residual of the person effect (or both are mutual residuals of one another), potential estimation bias in one of the two directly translates into an opposite bias in the other fixed effect. Hence, the correlation between the two is naturally downward biased. This is even more the case when we estimate the AKM model in relatively short intervals, where the average worker only switches the establishment once or twice. Hence, the individual fixed effect is estimated with very high standard errors but consistently, given our very large data set.

Figure 1.4: Variance decomposition of wage inequality by intervals, 1985–2010



Notes: The graph depicts the results of the decomposition of log wages using the AKM method by intervals. The variance of individual log wages (raw wage) can be described as the sum of the variance of the worker fixed effects (worker component), the variance of the firm fixed effects (firm component), the variance of observable worker characteristics, and their covariances. The sample includes full-time working men aged between 20 and 60 in the manufacturing sector in West Germany between 1985 and 2010.
Source: Own calculations, BeH.

1.3 Estimation strategy

To identify the determinants that impact wage inequality in Germany, we estimate separate empirical models for each dependent variable:

$$\Delta INEQM_{jt} = \beta_0 + \beta_1 \Delta TRADE_{jt} + \beta_2 \Delta RSH_{jt} + D_t + D_j + \varepsilon_{jt}. \qquad (1.3)$$

The dependent variables $\Delta INEQM$ are changes in the standard deviation of log wages, changes in the standard deviation of the firm and the worker component as well as changes in the covariance of both effects within three-digit industries. As the person and firm effects do not vary within the six-year intervals by construction, all changes are calculated in six-year differences. For example, $\Delta INEQM_{j,2005}$ describes the change in the standard deviation between Interval 5 (2005-2010) and Interval 4 (2000-2005) within industry j. Although yearly information on changes in raw wage inequality is available, we prefer to fit the raw wage data into the same intervals that we have for the wage components.

$\Delta TRADE$ is the change in an industry's exposure to trade. As both $\Delta TRADE$ and $\Delta INEQM$ are correlated with possible demand shocks, Equation 1.3 is subject to an endogeneity bias. To overcome this bias, we use two well-established strategies, which we describe in detail in section 1.3.1: First, we apply a gravity-based measure of trade, and second, we use an instrumental variable (IV) approach for trade. In practice, both measures take changes in import exposure and export opportunities into account and can therefore be seen as a measure of *net trade*.

To account for general trends in the German economy, we use time dummies, D_t, for each interval in all models. As we apply a first-difference methodology, our models already account for time-invariant industry differences. Thus, we abstain from further industry-level controls in our baseline specification, but add two-digit industry dummies, D_j, as a robustness exercise. We also control for technological progress in some models. We then include the routine share intensity, RSH, as a proxy for industries' labor substituting technologies, see section 1.3.2 for further details.

1.3.1 Trade exposure

When estimating the labor market effects of international trade, unobserved demand-side shocks may correspond with both firms' labor demand and firms' demand for imports of intermediate inputs. The correlation would typically lead the estimate by ordinary least squares (OLS) to understate the true effect of rising trade with the East on German labor market outcomes. To avoid this estimation bias, we have to isolate the effect of increased competitiveness and openness of the East from other distorting factors.

The literature commonly solves this endogeneity problem with instrumental variables (IV). ADH instrument trade between the US and China by China's rising trade relations worldwide, which are a consequence of its increasing competitiveness and the opening of its markets to world trade. As these events are exogenous to US demand-side shocks and simultaneously affect other trading partners of China, ADH can apply the increase of Chinese exports to other developed countries as an instrument for Chinese exports to the US.

A major problem with the IV approach is that a correlation between import growth and demand shocks cannot be completely ruled out if product demand shocks between the developed countries are correlated. ADH circumvent this problem by applying a gravity model to measure US imports from China as China's comparative advantage and market access to the US. In this paper, we use gravity residuals as our main measure of globalization because of their theoretical foundation and in order to rule out parallel demand shocks in the countries used for IV and the country under examination.

Gravity approach: One can assess the relative competitiveness of Germany vis-à-vis the East starting with the well-established standard gravity Equation 1.4 for trade values:[14]

$$X_{ijk} = \frac{y_{ij} y_{kj}}{Y_{Wj}} \left(\frac{\tau_{ik}}{P_{ij} P_{kj}}\right)^{1-\sigma}. \tag{1.4}$$

Here, trade between country i with a partner country k depends on the relative size of the two countries with respect to the world economy (y), the iceberg trade costs τ, and some prize indices P_i and P_k of the two countries. σ is the elasticity of substitution between commodities or industries j.

We empirically implement the gravity model by estimating the differences between the logs of German and Eastern trade with their respective trading partners in Equation 1.5. This difference can be interpreted as the relative competitiveness of the East compared to Germany. Country fixed effects control for multilateral trade barriers and distance and industry dummies control for path dependence and industry-specific idiosyncrasies. The difference in log trade is then regressed on these dummies. The residuals represent the rise in competitiveness of the East relative to Germany (after taking differences).

$$ln(X_{Ejk}) - ln(X_{Gjk}) = ln(z_{Ej}) - ln(z_{Gj}) - (\sigma_j - 1)[ln(\tau_{Ejk}) - ln(\tau_{Gjk})]. \tag{1.5}$$

A six-year differenced specification allows us to account for the interval structure of the dependent variables and implicitly allows for lagged effects. Formally, the trade shocks are constructed to affect the last period of an interval. They are defined as the sum of the one-year differences from the last period of the earlier interval to the

[14]See Appendix 1.A.2 for a detailed derivation of the gravity measure.

last period of the latter interval:

$$\Delta GRAVITY_{j,t}^{EAST} = \sum_{t=\tau}^{\tau+5}(GRAVITY_{j,t}^{EAST} - GRAVITY_{j,t-1}^{EAST}), \quad \forall\tau\epsilon\{1985, 1990, ..., 2005\}. \tag{1.6}$$

If trade follows the above-mentioned gravity structure, the gravity residuals account for endogeneity in the direct trade measures. In this case the IV approach is not necessary. By exploiting bilateral trade between many countries, the gravity approach uses more information and compares the rise in competitiveness and market access of Chinese and Eastern Europeans industries with German industries, accounting for multilateral resistance.

IV approach: We also use the conventional IV approach as a robustness check:

$$\Delta ImE_{j,t}^{D\leftarrow EAST} = \sum_{t=\tau}^{\tau+5} \frac{ImE_{j,t}^{D\leftarrow EAST} - ImE_{j,t-1}^{D\leftarrow EAST}}{ImE_{j,t-1}^{D\leftarrow WORLD}}, \qquad \forall\tau\epsilon\{1985, 1990, ..., 2005\}, \tag{1.7}$$

$$\Delta ExE_{j,t}^{D\rightarrow EAST} = \sum_{t=\tau}^{\tau+5} \frac{ExE_{j,t}^{D\rightarrow EAST} - ExE_{j,t-1}^{D\rightarrow EAST}}{ExE_{j,t-1}^{D\rightarrow WORLD}}, \qquad \forall\tau\epsilon\{1985, 1990, ..., 2005\}, \tag{1.8}$$

where $ImE_{j,t}^{D\leftarrow EAST}$ are industry j's imports from the East and $ImE_{j,t-1}^{D\leftarrow WORLD}$ are its imports from the rest of the world in year t. An industry's export exposure is derived analogously. The instruments are defined for the same set of countries as in DFS.[15] In our IV estimations we use the net imports (Equation 1.9) of German industries with respect to the East rather than the export and import measures separately (equations 1.7 and 1.8) because the results from using net imports are better comparable to our main results from the gravity model:

$$\Delta NetIm_{j,t}^{D\leftarrow EAST} = \Delta ImE_{j,t}^{D\leftarrow EAST} - \Delta ExE_{j,t}^{D\rightarrow EAST}. \tag{1.9}$$

In the first stage, we regress the instrument countries' net import measure on the German net import measure. The first stage shows that the IV is highly relevant.

[15] These are Australia, Canada, Japan, Norway, New Zealand, Sweden, Singapore, and the United Kingdom.

1.3.2 Technological change

In order to disentangle the effects of trade from those of technological change, we also control for the industries' exposure to computerization in some models. Ongoing computerization has an enormous impact on the economy and each sector has different conditions and possibilities to use new technologies as substitutes for labor. According to the task-based approach, the substitutability of labor by computers and thus labor demand is mainly determined by the degree of routineness (Autor *et al.*, 2003). Routine tasks are more easily codifiable and thus more likely to be taken over by a machine, robot, or computer. Autor *et al.* (2003) provide empirical evidence that indeed the routine-intensive tasks of a job are most easily replaced by automatization. As a result, jobs performing those tasks are more likely to become obsolete in the production process. In contrast, the demand for nonroutine tasks increases because they complement the work of computers.

Inspired by the work of Autor & Dorn (2013) and Autor *et al.* (2015), we look at the share of routine jobs in industries as a measure of their exposure to computerization. Given the possibility of technological substitution, we assume that there is elevated pressure on wages in industries with high shares of routine jobs. Moreover, we assume that technological progress contributes to rising wage inequality, as many routine occupations are found in the middle of the wage distribution (see, e.g., Dustmann *et al.*, 2009). In order to measure the routineness of an industry, we first calculate the routine task-intensity of each job l by applying the operationalization by Matthes (forthcoming). She uses the BIBB-IAB and BIBB/BAuA Employment Surveys (1979-2012) to determine how intensively various task categories (routine-manual [rm], routine-cognitive [rc], analytical [a], interactive [i], nonroutine-manual [nm]) are typically carried out in occupations. Based on this indicator, we calculate the routine task-intensity, RTI, of each job l following Autor & Dorn (2013):

$$RTI_l = ln(T^{rm}_{l,1979}) + ln(T^{rc}_{l,1979}) - ln(T^{a}_{l,1979}) - ln(T^{i}_{l,1979}) - ln(T^{nm}_{l,1979}). \qquad (1.10)$$

Similar to them, we classify an occupation as routine if it has an RTI above the 66-quantile of the employment-weighted RTI distribution in the initial year of 1979. In the next step, we determine the routine employment share, RSH, for each industry:

$$RSH_{jt} = \frac{\sum_{l=1}^{L} emp_{jlt} \cdot m(RTI_l > RTI^{P66})}{\sum_{l=1}^{L} emp_{jlt}}. \qquad (1.11)$$

emp_{jlt} is the number of employees in occupation l, industry j, and year t. $m(\cdot)$ is an indicator function which is either one if occupation l is routine-intensive as defined above, or zero if it is not. In this way, RSH reflects an industry's share of employees with routine-intensive jobs.

1.3.3 Trade, technological change, and wage inequality by industry

In Table 1.1 we list broader two-digit sectors and sort them by the change in log wage inequality (averages of three-digit industries). We also report changes in our main independent variables, the gravity residuals and routine-share measures, and total changes in employment (of full-time working men) over the observation period. The highest terciles of changes in the standard deviation of wages and in the gravity measure of trade are colored red and the lowest terciles are colored green. The highest and lowest decreases in technological change and in industry size are colored respectively.

Table 1.1 shows that over the entire sample period from Interval 2, starting in 1990, to Interval 5, starting in 2005, market access and competitiveness of the East rises in all sectors. We find by far the highest increase in the radio, TV, and other communication equipment industry, followed by the electrical and office machinery sector. These are typical fields of export for China. Looking at wage inequality, the manufacturing of radio, tv and communication equipment industry reports the highest increase. Also the electrical and office machinery sector is among those with the highest increase in wage dispersion. Regarding the routine-share intensity, we see that most of the sectors experience a decrease, with the automobile industry first and foremost. The textile and clothing and leather industry lists the highest losses in employment among manufacturing.

As expected, the colored terciles in the table indicate that the broad trends of wage inequality and trade move in the same direction. The same holds for the technology measure—with a little less obvious correlation. For a more in-depth analysis of the effect of trade and technological progress on wage inequality and especially on the inequality in the wage components, we apply regression analyses in the next section.

Table 1.1: Changes in trade, computerization, and wage inequality by industry, 1990–2010

		Δ Interval 5 and Interval 2 in %			
	Industry	Std. log wages	Gravity residuals	RSH	No. of workers
1	Wood	11.6	415.4	6.7	-26.1
2	Furniture and toys etc.	12.9	637.1	-2.2	-40.2
3	Paper	13.1	748.4	2.6	-20.6
4	Food and tobacco	14.9	250.2	5.7	-23.3
5	Textiles	16.3	623.7	-3.8	-61.5
6	Metals	17.4	663.1	-7.9	-31.1
7	Non-metallic minerals	17.5	857.6	-3.7	-38.6
8	Machinery	18.8	614.5	-10.6	-25.9
9	Chemicals	19.5	559.1	-0.9	-35.5
10	Medical and optical equip. etc.	20.6	1232.1	-18.0	-24.5
11	Rubber and plastic	21.3	915.6	-1.8	-14.1
12	Automobile	22.9	893.0	-26.3	-12.3
13	Electrical and office machinery	23.1	1752.6	-16.0	-27.5
14	Other transport equip.	23.3	654.5	-12.3	-18.2
15	Clothing and leather	24.2	736.3	-15	-58.9
16	Printing and publishing	24.7	1329.6	-4.1	-30.0
17	Radio, TV etc.	26.7	3148.8	-12.4	-14.3

Notes: The table lists changes in the dependent and independent variables, i.e., in wage inequality, in trade exposure, in technology and in number of employees, by broader two-digit sectors (averages of three-digit industries) between Interval 5 (1990-1995) and Interval 2 (2005-2010). Changes in wage inequality are measured as changes in the standard deviation (std.) of log wages. Increases in trade exposure are measured as changes in gravity residuals. Technological progress is measured as changes in the routine share intensity (RSH), i.e., the share of routine occupations. The decline in manufacturing employment is presented by the decline in the number of full-time male workers. Green (red) cells represents the lowest (highest) terciles of the increase in wage inequality and in trade exposure as well as in the decrease of the RSH and of the number of employees. Source: Own calculations, BeH and Comtrade.

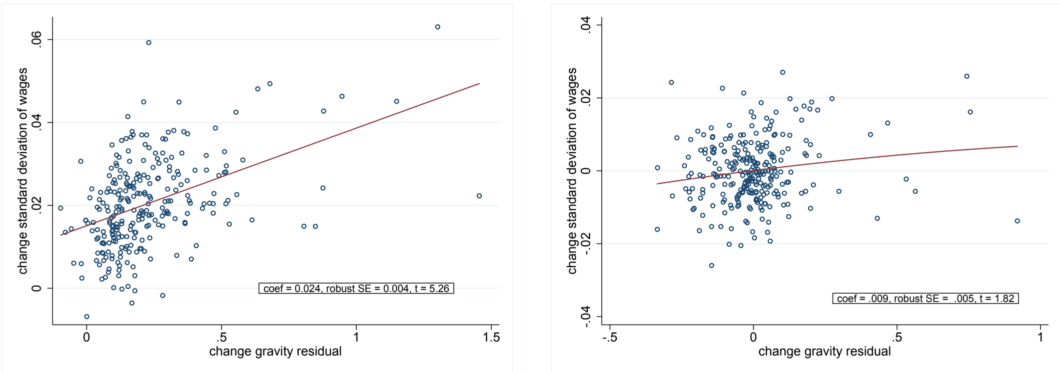
1.4 Results

In the following we present our empirical findings whether increased trade exposure contributes to rising wage inequality in Germany. We start with our main results that show which wage components are affected by trade with the East. Section 1.4.2 presents developments within skill groups and section 1.4.3 additionally controls for technological change. In section 1.4.5 we discuss how institutional developments may affect our results. In particular, we examine whether our findings differ between industries with a high or low decline in union coverage.

1.4.1 Effects of trade on wage components

Figure 1.5 illustrates the relationship between trade with the East and the dispersion of log wages within industries in Germany. The unconditional relationship depicted in Panel A shows that the rise of the East is positively correlated with increasing wage inequality. This relationship remains positive if we control for technological change and include time fixed effects, but the size of the coefficient is more than halved (Panel B).

Figure 1.5: Changes in import exposure and in wage inequality, 1995–2010



PANEL A: Unconditional PANEL B: Conditional on observables

Notes: The graphs plot first differences in the standard deviation of log wages within three-digit manufacturing industries against changes in West German industries' import exposures from the East. We consider changes between six-year intervals from 1995 to 2010. Panel A shows the unconditional correlation. Panel B shows the conditional correlation controlling for technological changes and including time fixed effects.
Sources: Own calculations, BeH and Comtrade.

Table 1.2 contains the regression results from estimating Equation 1.3. At first, we concentrate on the impact of trade and leave technological changes out. Table 1.2 compares the results for the different trade measures described in section 1.3.1. Column 1 and 2 include our main specification of trade exposure, i.e., changes in the industries' gravity residuals. Column 3 and 4 include IV estimations and Column 5 and 6 the OLS results with net trade.[16] Moreover, uneven columns include interval dummies to control for time trends that equally affect all manufacturing industries. Even columns additionally control for two-digit industries. The inclusion of the industry dummies reduces the effects of trade to some extent; however, the main effects remain significant.

[16]Note that Model 1 and 2 are also estimated by OLS because the gravity approach eliminates possible demand shocks (see section 1.3).

Table 1.2: Changes in import exposure and in inequality of wage components

	Gravity (1)	Gravity (2)	IV (3)	IV (4)	OLS (5)	OLS (6)
PANEL A | Dep. var.: Δ Std. of log wages						
Δ gravity	0.0175***	0.00936**				
	(0.001)	(0.024)				
Δ net imports			0.174**	0.160**	0.0617***	0.0383
			(0.045)	(0.035)	(0.005)	(0.135)
R2	0.266	0.483	0.138	0.433	0.212	0.503
PANEL B | Dep. var.: Δ Std. of worker fixed effects						
Δ gravity	0.0141***	0.00682**				
	(0.000)	(0.045)				
Δ net imports			0.144**	0.138*	0.0283	0.0151
			(0.026)	(0.050)	(0.247)	(0.596)
R2	0.0856	0.230	.	0.153	0.0306	0.236
PANEL C | Dep. var.: Δ Std. of firm fixed effects						
Δ gravity	0.000168	0.00290				
	(0.971)	(0.596)				
Δ net imports			0.0270	0.0255	0.0270	0.0243
			(0.788)	(0.828)	(0.397)	(0.486)
R2	0.166	0.226	0.163	0.214	0.163	0.214
PANEL D | Dep. var.: Δ Cov. of worker and firm fixed effects						
Δ gravity	0.00247*	0.00187				
	(0.067)	(0.153)				
Δ net imports			-0.00801	-0.00292	0.0105	0.0106
			(0.679)	(0.894)	(0.117)	(0.153)
R2	0.0520	0.215	0.0176	0.211	0.0436	0.223
N	263	263	262	262	262	262
Interval FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	No	Yes	No	Yes	No	Yes

Notes: Panel A shows the results of an industry's change in trade on changes in its distribution of log raw wages. The dependent variables in panels B, C, and D are changes in the standard deviation of the worker and firm wage component and in the covariance of both components, respectively. The independent variables are either trade measured as gravity residuals, instrumented net imports or net imports estimated with OLS. All models include interval dummies and a constant. In addition, columns 2, 4, and 6 include two-digit industry dummies. p-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations, BeH and Comtrade.

Raw wage inequality: In Panel A of Table 1.2 we regress changes in trade on changes in raw wage inequality. Independent of how we measure trade exposure, we find that an increase in trade affects the rise of wage inequality positively. The results in Column 1 of our preferred gravity measure reveal that for an average change in the gravity residual of 0.22, the effect of trade accounts for approximately 19% of the increase in the variation in raw wage inequality $(100 * [0.2239 * 0.0175]/0.0205 = 19.11\%)$. The effect remains significant even if we control for broader industry fixed effects. The IV estimations (Columns 3 and 4) confirm these positive and significant effects. Column 3 shows that an average increase in net import exposure of 0.0079 explains about 7% of the rise in overall wage inequality $(100*[0.0079*0.174]/0.0204 = 6.74\%)$.

However, the size of the IV and gravity coefficients are not comparable as they are based on different measures of import exposure. Thus, the increase in trade measured by the structural gravity parameter explains a larger share of the increase in wage inequality than the instrumented net import measure. While the IV measure also includes higher imports due to cheaper or better intermediate inputs, the gravity variable measures competitiveness and market access. It is therefore better suited capturing the effects on the labor market.

Overall, the effect size is plausible compared to previous studies which also come to the conclusion that increased international trade explains less than 20% of the rise in wage inequality (e.g., Van Reenen, 2011). Lastly, if we compare the OLS with the IV estimates, we see a factor three to four increase in the effect size, pointing to a sizable import endogeneity problem in the OLS specification.

Worker fixed effect: The main contribution of this paper is its focus on the effect of international trade on changes in the distribution of the wage components. We present the results for the individual-wage component in Panel B of Table 1.2. Column 1 shows that that increasing the change of the gravity residual by one changes the rise in the standard deviation of the worker-wage component by 0.014. Considering an average change in the gravity residual of 0.22, trade with the East explains about 18% of the increase of the standard deviation of the worker fixed effect $(100 * [0.2239 * 0.0141]/0.0179 = 17.64\%)$. The effect remains significant even if we control for broader industry effects (Model 2) or if we use IV (Columns 3 and 4).

Two developments can explain the positive effect of rising trade exposure on the inequality in worker-specific wages. At the intensive margin, the positive effect can be driven by an increase of the skill premium. At the extensive margin, it can be driven by a decrease in the relative demand for medium-skilled workers. Both developments are supposed to be highly affected by the rise in China's and Eastern Europe's competitiveness and market access. However, this interpretation requires a more in-depth view on changes in the skill-composition of German industries. We find that the number of low- and medium-skilled workers decreases in all industries, whereas the number of high-skilled workers increases (see Table 1.A.2 in the Appendix). Thus, considering the extensive margin, low- and medium-skilled workers lose their jobs. Newly hired workers apparently do not replace those workers, but rather fit into the new labor market structure that is more polarized regarding the returns to skill.

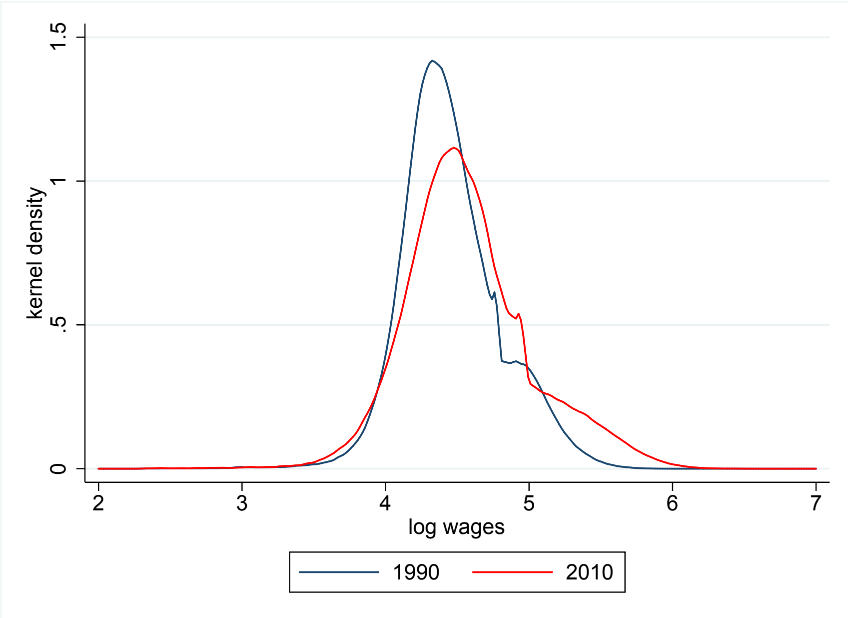
This hypothesis is also supported by Figure 1.6, which visualizes the polarization of wages in the manufacturing sector. The wage distribution in 2010 is wider than in 1990, with more mass at both ends and considerably less mass in the middle. The figure indicates that the size reduction of manufacturing industries is relatively strong in the middle of the wage distribution. Thus, workers with close to average wages leave the manufacturing sector and are not replaced accordingly. The increase at the lower end of the wage distribution does not mean that more low-skilled workers are employed in the manufacturing sector—we see the exact opposite in Table 1.A.2 in the Appendix. The table also shows that almost all manufacturing sectors employ more high-skilled workers, which contributes to an increase at the upper end of the wage distribution.

Overall, Germany has experienced a strong increase in formal education but relatively small changes in (real) average wages. Thus, a worker today has a lower position in the wage distribution compared to workers in the past with similar formal education. Moreover, the increase in jobs for high-skilled workers in almost all manufacturing sectors is in line with our findings of wage polarization because high-educated workers have substantially higher individual fixed effects (see Panel B of Figure 1.A.1 in the Appendix). In general, our results are consistent with the findings by Dauth *et al.* (2016), who show that workers are pushed out of industries that are highly exposed to imports from the East.

Firm fixed effect and covariance: Panel C of Table 1.2 shows that increased trade with the East does not significantly affect the firm-specific wage component.[17]

[17] The data includes firm sizes between one and about 50,000 workers. In an unweighted measure of deviation of the firm-wage component within industries both types of firms would count the same and the effect on inequality would be diluted. However, entry and exit of firms is determined by trade, leading to a reallocation of workers that would not be visible in the unweighted measurement. This reallocation is again dependent on the firm effect. Hence, we compute the distribution of the firm-specific wage component by weighting it by the number of full-time male workers in the firm.

Figure 1.6: Distribution of log wages, 1990–2010



Notes: The graph depicts the distribution of log wages in Interval 2 (1990-1995) and Interval 5 (2005-2010) of full-time working men aged between 20 and 60 in the sample of West German manufacturing industries that we use in our analysis.
Sources: Own calculations, BeH.

This finding contradicts recent contributions in trade theory and empirics, e.g., models of rent-sharing in the trade context (e.g., Egger & Kreickemeier, 2012; Amiti & Davis, 2012). Our findings do not contradict the existence of an exporter wage premium, as most manufacturing workers are employed by exporters.[18]

Finally, Panel D of Table 1.2 depicts the results for the covariance of the person and firm effects. The effect of our gravity measure is significant and economically large, indicating that increased import pressure from the East leads to more assortative matching in the manufacturing sector in Germany. Thus, trade-induced sorting contributes to the increase in wage inequality. These results are in line with other findings in the literature (e.g., Davidson *et al.*, 2008, 2014). For instance, Bloom *et al.* (2016) show that German firms under competitive pressure increasingly invest in high-tech products, thereby escaping low-wage competition. As a consequence, these firms become more productive and require better workers, which results in

[18]As we do not observe export status or export size of individual firms, we cannot rule out that trade affects rent-sharing and efficiency wages at the firm level (see, e.g., Amiti & Davis, 2012; Frias *et al.*, 2018).

more sorting. There is no effect for the IV measure of trade on the sorting component. The IV measure is based on trade between Germany and China, while the gravity measure takes all trade flows of both countries to third markets into account. We argue that Chinese competition leads to higher sorting particularly in the strong German export sectors. This could explain the different findings in the two measures.

To sum up, the more industries are exposed to competition from the East, the more inequality within industries increases. Firm-specific wage premiums do not drive wage inequality, it is rather trade that drives overall wage inequality through its impact on the inequality of the worker-specific wage component and through increased assortative matching.

Overall, our results are in line with those of other studies looking at the effects of trade on the German labor market. For example, Schank *et al.* (2007) show that most of the firm-wage premium is driven by observable and unobservable worker characteristics. DFS find a negative impact of trade integration with the East in form of job losses in regions that are marked by import-competing sectors. However, given their focus on regional labor markets, they do not find evidence of an effect of rising import exposure on wages within the region. In their recent working paper, Dauth *et al.* (2016) show that import competition leads to lower earnings within job spells and leads employees to leave exposed industries. Also, Dustmann *et al.* (2014) find an increase in wage inequality in tradable manufacturing sectors, where the wages of the lower percentile decrease, whereas the median and the 85th-percentile rise.[19]

Higher assortative matching is in line with the survival of relatively more complex production lines under low-wage competition. Bloom *et al.* (2016) describe in detail the transition of, e.g., the textile industry to advanced fibers and fabrics in the wake of Chinese competition. This then leads to a transition of some firms toward higher quality products or genuine innovations, e.g., reflected in patent counts. The comparative advantage of advanced economies like Germany, could therefore be seen in high quality products and innovation, which is then also reflected in the wage structure.

[19] Dustmann *et al.* (2014) define the tradable manufacturing sector according to high export volumes. Moreover, they find a strongest increase in wage inequality in the tradable service sector, which we do not consider in this paper.

1.4.2 Effects by skill group

In this section, we want to understand the mechanisms behind wage polarization better. Therefore, we look at the development of inequality in the wage components within skill groups. We build separate groups for workers without formal training (low-skilled), with vocational training (medium-skilled), or with a college or university degree (high-skilled). For this exercise, we assume that these skill groups are somewhat rigid and, e.g., workers without any training usually do not replace workers with vocational training. Vocational training is traditionally very important in Germany. Although Germany has experienced a strong increase in university enrollment in the last decades, the workforce composition is naturally changing more slowly, so that workers with a vocational degree form the largest group.

Table 1.A.4 in the Appendix shows that import pressure affects within skill-group inequality, but only for the group of medium-skilled workers. That holds for the dispersion of raw wages as well as for the worker-wage component. Again, we do not find any significant effect on the firm-pay premium, which confirms our main results of Table 1.2. There is no effect on the wage dispersion within the group of high- and low-skilled employees.[20] The trade effect on assortative matching is also significant for medium-skilled workers.

The fact that the effects of trade are only significant within the group of medium-skilled workers speaks in favor of the story that some jobs in the middle of the wage distribution are cut and not replaced accordingly. Table 1.A.2 in the Appendix shows that the employment of vocationally trained individuals decreases heavily in the manufacturing sector, supporting an offshoring story of those jobs. The remaining workers are either specialists whose work cannot be offshored and who are better paid, or workers that have to accept a rather low wage or a lower wage increase because of the import pressure. This argument is in line with Dauth *et al.* (2016) who show that people working in industries with a high import exposure are more likely to lose their job. Moreover, they find that if workers stay within the same firm or industry, they experience a negative effect on cumulative earnings.[21]

[20]Note that a large fraction of high-skilled workers are subject to top coding. Although we impute these wages, the effect on the college premium as a driver of inequality could still be larger than estimated.

[21]Moreover, Dauth *et al.* (2016) show that high increases in import exposure lead employees to leave the industry, especially toward the service sector where they earn less. This mobility pattern, however, is out of the scope of this paper, where we look at within-industry effects.

Table 1.A.2 in the Appendix also shows substantial workforce changes in the group without vocational training and in the group of those with a university degree. However, no effect of trade is found within any of these groups. These results might also indicate that competition from the East does not change the wage policy of firms to a large extent. Import penetration rather decreases the demand for certain occupations and also affects the between-skill-group wage redistribution, i.e., the skill premium.[22]

1.4.3 The role of technological change

In this section, we replicate the results of Table 1.2 but extend the regression by adding a measure for technological change (ΔRSH). The results are summarized in Table 1.3, which shows that the main results of Table 1.2 remain unchanged. If we control for technological change, the sign of the import competition coefficient is still in line with our expectations, while the size of the trade coefficient decreases up to 50% (compare Columns 1 with 3 and Columns 2 with 4).

Panel A of Table 1.3 indicates that an increase in the share of routine-intensive jobs within an industry reduces raw wage inequality, which conversely means that technological change increases wage inequality. The interpretation is straightforward: If an industry experienced a large decline in routine-intensive occupations in the preceding interval, the industry is assumed to be "trending" in automation and this pushes the increase in wage inequality. In our sample, the average decrease in an industry's share of routine occupations is -0.0084, explaining about 15% of the increase in wage inequality ($100 * [-0.0084 * (-0.362)]/0.0204 = 14.91\%$).[23] Panel B of Table 1.3 shows that a higher decrease in an industry's RSH leads to a significantly higher increase in the standard deviation of the worker-wage component, explaining about 11% of the rise in inequality of the worker fixed effect ($100 * [-0.0084 * (-0.236)]/0.0179] = 11.01\%$). Moreover, we find a significant negative effect of technological change on inequality in the firm-pay component and no effect on assortative matching.

[22] Note that the AKM model does not control for occupations as heterogeneity between occupations is included in the individual fixed effect (as long as the individual does not change the occupation).

[23] Using the IV approach for the trade variable in Model 4 of Table 1.3, we find a comparable effect size for computerization.

Because we measure technological progress as an industry's decrease in the share of routine occupations, it might be correlated with the trade variables to some degree. The reason is that routine jobs can typically not only be readily replaced by machines, but are also easily offshorable to labor-abundant countries (Blinder, 2009). As the trade coefficients stay significant when we additionally control for RSH, the correlation of the two measures keeps within limits.[24] Controlling for both trade and technology, we find that trade explains approximately 10% of the recent rise in inequality, while technology caused 15%. This is driven through the channel of the individual-wage component, with trade causing an approximate increase of 11% in inequality and technology of 18% of the recent increase. Firm-pay premiums are only affected by technology, while the increased sorting of workers and firms is only affected by trade.

[24] If we estimate Equation 1.3 only with RSH but without any variable for trade, technological change explains about 17% of the increase in raw wage inequality and 13% of the increase in worker-specific wage inequality.

Table 1.3: Changes in import exposure, in technology, and in inequality of wage components

	Gravity (1)	IV (2)	Gravity (3)	IV (4)	OLS (5)
PANEL A | Dep. var.: Δ Std. of log raw wages					
Δ gravity	0.0175***		0.00897*		
	(0.001)		(0.069)		
Δ net imports		0.174**		0.0784	0.0459**
		(0.045)		(0.369)	(0.028)
Δ RSH			-0.362***	-0.388***	-0.185***
			(0.000)	(0.000)	(0.000)
R2	0.266	0.138	0.233	0.175	0.323
PANEL B | Dep. var.: Δ Std. of worker fixed effects					
Δ gravity	0.0141***		0.00858**		
	(0.000)		(0.036)		
Δ net imports		0.144**		0.0795	0.0177
		(0.026)		(0.244)	(0.481)
Δ RSH			-0.236**	-0.260**	-0.125***
			(0.011)	(0.010)	(0.001)
R2	0.0856	.	0.0621	.	0.0901
PANEL C | Dep. var.: Δ Std. of firm fixed effects					
Δ gravity	0.000168		-0.00531		
	(0.971)		(0.375)		
Δ net imports		0.0270		-0.0223	0.0218
		(0.788)		(0.843)	(0.501)
Δ RSH			-0.233**	-0.200	-0.0620
			(0.044)	(0.115)	(0.106)
R2	0.166	0.163	0.124	0.124	0.171
PANEL D | Dep. var.: Δ Cov. of worker and firm fixed effects					
Δ gravity	0.00247*		0.00205		
	(0.067)		(0.174)		
Δ net imports		-0.00801		-0.0167	0.00991
		(0.679)		(0.485)	(0.159)
Δ RSH			-0.0176	-0.0351	-0.00700
			(0.516)	(0.297)	(0.585)
R2	0.0520	0.0176	0.0452	.	0.0457
N	263	262	263	262	262

Notes: Panel A shows the results of a change in trade and technology (measured as the change in an industry's routine-share intensity) on changes in the distribution of log raw wages, while panel B shows the individual fixed effect, panel C the firm fixed effects and panel D the covariance of the two as dependent variable. Trade is either measured as gravity residuals, instrumented net imports or net imports estimated with OLS. All models include interval dummies and a constant. p-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations, BeH and Comtrade.

1.4.4 Effects by industry

By considering broader industry categories in our analysis, we receive a deeper insight into which German industries are mainly affected by increased import exposure from the East. Moreover, we can assess the plausibility of the previous results. We expect that industries producing low-tech consumer goods are very prone to low-wage competition from China and Eastern Europe, as the tasks required in their production processes are easily offshorable. To analyze the effects by broader sectors, we interact our main trade variable, the gravity residual, with dummies for industries that produce three different product classes. The industries are classified as follows: Consumer industries are industries which, according to the input-output table of the Federal Statistical Office of Germany, sell most of their products to end consumers. Production goods industries sell their products predominantly to other industries, as intermediate inputs, and high-tech industries are characterized by R&D-intensive production and have no clear profile of producing intermediate or final goods.

Table 1.4 shows that the trade effects for the baseline category of industries producing high-tech goods are comparable with our main results in Table 1.2. First, we look at overall wage inequality in Model 1. The interaction effects show that the coefficients do not differ between high-tech goods and the other two classes of goods. Furthermore, we find that the effect of trade is only significant for high-tech- and consumer-good-producing industries. It is insignificant for the production goods sector. Although wage inequality rises slower for consumer goods industries, the effects for consumer products are the largest, though not significantly different from the effect on high-tech goods—which is in line with our expectations. We receive similar and plausible results for the effects on the worker-wage component and little to no effect on the change in the standard deviation of the firm-wage component—which confirms our main findings. Column 4 of Table 1.4 shows that the effect of trade on assortative matching in the high-tech goods-producing industry is economically large and almost twice as big as our baseline results in Table 1.2. Moreover, the effects for consumer and production goods are not significantly different from zero.

Our findings support previous evidence along the lines of Davidson *et al.* (2008, 2012, 2014), Baziki *et al.* (2016), and Bombardini *et al.* (2018). Overall, the findings by product classes reassure our baseline results that trade mainly contributes to the rise in wage inequality through the worker or skill channel and through increased

Table 1.4: Changes in import exposure and in inequality of wage components by product classes

	Δ Std. log wages (1)	Δ Std. worker FE (2)	Δ Std. firm FE (3)	Δ Cov. worker/firm FE (4)
Reference category: High-tech goods				
Δ gravity	0.0150***	0.0113**	0.0102	0.00470**
	(0.001)	(0.039)	(0.204)	(0.022)
Δ gravity × cons. goods	0.00997	0.00897	-0.0166*	-0.00318
	(0.171)	(0.201)	(0.066)	(0.141)
Δ gravity × prod. goods	-0.00775	-0.00137	-0.0105	-0.00484**
	(0.189)	(0.819)	(0.243)	(0.023)
Consumer goods	-0.00627***	-0.00229	0.00490	-0.000674
	(0.002)	(0.341)	(0.125)	(0.361)
Production goods	-0.00487***	-0.00291	0.00413	-0.0000785
	(0.006)	(0.145)	(0.146)	(0.902)
R2	0.348	0.113	0.178	0.114
N	263	263	263	263

Notes: The table shows the baseline gravity measure for trade interacted with three different industry groups: Consumption goods, production goods and high-tech goods (reference category). The dependent variables are changes in the distribution of log wage inequality, of individual and firm fixed effects and changes in the covariance of both effects, respectively. All models include interval dummies and a constant. p-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Source: Own calculations, BeH and Comtrade.

sorting. In particular, the sorting channel is strengthened by the previous analysis.

1.4.5 Effects by union coverage

Besides trade and technological change, labor market institutions can influence wage inequality in countries. Unions are an important institution because they bargain with employers' federations for wages and non-monetary benefits. Dustmann *et al.* (2014) show that the share of employees covered by a union agreement has strongly declined in Germany. In consequence, the wage-setting process is more decentralized and shifted away from the industry toward the firm level. This has led to more heterogeneity within industries. Dustmann *et al.* (2009) find that the decline in unionization rates can explain 28% of the increase in lower-tail income inequality. They argue that the share of workers covered by union agreements is the decisive measure to estimate the impact of unions in Germany. The reason is that collective bargaining results apply to all workers in a firm that recognizes a union and does not require the individual worker to be a union member in Germany.

If we assume that the decline of unions is exogenous, we would observe decreasing unemployment and larger wage inequality (if we abstract from the general decrease of manufacturing jobs). This is because low-paid workers benefit disproportionately from union bargaining, leading to a narrower range especially at the lower end of the wage distribution. In addition to direct effects of union coverage on wage inequality, unions can be seen as a factor determining the international competitiveness of an industry or firm. An industry's ability to adjust to trade shocks via wages can be restricted through bargaining agreements. Unions can also lower their wage demand if they primarily want to prevent employment losses because of trade (see, e.g., Egger & Etzel, 2012; Felbermayr *et al.*, 2014). Abstracting from the exogeneity assumption of unions, it is possible that the decline in unionization is a reaction to competitive pressure in the first place, so that firms can more easily adjust to trade.

In this section, we present some evidence on the correlation between changes in international trade, deunionization, and the inequality in wage components. The co-movement of these factors hints at a reinforcing character of trade and deunionization. To derive the union coverage rate for two-digit industries, we use information of the IAB Establishment Panel and construct a union coverage share for industry-level bargaining.[25,26] We then check whether the results of our main specification change if we differentiate between industries with a high or low decrease in the union coverage rate. In a way, this procedure gives us the possibility to consider the influence of labor market institutions, too.

Column 1 of Table 1.5 shows that the effect of trade on raw wage inequality is strong in industries with a high decrease in the union coverage rate. The interaction effect shows that the impact in industries with a lower decrease in unionization is significantly smaller and roughly halved, at least for raw wage inequality. The effect of import exposure on the inequality of the worker-wage component is significantly positive for the group of industries with a high decline in union coverage (Column 3). The effect does not substantially differ for industries with a low decline. The

[25]Firms can also implement firm-wide contracts. We do not include such house agreements in our measure of deunionization, because the effect would be part of the establishment-specific pay premium. It would certainly coincide with the firm effect.

[26]As the IAB Establishment Panel is not representative at the two-digit industry level, we have to accept that our measure of union coverage is based on less than 20 establishments for some industries.

same holds for the impact on changes in assortative matching (Column 5). Again, the establishment-pay premium remains unaffected within both groups (Column 4).

Table 1.5: Changes in import exposure, in inequality of wage components, and deunionization

	Δ Std. wage (1)	Δ Std. wage (2)	Δ Std. worker FE (3)	Δ Std. firm FE (4)	Δ Cov. worker/firm FE (5)
Δ gravity	0.0175***	0.0259***	0.0196***	0.000467	0.00242*
low union dec.		0.00132	0.000730	-0.000796	0.000155
(Δ gravity × low union dec.)		-0.0135*	-0.00868	-0.000498	0.0000804
R2	0.266	0.287	0.0963	0.167	0.0528
N	263	263	263	263	263

Notes: In columns 1 and 2, the dependent variable is the change in the standard deviation of log raw wages. In columns 3 to 5, the dependent variables are the change in the standard deviation of the worker fixed effect, the firm fixed effect and the change in the covariance of both effects, respectively. The baseline gravity results are included in Column 1. In column 2 to 5, we interact the changes in gravity measure of trade with a dummy that is one if the decrease in the union coverage rate in a two-digit industry is below the median (all decrease). All models include dummies for intervals and a constant. p-values in parentheses. * $p < 0.10$, **$p < 0.05$, *** $p < 0.01$.
Source: Own calculations, BeH, Comtrade, and IAB Establishment Panel.

1.5 Conclusion

This paper provides evidence on how international trade and technological progress influence the wage distribution within industries. We emphasize the impact of import competition with low-wage countries on changes in the wage components, i.e., worker- and firm-specific pay premiums, and assortative matching. In this way, our paper contributes to a better understanding of how labor markets adjust to globalization processes.

Our main finding is that increased market access and the competitiveness of China and Eastern Europe has lead to a rise in wage inequality in Germany. This rise is attributable to an increase in the inequality of the worker-wage component and partly to increased assortative matching of high-wage workers to high-wage firms. We find no evidence that trade with the East contributes to the rising inequality in the firm-pay premium. The paper also shows that trade exposure leads to increased wage inequality within education groups. The effect of trade on the inequality in the worker-wage component and on assortative matching is significant within the group of vocationally trained workers. Trade does not affect the inequality of any wage component within the group of low- and high-skilled workers.

At the same time, we observe a decline of the low- and medium-skilled workforce on the one hand and an increase of the high-skilled workforce in almost all manufacturing industries on the other. We conclude that trade leads to both a rising skill premium of qualified workers and changes in the composition of the workforce, resulting in wage polarization. Moreover, the positive effects of trade on assortative matching, especially in high-tech industries, are in line with the idea that more complex production processes, such as the O-ring production technology (Kremer, 1993), are more likely to survive low-wage competition. Finally, our analyses complement other work on technology upgrading (e.g., Bloom *et al.*, 2016), and export status at the firm level (e.g., Frias *et al.*, 2018). The fact that wage inequality has increased in Germany due to the rise in international trade with China and Eastern Europe is an important finding for economic policymakers. Our analyses show that this rise is not attributable to altered firms' pay policies in response to more globalized markers. Rather, the distribution and sorting patterns of workers, and the value of their skills changes under globalizing forces.

It should be noted that the effects we have found must be interpreted as a lower bound, because the AKM method may underestimate sorting effects (Postel-Vinay & Robin, 2006). In general, the German data appear to meet the relatively strong exogenous mobility assumption of the AKM approach (see Card *et al.*, 2013). They are therefore suitable for our analysis.

In addition to the effects of increased trade with the East, we find that the effect of declining routine-intensive jobs as a measure of technological change is equally important. Altogether, we are able to explain about a quarter of the recent increase in wage inequality within German manufacturing sectors through increased international trade with the East and technological progress.

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1.A Appendix

1.A.1 Additional results from wage decomposition

Table 1.A.1 reports the results of the AKM model. The high R^2, increasing from 87% to 92%, and low residual wage components indicate a high explanatory power of the AKM model. Our results are very close to the findings by Card *et al.* (2013), although we use a smaller sample and different time intervals.

Table 1.A.1: Summary statistics of the AKM effects

Interval 1: 1985-1990			
Observations	33,632,369	Corr. pers.& firm effect	-0.048
Std. log (daily wage)	0.367	Corr. pers. effect & Xb	0.066
Std. person effects	0.286	Corr. firm effect & Xb	0.068
Std. firm effects	0.162	RMSE of AKM residual	0.139
Std. Xb	0.118	Adjusted R-squared	0.873
Interval 2: 1990-1995			
Observations	35,845,173	Corr. pers. & firm effect	0.011
Std. log (daily wage)	0.383	Corr. pers. effect & Xb	0.140
Std. person effects	0.295	Corr. firm effect & Xb	0.087
Std. firm effects	0.171	RMSE of AKM residual	0.141
Std. Xb	0.091	Adjusted R-squared	0.878
Interval 3: 1995-2000			
Observations	33,813,314	Corr. pers. & firm effect	0.055
Std. log (daily wage)	0.419	Corr. pers. effect & Xb	0.109
Std. person effects	0.322	Corr. firm effect & Xb	0.097
Std. firm effects	0.189	RMSE of AKM residual	0.147
Std. Xb	0.091	Adjusted R-squared	0.892
Interval 4: 2000-2005			
Observations	32,605,834	Corr. pers. & firm effect	0.109
Std. log (daily wage)	0.463	Corr. pers. effect & Xb	0.094
Std. person effects	0.351	Corr. firm effect & Xb	0.122
Std. firm effects	0.212	RMSE of AKM residual	0.152
Std. Xb	0.089	Adjusted R-squared	0.909
Interval 5: 2005-2010			
Observations	31,291,419	Corr. pers. & firm effect	0.178
Std. log (daily wage)	0.510	Corr. pers. effect & Xb	0.073
Std. person effects	0.375	Corr. firm effect & Xb	0.132
Std. firm effects	0.231	RMSE of AKM residual	0.157
Std. Xb	0.104	Adjusted R-squared	0.921

Notes: The table follows Table III in Card *et al.* (2013) for slightly different intervals and for a 50% sample of the BeH including full-time working men aged between 20 and 60 in the manufacturing sector in West Germany between 1985 and 2010. Xb includes interaction terms of year dummies with education dummies as well as the interaction of quadratic and cubic terms in age with education dummies.
Source: Own calculations, BeH.

1.A.2 Gravity measure of trade exposure

To derive our gravity measure of trade,[27] we start from the basic gravity equation that describes the trade values of industry j between country i and k. It depends

[27] For a detailed description see Autor *et al.* (2013).

on the size of the respective industries in both countries, relative to size of the world industry. It negatively depends on the iceberg transport cost τ and positively on the respective price indexes in nominal values.

$$X_{ijk} = \frac{y_{ij}y_{kj}}{Y_{Wj}}(\frac{\tau_{ik}}{P_{ij}P_{kj}})^{1-\sigma_j}. \tag{1.12}$$

We look at trade between country G (Germany) and E (the East). We take the natural logs of Equation 1.12. World and destination industries' sizes vanish by taking differences. We receive relative exports:

$$ln(X_{Ejk}) - ln(X_{Gjk}) = ln\left(\frac{y_{Ej}}{P_{Ej}^{1-\sigma_j}}\right) - ln\left(\frac{y_{Gj}}{P_{Gj}^{1-\sigma_j}}\right) - (\sigma_j - 1)[ln(\tau_{Ejk}) - ln(\tau_{Gjk})]. \tag{1.13}$$

We reduce Equation 1.13:

$$ln(X_{Ejk}) - ln(X_{Gjk}) = ln(z_{Ej}) - ln(z_{Gj}) - (\sigma_j - 1)[ln(\tau_{Ejk}) - ln(\tau_{Gjk})]. \tag{1.14}$$

This gives us the relative trade with a third country k for Germany and the East explained by relative real industry sized z or export capabilities and as a function of the relative access cost to these markets for both countries. To extract the relative competitiveness, we then estimate the following equation for years t:

$$ln(X_{Ejkt}) - ln(X_{Gjkt}) = \alpha_j + \alpha_k + \epsilon_{jkt}. \tag{1.15}$$

We estimate the log difference in exports to a third country by industry and third country fixed effects. Substituting Equation 1.14 for the term on the left-hand side yields:

$$ln(z_{Ej}) - ln(z_{Gj}) - (\sigma_j - 1)[ln(\tau_{Ejk}) - ln(\tau_{Gjk})] = \alpha_j + \alpha_k + \epsilon_{jkt}. \tag{1.16}$$

Solving for the error term, the gravity residuals are as follows:

$$\epsilon_{jkt} = ln(z_{Ej}) - ln(z_{Gj}) - (\sigma_j - 1)[ln(\tau_{Ejk}) - ln(\tau_{Gjk})] - \alpha_j - \alpha_k \tag{1.17}$$

and reshaping:

$$\epsilon_{jkt} = \left[ln\left(\frac{z_{Ej}}{z_{Gj}}\right) - \alpha_j \right] - \left[(\sigma_j - 1) \cdot ln\left(\frac{\tau_{Ejk}}{\tau_{Gjk}}\right) - \alpha_k \right]. \tag{1.18}$$

We end up with two terms. First, the relative export capabilities *demeaned* by the average of all industries, and second, the relative cost of exporting demeaned by the average cost difference for that country. Note that the second term is negative if the East has worse market access than Germany and then enters positively to the first term. Finally, we take six-year differences of these residuals to capture the change in relative market access and export capabilities for our interval periods.

1.A.3 Changes in industries' workforce composition

Table 1.A.2 shows the workforce changes by skill group over time. The number of low-skilled workers without a vocational degree, and the number of medium-skilled workers with a vocational degree, has decreased in all manufacturing sectors. Whereas, the number of high-skilled workers with a degree from university (of applied sciences) has increased in all but the textile industry. These results indicate a rise in the education level of the German manufacturing workforce.

In addition to Table 1.A.2, Table 1.A.3 shows the within-industry changes in the distribution of the worker-wage component. The dispersion of the individual-wage component increases for all workers within their education group in all industries. Thus, the between-education group effects of wages cannot explain all of the dispersion in overall wages and in the worker fixed effect.

Figure 1.A.1 shows changes of the employment shares of different skill groups. We find a general increase in college-educated workers and moderate to strong declines in non-college-educated workers. This pattern alone cannot explain the polarization of wages found in Figure 1.6, although the increase in wages at the right of the distribution is partly attributable to the rise in high-skilled workers. These findings emphasize the necessity to look at wage inequality within skill groups. Note that around 80% of workers are in the medium-skilled category.

Table 1.A.2: Workforce changes by industry and skill group in %, 1990–2005

Industry	No voc. training	Voc. training	College / Univ.
Textiles	-73.5	-56.3	-16.5
Clothing and leather	-73.4	-57.6	50.0
Chemicals	-64.8	-32.5	1.5
Non-metallic minerals	-63.4	-28.9	11.3
Electrical and office machinery	-57.8	-30.9	9.8
Automobile	-57.0	-4.4	99.5
Machinery	-57.0	-25.9	31.9
Furniture and toys etc.	-56.6	-38.5	39.6
Other transport equip.	-56.5	-19.6	18.1
Metals	-55.6	-24.2	27.1
Radio, TV etc.	-52.1	-23.8	51.8
Paper	-51.2	-7.4	25.6
Wood	-50.9	-17.6	64.6
Medical and optical equip. etc.	-50.3	-30.6	20.4
Printing and publishing	-45.2	-34.3	69.2
Rubber and plastic	-42.5	-4.8	59.4
Food and tobacco	-33.3	-23.0	39.1
Mean	-55.4	-27.1	35.4

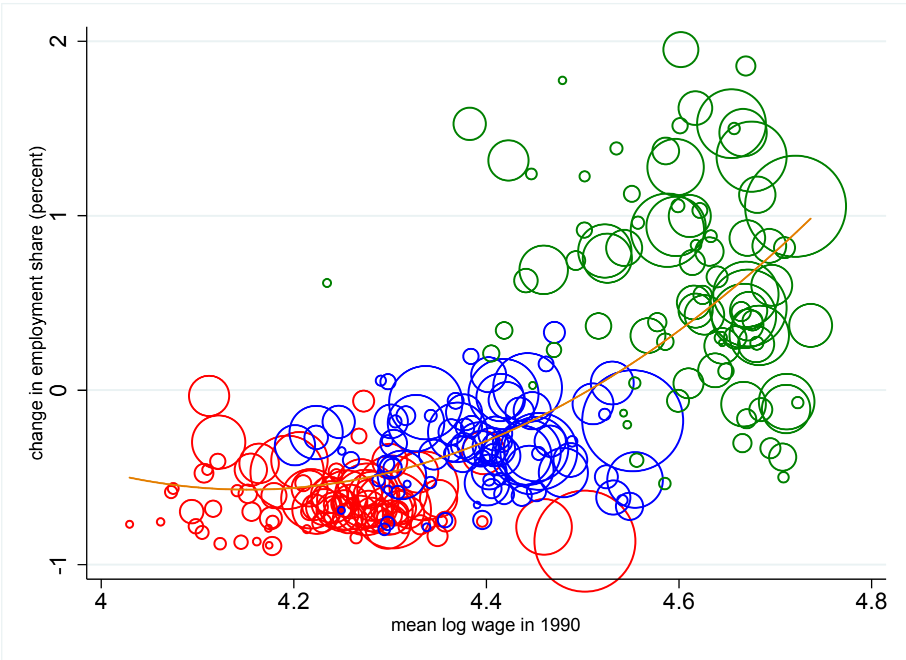
Notes: The table depicts changes in the number of workers (full-time men between 20 and 60 in West Germany) by skill group between the first year of Interval 5 (1990) and Interval 2 (2005) in broader two-digit manufacturing industries. For example, the textile industry lost 73.5% of its low-skilled workforce.
Source: Own calculations, BeH.

Table 1.A.3: Changes in the worker-wage component by industry and skill group in %, 1990–2005

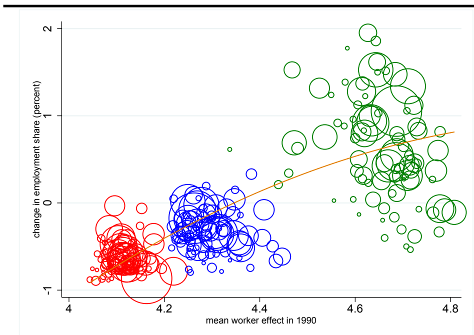
Industry	No voc. training	Voc. training	College / Univ.
Textiles	9.4	12.1	5.1
Clothing and leather	9.7	16.8	20.5
Chemicals	17.4	11.6	29.2
Non-metallic minerals	12.2	14.3	21.9
Electrical and office machinery	30.4	18.7	28.7
Automobile	34.1	19.3	28.6
Machinery	19.4	11.0	16.1
Furniture and toys etc.	23.5	12.8	14.6
Other transport equip.	24.7	15.3	19.5
Metals	17.0	10.6	23.5
Radio, TV etc.	51.6	20.1	24.4
Paper	5.8	7.7	21.7
Wood	9.8	9.9	24.9
Medical and optical equip. etc.	28.4	15.0	34.2
Printing and publishing	34.6	30.1	19.3
Rubber and plastic	21.9	11.6	23.0
Food and tobacco	9.5	12.6	22.1
Mean	21.1	14.7	22.2

Notes: This table shows changes in the dispersion of the worker fixed effect by skill group and industry. For example, the standard deviation of the worker wage component of low-skilled textile workers has increased by 9.4% in the period the first year of Interval 2 (1990) and Interval 5 (2005).
Source: Own calculations, BeH.

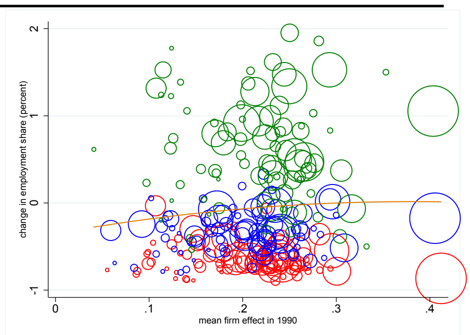
Figure 1.A.1: Changes in industry-skill group employment



PANEL A: Raw wages



PANEL B: Person fixed effect



PANEL C: Firm fixed effect

Notes: The y-axis depicts changes in employment shares of industry-skill-groups from 1990 to 2010. On the x-axis these industry-skill-groups are ranked according to their position in the distribution of mean log wages (Panel A), mean worker fixed effects (Panel B) and mean firm fixed effects (Panel C) in 1990. The skill groups are no training (red), vocational training (blue), and college or university degree (green). Circle sizes represent overall industry sizes.
Source: Own calculations, BeH.

1.A.4 Additional results by skill group

Table 1.A.4 summarizes the results if we estimate our regression model within conventional skill groups. We group all workers with no training, with vocational training, and those with a college or university degree.

Table 1.A.4: Changes in import exposure and in inequality of wage components within education groups

	No vocational training				Vocational training				College/university degree			
	Gravity	Gravity	IV	IV	Gravity	Gravity	IV	IV	Gravity	Gravity	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
PANEL A | Dep. var.: Δ Std. of log wages												
Δ gravity	0.0174	0.00139			0.0285**	0.0285**			0.00117	0.0191		
	(0.333)	(0.939)			(0.026)	(0.028)			(0.956)	(0.406)		
Δ net imports			-0.254	-0.361			0.364	0.116			0.0612	0.347
			(0.444)	(0.287)			(0.132)	(0.665)			(0.783)	(0.182)
R2	0.201	0.304	0.168	0.276	0.146	0.238	0.0680	0.220	0.0602	0.167	0.0497	0.115
PANEL B | Dep. var.: Δ Std. of worker fixed effects												
Δ gravity	0.0157	-0.00595			0.0267**	0.0208*			0.000799	0.0236		
	(0.336)	(0.738)			(0.026)	(0.086)			(0.968)	(0.246)		
Δ net imports			-0.311	-0.434			0.414*	0.0815			-0.117	-0.106
			(0.318)	(0.175)			(0.089)	(0.777)			(0.609)	(0.731)
R2	0.232	0.332	0.202	0.302	0.318	0.394	0.242	0.385	0.0128	0.174	0.0191	0.172
PANEL C | Dep. var.: Δ Std. of firm fixed effects												
Δ gravity	0.000168	0.00290			0.000168	0.00290			0.00195	0.00593		
	(0.971)	(0.596)			(0.971)	(0.596)			(0.747)	(0.373)		
Δ net imports			0.0270	0.0255			0.0270	0.0255			0.0184	0.170
			(0.788)	(0.828)			(0.788)	(0.828)			(0.867)	(0.258)
R2	0.166	0.226	0.163	0.214	0.166	0.226	0.163	0.214	0.0817	0.211	0.0788	0.169
PANEL D | Dep. var.: Δ Cov. of worker and firm fixed effects												
Δ gravity	0.00265	0.00195			0.00359**	0.00275*			-0.0000943	0.000404		
	(0.227)	(0.411)			(0.013)	(0.063)			(0.974)	(0.909)		
Δ net imports			-0.00876	-0.000338			0.00998	0.00694			0.0378	0.0535
			(0.809)	(0.993)			(0.615)	(0.766)			(0.415)	(0.375)
R2	0.00949	0.114	.	0.113	0.0861	0.187	0.0676	0.194	0.00559	0.0423	.	.
N	263	263	262	262	263	263	262	262	263	263	262	262
Interval FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Panel A shows the results of a change in trade on the change in the distribution of log raw wages. In panels B to D the dependent variables are the change in the standard deviation of the worker fixed effect, the firm fixed effect and the change in the covariance of both effects, respectively. The independent variables for trade are either measured as gravity results or instrumented net trade. All models include interval dummies and a constant. In addition even columns include two-digit industry dummies. p-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations, BeH and Comtrade.

Chapter 2

The Effects of Foreign Direct Investment on Job Stability: Upgrades, Downgrades, and Separations

Co-authored with Johann Eppelsheimer

2.1 Introduction

Multinational enterprises (MNEs) are one of the most controversial aspects of globalization. While firms benefit from foreign direct investment (FDI) by saving production costs or by exploiting new markets, MNEs are often criticized for replacing domestic with foreign labor. Empirical results on the employment and wage effects of FDI are ambiguous and can neither support nor fully reject these fears (see Crinò, 2009 and Hummels *et al.*, 2018 for recent surveys). We argue that the literature has overlooked another important channel by which firms adjust their workforce following FDI—namely, firm-internal restructuring. Our data imply that the rate of job transitions within MNEs is 1.5 times higher than that in domestic firms. In this paper, we therefore investigate whether internal transitions increase when firms turn multinational. Moreover, we distinguish between up- and downgrading of workers to more- or less-complex jobs.

The question how FDI affects job transitions is closely related to that of how FDI affects labor demand. Managing foreign affiliates plausibly requires coordination and administration. Thus, the demand for interactive and analytical tasks should increase when firms turn multinational, as shown by previous studies (e.g., Becker *et al.*, 2013; Nilsson Hakkala *et al.*, 2014; Laffineur & Mouhoud, 2015). Moreover, if FDI is accompanied by global fragmentation of production chains, MNEs can specialize their domestic workers in fewer tasks. Such fragmentation might lead to simpler task sets for some workers, while others might specialize in more-complex tasks. To adjust to these changes in labor demand, MNEs can rely on internal labor markets. Incumbent workers possess firm-specific human capital (Becker, 1962), which represents a productivity advantage over outsiders. Further, hiring internally reduces asymmetric information on the skills and abilities of workers (e.g., Waldman, 1984; Greenwald, 1986) and might cost less (Demougin & Siow, 1994) compared to hiring outside the firm. Moreover, it can be cheaper for MNEs to demote workers whose tasks become redundant over the course of FDI than to dismiss them. This might especially apply to labor markets with strict dismissal protection laws, strong works councils and unions. Thus, in addition to the extensive margin of hires and layoffs, MNEs have incentives to restructure their workforce internally after investing abroad.

No extant study has investigated the impact of FDI on internal job transitions. The two papers most closely related to the topic are Liu & Trefler (2011) and Baumgarten (2015). Both consider the effect of offshoring on occupational switches. Liu & Trefler (2011) find a positive effect on switches to occupations with higher and lower average wages for US service offshoring. For Germany, Baumgarten (2015) finds that offshoring is not associated with greater occupational instability on average. However, he shows that workers with more non-routine tasks face less occupational uncertainty through offshoring. In contrast to our paper, both studies examine the impact of offshoring in general, not FDI in particular. Moreover, they do not separately consider firm-internal occupational switches. We believe that firm-internal restructuring processes play a crucial role over the course of FDI because establishing or acquiring foreign firms entails deep organizational changes. Conversely, offshoring does not require comparably extensive organizational changes, as it mainly covers trade with unaffiliated firms. Moreover, industry-level offshoring data only permit indirect conclusions for individual workers within industries. By contrast, we can draw direct conclusions on how a firm's decision to invest abroad affects the job stability of its workers.

To investigate the impact of FDI on job stability, we exploit a unique administrative micro-panel dataset. By using these data, we can follow MNEs, domestic firms and their workers for two decades with quarterly precision. Specifically, our data comprise the entire universe of German firms with Czech affiliates as of 2010 and a large pool of domestic control firms that never conducted FDI in any country. German FDI in the Czech Republic represents a compelling case of FDI flows, as Germany is the largest economy in Europe, and the Czech Republic is one of its main recipients of investment among the Central and Eastern European Countries (CEEC).[1] In contrast to previous studies, our data also cover small firms with low investment volumes.[2] This is an advantage, as the geographic proximity and low labor costs of the Czech Republic allow small firms to also invest beyond the border. Our data further include the complete administrative employment biographies of all workers in the investing and domestic firms.

[1] In 2010, approximately 24% of the workers employed by German firms in the CEEC worked in the Czech Republic (Deutsche Bundesbank, 2014).

[2] In the majority of datasets on FDI, small firms with low investment volumes are underrepresented because only investments above a certain threshold need to be registered officially (see Pflüger *et al.*, 2013). With regard to our analysis, Schäffler (2016) shows that only one-fourth of Czech affiliates with a German owner appear in the Microdatabase Direct Investment (MiDi) provided by the Federal Bank of Germany, which is commonly used to study the FDI of German firms.

To identify the effects of FDI on the occurrence of job upgrades and downgrades and separations of workers and firms, we pursue a three-step procedure. As Helpman *et al.* (2004) show, only the most productive firms conduct FDI. We therefore first construct a balanced sample of MNEs and domestic firms with equal probabilities of investing. We propose an iterative matching procedure that allows us to achieve a distinct one-to-one matching of MNEs and domestic firms over the entire observation period. Additionally, our matching approach ensures that we match firms exactly in the same year. Standard propensity score matching cannot meet both requirements. Further, our matching approach allows us to assign the investment dates of matched MNEs as *pseudo* investment dates to domestic firms. We match firms two years before investment. Because of the equal probabilities of conducting FDI and the significant time lag between the matching and the (pseudo) investment, it should be impossible for workers to distinguish between future MNEs and domestic firms at the time of matching. Second, to overcome ability-driven sorting of workers (e.g., Card *et al.*, 2013) into MNEs, we restrict our data to individuals who already worked in the firm in the year of matching. Third, we compare the likelihood of job upgrades and downgrades and separations between MNEs and domestic firms at the worker level. To reap the benefits of the event history design of our data, we use Cox (1972) proportional hazard models to estimate the effects. We define job upgrades (downgrades) as job switches within the firm to occupations with a higher (lower) share of analytical and interactive tasks, which we refer to as *complex tasks*.

This article is the first to show that firms meet altered labor demand due to FDI by internally restructuring their workforce. More precisely, when firms invest abroad, the likelihood that workers will upgrade internally to more-complex jobs increases by 24%. Simultaneously, the hazard to downgrade to less-complex jobs increases by 34%. Both effects increase over time and become traceable two years after investment. However, we find that only workers in relatively non-routine and interactive jobs receive the opportunity to internally switch occupations. In line with these results, the same group of workers faces lower hazards of employment separations in MNEs. Altogether, we find only weak effects of FDI on separations. The average worker has a higher chance of remaining shortly after the investment, but this lock-in effect disappears after several quarters. We further investigate whether worker productivity influences their job stability in the investing firms. Although workers in MNEs are considerably more likely to switch occupations, MNEs follow the same pattern as domestic firms do when choosing who to upgrade, downgrade or dismiss.

Independent of FDI, firms promote more productive workers and dismiss or demote less productive workers.

This paper relates to several strands of the theoretical and empirical literature on the employment effects of FDI in the source country. Theory predicts both positive (e.g., Grossman & Rossi-Hansberg, 2008) and negative (e.g., Feenstra & Hanson, 1996) effects of FDI on the employment and wages of domestic workers. Thus, determining the net effects remains an empirical question. Within the empirical literature, our paper is related to studies on the employment effects of FDI, especially those differentiating between tasks (e.g., Becker *et al.*, 2013; Laffineur & Mouhoud, 2015). Specifically, our paper relates to the empirical literature considering the effects of FDI on employment stability. Becker & Muendler (2008) were the first to consider job-separation rates of German MNEs. They find them to be four percentage points lower than those of domestic firms—half of this difference can be explained by foreign employment expansions of MNEs. Bachmann *et al.* (2014) estimate the effects of both inward and outward FDI on employment security in Germany. They find that FDI, particularity to CEEC, reduces employment security for low-skilled and older workers. In contrast to our paper and to Becker & Muendler (2008), Bachmann *et al.* (2014) use industry-level data on FDI and cannot analyze the direct effects of firm-level decisions on FDI.

A larger body of literature considers the job security effects of offshoring, which, in contrast to FDI, also includes trade with unaffiliated foreign firms. These papers yield ambiguous results (see, e.g., Liu & Trefler, 2011, Ebenstein *et al.*, 2014 for the US; Munch, 2010 for Denmark; Egger *et al.*, 2007 for Austria; and Geishecker, 2008, Bachmann & Braun, 2011, Baumgarten, 2015 and Görg & Görlich, 2015 for Germany). Within this strand of literature, some studies also consider occupational switches, although not exclusively within the borders of the firm. Baumgarten (2015) finds that offshoring—measured by an occupation-specific exposure to imported intermediates—decreases the risk of occupational switches for highly non-routine jobs. However, these effects are strongest for transitions to non-employment. He does not distinguish between occupational up- and downgrades. The only other paper that considers up- and downgrades is by Liu & Trefler (2011). They are the first to show theoretically and empirically that promotions and demotions are a common reaction to offshoring in general. They find that US offshoring to China and India increases job downgrades by 17% and job upgrades by 4%.

The remainder of the paper is structured as follows. The next section explains our identification strategy. Section 2.3 describes the data. Section 2.4 reports our results and discusses implications. Section 2.5 summarizes several robustness exercises, and Section 2.6 concludes.

2.2 Empirical strategy

Our aim in the empirical analysis is to measure the effect of FDI on job stability. Our approach consists of three steps. First, we construct a panel dataset of MNEs and domestic firms by using an iterative matching approach. Second, we address endogenous sorting of workers into firms. Third, we use proportional hazard models to estimate the influence of FDI on the probability of employment separations and occupational up- and downgrades.

As Helpman *et al.* (2004) show, only certain types of firms are likely to invest abroad. Thus, in a first step, we use a broad database of firm characteristics to estimate firm-specific investment probabilities for each MNE and control firm. We begin with propensity score matching to create a homogeneous dataset of MNEs and domestic firms with equal probabilities to invest.[3] The resulting dataset consists of comparable MNEs and domestic firms with a balanced distribution of firm characteristics across the two groups. One benefit of a matched sample is that it increases the robustness of statistical inference (Imbens & Rubin, 2015). Furthermore, matching allows us to assign pseudo investment dates to domestic firms. For workers in MNEs, the onset of the risk of switching occupations or leaving the firm begins with the investment. For workers in domestic firms, there is no investment date and thus no inherent interval to observe their risk of each event. We therefore assign the investment date of the best matched MNE to the domestic firm.

To assign appropriate investment dates, we match firms exactly in the same year. Further, we require a one-to-one matching of firms over the whole observation period. Because standard matching procedures cannot satisfy both requirements, we proceed

[3]Propensity score matching has previously been used in the FDI context by a wide range of studies, e.g., Bronzini (2015), Crinò (2010) and Barba Navaretti & Castellani (2004) for Italy, Hijzen *et al.* (2011) for France, Debaere *et al.* (2010) for Korea, Barba Navaretti *et al.* (2010) for France and Italy, Becker & Muendler (2008) and Kleinert & Toubal (2007) for Germany, Hijzen *et al.* (2007) for Japan, and Egger & Pfaffermayr (2003) for Austria. However, the majority of these studies consider FDI effects at the firm, not the individual, level.

as follows.[4] We assign MNEs two years prior to investment and domestic firms in every observation year to our pool of firms for the matching. We select a lag of two years for MNEs to avoid that their investment decision may already affect firm characteristics (see also Hijzen *et al.*, 2011). For every MNE, we use propensity score matching to find the three best matched domestic firms exactly in the same year (e.g., matches MNE A: a_{2004}, b_{2004}, c_{2004}; matches MNE B: a_{2006}, d_{2006}, e_{2006}). After this first step, domestic firms can appear multiple times as matches for different MNEs (e.g., a_{2004} and a_{2006}). In the second step of the matching approach, we thus find the single best match of treatment and control firms over the whole observation period by an iterative procedure (see Algorithm 1 in Appendix 2.A.1 for details). Initially, we select the best match out of the three potential matches for each *MNE* (e.g., matches MNE A: $\mathbf{a_{2004}}$, b_{2004}, c_{2004}; matches MNE B: $\mathbf{a_{2006}}$, b_{2006}, c_{2006}).[5] From the resulting list of potentially best matches, we retain only the best match for a *domestic firm* over the whole observation period (matches MNE A: $\cancel{\mathbf{a_{2004}}}$; matches MNE B: $\mathbf{a_{2006}}$). Then, we update the list of potential matches for *MNEs* and move up second-ranked matches if necessary (matches MNE A: $\mathbf{b_{2004}}$, c_{2004}; matches MNE B: a_{2006}, d_{2006}, e_{2006}). Finally, we repeat the procedure two times, which results in a one-to-one matching of firms exactly in the same year without using any domestic control firm multiple times (e.g., final best match MNE A: b_{2004}; final best match MNE B: a_{2006}). This matching procedure results in a balanced dataset of MNEs and domestic firms with equal probabilities to invest (for details, see Appendix 2.A.2).

In the second step of our empirical analysis, we link the full employment histories of workers to the matched firm data. To ensure that workers do not self-select into MNEs, we restrict our data to individuals who already worked in the firm at the time of the matching (i.e., two years prior to the (pseudo) investment). It should be impossible for workers to distinguish between future MNEs and domestic firms at the time of the matching because of the firms' equal probabilities of conducting FDI and the significant time lag between the matching and the (pseudo) investment.

[4] Although matching without replacement ensures that observations—firm-years in our case—are matched only once, it does not guarantee that associated observations—firms in our case—are matched only once. Thus, control firms could be matched to multiple treatment firms in different years.

[5] The goodness of a match is defined by the smallest differences in the estimated propensity scores, which we obtain from first step of our matching procedure. For a detailed description, see Appendix 2.A.2.

In the final step of our empirical analysis, we estimate the effects of FDI on the individual likelihood to switch jobs within the firm and to separate from the firm. To reap the benefits of the event history design of our data, we use Cox (1972) proportional hazard models to measure the effects of FDI on job stability.[6] We estimate the hazard ratios of employment separations and occupational up- and downgrades in separate models and treat competing events as censoring:

$$\log h_e(t|x_{ijtyro}) = h_0(t) + \gamma I(\mathrm{FDI}_j) + x_{ijt}\beta_1 + x_{ijt}t\beta_2 + \tau_y + \omega_r + \theta_o + u_{ijtyro}. \quad (1)$$

Here, $h_e(t|x_{ijtyro})$ is the hazard rate of event $e \in \{\text{separation, upgrade, downgrade}\}$, $h_0(t)$ is the baseline hazard rate, $I(\text{FDI})$ is an indicator variable for the investment, and γ measures the according treatment effect. Further, x_{ijt} is a vector of time-varying worker (i) and firm (j) characteristics, and $x_{ijt}t$ is an interaction of these characteristics and time since the (pseudo) investment. Our model further purges investment effects from year (τ_y), region (ω_r) and occupation (θ_o) fixed effects.

We measure the events e with quarterly precision. In our setting, workers become at risk of separation or up- or downgrade at the quarter of the (pseudo) investment, and we then follow them for 20 quarters. We define occupational switches within the firm as upgrades if the intensity of analytical and interactive tasks is higher in the new job than in the old one and as downgrades if the intensity of analytical and interactive tasks decreases. We summarize analytical and interactive tasks by the term *complex tasks*. Because task compositions also vary within occupations, we compare old and new jobs at the same point in time (i.e., immediately after the job switch). Employment separations occur if workers leave the firm.

As indicated previously, we treat competing events as censoring. This means that after the occurrence of an event (e.g., an occupational upgrade), we remove workers from the risk set of the other two events (e.g., occupational downgrades and job separations). The underlying rationale is that each possible event is the outcome of a distinct causal mechanism. In essence, the likelihood of an event e depends on worker performance and the objective of a firm ($P(e) = f(\text{worker performance, firm objective})$). Clearly, worker performance increases the likelihood of occupational upgrades and reduces the probability of downgrades or separations. In essence, the objective of

[6]Compared to linear probability models and logit or probit models, proportional hazard models offer several advantages. For instance, they are robust to deviations from the normality assumption and censored events, and they allow us to include time-varying covariates.

a firm consists of two dimensions: (1) firm size and (2) internal task structure. A firm might want to shrink or grow its domestic plant after FDI and simultaneously might plan to perform more- or less-complex tasks. Importantly, the objective of the firm distinctly alters the likelihood of each event for each individual.

For instance, if a firm follows the classical factor-seeking motive of FDI (see, e.g., Helpman, 1984; Markusen, 2002) and seeks to reduce labor costs by relocating offshorable tasks to a foreign plant, it attempts to (1) shrink, which raises the hazard of separations, and (2) perform more complex supervisory and management tasks, which increases the likelihood of upgrades. Since neither of the two objectives facilitates downgrades, the according probability remains constant. However, fragmentation is often not as simple as described above because modern production processes are interwoven. Therefore, offshoring certain production stages also affects tasks in the up- and downstream processes of the firm. These changes in the task structure might lead some incumbent workers to take over new tasks, which results in (2) up- and downgrades in all areas of the firm. Another motive is market-seeking FDI (see, e.g., Markusen, 1984, 2002), where a firm intends to serve the foreign market by production on site. Thus, (1) firm size remains unchanged or even increases, which reduces the hazard of separations and (2) the firm requires more complex supervisory and management tasks, which increases the likelihood of upgrades. Downgrades are not affected. In summary, the complex interplay of worker performance and firm objectives portrays parallel causal mechanisms that idiosyncratically influence the probabilities of separations and up- and downgrades. Thus, we regard competing events as censoring. However, as we show in the robustness section, alternative strategies that do not treat events as mutually exclusive do not affect the results.

The baseline model (Equation (1)) captures time-constant effects of FDI on job stability, i.e., the average effect over the five-year interval after the investment. However, it is possible that the effect of FDI varies over time. If, e.g., workers need further training to switch occupations within the firm, we will not observe an effect of FDI immediately after the investment. Thus, we estimate the influence of FDI on job stability over time by:

$$\log h_e(t|x_{ijtyro}) = h_0(t)+\gamma_0 I(\mathrm{FDI}_j)+\gamma_1 I(\mathrm{FDI}_j)t+x_{ijt}\beta_1+x_{ijt}t\beta_2+\tau_y+\omega_r+\theta_o+u_{ijtyro}, \tag{2}$$

where $I(\mathrm{FDI}_j)t$ is the interaction of the investment dummy and time since the investment. The remainder of Equation (2) is identical to Equation (1). Because treatment is assigned to firms (not workers), we cluster standard errors at the firm level in both models (see Abadie *et al.*, 2017).

2.3 Data and descriptive statistics

2.3.1 Data

To analyze the effects of FDI on workers' job stability, we synthesize four data sources. We retrieve information on German FDI in the Czech Republic from the *Research on Locational and Organisational Change* database (*ReLOC*).[7] The ReLOC data identify the entire universe of German firms with affiliates in the Czech Republic in the Czech commercial register 2010. ReLOC covers 3,406 German investors and the exact date of their investment.[8] To compare developments in investing firms to those in domestic firms, a control group of 9,700 German firms without any foreign affiliate (in any country) completes the ReLOC data.

We link the ReLOC data to two administrative micro-datasets from the Institute for Employment Research (IAB). We receive the establishment-level information from the *Establishment History Panel (BHP 7514v1)* and individual-level data from the *Integrated Employment Biographies (IEB V10.00).* The BHP contains information on the employment and wage structure of all German establishments with at least one employee subject to social security contributions as of June 30 between 1975 and 2014.[9] The IEB includes the complete employment biographies of all individuals in the German social security system after 1975. In particular, the data offer information on occupations and employment spells with daily precision. Because both the BHP and the IEB use mandatory social security notifications for all German employers, they are highly reliable. Applying record linkage, Schäffler (2014) joins the ReLOC data and the BHP. The resulting dataset groups establishments into firms and provides investment information at the firm level. We attribute to the firm the region or industry of the largest establishment. Further, we merge the IEB with the BHP by using their readily available shared identifiers. Our observation

[7] Refer to Hecht *et al.* (2013b) for details on the ReLOC dataset.

[8] Hecht *et al.* (2013a) show in their survey of 459 firms of the ReLOC dataset that almost 70% of the firms with FDI in the Czech Republic have not invested anywhere else before.

[9] Refer to Eberle & Schmucker (2017) for details on the BHP.

period begins after the fall of the *iron curtain*, 1990, and ends with the most recent registered investments in the ReLOC data, 2010.

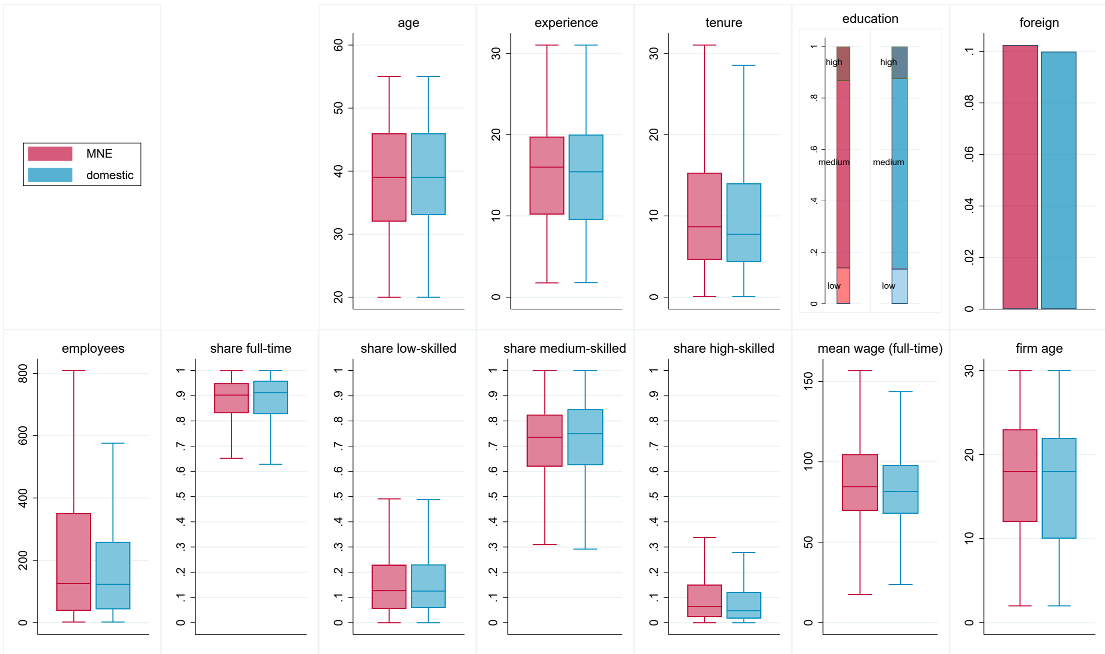
To identify occupational up- and downgrades, we extend our data with the task structures of occupations. Therefore, we use data from the *BIBB-IAB Employment Surveys* 1991, 1999 and the *BIBB/BAuA Employment Survey* 2006 (see Hall & Tiemann, 2006). For each occupation and survey year, we receive the share of each of the five task categories—i.e., routine-manual, routine-cognitive, non-routine-manual, analytical, and interactive activities—by using an algorithm described in Matthes (forthcoming).

From the spell data, we construct a quarterly panel with March 31, June 30, September 30, and December 31 as reference dates. If an employee has more than one job notification per reference date, we only use the one with the highest earnings. To ensure that we do not mistake maternity leave or retirement for job separations, we restrict the sample to male workers between 20 and 55 at the time of the investment. Further, we only consider regular full-time workers for two reasons. First, we are only interested in *regular* job changes and not in, e.g., switches from part- to full-time or from marginal to regular employment. Second, workers in marginal employment might intrinsically aim to improve their labor market positions and thus might distort our findings. To strengthen our identification strategy, we restrict the sample used for our baseline estimates to workers who, at the time of the (pseudo) investment, worked for at least for two years in their firm. We correct inconsistent information on individual education following Fitzenberger *et al.* (2005). Furthermore, the wages of approximately 10% of the spells are right-censored due to the contribution assessment ceiling in Germany. We impute these records using an imputation procedure that follows Dustmann *et al.* (2009) and Card *et al.* (2013).

2.3.2 Descriptive statistics

Figure 2.1 presents an overview of individual (first row) and firm (second row) characteristics after applying our matching algorithm. The box plots and bar charts illustrate that the worker and firm characteristics of MNEs and domestic firms are well balanced in the quarter of the (pseudo) investment, i.e., two years after matching. Although they were not part of the matching, worker characteristics are also well balanced. In both samples, the distributions of employees' age, experience and

Figure 2.1: Worker and firm characteristics after matching

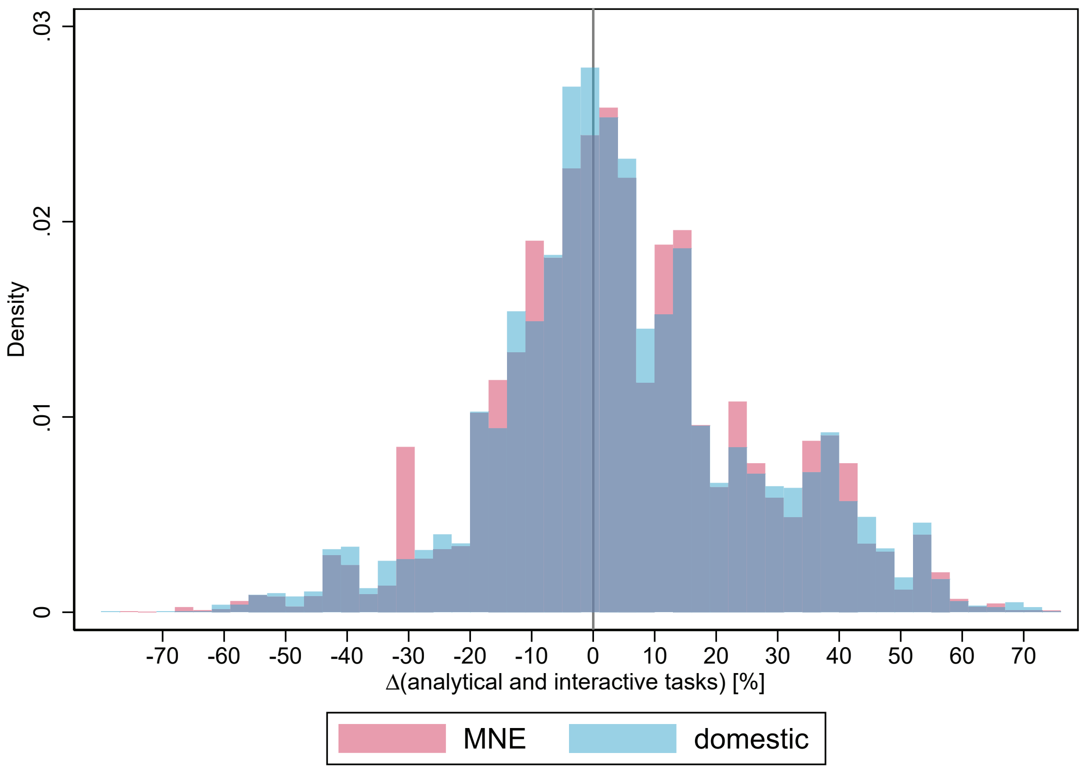


Notes: The figure shows box plots and bar charts of various worker (first row) and firm (second row) variables. The horizontal line in the middle of a box represents the median. The edges of a box indicate the first and the third quartiles. The range of the whiskers illustrates minima and maxima, limited to $\frac{3}{2}$ of the first or third quartile, respectively. For the education and foreign variables, the figure presents bar charts, which depict the shares of individuals in the corresponding group.
Source: ReLOC, IEB and BHP, own calculations.

tenure are almost identical. Additionally, the composition of the workforce with respect to education and nationality are highly comparable. Moreover, the firm-level characteristics of the treatment and control firms are almost equivalent in their medians and first and third quartiles. The figure shows that both firm groups are similar in size, age, average wages, and shares of different worker groups. Only the firm size of MNEs has a larger variation in the upper part of the distribution.

The focus of this article is on occupational up- and downgrades. Figure 2.2 therefore visualizes changes in analytical and interactive tasks for workers that switch occupations within the firm. Based on these changes, we define occupational upgrades as job switches accompanied by an increase in analytical and interactive tasks (bins to the right of zero) and downgrades as job switches accompanied by a decrease in analytical and interactive tasks (bins to the left of zero). Common upgrades in our data include, e.g., promotions from instrument mechanics to technicians or line

Figure 2.2: Histograms of up- and downgrades in MNEs and domestic firms



Notes: The figure shows the distribution of up- and downgrades by percentage point changes of the share of analytical and interactive tasks for job switches within investing (MNE) and domestic firms. We define job upgrades (downgrades) as firm-internal job transitions to occupations with a higher (lower) share of analytical and interactive tasks. Therefore, all upgrades are found to the right of the zero line and all downgrades to its left.
Source: ReLOC, IEB and BHP, own calculations.

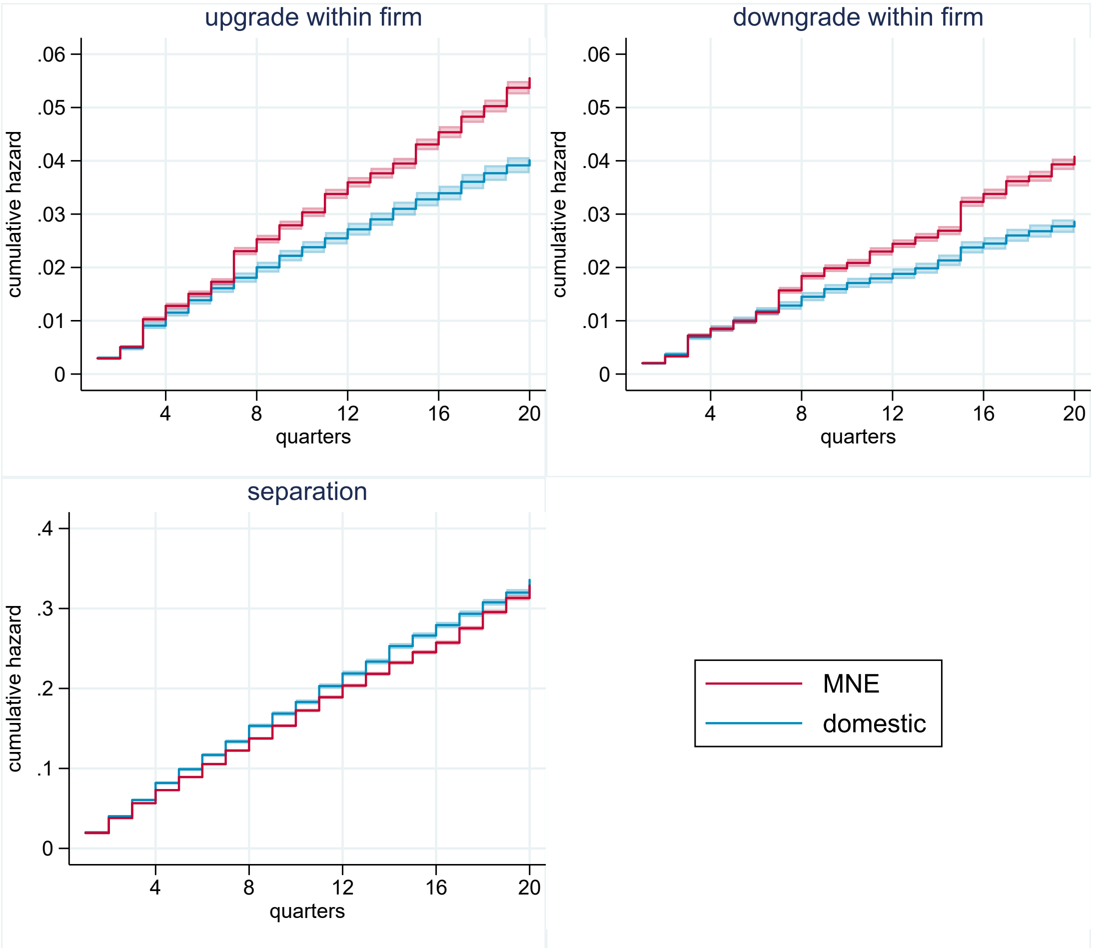
workers to stock managers. The former example leads to a broader, less routine set of tasks; the latter example enhances supervisory responsibilities. Frequent downgrades include, e.g., electricians to metal workers or metal workers to welders. Both examples lead to a less-complex task set. The graph shows that for the majority of workers, an occupational switch changes the complexity of their job by up to 40 percentage points. Of all up- and downgrades, 60% entail changes in complexity of more than 10 percentage points.

Having defined up- and downgrades, let us now descriptively assess their relative frequencies in MNEs and domestic firms. Figure 2.3 illustrates the cumulative hazards of separations and up- and downgrades. The cumulative hazard indicates the probability of an event within a given timeframe. The upper-left panel of Figure 2.3

shows that the hazard of receiving a job upgrade is larger for workers in investing firms than for those in domestic firms. In the quarters immediately following the investment, the difference is negligible. However, approximately two years after the investment, the likelihood of a job upgrade in MNEs clearly exceeds that in the control group. After 20 quarters, the probability of receiving an occupational upgrade is 5.7% in MNEs. In domestic firms, it is only 4%. The development of the risk of downgrades is similar. However, the hazard of a downgrade is lower than the hazard of an upgrade. Figure 2.3 further illustrates that the risk of separation is higher than the likelihood of both types of occupational changes within the firm. However, separation rates differ only barely between MNEs and domestic firms. In fact, they are slightly lower in MNEs than in domestic firms.

In summary, Figure 2.3 suggests that most of the adjustments over the course of FDI take place within the firm. Although the described hazards only provide descriptive evidence, they mirror well our multivariate findings that follow in the next sections.

Figure 2.3: Cumulative hazards of up- and downgrades and separations in MNEs and domestic firms



Notes: The figure shows the cumulative hazards for the three events, *separation from the firm* as well as *internal up- and downgrades*, by quarters after the (pseudo) investment, distinguishing between investing (MNE) and domestic firms. Light blue and light red colors indicate 95% confidence bands. The cumulative hazard indicates the probability of an event within a given timeframe. For instance, the individual hazard of receiving an occupational upgrade within 20 quarters in FDI firms is 5.7% (first panel). Hazards of occupational up- and downgrades are significantly larger in MNEs than in domestic firms. By contrast, the hazard of separations is slightly larger in domestic firms.
Source: ReLOC, IEB and BHP, own calculations.

2.4 Results

2.4.1 Main results

This section presents the estimation results for the impact of FDI on the job stability of the average worker in the investing firm. We distinguish between effects on the likelihood of separations of workers and firms and upgrades into more-complex jobs and downgrades into less-complex jobs within the firm. Table 2.1 summarizes the main results. Columns 1, 3 and 5 show the time-independent impact of FDI on the hazard of separation and up- and downgrades, respectively (see Equation (1)). Columns 2, 4 and 6 provide the results from a dynamic specification. Here, the FDI indicator is interacted with the quarters since the investment (see Equation (2)). The table denotes the effects as hazard ratios, which have the same interpretation as odds ratios.

Table 2.1: Effects of FDI on the hazard ratios of separations and up- and downgrades

	separation		upgrade		downgrade	
	(1)	(2)	(3)	(4)	(5)	(6)
FDI	0.9630	0.8092**	1.2422**	0.9960	1.3413**	0.9781
	(0.0440)	(0.0691)	(0.1315)	(0.1896)	(0.1980)	(0.2094)
FDI × quarter		1.0190**		1.0252*		1.0352*
		(0.0083)		(0.0141)		(0.0205)
Subjects	383,098	383,098	383,098	383,098	383,098	383,098
Events	102,661	102,661	15,880	15,880	11,731	11,731

Notes: The table presents exponentiated coefficients (hazard ratios) and cluster robust standard errors at the firm level (in parentheses). *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Additional control variables in all models are: age, age squared, experience, tenure, a foreign dummy, skill dummies, firm age and a dummy if the firm existed in 1975 (all interacted with quarters since treatment), as well as occupation, year, and state dummies. The deviation of the estimated hazard ratios from one can be interpreted as changes in the probabilities of the events attributable to FDI. For example, an estimated hazard ratio for separation of 0.8092 indicates that FDI reduces the individual risk of separation by 19.08% in the quarter of investment. Estimates are based on a matched sample of MNEs and domestic firms. The full table, including estimates on control variables, can be found in the Appendix (Table 2.A.4).
Source: ReLOC, IEB and BHP, own calculations.

As Column 1 indicates, we find no effect of FDI on separations in the static model. In contrast, the hazard ratios of 1.24 and 1.34 imply that FDI increases the likelihood of a job upgrade by 24% and the likelihood of a downgrade by 34%. Table 2.1 further shows that the absolute number of promotions and demotions in our sample is much lower than the number of separations. Nevertheless, the estimated hazard rations indicate that MNEs adjust their workforce to meet changing labor demand over the course of FDI mainly through internal occupational changes. However, separations do not seem to be an important adjustment channel.

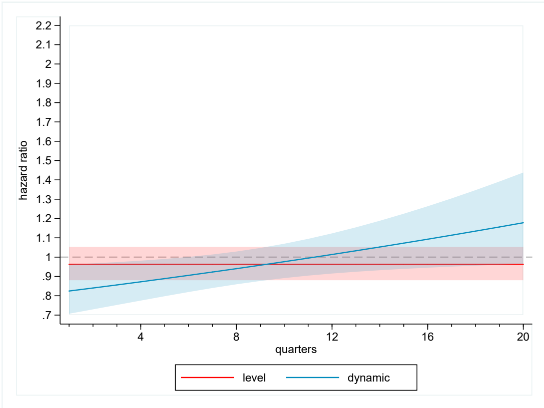
The static model provides average hazard ratios over the five-year period after (pseudo) investment. However, it is possible that the hazard of each event changes over time. Because estimates from the dynamic models are not directly interpretable from Table 2.1, we illustrate the time-varying impact of FDI on job stability in Figure 2.4. The blue lines in Panels A, B and C show time-dependent hazard ratios for separations and up- and downgrades. For comparison, the horizontal red lines indicate estimates from the static models. Shaded areas show 95% confidence intervals. The dashed line in each panel has an intercept of one and therefore serves as reference to a scenario with no influence of FDI.[10]

While time-invariant hazard ratios indicate no effect of FDI on workers' separation rates, more flexible time-variant estimates imply a short lock-in effect immediately after the investment. Specifically, the hazard of separation is 19% lower in MNEs in the quarter of the investment. It increases by 1.9% in each following quarter. However, over five years, the effect never becomes significantly positive. We conclude that there is no evidence that FDI increases the risk of separations for the average worker. On the contrary, FDI has an advantageous lock-in effect, which, however, vanishes approximately six quarters after the investment.

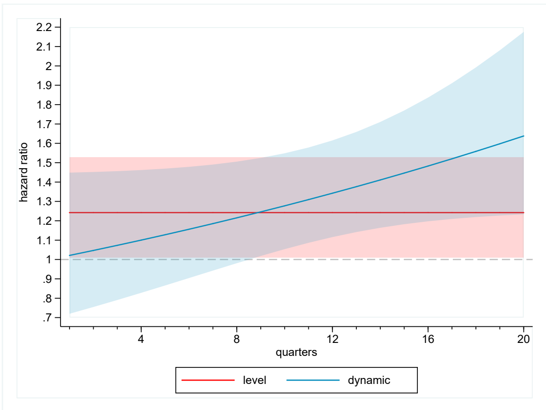
Panels B and C of Figure 2.4 illustrate the effect of FDI on the hazard ratios of up- and downgrades. Both graphs show that there is no instantaneous effect of FDI on the likelihood of job switches within the firm. Instead, the effects evolve over time and become statistically significant approximately two years after the investment. The likelihood of upgrading to a more-complex job increases by 2.5% every quarter due to FDI. The risk of downgrading to a less-complex job increases by 3.5% per quarter. There are several possible explanations for the time lag between FDI and the occurrence of job switches. For instance, it might well be that firms do not restructure their domestic plants immediately after the investment. Further,

[10]Because hazard ratios are exponentiated coefficients, the impact of FDI on the hazard ratio t quarters after the investment is $\exp(\gamma_0) \times \exp(\gamma_1)^t$. As an example, the hazard ratio for job upgrades due to FDI eight quarters after the investment increases by a factor of $0.996 \times 1.025^8 = 1.21$. Note that confidence intervals depend on the variance of the estimands γ_0 and γ_1, as well as their covariance. Thus, standard errors from Table 2.1 do not suffice to infer the significance of the effects.

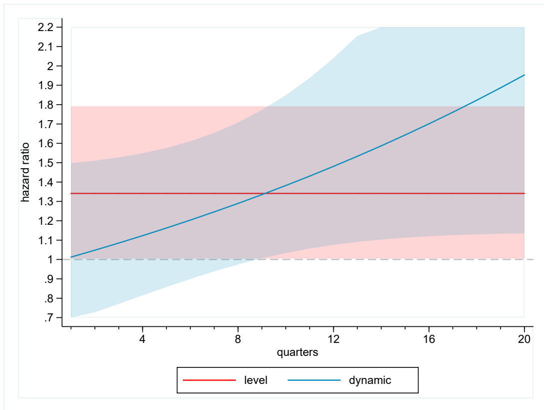
Figure 2.4: Dynamic effects of FDI on the hazard ratios of separations and up- and downgrades



PANEL A: Separations



PANEL B: Upgrades



PANEL C: Downgrades

Notes: The figures provide a graphical representation of the hazard ratios and 95% confidence intervals of the estimated effects of FDI on separations and up- and downgrades. The results are obtained from the Cox regressions presented in Table 2.1. The red lines display the level effects of FDI, i.e., the average effects over five years after investment. The blue lines show the development of the estimated hazard ratios over time (see the interaction effects of FDI × quarter in Table 2.1). The deviation of the estimated hazard ratios from one can be interpreted as changes in the probabilities of the events attributable to FDI. For example, an estimated hazard ratio for separation of 0.8092 indicates that FDI reduces the individual risk of separation by 19.08% in the quarter of investment. Sources: ReLOC, IEB and BHP, own calculations.

it takes time to negotiate new positions with incumbent workers, and it might be necessary to re-train workers before they can fill new positions.

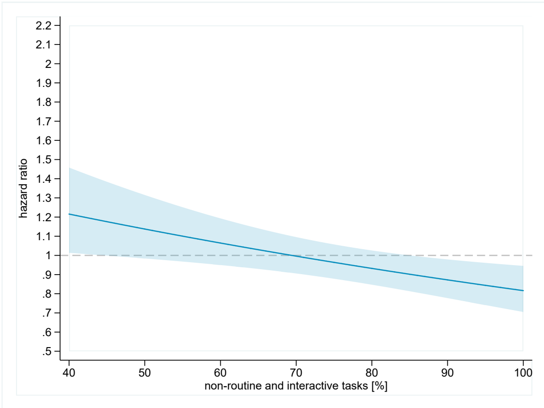
2.4.2 Job stability and tasks

Not all workers in the investing firms might be equally affected by FDI. Recent literature shows that the effects of offshoring depend substantially on the tasks that are performed on a job (e.g., Blinder, 2006; Grossman & Rossi-Hansberg, 2008). In particular, scholars classify routine (Levy & Murnane, 2004), codifiable (Leamer & Storper, 2001), and non-interactive tasks (Blinder, 2006) as easily offshorable. In this section, we therefore explore heterogeneous effects of FDI depending on the offshorability of the tasks of the initial job. Following the literature, we define the level of offshorability for every occupation as the share of routine and non-interactive tasks.

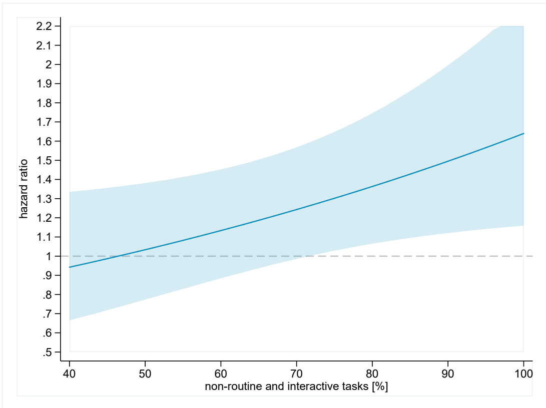
Figure 2.5 illuminates the impact of FDI on job stability depending on the initial offshorability of jobs. For ease of interpretation, the share of non-routine and interactive tasks increases from left to right. Thus, more easily offshorable jobs are on the left and jobs that are theoretically more resistant to offshoring on the right of the x-axis. Technically, the graphs show the interaction effect of FDI with the share of non-routine and interactive tasks (see Table 2.A.5 in the Appendix). Note that the x-axis scale ranges from 40% to 100% because there are practically no occupations comprising less than 40% non-routine and interactive tasks (see Figure 2.A.1 in the Appendix).

As can be seen from Panel A of Figure 2.5, the likelihood to separate from the firm increases with the offshorability of occupations (right to left). While FDI significantly reduces the hazard of separation for workers in highly non-routine and interactive jobs, FDI barely increases the risk of separations for workers in offshorable occupations. These results are in line with what we would expect theoretically. Internationalization means that investing firms require more administration, management and supervision. Because these tasks are mainly undertaken by workers with highly non-routine and interactive jobs, it seems plausible that they stay. On the contrary, workers with jobs with a high share of offshorable tasks could lose their jobs due to FDI. However, as argued by Grossman & Rossi-Hansberg (2008), foreign activity can increase a firm's productivity. This productivity effect can save workers with offshorable jobs from dismissal. This argumentation might explain why we barely observe an effect of FDI on the separation rate for employees with routine and non-interactive jobs.

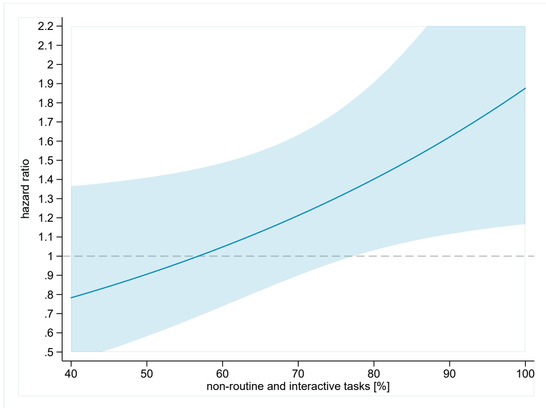
Figure 2.5: Effects of FDI on the hazard ratios of separations and up- and downgrades depending on the share of non-routine and interactive tasks



PANEL A: Separations



PANEL B: Upgrades



PANEL C: Downgrades

Notes: The figures provide a graphical representation of the hazard ratios and 95% confidence intervals of the estimated effects of FDI on separations and up- and downgrades. The blue lines plot these estimated hazards against a worker's share of offshorable tasks, i.e., routine and non-interactive tasks, before investment. The results are obtained from Cox regressions presented in Table 2.A.5 in the Appendix with an interaction between FDI and the share of non-routine and interactive tasks. The estimated hazard ratios are averages over the five-year post-investment period. As Figure 2.A.1 in the Appendix shows, the share of non-routine and interactive tasks ranges between 40% and 100% in the data. The range of non-routine and interactive tasks in Figure 2.5 is restricted accordingly. Sources: ReLOC, IEB and BHP, own calculations.

The results for up- and downgrades in Panels B and C imply that the probability of switching positions within the firm increases with the share of non-routine and interactive tasks. In the following, we discuss several explanations for this pattern. Generally, switching occupations requires adaptations. The share of non-routine and interactive tasks presumably also reflects a worker's ability and willingness to adopt. Therefore, the likelihood of switching should be higher for workers with non-routine and interactive jobs.

Moreover, occupational upgrades requires further training and are therefore more expensive than downgrades, which merely require a reduction in tasks. Thus, to fill jobs with medium complexity, it is less expensive to downgrade workers with initially high shares of non-routine and interactive tasks than to upgrade workers with initially low shares. Our data reflect this argumentation. For instance, common downgrades in our data are locksmiths (very non-routine) to welders. Welders typically only carry out some of the locksmiths' tasks. This reduction in the complexity of tasks is a plausible reaction to the fragmentation of production processes where only some tasks of the locksmiths remain at the home firm, while others become obsolete.

Similarly, to fill jobs with high complexity it is cheaper to upgrade workers with initially high shares of non-routine and interactive tasks than to upgrade workers with lower shares. Furthermore, if FDI raises the demand for management and coordination, and only non-routine and interactive workers possess the abilities to take over such complex tasks, the likelihood additionally increases for these workers.

In summary, these arguments imply that firms have strong incentives to up- and downgrade workers in non-routine and interactive jobs. Moreover, our results reveal that firms adopt their workforce after FDI by relocating their most flexible individuals. Separations do not appear to be a popular adjustment channel.

2.4.3 Job stability and unobserved worker productivity

In this section, we shed further light on the mechanisms of separations, promotions and demotions by investigating whether unobserved worker productivity influences the likelihood of these events. To this end, we first obtain residual wages from Mincer-type wage estimates. We use standard controls from the labor literature, such as age, experience, tenure (and their squares), skill level as well as dummies for foreign nationality, two-digit occupations and year. We then rank all workers according to their estimated wage residual within the firm (in bins of 100). Technically, the wage residual captures positive or negative wage premiums that workers earn compared to workers with identical observable characteristics (e.g., same education, work experience, occupation). Ranking residual wages within firms additionally nullifies all time-invariant firm-specific effects on wages. Economically, the ranking of residual wages within the firm should reflect unobserved worker productivity. We expect that workers with high (low) unobserved productivity have better (lower) chances of upgrades and be less (more) likely to downgrade or leave the firm.

Table 2.2 presents estimates of our main specification extended with the workers' position in the wage ranking and an interaction of the ranking with the FDI indicator. Compared to our baseline estimates (Table 2.1), the sizes of the coefficients on FDI change somewhat. However, these changes are simply the result of the interaction of FDI and the wage ranking. For workers exactly in the middle of the ranking, the effects are identical to our baseline estimates (e.g, for upgrades, $1.3918 \times 0.9981^{50} = 1.2656 \approx 1.2422$). In particular, we find no effect of FDI on separations. At the median of the wage ranking, FDI increases the likelihood of up- and downgrades by 27% and 36% percent, respectively. Both effects are statistically significant.

Table 2.2: Effects of FDI on the hazard ratios of separations and up- and downgrades depending on unobserved worker productivity

	separation (1)	upgrade (2)	downgrade (3)
FDI	0.9477	1.3918**	1.2723
	(0.0492)	(0.1937)	(0.2167)
FDI × wage rank	1.0002	0.9981	1.0013
	(0.0006)	(0.0012)	(0.0018)
Wage rank	0.9962***	1.0093***	0.9914***
	(0.0005)	(0.0010)	(0.0014)
Subjects	376,411	376,411	376,411
Events	99,866	15,687	11,611

Notes: The table presents exponentiated coefficients (hazard ratios) and cluster robust standard errors at the firm level (in parentheses). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. "Wage rank" indicates the ranking of a worker's unobserved productivity within the firm. Additional control variables in all models are: age, age squared, experience, tenure, a foreign dummy, skill dummies, firm age and a dummy if firm existed in 1975 (all interacted with quarters since treatment), as well as occupation, year, and state dummies. Estimates are based on a matched sample of MNEs and domestic firms.
Source: ReLOC, IEB and BHP, own calculations.

The main coefficients on the wage ranking indicate that the job stability of workers indeed depends on their unobserved ability. These results are in line with what we would expect, i.e., more productive workers are less likely to be dismissed or demoted and more likely to receive occupational upgrades. Specifically, an increase in the residual wage ranking of one (on a scale between one and 100) reduces the hazard of separations by 0.38% and that of demotions by 0.86%. The likelihood of promotions increases by 0.93%. However, this effect does not differ between firms that invest abroad and domestic firms.

The insignificant interaction effects of FDI and wage ranking in all three models indicate that MNEs follow the same patterns as domestic firms when choosing whom to upgrade, downgrade or dismiss in terms of individual productivity. This result is not surprising. Although this paper finds that MNEs are more likely to restructure, restructuring is comparable to the dynamics in domestic firms. Workers with lower productivity always face higher risks of dismissals and downgrades independent of FDI.[11]

2.4.4 Discussion of the empirical findings

In the following, we discuss our findings and derive their main implications. Contrary to the widespread concern that MNEs substitute foreign for domestic labor, our main findings suggest no effect of FDI on average separation rates. However, further investigations with time-sensitive models and heterogeneous groups of workers reveal some exceptions. First, we find a brief lock-in effect that saves workers from separations immediately after their employers go multinational. Second, the positive effect on employment security is significant only for workers in highly non-routine and interactive occupations. These workers experience a 10% to 20% greater likelihood of remaining employed at the firm over the course of FDI.

Overall, the results on separations are in line with the literature, which generally finds no or very limited employment effects of FDI. For instance, Bachmann *et al.* (2014) find no significant evidence that industry-level FDI affects individual separation rates.[12] In line with our results, Becker & Muendler (2008) find lower separation rates in MNEs, particularly among high-skilled workers.[13] Empirical evidence on the

[11]In Appendix 2.A.5, we present additional results for different skill and age groups.

[12]In their paper, separation rates comprise both transitions to other firms and to non-employment. When Bachmann *et al.* (2014) exclusively consider transitions to non-employment, which is their main measure of employment security, they find that FDI—especially to CEEC—significantly increases workers' risk of non-employment.

[13]We also estimate occupational hazard ratios by skill levels; see Table 2.A.6 and Figure 2.A.2 in the Appendix. Our results support the findings by Becker & Muendler (2008) inasmuch as the positive lock-in effect of FDI exists among high-skilled workers. We additionally find an impact for medium-skilled workers.

employment effects of FDI by tasks is scarce. Thus, we compare our findings with the offshoring literature. Comparable to our results, Baumgarten (2015) finds no significant effect of offshoring on the hazard of non-employment on average. Moreover, he also shows that over the course of offshoring, workers in non-routine occupations experience a decrease in the hazard of non-employment.

Generally, our results are in line with the theoretical predictions by Grossman & Rossi-Hansberg (2008). They argue that the positive productivity effect of offshoring could outweigh the negative effects for workers with offshorable jobs. Thus, even if firms want to save labor costs and offshore parts of their production abroad, this does not necessarily lead to dismissals of domestic workers. Additionally, our results are in line with the predicted employment effects of market-seeking FDI. To serve the foreign market on-site, more complex coordination and management services are required at the headquarters, and there is thus no need for separations. We show that instead of separations, MNEs adjust their workforce internally through promotions and demotions. For the average worker, the likelihood of upgrading to a more-complex job increases by 24% due to FDI. The likelihood of downgrading to a less-complex job increases by 34%. Both effects become measurable with a time lag of two years after the investment. Explanations for the time lag of up- and downgrades include, e.g., time-intensive negotiations between firms and employees over occupational changes. Moreover, it might be necessary to re-train workers before they can fill new positions. Further, the positive impact of FDI on internal job transitions applies only to workers in occupations with at least moderate shares of non-routine and interactive tasks. Their likelihood of upgrading to more-complex jobs increases by between 30% and 60%. For the same group of workers, the probability of downgrading to less-complex jobs increases by between 30% and 80%. The likelihood of both types of switches does not increase for workers performing mostly routine and non-interactive tasks.

The greater opportunities to climb the career ladder through occupational upgrades in MNEs are in line with the theoretical expectations that MNEs require more administration and management tasks and with our hypothesis that these firms attempt to fill these vacant complex positions internally. Moreover, the increased risk of demotions through FDI is in line with our expectation that MNEs might avoid the costs of dismissals by demoting workers whose tasks become redundant over the course of FDI. Generally, the positive effect of FDI on firm-internal job switches speaks in favor of our hypothesis that internal labor markets are an important way in which MNEs can meet the changes in labor demand due to FDI.

The task-specific analyses show that the hazard of up- and downgrades is significant only for workers in jobs with medium-to-high initial shares of non-routineness and interactivity. As explained in Section 2.4.2, they have the opportunity to upgrade to new and more-complex positions because routine and non-interactive workers might not possess the prerequisites for these positions. However, highly non-routine and interactive workers also face an increased risk of downgrades. A possible explanation for this is that in the case of fragmentation, jobs at the middle of the complexity scale of tasks need to be filled, and it might be less expensive for MNEs to downgrade these workers than to upgrade workers with a low initial level of non-routine and interactive tasks.

There is no comparable study in the FDI literature that analyzes effects on job switches. Instead, we take up some results of the offshoring literature. However, the offshoring literature considers imports of intermediate inputs mostly at the industry level and does not specifically examine firm-internal transitions. Our results are in line with the positive effect of offshoring to CEEC on job-to-job transitions observed by Baumgarten (2015). Additionally, our results on job switches are, to some extent, comparable with studies on workforce composition. In line with our results Becker *et al.* (2013) and Nilsson Hakkala *et al.* (2014) find evidence for a shift in tasks in German and Swedish MNEs. In contrast to our results for FDI to the Czech Republic, Becker *et al.* (2013) do not find significant effects on the workforce composition for FDI to CEEC.

Overall, our results provide unique evidence that firms restructure their labor forces internally over the course of FDI. Some incumbent workers are promoted, while others are demoted. Although demotions are per se not a positive occupational change, they might be a more minor career disruption than dismissals. Further, the results suggest that although FDI opens career opportunities for some workers, it might also exert pressure to adapt and keep up for others. The perceived pressure to adapt might partly explain the fear of globalization in the public debate.

2.5 Robustness checks

In this section, we perform several robustness exercises. Specifically, we assess the competing risk assumption, employ alternative estimators and test further definitions of occupational up- and downgrades. The section concludes with a brief description of additional robustness checks.

2.5.1 Non-competing risks

In Section 2.2, we argue that separations and up- and downgrades follow distinct causal mechanisms. Therefore, we treat these events as competing risks and estimate separate models in which we remove workers from the risk set after any other event. As a robustness exercise, we now test an alternative specification for separations in which we retain individuals after job switches within the firm. Table 2.3 shows the results (Column 2) and repeats the estimates from our baseline specification (Column 1) for comparison. Both models yield the same results and obtain no effect of FDI on job separations. Thus, our conclusions from the main specification are not driven by the assumption of competing risks.

Moreover, we control for preceding up- and downgrades within the firm in Column 2 of the same table. Independent of FDI, a promotion reduces the hazard of a separation by 25%. This finding is in line with the expectation that only *good* workers receive promotions and are therefore less likely to be dismissed. The robustness exercise further indicates that past downgrades do not influence separations.

2.5.2 Alternative estimators

To ensure that our findings are independent of the chosen estimator, we further compute the effects of FDI on job stability with simple logit and multinomial logit models. To do so, we construct a cross-sectional dataset that assigns the first event $e \in \{\text{separation, upgrade, downgrade}\}$ within five years after the (pseudo) investment to individuals. Obviously, logit estimates ignore the chronological order of events. In the simple logit models, we estimate each event separately, as we also do in our baseline specification. In the multinomial logit model, we jointly esti-

Table 2.3: Effect of FDI on separations: competing vs. non-competing risks model

	baseline	no competing risks
	(1)	(2)
FDI	0.9630	0.9595
	(0.0440)	(0.0437)
Upgrade		0.7471***
		(0.0291)
Downgrade		0.9525
		(0.0504)
Subjects	383,098	383,098
Events	102,661	106,613

Notes: The table presents exponentiated coefficients (hazard ratios) and cluster robust standard errors at the firm level in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Additional control variables in all models are: age, age squared, experience, tenure, a foreign dummy, skill dummies, firm age and a dummy if a firm existed in 1975 (all interacted with quarters since treatment), as well as occupation, year, and state dummies. Estimates are based on a matched sample of MNEs and domestic firms.
Source: ReLOC, IEB and BHP, own calculations.

mate the likelihood of all events (against the baseline outcome *no event*). Table 2.4 summarizes the results.

Overall, the estimates of the separate logit models and the multinomial logit model are well in line with our main findings. The computed odds ratios are only marginally larger than in the proportional hazard models. We conclude that our results are independent of the chosen estimator. Nevertheless, we prefer hazard models because they allow us to explicitly model the time structure of the impact of FDI.

2.5.3 Alternative definitions of up- and downgrades

Throughout the paper, we interpret switches to occupations with higher (lower) shares of analytical and interactive tasks as upgrades (downgrades). We now corroborate the validity of this interpretation with a range of alternative definitions.

We begin with the possible concern that switches with only marginal changes in the complexity of tasks might not reflect real up- or downgrades. For instance, a switch from metalworker to mechanic increases the share of complex tasks by only five percentage points and thus might not be considered a significant promotion.

Table 2.4: Logit estimates of the effect of FDI on separations and up- and downgrades

	separate logit models by events			multinomial logit model (base category: no event)		
	separation	upgrade	downgrade	(4)		
	(1)	(2)	(3)	separation	upgrade	downgrade
FDI	0.9722	1.2607**	1.4248**	0.9868	1.2968**	1.3973**
	(0.0534)	(0.1236)	(0.2236)	0.0541	0.1375	0.2232
N	383,097	382,776	382,664		383,098	
Log lik.	-211949.4449	-61383.9606	-48363.6299		-316837.68	
Chi-squared	3624.3332	3388.7212	4093.1349		6064.85	

Notes: The table presents exponentiated coefficients (odds ratios) and cluster robust standard errors at the firm level (in parentheses). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Additional control variables in all models are: age, age squared, experience, tenure, a foreign dummy, skill dummies, firm age and a dummy if a firm existed in 1975, as well as occupation, year, and state dummies. The multinomial logit does only include one-digit occupational dummies. Estimates are based on a matched sample of MNEs and domestic firms.
Source: ReLOC, IEB and BHP, own calculations.

As a robustness exercise, we therefore define *significant* up- and downgrades as job switches with changes in complex tasks of at least ten percentage points. In Figure 2.2, these switches are in the bins to the left of -10% and in the bins to the right of +10%. The estimates for significant up- and downgrades in Panel B of Table 2.5 are comparable in sign and significance to our baseline results (Panel A). We conclude that our main findings are not biased by including job switches with only marginal changes in the complexity of tasks.

Next, we assess whether considering an alternative definition of the complexity of tasks alters our results. In our main specification, we measure the complexity of tasks as the share of analytical and interactive tasks. We now quantify the complexity of occupations by the share of all non-routine tasks. Accordingly, workers receive upgrades (downgrades) if the percentage of routine tasks decreases (increases). As the share of routine tasks is analogous to one minus the share of interactive, analytical and non-routine manual tasks, our alternative definition essentially extends our original definition of complexity along the manual dimension. Importantly, this definition also corresponds to the definition of offshorable tasks in the trade literature. As Panels A and C of Table 2.5 indicate, adding the manual dimension to our task measure does not affect the results.

Finally, inspired by Liu & Trefler (2011), we completely refrain from a task-based classification and identify occupational up- and downgrades based on wages. Therefore, we use a large, representative register sample of workers in Germany (Sample

Table 2.5: Effects of FDI on the hazard ratios of up- and downgrades (alternative definitions)

	upgrade		downgrade	
	(1)	(2)	(3)	(4)
Panel A: Baseline model (complex tasks):				
FDI	1.2422**	0.9960	1.3413**	0.9781
	(0.1315)	(0.1896)	(0.1980)	(0.2094)
FDI × quarter		1.0252*		1.0352*
		(0.0141)		(0.0205)
Subjects	383,098	383,098	383,098	383,098
Events	15,880	15,880	11,731	11,731
Panel B: Significant up- and downgrades with at least 10 percentage points changes:				
FDI	1.3448***	1.2031	1.4476**	1.0333
	(0.1231)	(0.1786)	(0.2489)	(0.2731)
FDI × quarter		1.0121		1.0365
		(0.0109)		(0.0246)
Subjects	383,098	383,098	383,098	383,098
Events	10,580	10,580	6,255	6,255
Panel C: All non-routine tasks:				
FDI	1.2501**	0.9847	1.3248*	0.9962
	(0.1267)	(0.1662)	(0.1903)	(0.2435)
FDI × quarter		1.0270**		1.0321
		(0.0135)		(0.0205)
Subjects	383,098	383,098	383,098	383,098
Events	14,017	14,017	13,594	13,594
Panel D: Median wages:				
FDI	1.2533**	1.1140	1.3137*	0.8730
	(0.1305)	(0.2039)	(0.1898)	(0.1905)
FDI × quarter		1.0132		1.0467***
		(0.0154)		(0.0180)
Subjects	383,098	383,098	383,098	383,098
Events	14,006	14,006	13,605	13,605

Notes: The table presents exponentiated coefficients (hazard ratios) and cluster robust standard errors at the firm level (in parentheses). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Panel A repeats our main findings, where upgrades (downgrades) are defined as increases (decreases) in non-routine and analytical tasks. Panel B classifies upgrades (downgrades) as job switches with at least a ten percentage points increase (decrease) in analytical and interactive tasks. Panel C identifies upgrades (downgrades) as job switches with increases (decreases) in analytical, non-routine manual and interactive tasks. Panel C specifies job switches as upgrades (downgrades) if the occupational median wage increases (decreases) with the job switch. Control variables in all models are: age, age squared, experience, tenure, a foreign dummy, skill dummies, firm age and a dummy if the firm existed in 1975 (all interacted with quarters since treatment), as well as occupation, year, and state dummies. Estimates are based on a matched sample of MNEs and domestic firms.
Source: ReLOC, IEB and BHP, own calculations.

of Integrated Labour Market Biographies, SIAB) and compute yearly median wages in two-digit occupations. To remove noise, we further fit a quadratic time trend to the data. The result is an occupational panel with smooth median wages over the time frame of our analysis. We link the occupational panel to our main dataset and re-define upgrades (downgrades) as job switches within the firm to occupations with higher (lower) median wages. Panel D of Table 2.5 summarizes the according estimates. Both our task-based definition from the baseline model and the alternative wage-based definition of job switches lead to similar results. Overall, our main finding that FDI leads to notably more up- and downgrades within the firm, holds independent of the exact definition of up- and downgrades.[14]

2.5.4 Additional robustness checks

In an additional robustness check, we test whether our main findings are driven by small firms. To ensure that the investment decision is independent of the individual worker, we exclude small firms with fewer than 50 employees in Panel B of Table 2.A.8 in the Appendix. The results point in the same direction, and deviations from our main specification are minor (see Panel A of the same table). We conclude that small firms do not drive our results.

While for workers in MNEs, the onset of the risk of job changes naturally begins with FDI, there is no such inherent start date for domestic firms. For this and other reasons, we match domestic firms to MNEs and assign the investment quarter of the MNE to its domestic counterpart. To determine whether this assignment influences our findings, we now randomly change the pseudo investment date of domestic firms. In particular, we randomly draw pseudo investment quarters from a uniform distribution ranging from four quarters before to four quarters after the initial assignment. We do not alter the investment dates of MNEs. As Table 2.A.8 in the Appendix shows, this robustness exercise does not affect the results on separations. In the static model, the effects on job switches are also stable and even larger for upgrades. However, the dynamic effects on up- and downgrades are insignificant. If the likelihood of job switches within the firm follows time-dependent trajectories, it is substantial for a dynamic analysis to compare temporal twins of MNEs and domestic firms and not just time-averaged twins. Shuffling pseudo investment dates breaches such a prerequisite and therefore potentially leads to insignificant estimates.

To identify the causal effects of FDI on job stability, we restrict our sample to workers who were already employed two years prior to the (pseudo) investment. This restriction ensures that individuals do not self-select into future MNEs. However, it

[14]Figure 2.A.4 in the Appendix present the unconditional hazard rates for the alternative definitions of up- and downgrades.

also removes 20% of workers from our sample, to whom our findings might not be applicable. To test the generalizability of our findings to workers with less than two years' tenure, we discard this restriction and re-estimate our models. The resulting estimates are almost identical to our main findings (see Table 2.A.8 in the Appendix). Although the unrestricted estimates should not be interpreted causally, they suggest that our findings also apply to workers with less than two years' tenure.

2.6 Conclusion

The objective of this paper is to analyze how FDI affects the job stability of workers. In an extension of the results in the previous literature, we suggest that firms use internal reorganizations of their workforce as an important adjustment channel to the changes in labor demand induced over the course of FDI. Particularly, we consider occupational up- and downgrades of workers to more- or less-complex jobs, respectively. Especially in labor markets with strong labor protection laws and rigid wages, internal labor markets offer investing firms the opportunity to adjust their incumbent workforce to changes in labor demand. Internal restructuring circumvents the costs of hires and dismissals and information asymmetries and retains firm-specific human capital. To identify occupational switches within and out of the firm, we use employer-employee data on German firms that invest in the Czech Republic and those on comparable domestic firms.

Our results show that workers in MNEs have a significantly greater likelihood of upgrading to more-complex jobs over the course of FDI. However, the risk of downgrading to less-complex occupations also increases. The probability of up- and downgrades grows with the workers' share of non-routine and interactive tasks in their job before FDI. Both effects become significant two years after investment. Further, we show that FDI has no impact on separations of workers and firms on average. At most, we find a temporal lock-in effect of FDI shortly after investment.

In summary, our results imply that MNEs use internal restructuring rather than dismissals as an important adjustment channel to meet labor demands that change over the course of FDI. Our findings therefore rebut the common fear that foreign labor substitutes for domestic labor in MNEs. However, workers in investing firms need to be more flexible and willing to take on new tasks. As further training is indispensable for successful occupational transitions, this paper underpins the importance of lifelong learning.

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2.A Appendix

2.A.1 Iterative matching algorithm

Algorithm 1: Iterative matching

input : List with three potentially best matches: $\mathbf{P}$.
output: List with single best matches: $\mathbf{M}$

define : Match of treatment firm t and control firm c: m_{tc}
Distance of logit propensity scores of m_{tc}:
$\Delta_{tc} = |logit(\mathrm{PS}_t) - logit(\mathrm{PS}_c)|$

repeat $3\times$
  for *each treatment firm t*
    find best match $\tilde{m}_{t\cdot} \in \mathbf{P}_t$ with smallest $\Delta_{t\cdot}$
    add match $\tilde{m}_{t\cdot}$ to $\mathbf{M}$
  for *each control firm c*
    find best match $\tilde{m}_{\cdot c} \in \mathbf{M}$ with smallest $\Delta_{\cdot c}$
    drop other matches $m_{\cdot c} \neq \tilde{m}_{\cdot c}$ from $\mathbf{M}$
  for *each treatment firm t*
    if *match* $\tilde{m}_{t\cdot} \notin \boldsymbol{M}$
      drop match $\tilde{m}_{t\cdot}$ from $\mathbf{P}_t$
drop matches $m_{\cdot\cdot}$ with $\Delta_{\cdot\cdot} < [0.2 \times sd(logit(\mathrm{PS}))]$ from $\mathbf{M}$

2.A.2 Matching results

Table 2.A.1 illustrates the distribution of firm characteristics of (future) MNEs and domestic firms in our raw data. Notably, the sizes and average wages of MNEs are considerably larger and show higher variability.

To create a homogeneous dataset of MNEs and domestic firms with equal probabilities of investing, we propose an iterative matching procedure. We match firms between 1990 and 2010. Firms with just one employee in the year of treatment are excluded. Further, we restrict our sample to MNEs smaller than 30,000 employees because the largest control firm has only 23,000 workers. We also drop firms in the public sector as well as private households and extra-territorial organizations.

First, we estimate propensity scores based on the following variables: log number of employees, average age and wage of the workers, the share of female, regular,

Table 2.A.1: Firm characteristics (unmatched sample)

	MNEs two years prior FDI			MNEs two years after FDI			domestic firms		
	obs.	mean	std.	obs.	mean	std.	obs.	mean	std.
No. of employees	1,996	383.8868	1133.8480	2,164	382.5873	1113.0740	7,767	185.5134	420.8820
Employment growth rate	1,992	0.3699	2.4167	1,870	0.3816	5.0398	7,767	0.6443	4.2290
Firm age	1,996	15.2169	8.5461	2,164	17.4205	9.5959	7,767	16.0055	8.8191
Av. wage of emp.	1,996	88.8361	38.0267	2,164	98.4771	43.4784	7,767	82.9027	32.1573
Wage growth rate	1,992	0.0717	0.1748	1,867	0.0736	0.1796	7,767	0.0584	0.1252
Av. age of emp.	1,996	38.3412	4.9081	2,164	39.4725	4.8128	7,767	39.3095	4.4908
Share of female emp.	1,996	0.3539	0.2322	2,164	0.3559	0.2239	7,767	0.3828	0.2589
Share of trainees	1,996	0.0341	0.0507	2,164	0.0362	0.0595	7,767	0.0445	0.0620
Share of regular emp.	1,996	0.9141	0.1255	2,164	0.8912	0.1381	7,767	0.8496	0.1537
Share of full-time emp.	1,996	0.8609	0.1509	2,164	0.8367	0.1639	7,767	0.7707	0.2082
Share of low-sk. emp.	1,996	0.1486	0.1396	2,164	0.1358	0.1257	7,767	0.1519	0.1280
Share of med.-sk. emp.	1,996	0.7065	0.1922	2,164	0.7007	0.1897	7,767	0.7289	0.1720
Share of high-sk. emp.	1,996	0.1304	0.1794	2,164	0.1499	0.1839	7,767	0.1019	0.1447
Share of German emp.	1,996	0.9160	0.1101	2,164	0.9188	0.1076	7,767	0.9258	0.1077
Share of unsk.-man. emp.	1,996	0.2197	0.2585	2,164	0.1986	0.2429	7,767	0.1786	0.2399
Share of engineers etc.	1,996	0.0303	0.0800	2,164	0.0311	0.0758	7,767	0.0226	0.0671

Notes: The table compares the number of firms, the means and standard deviations of various characteristics of investing and domestic firms in the raw data before matching. For MNEs we report the values two years prior to investment and two years after the investment. For the control group of domestic firms we show averages over all years they are in the data.
Source: ReLOC and BHP, own calculations.

German, unskilled-manual, full-time, low-, medium- and high-skilled employees, the share of trainees, the share of engineers and scientists, wage and employment growth rates over the last two years, firm age, a dummy for whether the firm existed before 1975 and federal state, year and industry dummies. These variables either directly affect the firms' probability of investing (e.g., firm age) or are a good proxy for variables that have a direct impact on the firms' decision to conduct FDI (e.g. firm-size for productivity.) All variables are measured two years prior to investment to avoid adjustments to FDI already having been made. If a firm did not exist two years before, we do not receive a growth rate of wage and employment. Growth rates in these firms are imputed with the average growth rate for the year in question. We include a dummy to tag these observations in the logit model.

We match every MNE two years before investment to its three nearest neighbors according to the estimated propensity score among the control firms exactly in the same year.

To obtain an unambiguous start date for domestic firms, we need to ensure that every control firm is only matched once to a treatment firm (see Section 2.2). Therefore, we propose an iterative matching procedure (see Algorithm 1) to identify the single best pairs of MNEs and domestic firms over the entire observation period. To ensure

Table 2.A.2: Balancing test results after matching

	standardized mean differences			variance ratios		
	raw	1st match	2nd match	raw	1st match	2nd match
Log no. employees	0.0126	-0.0129	-0.0055	1.4268	1.3381	1.3067
Av. wage	0.2569	0.0714	0.0872	1.6021	1.1804	1.1692
Firm age	-0.1861	-0.0238	0.0233	0.9263	0.9375	0.9036
Av. age	-0.0674	-0.0229	-0.0023	0.9634	1.0325	1.0416
Share female emp.	-0.0690	0.0133	0.0384	0.7704	0.8431	0.8521
Share trainees	-0.1850	0.0216	0.0273	0.5424	1.0195	1.0923
Share regular emp.	0.2035	0.0043	0.0330	0.6570	0.9031	0.8697
Share full-time emp.	0.2973	0.0047	0.0217	0.5262	0.8325	0.7948
Share low-skilled emp.	-0.1044	-0.0480	-0.0356	0.8927	0.9241	0.9718
Share medium-skilled emp.	-0.1477	-0.0397	-0.0362	1.1235	1.0230	0.9784
Share high-skilled emp.	0.2666	0.0859	0.0690	1.6013	1.1090	1.0281
Share german emp.	-0.0464	0.0229	0.0141	0.8735	0.8854	0.9236
Share unskilled-manual emp.	0.1155	-0.0677	-0.0518	1.0628	0.9187	0.9261
Share engineers etc.	0.1206	0.0130	0.0154	1.3399	0.9416	0.9979
Employment growth	-0.0201	-0.0204	-0.0348	0.0491	0.2411	0.1758
Av. wage growth	0.0348	0.0159	-0.0025	0.2527	1.1729	0.8502

Notes: The table compares the standardized mean differences and variance ratios of the variables used for matching. "Raw" represents the standardized mean differences and variance ratios before matching. "1st match" give the results for the first part of our matching procedure two years prior to investment with three-nearest neighbor propensity score matching exactly by year. "2nd match" presents the results after applying our iterative matching algorithm (1). The cells with the best balance statistic are highlighted, i.e., figures closest to zero in case of the standardized mean differences and figures closest to one for variance ratio.
Source: ReLOC and BHP, own calculations.

that nearest neighbors are not too far away, we calculate the optimal caliper width as recommended by Austin (2011b).[15]

Table 2.A.2 presents the balancing test results of our matching approach. We calculate the standardized differences and variance ratios of our resulting sample according to Austin (2011a). Standard propensity score matching (1st Match) and Algorithm 1 (2nd Match) reduce the standardized differences of almost all variables (expect for the log number of employees and employment growth) and lead to a variance ratios closer to one. The results indicate that matching substantially improves the balancing of firm characteristics.

Further, Table 2.A.3 shows that the distribution of firms across industries is also remarkably similar after matching. The matched dataset consists of 1,876 matched treatment and control pairs.

[15] We use a logit of the estimated propensity score for matching. Here, we follow Austin (2011b), who recommend setting the optimal caliper width to 0.2 of the standard deviation of the logit of the propensity score.

Table 2.A.3: Balance of industries after matching

Industry	No. domestic f.	No. MNEs	Total
Manuf. food products, beverages and tobacco	21	28	49
Manuf. textiles and textile products	35	34	69
Manuf. pulp, paper and paper products; publishing and printing	30	38	68
Manuf. chemicals, chemical products and man-made fibres	48	44	92
Manuf. rubber and plastic products	79	66	145
Manuf. other non-metallic mineral products	36	32	68
Manuf. basic metals and fabricated metal products	147	142	289
Manuf. machinery and equipment n.e.c.	130	130	260
Manuf. electrical and optical equipment	129	147	276
Manuf. transport equipment	30	25	55
Manuf. n.e.c.	21	25	46
Construction	79	72	151
Wholesale/retail; repair of motor vehicles/household goods etc.	262	247	509
Transport, storage and communication	95	83	178
Real estate, renting and business activities	130	150	280
Total	1,344	1,340	2,684

Notes: The table presents the balance of firms over industries after applying our iterative matching algorithm. For reasons of data protection the table only includes industries with more than 20 firms.
Source: ReLOC and BHP, own calculations.

2.A.3 Main results of Cox regression

Table 2.A.4: Effects of FDI on the hazard ratios of separations and up- and downgrades (full table)

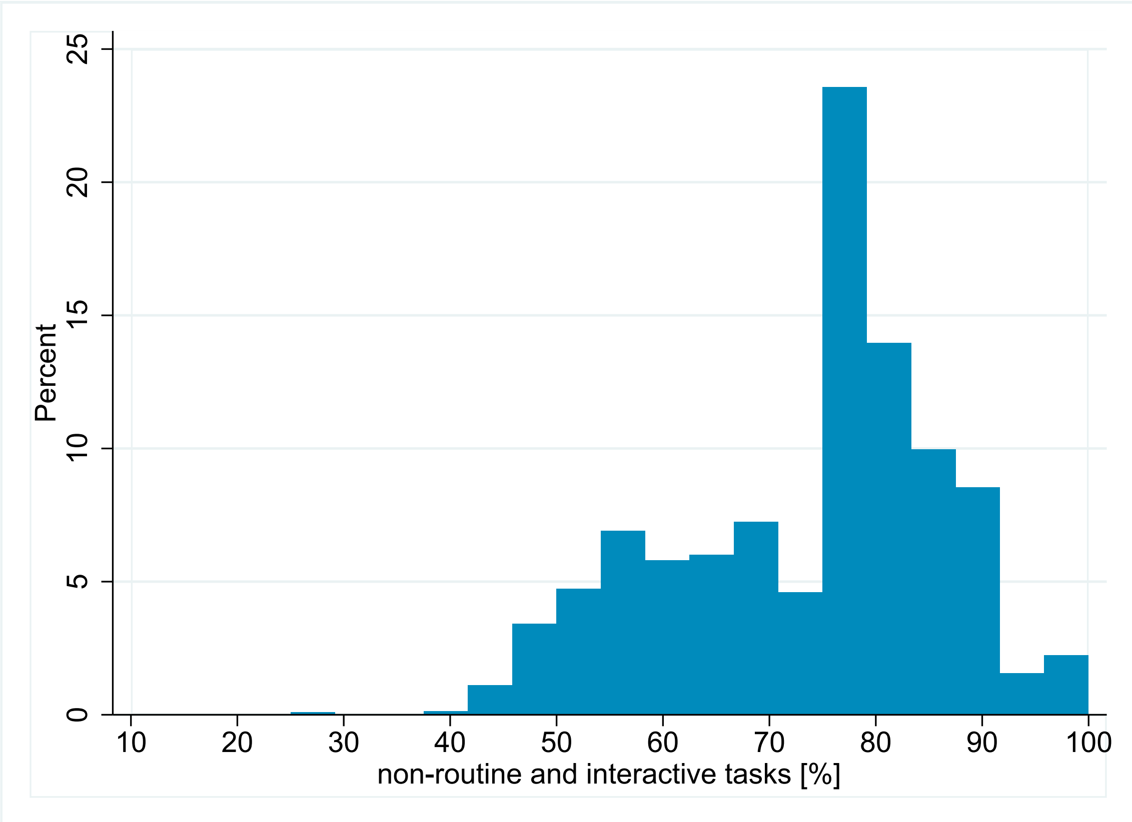
	separation		upgrade		downgrade	
FDI	0.9630	0.8092**	1.2422**	0.9960	1.3413**	0.9781
	(0.0440)	(0.0691)	(0.1315)	(0.1896)	(0.1980)	(0.2094)
FDI × quarter		1.0190**		1.0252*		1.0352*
		(0.0083)		(0.0141)		(0.0205)
Age	0.7875***	0.7875***	1.0070	1.0068	0.9761	0.9762
	(0.0156)	(0.0155)	(0.0333)	(0.0335)	(0.0543)	(0.0552)
Age squared	1.0030***	1.0030***	0.9997	0.9997	1.0003	1.0003
	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0006)	(0.0006)
Experience	0.9999***	0.9999***	1.0000	1.0000	1.0000	1.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0001)
Tenure	0.9998***	0.9998***	1.0000	1.0000	0.9999***	0.9999***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Foreign	1.2165***	1.2170***	0.8614	0.8646	1.2759	1.2687
	(0.0915)	(0.0892)	(0.1359)	(0.1351)	(0.2026)	(0.1976)
Medium skilled	0.9779	0.9788	1.3636***	1.3647***	0.9639	0.9646
	(0.0460)	(0.0458)	(0.1141)	(0.1154)	(0.1086)	(0.1105)
High skilled	1.2300***	1.2449***	2.9824***	3.0368***	0.5418**	0.5494**
	(0.0851)	(0.0871)	(0.6202)	(0.6263)	(0.1373)	(0.1405)
Firm age	1.0201*	1.0206**	1.0623**	1.0629**	1.0388	1.0398
	(0.0106)	(0.0105)	(0.0273)	(0.0271)	(0.0394)	(0.0390)
Dummy firm < 1975	0.7874	0.8074	0.5501**	0.5779*	0.5013*	0.5394
	(0.1825)	(0.1854)	(0.1559)	(0.1624)	(0.1932)	(0.2087)
Interaction with quarters since treatment:						
Age	1.0050***	1.0050***	0.9920**	0.9920**	0.9970	0.9970
	(0.0011)	(0.0011)	(0.0033)	(0.0033)	(0.0053)	(0.0054)
Age squared	0.9999***	0.9999***	1.0001**	1.0001**	1.0000	1.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0001)
Experience	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Tenure	1.0000***	1.0000***	1.0000	1.0000	1.0000**	1.0000**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Foreign	1.0013	1.0013	0.9921	0.9917	1.0118	1.0126
	(0.0079)	(0.0077)	(0.0136)	(0.0134)	(0.0137)	(0.0132)
Medium skilled	1.0007	1.0006	0.9889	0.9888	0.9610**	0.9609**
	(0.0048)	(0.0048)	(0.0073)	(0.0072)	(0.0171)	(0.0170)
High skilled	0.9884*	0.9871*	0.9772	0.9752	0.9531	0.9517
	(0.0067)	(0.0069)	(0.0203)	(0.0203)	(0.0310)	(0.0311)
Firm age	1.0002	1.0001	1.0011	1.0010	1.0027	1.0027
	(0.0010)	(0.0010)	(0.0023)	(0.0022)	(0.0035)	(0.0035)
Dummy firm < 1975	0.9846	0.9819	0.9915	0.9859	1.0071	0.9992
	(0.0225)	(0.0224)	(0.0225)	(0.0230)	(0.0354)	(0.0359)
Subjects	383,098	383,098	383,098	383,098	383,098	383,098
Events	102,661	102,661	15,880	15,880	11,731	11,731

Notes: The table presents exponentiated coefficients (hazard ratios) and cluster robust standard errors at the firm level in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The models further include occupation, year, and state dummies. The deviation of the estimated hazard ratios from one can be interpreted as changes in the probabilities of the events attributable to FDI. For example, an estimated hazard ratio for separation of 0.8092 indicates that FDI reduces the individual risk of separation by 19.08% in the quarter of investment. Estimates are based on a matched sample of MNEs and domestic firms. Source: ReLOC, IEB and BHP, own calculations.

2.A.4 Additional material on job stability and tasks

Distribution of non-routine and interactive tasks

Figure 2.A.1: Histogram of non-routine and interactive tasks



Notes: The histogram shows the share of non-routine and interactive tasks that workers perform in our data (in the quarter of the (pseudo) investment). The actual range of non-routine and interactive tasks is between 40% and 100%. Only 0.1% of workers are in occupations with less than 40% non-routine and interactive tasks.
Source: ReLOC, IEB and BHP, own calculations.

Job stability by initial share of non-routine and interactive tasks

Table 2.A.5: Effects of FDI on the hazard ratios of separations and up- and downgrades depending on the share of non-routine and interactive tasks

	separations		upgrades		downgrades	
	(1)	(2)	(3)	(4)	(5)	(6)
FDI	1.5853***	1.9558	0.6514	0.6542	0.4373	0.2429
	(0.2825)	(1.4482)	(0.2155)	(0.7855)	(0.2399)	(0.6932)
Non-routine & interactive	1.0100***	0.9997	0.9805***	0.9511*	1.0078	1.0414
	(0.0017)	(0.0143)	(0.0027)	(0.0284)	(0.0051)	(0.0565)
FDI × non-routine	0.9934***	0.9872	1.0093**	1.0096	1.0147**	1.0308
& interactive	(0.0023)	(0.0206)	(0.0044)	(0.0375)	(0.0073)	(0.0792)
FDI × (non-routine		1.0000		1.0000		0.9999
& interactive)squared		(0.0001)		(0.0003)		(0.0005)
Subjects	383,009	383,009	383,009	383,009	383,009	383,009
Events	102,626	102,626	15,862	15,862	11,730	11,730

Notes: The table presents exponentiated coefficients (hazard ratios) and cluster robust standard errors at the firm level in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Additional control variables in all models are: age, age squared, experience, tenure, foreign dummy, firm age and a dummy if firm existed in 1975 (all interacted with quarters since treatment), as well as year and state dummies. Estimates are based on a matched sample of MNEs and domestic firms.
Source: ReLOC, IEB and BHP, own calculations.

2.A.5 Additional results

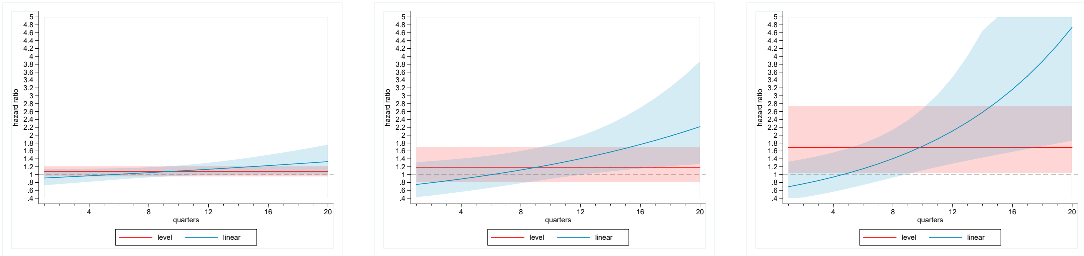
Job stability by skill level

Figure 2.A.2 shows the effect of FDI on job stability by skill level. We distinguish between low-skilled workers, without any occupational degree, medium-skilled workers, with an occupational degree, and high-skilled workers, who hold a university degree. The graphs show the estimated hazard ratios and 95% confidence bands for the Cox models presented in Table 2.A.6.

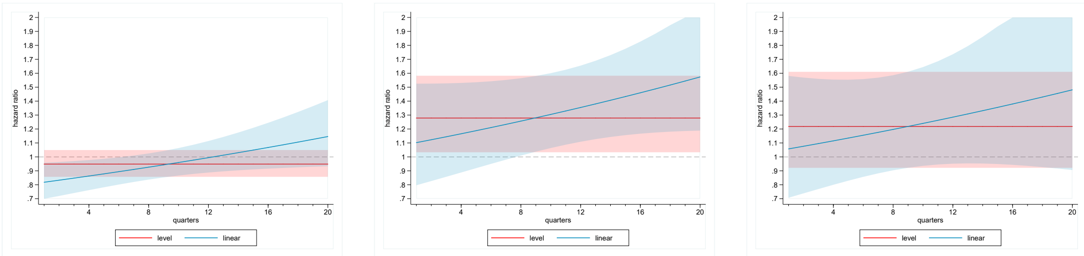
None of the average separation rates of the skill groups is significantly affected by FDI (red lines). However, the time-flexible models (blue curves) show that low-skilled workers in MNEs face a small but significantly higher risk of separation that sets in with some delay. This result is in line with theoretical considerations (e.g., Feenstra & Hanson, 1996) that predict that to save labor costs, FDI in low-wage countries is particularly harmful to low-skilled workers. By contrast, medium- and high-skilled workers in MNEs face significantly lower risk of losing employment immediately after the investment than comparable workers in domestic firms. This

Figure 2.A.2: Dynamic effects of FDI on the hazard ratios of separations and up- and downgrades by skill groups

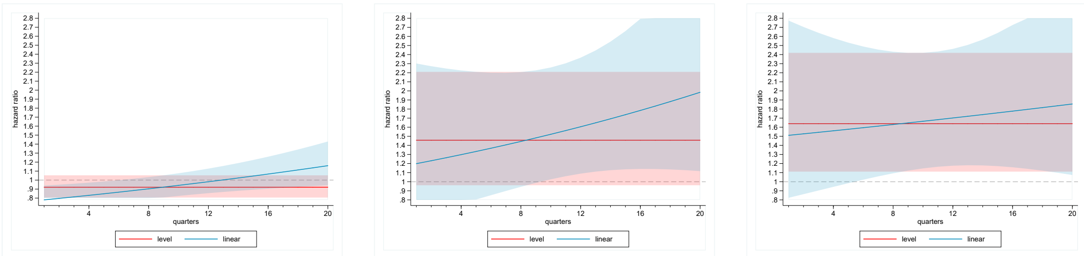
separations upgrades downgrades



PANEL A: Low-skilled workers



PANEL B: Medium-skilled workers



PANEL C: High-skilled workers

Notes: The figures provide a graphical representation of the hazard ratios and 95% confidence intervals of the estimated effects of FDI on separations and up- and downgrades by skill groups. The regression results are shown in Table 2.A.6 in the Appendix. The red lines display the level effects of FDI, i.e., the average effect over the five years after investment. The blue lines show the interaction effects of FDI and time, i.e., quarters.
Source: ReLOC, IEB and BHP, own calculations.

outcome is in line with the expectation that MNEs require more communication, management and organizational tasks, which are typically possessed by workers with at least a vocational degree. These findings are also in line with earlier studies, e.g., Görg & Görlich (2015), who find that offshoring increases the risk of unemployment for low-skilled workers and reduces the risk for high- and medium-skilled workers.

Investigating job changes within the firm, we find a significant level effect of FDI on job upgrades only for medium-skilled workers. Their likelihood of experiencing

Table 2.A.6: Effects of FDI on the hazard ratios of separations and up- and downgrades by skill groups

	separations			upgrades			downgrades		
	low	medium	high	low	medium	high	low	medium	high
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Level estimates:									
FDI	1.0742	0.9484	0.9210	1.1738	1.2786**	1.4573*	1.6877**	1.2183	1.6393**
	(0.0652)	(0.0489)	(0.0629)	(0.2236)	(0.1389)	(0.3093)	(0.4155)	(0.1733)	(0.3254)
Panel B: Time-variant estimates:									
FDI	0.8960	0.8039**	0.7639***	0.7075	1.0818	1.1674	0.6255	1.0388	1.4946
	(0.1124)	(0.0698)	(0.0765)	(0.2171)	(0.1897)	(0.4129)	(0.2256)	(0.2275)	(0.4922)
FDI × quarter	1.0199	1.0179**	1.0211***	1.0588**	1.0189	1.0269	1.1065***	1.0179	1.0109
	(0.0122)	(0.0082)	(0.0079)	(0.0254)	(0.0124)	(0.0262)	(0.0397)	(0.0190)	(0.0241)
Subjects	52,591	282,017	48,490	52,591	282,017	48,490	52,591	282,017	48,490
Events	14,461	73,104	15,096	2,353	12,348	1,179	2,120	8,476	1,135

Notes: The table presents exponentiated coefficients (hazard ratios) and cluster robust standard errors at the firm level in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Additional control variables in all models are: age, age squared, experience, tenure, foreign dummy, firm age and a dummy if firm existed in 1975 (all interacted with quarters since treatment), as well as year and state dummies. Estimates are based on a matched sample of MNEs and domestic firms and separably shown by the skill-level of workers. "Low" classifies all workers without an occupational degree, "medium" all workers with a vocational degree and "high" all workers with a degree from an university or an university of applied sciences.
Source: ReLOC, IEB and BHP, own calculations.

an upgrade increases by 28% following FDI. However, time-flexible estimates show that after some delay, the likelihood for low- and high-skilled workers to upgrade to more-complex jobs increases due to FDI. Overall, five years after investment, all skill groups have 20% to 30% greater likelihoods of occupational upgrades than workers in domestic firms.

For low- and high-skilled workers, we also find a significant effect on the risk of downgrades through FDI. On average, low-skilled workers in MNEs face a 69% higher risk of switching to a less-complex job than workers in domestic firms. This large effect might explain why we do not observe a significant effect on average separation rates for this group and only find a slight increase in their separation rates some years after investment. Although they might perform labor-intensive tasks that can be offshored easily, low-skilled employees without any occupational degree are rather inexpensive. Thus, instead of dismissing them, the investing companies may retain the most productive low-skilled employees and assign them new tasks. Following such a strategy would allow MNEs to preserve their firm-specific human capital.

Job stability by age

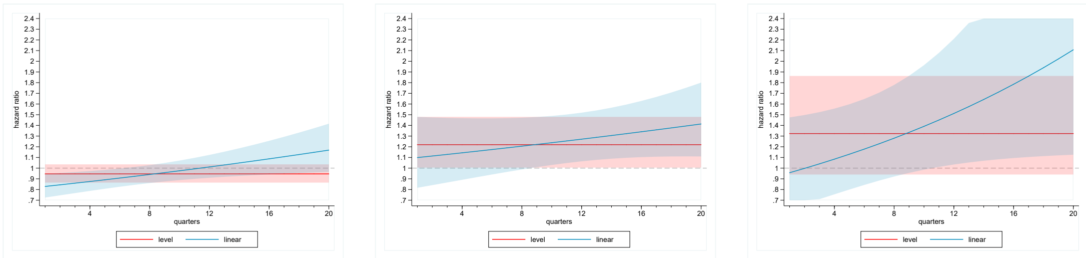
In our paper, we have shown that the effects of FDI depend on the task composition of employees. We might also expect the results to vary by worker age. Medoff & Abraham (1981), e.g., find that younger workers are more likely to receive a promotion than older workers. In this section, we therefore separate the sample into three age groups. We categorize workers as "young" if they are younger than 33, "medium" if they are between 33 and 45, and "older" if they are older than 45. We expect young workers in particular to have less firm-specific human capital given their lower seniority. They might not be as valuable for the firm as older workers and might face a higher risk of being fired.[16] Moreover, we expect that young workers might be particularly likely to be promoted after their firm goes multinational because firms might invest in further training these workers to benefit from their new knowledge for the longest possible time.

Figure 2.A.3 provides the results that support the hypotheses discussed above. On average, we find no effect of FDI on the probability of separations for all age groups. However, young and middle-aged workers in MNEs have a significantly lower risk of losing employment in the year of investment (19% and 22%; see also Table 2.A.7). Thus, we refute our hypothesis that younger workers, who likely have less firm-specific human capital, are among those dismissed over the course of FDI. The risk of separation due to FDI does not seem to depend on workers' age. Across all age groups, we find a significant positive effect of FDI on the likelihood of experiencing a job upgrade that appears with a delay of approximately 2.5 years after investment. This result contradicts our consideration that firms might be particularly likely to offer promotions to younger employees. On the contrary, the last column of Figure 2.A.3 shows that younger and middle-aged workers face a higher risk of demotion. Older workers, however, are not affected. Overall, FDI primarily affects the job stability of young and middle-age workers. They face a lower risk of separation from the firm, but at the same time, their risk of demotion is higher in MNEs after investment. Moreover, we find that independent of age, all workers seem to have a higher chance of promotion due to FDI.

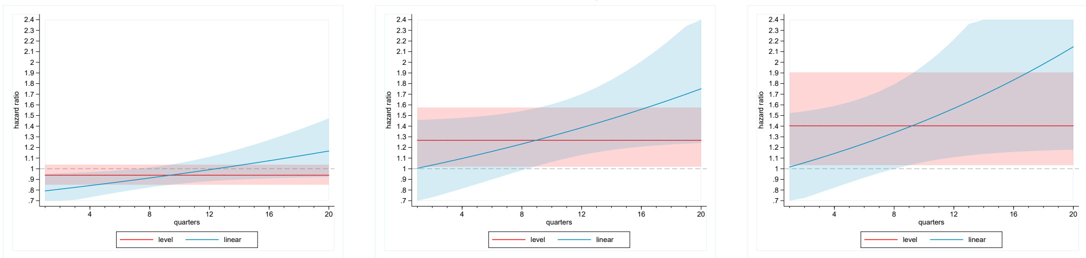
[16] All workers in the sample have seniority of at least two years with the firm because one prerequisite of our estimation is that all workers already worked for the firm two years prior to investment. Thus, the estimates for separations cannot depend on lower employment protection rules during a probation period. Panel D in Table 2.A.8 includes a robustness check without the tenure restriction. The results are highly comparable with our main findings in Table 2.1.

Figure 2.A.3: Dynamic effects of FDI on the hazard ratios of separations and up- and downgrades by age groups

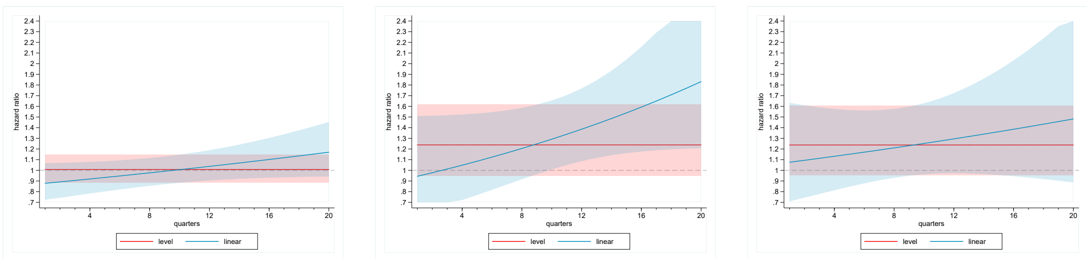
separations	upgrades	downgrades



PANEL A: Younger than 33



PANEL B: Between 33 and 45



PANEL C: Older than 45

Notes: The figures provide a graphical representation of the hazard ratios and 95% confidence intervals of the estimated effects of FDI on separations and up- and downgrades by age groups. The regression results are shown in Table 2.A.7 in the Appendix. The red lines display the level effects of FDI, i.e., the average effect over the five years after investment. The blue lines show the interaction effects of FDI and time, i.e., quarters.
Source: ReLOC, IEB and BHP, own calculations.

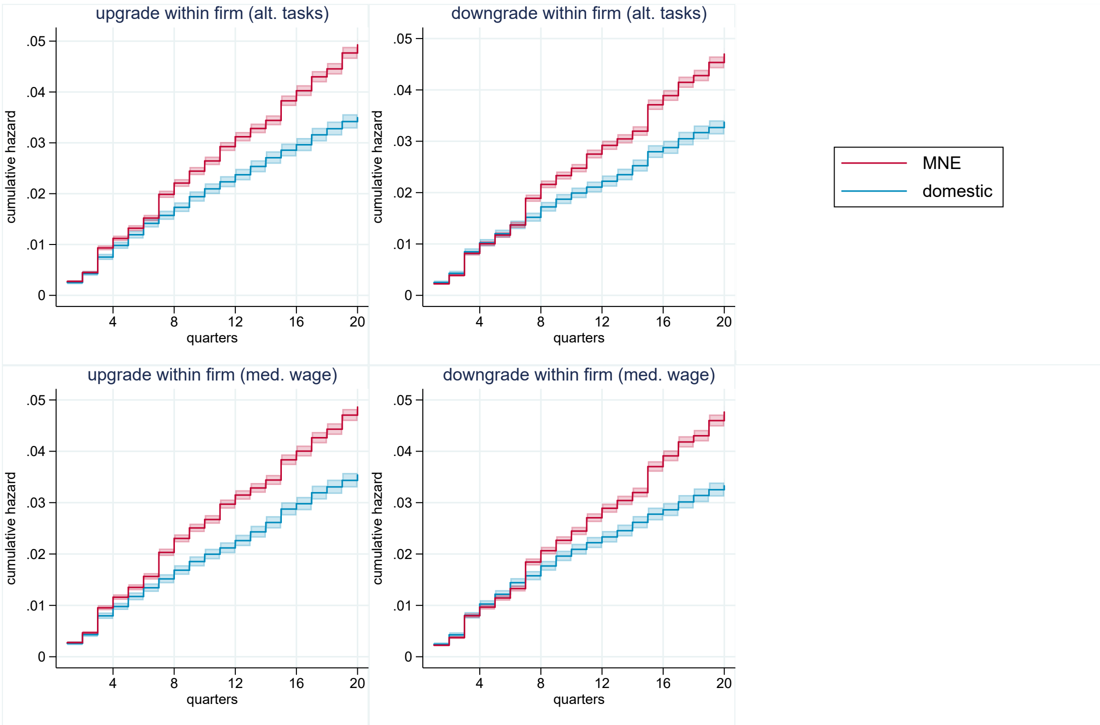
Table 2.A.7: Effects of FDI on the hazard ratios of separations and up- and downgrades by age groups

	separations			upgrades			downgrades		
	< 33	$33-45$	> 45	< 33	$33-45$	> 45	< 33	$33-45$	> 45
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Level estimates:									
FDI	0.9461	0.9392	1.0075	1.2194**	1.2669**	1.2385	1.3234	1.4030**	1.2376
	(0.0435)	(0.0482)	(0.0673)	(0.1204)	(0.1410)	(0.1697)	(0.2308)	(0.2191)	(0.1651)
Panel B: Time-variant estimates:									
FDI	0.8138***	0.7758**	0.8652	1.0844	0.9755	0.9114	0.9181	0.9768	1.0577
	(0.0601)	(0.0797)	(0.0915)	(0.1741)	(0.1985)	(0.2341)	(0.2162)	(0.2166)	(0.2426)
FDI × quarter	1.0183**	1.0206**	1.0152*	1.0133	1.0297*	1.0355*	1.0424*	1.0401*	1.0170
	(0.0074)	(0.0100)	(0.0087)	(0.0110)	(0.0163)	(0.0206)	(0.0228)	(0.0224)	(0.0210)
Subjects	96,579	181,562	104,957	96,579	181,562	104,957	96,579	181,562	104,957
Events	30,530	42,924	29,207	5,460	7,103	3,317	3,309	5,329	3,093

Notes: The table presents exponentiated coefficients (hazard ratios) and cluster robust standard errors at the firm level in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Additional control variables in all models are: age, age squared, experience, tenure, foreign dummy, skill dummies, firm age and a dummy if firm existed in 1975 (all interacted with quarters since treatment), as well as occupation, year, and state dummies. Estimates are based on a matched sample of MNEs and domestic firms and separably shown by the age of workers.
Source: ReLOC, IEB and BHP, own calculations.

2.A.6 Alternative definitions of job up- and downgrades

Figure 2.A.4: Cumulative hazards for alternative definitions of up- and downgrades



Notes: The figure shows the cumulative hazards for the two events *internal up- and downgrades* by quarters after (pseudo) investment and by investing (MNE) and domestic firms. The graphs depict the cumulative hazards for alternative definitions of up- and downgrades. The graphs in the first row include switches defined by changes in the share of analytical, interactive and non-routine manual tasks. The graphs in the second row show switches defined by changes to jobs with a higher or lower median wage. For details, see Section 2.5.3. Light blue and light red colors indicate 95% confidence bands. The cumulative hazard indicates the probability of an event within a given timeframe.
Source: ReLOC, IEB and BHP, own calculations.

2.A.7 Additional robustness checks

Table 2.A.8: Estimated hazard ratios for the effect of FDI on separations and up- and downgrades

	separations		upgrades		downgrades	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Main results:						
FDI	0.9630	0.8092**	1.2422**	0.9960	1.3413**	0.9781
	(0.0440)	(0.0691)	(0.1315)	(0.1896)	(0.1980)	(0.2094)
FDI × quarter		1.0190**		1.0252*		1.0352*
		(0.0083)		(0.0141)		(0.0205)
Subjects	383,098	383,098	383,098	383,098	383,098	383,098
Events	102,661	102,661	15,880	15,880	11,731	11,731
Panel B: Without small firms (>50 employees):						
FDI	0.9576	0.8019**	1.2463**	1.0019	1.3425**	0.9756
	(0.0450)	0.0700	(0.1337)	(0.1928)	(0.2013)	(0.2123)
FDI × quarter		1.0193**		1.0249*		1.0354*
		0.0085		(0.0143)		(0.0208)
Subjects	376,847	376,847	376,847	376,847	376,847	376,847
Events	100,316	100,316	15,708	15,708	11,592	11,592
Panel C: Random starts (plus minus 4 quarters):						
FDI	0.9494	0.8573**	1.3317***	1.2961	1.3600**	1.1711
	(0.0427)	(0.0620)	(0.1331)	(0.2140)	(0.2028)	(0.2160)
FDI × quarter		1.0112*		1.0030		1.0166
		(0.0064)		(0.0112)		(0.0170)
Subjects	264,427	264,427	264,427	264,427	264,427	264,427
Events	71,722	71,722	11,157	11,157	8,073	8,073
Panel D: No restriction to workers' tenure:						
FDI	0.9395	0.8027**	1.2523**	1.0484	1.3209*	0.9930
	(0.0541)	(0.0805)	(0.1244)	(0.1843)	(0.1888)	(0.1976)
FDI × quarter		1.0190**		1.0205		1.0323*
		(0.0083)		(0.0131)		(0.0189)
Subjects	490,679	490,679	490,679	490,679	490,679	490,679
Events	158,350	158,350	19,471	19,471	14,049	14,049

Notes: The table presents exponentiated coefficients (hazard ratios) and cluster robust standard errors at the firm level in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Panel A repeats our main findings. Panel B shows estimates without firms with less than 50 employees. Panel C summarizes estimates where we randomly shuffled the pseudo investment quarter of domestic firms. Panel D shows estimates without restrictions on the tenure of workers. Control variables in all models are: age, age squared, experience, tenure, foreign dummy, skill dummies, firm age and a dummy if a firm existed in 1975 (all interacted with quarters since treatment), as well as occupation, year, and state dummies. Estimates are based on a matched sample of MNEs and domestic firms.
Source: ReLOC, IEB and BHP, own calculations.

Chapter 3

The Effects of Outward Foreign Direct Investment on Domestic Earnings: Evidence from German Investments in the Czech Republic

3.1 Introduction

In the public debate about globalization, a common argument for protectionist trade policies is that international trade has negative side effects. Especially when firms set up or acquire affiliates in low-wage countries, workers at the investing firms are concerned about job losses and wage cuts. However, economic theory suggests ambiguous labor market effects of foreign direct investment (FDI). On the one hand, factor price differences across countries (e.g., Helpman, 1984; Feenstra & Hanson, 1996) let firms relocate labor-intensive production stages to foreign countries where labor costs are lower. Such "vertical investments" will have negative effects for low-skilled workers if they perform labor-intensive tasks that are substituted by foreign workers. On the other hand, vertical FDI is accompanied by cost savings that can enhance firms' productivity, and may therefore benefit all domestic workers (Grossman & Rossi-Hansberg, 2008). Moreover, if firms follow market-seeking motives, they will invest abroad to serve foreign demand with goods and services on-site (Markusen, 1984). Such "horizontal investments" will have negative effects for domestic workers who produce export goods if these exports are replaced by on-site production abroad (see, e.g., Buch *et al.*, 2007; Bachmann *et al.*, 2014). Thus, how FDI affects labor market outcomes of the domestic workforce is unclear in theory and remains an empirical question.

In this paper, I study how workers' labor market outcomes evolve over time once their employers engage in FDI. I focus on workers already employed before the investment, and follow their annual earnings independent of whether they stay with the employer after the investment. Thus, my analyses of annual earnings account not only for wage effects within the firms that invest, but also effects induced by workers' transitions into unemployment and jobs at other employers. Additionally, I analyze whether the changes in workers' earnings go back to changes in their daily wages on the one hand, and their number of days in employment on the other. In this way, I provide comprehensive insights into the effects of FDI over workers' careers and complement previous research on FDI-induced outcomes at the individual level.

Thus far, empirical studies on individual-level effects of FDI have remained rare. The reasons are manifold. First, in the absence of suitable employer-employee data, effects of FDI have often been analyzed using aggregate data at the firm or industry level.[1] The problem with identifying effects of FDI using aggregate data is that average wages depend on the composition of the workforce, which may also has changed due to FDI. Thus, most studies using aggregate data examine the substitutability between foreign and domestic employment and the relative skill demand and come to mixed results (see, e.g., Slaughter, 2000; Head & Ries, 2002; Barba Navaretti & Castellani, 2004; Hansson, 2005; Konings & Murphy, 2006; Desai *et al.*, 2009; Hijzen *et al.*, 2011; Becker *et al.*, 2013).[2] In contrast to these studies, the paper at hand uses an employer-employee data set, which makes it possible to assess whether a change in the firms' FDI status affects individual workers' earnings and whether these earnings changes depend on the workers' education. By measuring the outcome at the individual level for a fixed group of workers, I am able to abstract from compositional changes of the workforce within firms or industries.

Second, when analyzing wage effects of FDI, one has to cope with endogeneity. As only the most productive firms are able to cover the costs of investing (Helpman *et al.*, 2004), the wage premium of multinational enterprises (MNEs) is hardly distinguishable from wage premia associated with other firm characteristics that are highly correlated with productivity, such as firm size. By applying propensity score matching, I identify domestic firms that do not engage in FDI in any country but according to a defined set of characteristics have the same investment probability as the firms that engage in FDI. Due to exact matching per year, the investment dates of matched MNEs can serve as pseudo-investment dates for domestic firms. In this way, I consider earnings developments of workers from matched domestic firms as counterfactual developments to workers from MNEs.

Moreover, theoretical models with labor market frictions show that more productive firms screen employees more rigorously according to skills and unobservable abilities and therefore realize higher-quality matches between workers and firms. Because it is costly to replace workers that are well-matched, firms pay them higher wages (e.g., Helpman *et al.*, 2010). If one wants to estimate the effect of FDI on workers' wages, one has to account for the distinct sorting of workers into MNEs. One approach is to

[1]For Germany, Jäckle & Wamser (2010) find that firms engaged in FDI have higher average wages and productivity than domestic firms, but lower employment growth rates. They explain that average wages rise because investing firms maintain capital-intensive production stages in Germany. Marin (2004, 2010) finds that German and Austrian firms search for high-skilled workers abroad to cope with domestic skill shortages, leading to a wage decline for high-skilled domestic workers. In contrast, Temouri & Driffield (2009) find neither significant employment nor wage effects due to outward FDI of German firms.

[2]See Crinò (2009) for a comprehensive overview of the literature.

control for unobserved worker-firm matching using job-spell fixed effects (see, e.g., Laffineur & Gazaniol, 2019). However, I cannot apply such within-job-spell wage regressions, as I am also interested in covering earnings developments over periods of unemployment. Instead, I focus on the cohort of workers already employed at investing and domestic firms two years before the investment. At that point in time, worker sorting into future MNEs on the one hand and domestic firms on the other should be quite similar, provided that the matched firms in my sample have comparable characteristics which are highly correlated with productivity. In any case, match-specific pay premia will cancel out in my estimations, because I estimate the effect of FDI on workers' earnings and wages relative to their pre-investment earnings and wages.

Studying the longer-run effects on individual earnings of predefined cohorts of workers has a number of advantages (see Autor *et al.*, 2014; Hummels *et al.*, 2014). First, I can account for adjustment costs that workers may face in the aftermath of FDI, such as job losses, spells of unemployment, or wage losses following transitions into new jobs where the firm- or sector-specific human capital that workers have acquired may be less applicable. Second, I can avoid that my analytical sample is selective with regard to certain groups of workers leaving or staying at the firm (Hummels *et al.*, 2014).

The empirical analyses in this paper rely on data from the project "Research on Locational and Organisational Change" (ReLOC) that cover all German firms with Czech affiliates listed in the Czech Commercial Register in 2010. Despite their geographical proximity, trade between these neighboring countries officially started only after the fall of the Iron Curtain in 1989. Ever since, the Czech Republic has been emerging as a top destination for German FDI among the Central and Eastern European Countries (CEEC). In 2010, Czech affiliates alone accounted for approximately 24% of the workforce employed by German firms across the CEEC (Deutsche Bundesbank, 2014). The ReLOC data at hand allow me to contribute to the body of research that studies how FDI in lower-wage CEEC affects labor market outcomes in the home country.[3] The data have several advantages over conventional data sets regarding their representativeness. Whereas data sets used in previous

[3]For example, Becker *et al.* (2005) show that German and Swedish FDI in CEEC lead to job losses in the investing countries. Kleinert & Toubal (2007), on the other hand, do not find any significant employment effects of German FDI in countries worldwide or the CEEC in particular, but show that German FDI in the EU-15 countries increases employment at home.

studies commonly underrepresent small firms with low investment volumes (Pflüger *et al.*, 2013), the ReLOC data cover firms of all sizes irrespective of any investment thresholds.[4] Covering small firms is all the more important in the German-Czech context, because the geographical proximity of the two countries implies low transaction costs that make FDI more likely affordable for small firms. For the analyses, I link the ReLOC data to highly reliable administrative data on German firms and workers.

In the empirical analyses, I use these linked employer-employee data to estimate outcomes of the predefined cohort of workers. I apply an event study difference-in-difference (DiD) design that sheds light on how the effects of FDI evolve relative to the pre-investment period. In order to provide comprehensive insights into how FDI affects individual labor market outcomes, I consider several outcome variables at the worker level. My baseline model estimates effects on annual earnings, including periods of zero earnings. Because changes in annual earnings are driven by changes in days worked, daily wages, or a combination of the two, I additionally examine effects of FDI on days in employment and average daily wages.

In the first step of the analysis, I examine effects of FDI on the annual earnings of workers, i.e., earnings irrespective of whether workers stay with their employers in the following years. I find that FDI affects earnings of medium-skilled workers, whereas earnings of low- and high-skilled workers remain unaffected. Further analyses show that the positive earnings effect for medium-skilled workers is mainly driven by an increase in their days in employment. In further analytical steps, I consider the employment and wage effects at the initial employers. These within-firm analyses show that FDI does not affect medium-skilled workers' number of days in employment at the initial firm. Thus, the increase in days in employment independent of the firm must come from workers switching to other firms, who appear to have better reemployment chances than workers from domestic firms. Moreover, the within-firm results show that low- and medium-skilled workers benefit from FDI in terms of higher average daily wages, and high-skilled workers benefit from FDI in terms of more days in employment at the initial firm. Most effects appear shortly after the investment and last over a few years only.

[4] The most commonly used data on German FDI is the *Microdatabase Direct Investment* (MiDi). Since 2007, it has been restricted to foreign affiliates with a balance sheet total of more than three million Euros (Schild & Walter, 2015). In fact, Schäffler *et al.* (2017) show that when contrasting MiDi data against ReLOC data, MiDi will only identify about one fourth of the German firms with affiliates in the Czech Republic.

To sum up, FDI has either non-significant or positive effects on the outcomes under study. In light of these findings, German workers need hardly be concerned about their employers' investments in the Czech Republic. Even when taking into account subsequent periods of unemployment and job transitions, I find no negative effects of FDI on workers' earnings on average. The analyses corroborate that FDI does not negatively affect employment in general and employment at the initial firm in particular. Thus, FDI does not seem to cause any major layoffs.

The results add to recent literature on the effects of FDI on individual wages (e.g., Laffineur & Gazaniol, 2019) and job security (e.g., Becker & Muendler, 2008; Bachmann *et al.*, 2014) as well as research on the individual-level effects of offshoring.[5] Although the information on FDI does not cover arm's-length trade, looking at firms that become multinational provides evidence on the effects of firm-internal offshoring and additionally covers the effects of horizontal FDI (Barba Navaretti *et al.*, 2010).

The remainder of the paper is structured as follows. In the next section, I outline the empirical strategy. In Section 3.3, I introduce the data used in the analysis, and provide a comparison of the multinational and the domestic firms under analysis along relevant descriptive statistics. In Sections 3.4 and 3.5, I discuss the results and present a series of robustness checks. Section 3.6 concludes.

3.2 Empirical strategy

Estimating effects of FDI on individual earnings is challenging because one must account for potential endogeneity bias on two levels. At the firm level, if only the most productive firms select into investment (Helpman *et al.*, 2004), these firms will typically pay higher wages from the outset. At the individual level, more productive firms can realize better worker-firm matches which come with higher wages (e.g, Helpman *et al.*, 2010) and more productive workers may sort into more productive firms (e.g, Card *et al.*, 2013, 2018). Moreover, the estimation strategy must consider that the sample under analysis may be selective if certain groups of workers leave or stay at the firms following the investment.

[5] Studies on effects of offshoring mainly consider imported intermediate inputs that also capture trade with unaffiliated foreign firms that are not part of FDI (see, e.g., Liu & Trefler 2008, Ebenstein *et al.* 2014 for the US; Hummels *et al.* 2014 for Denmark; and Geishecker & Görg 2008, Wagner 2011, Schwörer 2013, Baumgarten *et al.* 2013, Hogrefe & Wrona 2014 for Germany). Hummels *et al.* (2018) provide a comprehensive overview of this strand of the empirical literature.

In this paper, I apply a three-step procedure that avoids endogeneity on both levels and accounts for potential selectivity. First, I deal with endogeneity at the firm level by restricting the sample to firms that have a similar probability of engaging in FDI at a given point in time. Using propensity score matching (Rosenbaum & Rubin, 1983), I identify statistical twins of future MNEs and domestic firms that are equally likely to engage in FDI. The matching procedure and matched firm sample are the same as in Chapter 2 and described in full detail there. Second, I cope with sorting at the individual level by applying a cohort design that restricts the sample to workers who were already employed by the future MNEs and domestic firms two years prior to the (pseudo-)investment. In combination with the event study DiD design, which estimates relative changes to this pre-investment period, I can isolate the effect of FDI from any worker- or match-specific pay premia. Third, by following predefined cohorts of workers, I can capture effects of FDI over workers' careers independent of the employer.

In the first step, propensity score matching assigns each treatment firm, i.e., each MNE, to its three nearest neighbors in the group of control firms, i.e., domestic firms, according to their predicted probability to invest in the common support. It builds on the assumption that conditional on a set of control variables, FDI will occur randomly among firms with the same propensity score.[6] The logit regression for obtaining the propensity scores includes the following pre-investment variables: log number of employees; average age and wage of the workforce; the share of female, regular, German, unskilled-manual, full-time, low-, medium- and high-skilled employees; the share of trainees, engineers and scientists;[7] wage and employment growth rates over the last two years; firm age and a dummy for firms older than 1975;[8] and state and industry dummies. Following Hijzen *et al.* (2011), pre-investment characteristics are measured two years prior to the investment to ensure that the control variables are unaffected by the decision to invest. Note that

[6] Propensity score matching is a common approach to analyzing effects of FDI (see, e.g., Bronzini 2015, Crinò 2010 and Barba Navaretti & Castellani 2004 for Italy; Hijzen *et al.* 2011 for France; Debaere *et al.* 2010 for Korea; Barba Navaretti *et al.* 2010 for France and Italy; Becker & Muendler 2008 and Kleinert & Toubal 2007 for Germany; Hijzen *et al.* 2007 for Japan; and Egger & Pfaffermayr 2003 for Austria). Except for Becker & Muendler (2008), these studies consider aggregate outcomes at the firm level only.

[7] See Schmucker *et al.* (2016) for a detailed description of the worker categories in the Establishment History Panel (BHP) data.

[8] The age of a firm is deduced from its oldest establishment according to the BHP. Because the data set starts in 1975, firm age is left-censored. Firms with establishments that date back to years before 1975 are identified by a separate dummy.

I can only match firms according to their observable characteristics, whereas their productivity is unobserved. However, I match on several firm characteristics which are highly correlated with firms' productivity and, thus, make for good proxies for productivity.

Propensity score matching results in an exact three-to-one matching by year. In principle, a given control firm could be matched to more than one treatment firm in different years. As I require an unambiguous pseudo-investment date for each control firm for the event study analysis, I rely on an "iterative nearest neighbor approach" that takes into consideration all potential matches and assigns each treatment firm to its best-matching control firm according to the estimated propensity scores, under the condition that the control firm is not better matched to another treatment firm. Each control firm is assigned a pseudo-investment date that corresponds to the investment date of its matched treatment firm. According to their covariates, the firms in the matched sample are equally likely to turn into MNEs.[9] Thus, the sample allows for me to estimate the counterfactual scenarios that would have emerged if the treatment firms had not invested abroad.

In the second step, I identify all workers in the matched sample of firms that were employed two years prior to the investment, and follow this cohort of workers irrespective of whether they stay with their initial employers. This approach of following cohorts is adapted from Autor *et al.* (2014), who use it to estimate how Chinese import exposure affects US workers, and Hummels *et al.* (2014), who estimate how offshoring affects Danish workers.[10] Whereas they calculate average effects over the entire worker cohort, I follow individuals within the worker cohort. Fixing the cohort from the outset has two advantages. First, once the cohort is fixed, one can track workers independent of whether they stay with their initial employers. Thus, one can also capture the costs of job changes among workers who leave firms in the aftermath of FDI and avoid sample selection of within-firm wage regressions at the same time. Second, two years before the investment, worker sorting into future MNEs and domestic firms should be quite similar, provided that the matched firms have comparable characteristics which are highly correlated with productivity. Moreover, one can assume that at that point in time it is unforeseeable for workers

[9] See Chapter 2 for balancing test results. An overview of the covariates before and after matching is also given in the Appendix 3.A.1.

[10] Both papers refer to Walker (2013), who applies a similar approach to estimate the costs that workers incur when they switch between industries following changes in environmental regulations.

whether their employer will make investments in foreign countries or not. Notwithstanding whether the firm-level matching and cohort sampling will perfectly account for worker sorting into MNEs, the event study DiD models with worker fixed effects in the next step will cancel out any worker- or match-specific pay premia by design.

In the third step, with the matched firm sample and the cohort sample at hand, I estimate the effects of FDI on workers' labor market outcomes using an event study design.[11] My identification strategy builds on the change in MNE status, i.e., I only consider the firms' first investment in the Czech Republic.[12] The event study design follows a simple panel data model where the outcome variable is regressed on a separate set of non-parametric time-to-event indicators, one for the treatment and one for the control group. Equation (1) describes the model:

$$y_{it} = \sum_{k=-5}^{5} \beta_k T(t = t^* + k) + \sum_{k=-5}^{5} \gamma_k C(t = t^* + k) + \alpha_i + \theta_t + \epsilon_{it}, \tag{1}$$

where y_{it} is a dependent variable for worker i in year t, namely annual earnings, annual days in employment, or log average daily wages per year. Earnings are scaled relative to their value in year $k = -2$.[13] In the baseline model, outcomes are measured independent of the firm at which a worker is employed. Additionally, I present results for wages and employment at the initial firm at which a worker is employed in year -2.

$T(t = t^* + k)$ and $C(t = t^* + k)$ are sets of time-to-event dummies for individuals at the treated firms, T, and control firms, C. They indicate the timing of year t relative to the year of (pseudo-)investment t^*. The base year -2 is the reference category and, thus, estimated coefficients reflect changes relative to two years before the investment.

The effect of FDI on workers' earnings, wages, and employment is not provided by

[11] In the labor market literature, event studies are typically applied to study mass layoffs (see, e.g., Jacobson *et al.*, 1993).

[12] Barba Navaretti *et al.* (2010) argue that the largest changes for the workforce can be expected at the extensive margin and less by expanding existing foreign production. This is confirmed by Muendler & Becker (2010), who show that the extensive margin of FDI has a major impact on domestic employment. Similarly, Bachmann *et al.* (2014) find that the extensive margin of investment has the largest effect on the employment security of German workers.

[13] Scaling is preferred over log transformations because logs remove zero earnings from the analysis (see also Autor *et al.*, 2014; Bessen *et al.*, 2019).

estimating Equation (1) right away. For each year k relative to the investment, the effect of FDI on the individual outcome is the difference between the two estimated coefficients for the treatment and control group, $\beta_k - \gamma_k$. If this difference significantly deviates from zero, the investment has an impact on the individual outcome variable. Thus, the estimation strategy of this paper is comparable to a conventional DiD estimation. However, a DiD approach would hinge on the assumption that the effect of FDI is static for the year of treatment and following years, whereas the event study approach is more flexible in this respect.

α_i are individual fixed effects. The individual fixed effect captures all time-invariant observable and unobservable worker characteristics. At the same time, it absorbs any effects of industry, region, and other firm characteristics in the base year. Note that I cannot control for any worker or firm characteristics after the base year. Any changes observed in the years after the event—for example, occupation or industry switches—can be outcomes of the treatment themselves and, thus, make for bad controls that should not be included in the model (see Angrist & Pischke, 2009). Moreover, it is not necessary to control for other observable characteristics in the base year because they are already captured by the worker fixed effect. Last, the individual fixed effects in the event study DiD design imply that the effects of FDI on workers' earnings and wages are estimated relative to their individual earnings and wages in the base year. In the base year, workers are necessarily employed at the future MNEs and domestic firms, respectively, due to the cohort sampling. The estimations of relative changes will therefore cancel out any worker-firm specific pay premia.

θ_t is a vector of year dummies. It accounts for calendar-year specific shocks, like macroeconomic events, that affect all workers exposed to FDI in a particular year. ϵ_{it} is an error term. Standard errors are clustered at the level of the initial firm because they are supposedly correlated among workers who have the same employer in year -2.[14]

I choose year -2 as the reference category for two reasons. First, it is unlikely that MNEs will start adjusting to FDI two years before the investment and it is still sufficiently close to the investment date. In addition, I can observe whether the investment decision induces adjustments in advance, comparable to an Ashenfelter

[14] I estimate Equation (1) with the user-written regression command for high-dimensional fixed effects (reghdfe) by Correia (2016).

(1978) dip. Second, the reference year thereby equals the year when the firms are matched and the cohort sample is fixed, which precludes early sorting into MNEs.

According to Schmidheiny & Siegloch (2019), the correct modeling of the endpoints of the effect window is crucial in event study designs. They explain that it is necessary to stop measuring the effects at some point in time for practical reasons. Referring to previous work on the labor market effects of German firms that invest in the Czech Republic (Schäffler & Moritz 2018 and Chapter 2), I consider an effect window of five years before and five years after the investment. All effects prior to year -5 and after year five are summarized under the corresponding dummy. Thus, the interpretation of the respective coefficients slightly differs from that of the other time-to-event dummies. The coefficients at the ends provide an average effect for all years outside of the effect window. In a robustness test, I drop all years outside the considered time interval to see if it changes the results for the respective years.

3.3 Data and descriptive statistics

3.3.1 Data

This paper relies on three databases. First, I use unique data from the ReLOC project. With respect to the treatment group, the data cover the full population of about 3,400 German firms that had made investments in the Czech Republic between 1990 and 2010 according to the Czech Commercial Register in 2010, including their date of investment.[15] Because my focus lies on the extensive margin of FDI, I consider the firms' first investment in the Czech Republic only.[16] ReLOC provides additional data on another 9,700 German firms that have never invested in any foreign country, which I use as a control group in my analyses.

Second, I merge the data on the treatment and control firms with two administrative data sets of the Institute for Employment Research (IAB). I use the Establishment History Panel (BHP 7514V1) for information at the establishment level, and the Integrated Employment Biographies (IEB V10.00) for information on worker

[15] See Hecht *et al.* (2013b) for more detailed information on the ReLOC data set.

[16] In principle, the MNEs can have invested in other foreign countries, too. However, a survey among 459 ReLOC firms shows that almost 70% of German MNEs with FDI in the Czech Republic are first-time investors (Hecht *et al.*, 2013a).

characteristics. Both data sets stem from mandatory social security notifications of employers and therefore provide highly reliable data. The BHP covers all establishments with at least one employee subject to social security contributions in Germany between 1985 and 2014. For each establishment, it provides information on the workforce as of June 30 of a given year (see Eberle & Schmucker, 2017). In order to match the BHP with the ReLOC sample, all establishments that belong to a firm are identified by means of record linkage techniques by Schäffler (2014). Identifying the firms that the establishments belong to is essential, because decisions on FDI will typically take place at the firm rather than the establishment level. For firms that consist of more than just one establishment, I aggregate the firm-level information over all establishments. Moreover, I use the data available for a firm's largest establishment (in terms of the number of employees) to assign regions and industries to the parent firm. The IEB provides individual-level information for all employees in Germany between 1975 and 2010. They can be directly linked to the BHP. The wage information in the IEB is top-coded and must be imputed for approximately 10% of the sample. The imputation procedure follows Dustmann *et al.* (2009).[17] In addition, I follow Fitzenberger *et al.* (2005) and impute any missing or inconsistent information on education in the IEB.

Next, I create a cohort data set by identifying all workers employed in the matched treatment and control firms two years prior to the investment. With the administrative data at hand, I can follow workers' employment biographies across different firms as long as they work subject to social security contributions. To capture the effects of FDI rather than other events in workers' careers that might bias the effects of interest, I apply a series of sample restrictions. First, I only keep workers aged 20 to 53 in the base year. Five years after the investment, these workers reach a maximum age of 60, and are thus unlikely to retire during the observed period. Moreover, I ensure that the workers under analysis are highly attached to the labor force and have rather stable employment biographies by restricting the cohort to male workers with a minimum tenure of two years at the firm. In addition, I exclude workers in other unstable jobs from the analysis who can be assumed to have an intrinsic motivation to improve their labor market positions and are therefore more likely to leave the firms voluntarily. These include part-time workers, trainees,

[17] I drop all workers with earnings above the 99th percentile of the annual earnings distribution because these are likely to be imputations by error. Moreover, I exclude workers with zero annual earnings but non-zero days in employment as these are likely to be mistaken reports.

and workers with earnings below the marginal employment threshold.[18] Moreover, I only consider individuals who had their main job-spell in the treatment or control firm in the base year and did not incur substantial earnings losses in the base year compared to previous years.[19] In a last step, I only keep workers whose matched firm exists in the data set after the above restrictions are applied. This leaves me with a final sample of 203,695 employees in the base year, which I follow until five years after the investment. For every worker, I calculate his real annual earnings, average real daily wages per year, and days in employment per year.

I focus on investments between 1995 and 2005, which allows me to observe a worker five years before and five years after the investment. The corresponding base years are between 1993 and 2003. As I am interested in identifying the effects of FDI on outcomes of all workers initially employed in the treatment or control firms, I takes measures to ensure that findings are not distorted by workers who leave the labor market and are unobservable in the data. Therefore, I create a balanced panel for all years following the base year by setting annual earnings and days in employment to zero for any unobserved worker. I thereby assume that workers with zero annual earnings and zero days in employment are unemployed. This assumption is reasonable given that the administrative data under study provides all employment spells liable to social security contributions. Nevertheless, it is possible that workers with zero earnings or employment are self-employed or work as civil servants. In a robustness check, I drop the assumption that unobserved workers have zero earnings to analyze whether any of these unobserved workers affect the results.[20]

[18] Marginal employment is defined as jobs where workers yield earnings below the threshold for income tax and social security contributions. These jobs hold a specific status in the German labor system and were restricted to a maximum income of 400 Euros per month during the period under investigation.

[19] The main job in a given year is defined by the longest job-spell in terms of days in employment. If there are several job-spells of equal length, I use the one with the highest earnings. If the worker is employed in more than one firm of the matched sample in the base year, I consider the earlier employment. Additionally, I run a robustness test in which I exclude workers employed by another MNE after the first treatment.

[20] The panel does not require balancing for years preceding the base year, because workers can enter the sample for reasons other than FDI in the base year. Most workers are just entering the labor force and younger than 20. Thus, there is no reason to balance their observations before their first regular employment spell in a treatment or control firm.

3.3.2 Descriptive statistics

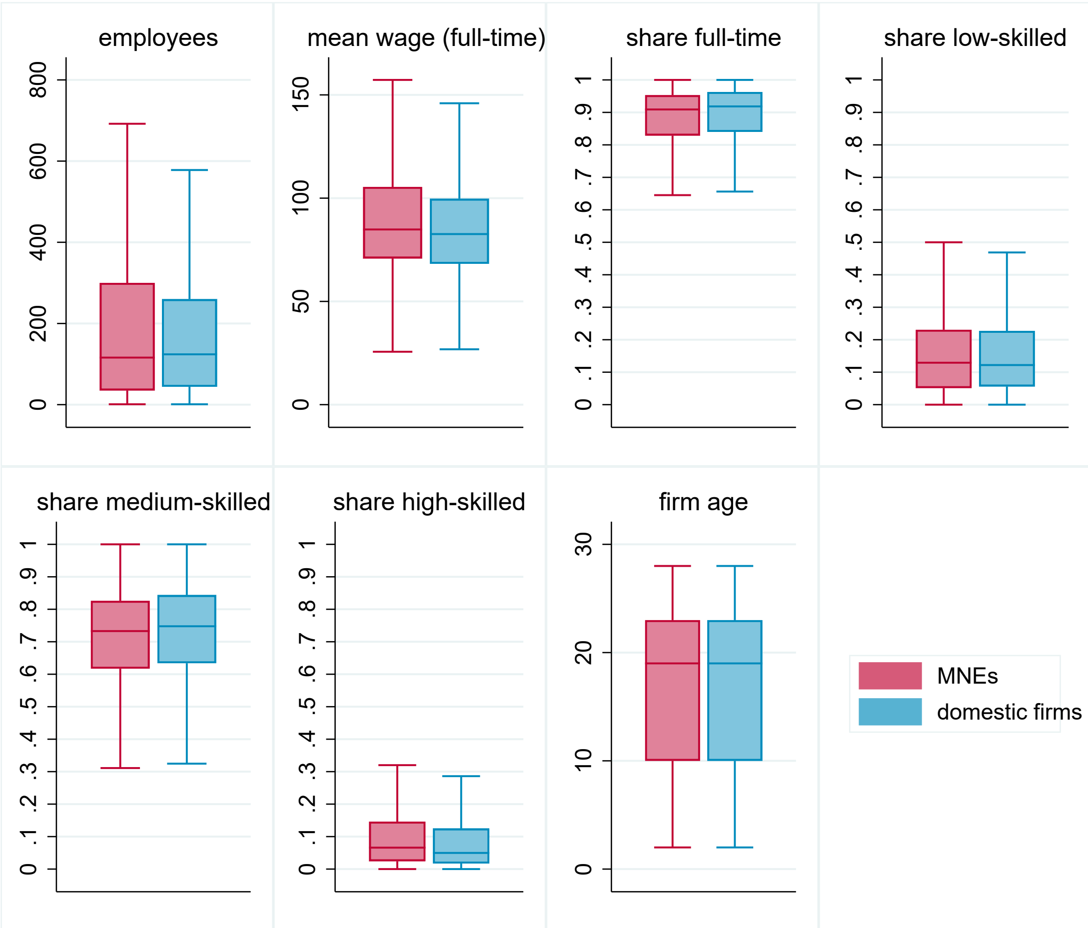
In this section, I give a descriptive overview of the sample of firms and workers used in the event study analysis. I start out by comparing some core characteristics of the treatment and control firms in the year of matching, i.e., two years before the investment. The box plots in Figure 3.1 show that the MNEs and domestic firms are very similar in size, average wages, workforce composition, and firm age. Overall, their medians, first and third quartiles are very similar for all variables. Table 3.A.2 in the Appendix lists average values and standard deviations for these variables as well as additional variables used for matching. Overall, the descriptive statistics corroborate that the treatment and control firms are highly comparable. Although the average firm size, wages, and share of high-skilled workers are slightly higher in the MNEs than in the control firms after matching, the matching procedure diminishes the pre-investment differences between treatment and control firms.[21] 54% of the firms are located in the manufacturing industry, 19% in the transport and business service sector, and another 19% in the retail and hospitality sector.

It is interesting to see not only the firm characteristics in the base year but also their development following the investment. Table 3.A.3 and 3.A.4 in the Appendix summarize the firm characteristics in the year of investment and two years after. Two years after investing, MNEs are larger, pay higher average wages and employ a larger share of high-skilled employees and a lower share of low-skilled workers than before. In addition, their share of unskilled-manual workers, which are often assumed to have the highest risk of being offshored, is slightly lower after the investment.[22] Developments among the control firms are quite similar. Over time, they also have an increase in employment, wages, and the share of high-skilled workers as well as a decrease in the share of unskilled-manual workers. Trends towards more skilled labor independent of FDI may also reflect the shift in skill demand due to technological change (see, e.g., Autor *et al.*, 2003). Note that the compositional changes among the domestic firms only slightly diverge from the changes among the MNEs. However, aggregated characteristics at the firm level do not necessarily reflect how FDI affects labor market outcomes at the individual level. The subsequent event study based

[21] A short description of the complete sample of firms that is used for matching is provided by Table 3.A.1 in the Appendix. A more comprehensive description of the firm-level data before matching and the results of balancing tests after matching can be found in Chapter 2.

[22] Unskilled-manual workers in the BHP are defined according to Blossfeld-occupations (see Schmucker *et al.*, 2016).

Figure 3.1: Characteristics of the matched firm sample in the year of matching

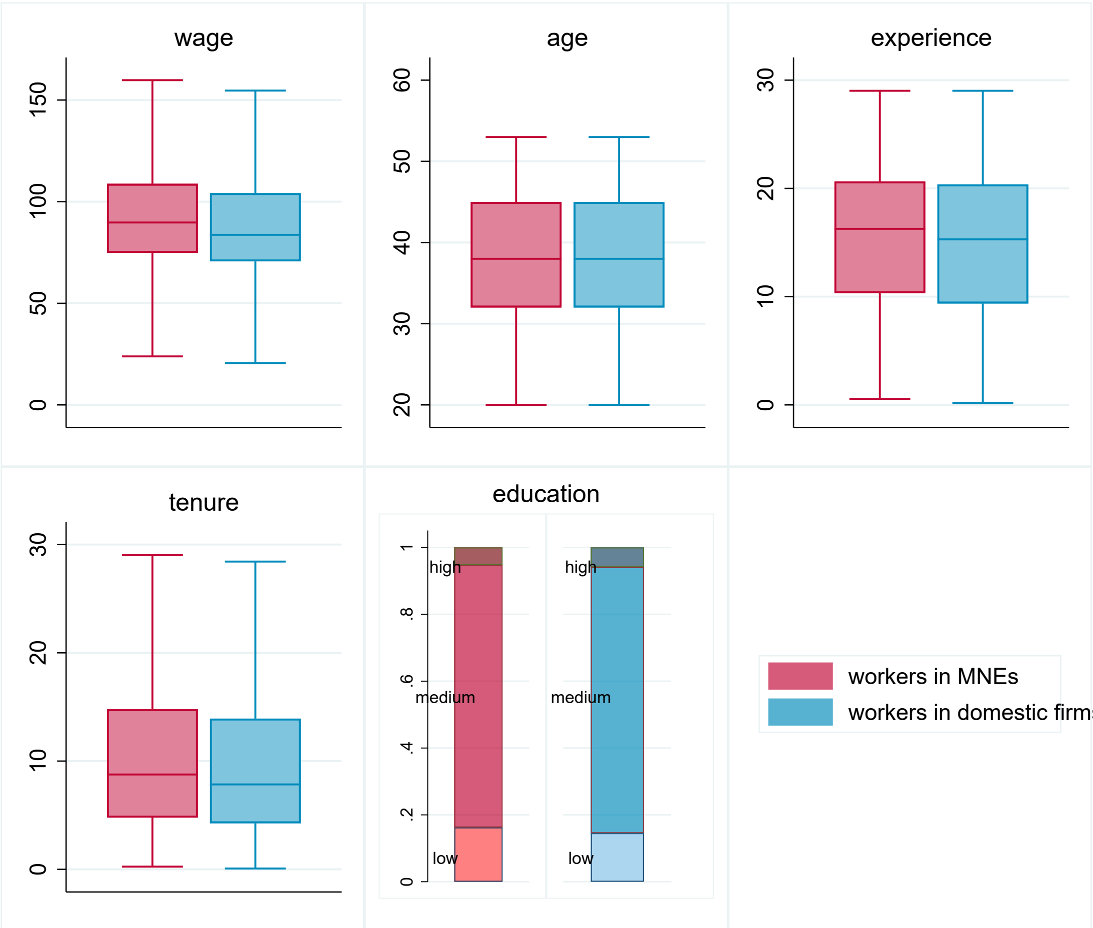


Notes: The figure shows box plots for selected firm characteristics in the final data set used for the event study. The horizontal line in the middle of a box represents the median. The edges of a box indicate first and third quartiles. The whiskers illustrate minima and maxima, limited to 3/2 of the first or third quartile, respectively. Average wages are calculated for full-time workers only. Sources: ReLOC and BHP, own calculations.

on the matched firm sample will deliver a more in-depth analysis and understanding how FDI affects outcomes of individual workers.

Turning to the worker level, Figure 3.2 presents essential characteristics in the base year for the cohort of workers used in the event study estimations. The cohort comprises 130,972 workers at the matched MNEs and 72,723 workers at the matched domestic firms two years before the investment. Although I do not match on worker characteristics, Figure 3.2 shows that the workforce of the matched firms is highly comparable according to observable individual characteristics, such as wage, age, experience, tenure, and education. Additional characteristics are summarized in Table 3.A.5 in the Appendix. Workers at the MNEs exceed workers at the domestic firms in their average daily wage by four Euros, whereas other worker-level variables are very similar.

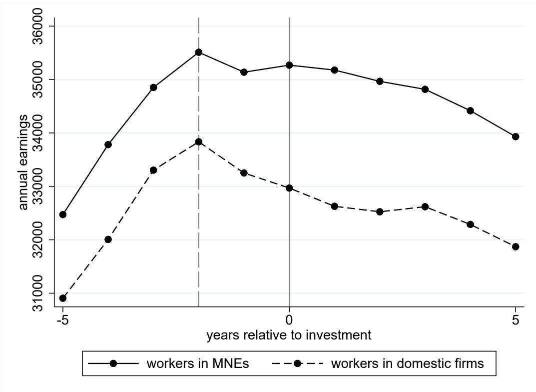
Figure 3.2: Characteristics of workers in the matched firms in the year of matching



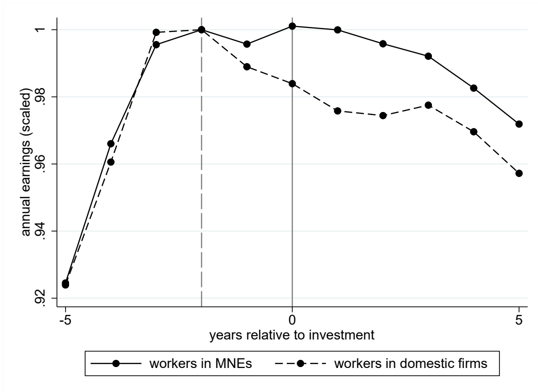
Notes: The figure shows box plots and bar charts for various worker characteristics of the final data set used for the event study. The horizontal line in the middle of a box represents the median. The edges of a box indicate first and third quartiles. The whiskers illustrate minima and maxima, limited to 3/2 of the first or third quartile, respectively. The bar charts for the education variable depict the share of individuals that falls into a skill group.
Sources: ReLOC, IEB, and BHP, own calculations.

Figure 3.3 summarizes how the main outcome variables of workers employed at MNEs and control firms two years before the investment evolve over time, namely their annual earnings, days in employment, and average daily wages. The dashed vertical line at year -2 marks the base year, when the firms are matched and the sample is fixed. The solid vertical line at year zero marks the year of investment for MNEs and pseudo-investment for domestic firms, respectively.

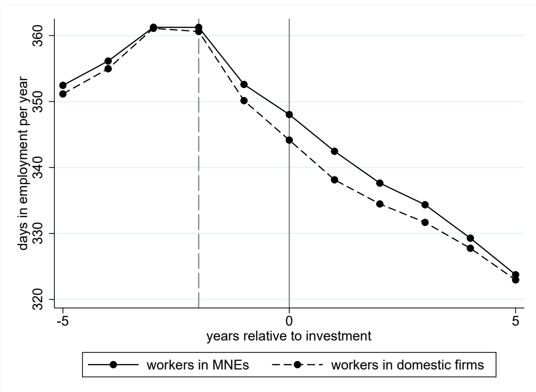
Figure 3.3: Trends in the workers' outcome variables



Panel A: Average annual earnings



Panel B: Average annual earnings (scaled)



Panel C: Average days in employment

Panel D: Log average daily wages

Notes: The graphs depict the trends in annual earnings, days in employment, and average daily wages from five years before until five years after the investment. The graphs show the group averages calculated for the cohort of workers that worked in a matched MNE or domestic firm two years prior to investment. Dashed vertical lines mark the base year -2, and solid lines mark the year of investment. Annual earnings in Panel B are scaled by the individual value in the base year -2. Log average wages in Panel D are conditional on being in employment.
Sources: ReLOC, IEB, and BHP, own calculations.

Panel A of Figure 3.3 shows the trends in workers' annual earnings independent of where they are employed. Employers are only fixed for the base year and the two years before. In these years, workers must be employed at either a treatment or a control firm. Throughout the depicted period, earnings of workers who start out at future MNEs are larger than earnings of workers who start out at domestic firms. Before the base year, trends in annual earnings for workers from future MNEs and domestic firms are parallel. After the base year, they slightly diverge. Panel B shows similar trends for annual earnings scaled by annual earnings in the base year. Before the base year, the scaled average annual earnings are very similar for workers in treatment and control firms. This is plausible, given that the cohort sample is restricted to workers with high firm attachment. The graphs reveal that after the event, average annual earnings of workers from domestic firms decrease at a higher rate than the earnings of workers from MNEs. Thus, in terms of annual earnings, workers seem to benefit from FDI. This potential positive effect of FDI is further examined in the event study estimation in Section 3.4. The decrease in average annual earnings after the base year can be attributed to workers who become unemployed or do not have a job liable to social security contributions and thus have their earnings fall to zero.

The follow-up question regarding these earnings trends is whether they are driven by changes in the employment margin or by changes in wages. Panels C and D show the trends in workers' number of days in employment per year and the trends in their average daily wages. The cohort sample consists of workers with a minimum tenure of two years in the base year and accordingly, the number of days employed is largest in the base year and the two years before. From the base year onwards, workers may become unemployed and, thus, their average number of days in employment declines. Moreover, there is a small gap in trends for the treatment and control group after the event. The control group has a lower average number of days in employment, which suggests that on average workers who start out at future MNEs face less days of unemployment after investment. In the event study analyses in Section 3.4, I further examine whether the diverging trends in employment between the two groups are attributable to FDI and whether they are driven by continued employment at the initial firm or outside employment opportunities.

Beyond the employment adjustment margin, I consider the impact of FDI on average daily wages. Panel D shows the trends in log average daily wages. As workers receive wages only when being employed, the graphs depict wage trends conditional on employment. Whereas the trends are parallel before the base year, the positive trend in wages of workers starting out in future MNEs exceeds that of workers starting out in domestic firms later on. Two years after the investment, the wage increase for the treated group has flattened out, and three years after the investment, average daily wages start decreasing for both groups. Overall, the average daily wages of workers from MNEs remain at a higher level throughout the observation period. In view of the descriptive evidence, the gap appears to widen after the investment.

In the next section, I use event study DiD models to analyze the extent to which the presented group differences in the outcome variables are effects of FDI. The DiD identification strategy requires that the outcome variables follow parallel trends in the absence of treatment. Despite some level differences, all outcome variables under study meet the parallel trends assumption before the base year according to Figure 3.3. I therefore assume that the outcome variables would follow similar trends in a counterfactual situation without treatment.

3.4 Results

In this section, I estimate the effects of employers' investment in the Czech Republic on workers' labor market outcomes. First, I analyze the effects of FDI on workers' annual earnings. Second, I examine whether the effects on annual earnings are driven by changes in employment or changes in average daily wages. Third, I analyze wage and employment changes within the initial firm.

The estimations refer to the event study DiD model introduced in Section 3.2. The effect of FDI is given by the difference in the time-to-event coefficients of the treatment and the control group, $\beta_k - \gamma_k$. For better overview, I present graphs that depict the effects of FDI in single years, starting five years before and ending five years after the investment. Observing potential pre-investment effects is sensible for two reasons. First, although the year of investment is available in the data, it is less clear when firms made the investment decision. By defining two years prior to the investment as the reference year, I consider that the decision may affect workers even before the actual year of investment. The event study design makes it possible to detect these pre-investment changes, following the same logic as an Ashenfelter (1978) dip. Second, if the matching procedure, cohort sampling, and event study specification make trends between future MNEs and domestic firms effectively comparable in absence of FDI, the DiD estimates before the base year should be small and insignificant.

3.4.1 Effects on annual earnings

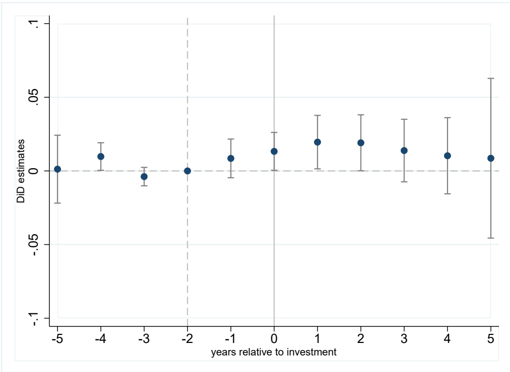
Figure 3.4 presents the effects of FDI on workers' annual earnings. Annual earnings are sensitive to wage changes as well as changes in the employment margin and can be computed independent of whether workers are in employment and the firms that employ them. Thus, they capture not only effects on workers who stay within the MNEs, but also effects on workers who leave the MNEs after the investment. For example, if workers lose their job in the aftermath of FDI, struggle with re-entering employment and take on new jobs that pay lower wages, annual earnings will include these effects, too.

In Panel A of Figure 3.4, I consider the entire cohort of workers employed in the treatment and control firms in the base year. The results support the assumption that relative earnings before treatment do not differ significantly between workers in MNEs and domestic firms, given that most of the estimates before the base year are insignificant. The only exception is the estimate for year -4, which is weakly significant at the 5% level. However, as the deviations in the period before the base year are not systematically higher or lower than zero, I conclude that a single weakly significant effect does not harm the overall assumption that pre-investment differences are zero.

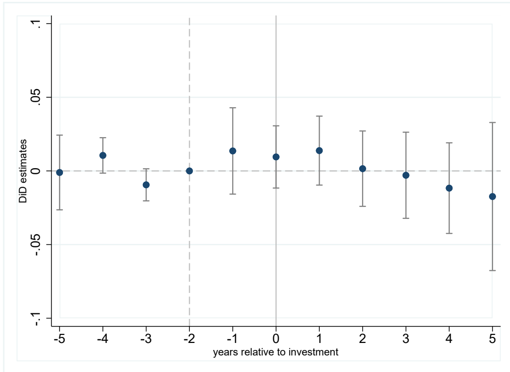
After the base year, estimates are positive, indicating that FDI has some positive effect on annual earnings. The effect becomes weakly significant at the 5% level in the year of investment and the two following years, before turning insignificant three years after the investment and onwards. The DiD estimate of 0.0133 in the year of investment indicates that annual earnings are 1.33 percentage points higher than they would have been if the firm had not invested. One and two years after the investment, the effect of FDI amounts to 1.96 and 1.91 percentage points, respectively.

To illustrate how these effects are calculated, Column 1 of Table 3.A.6 in the Appendix provides the corresponding event study coefficients from estimating Equation (1). In the year of investment, the estimate is -0.0011 for treated individuals and -0.0144 for workers in the control group. Thus, relative to the base year, both groups experience a decline in earnings. Workers from MNEs earn 0.11% less and workers in the control group earn 1.44% less on average. However, the point estimate is only significant for the control group. The decline in real earnings is in line

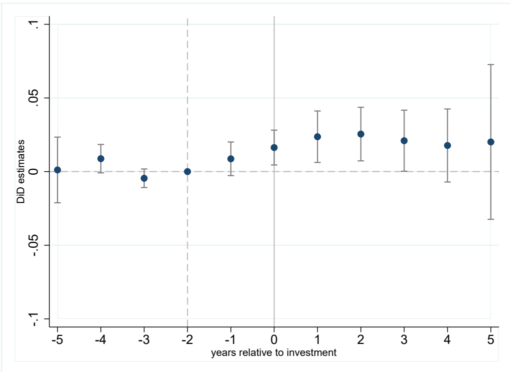
Figure 3.4: Effects of FDI on annual earnings



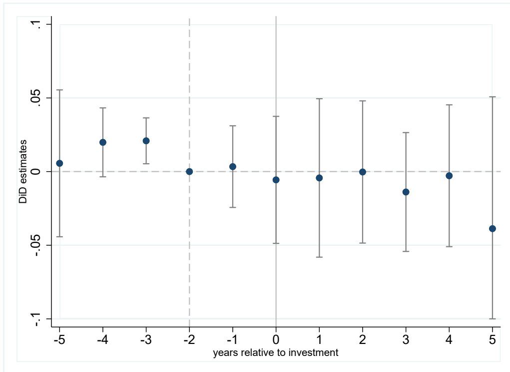
Panel A: All workers



Panel B: Low-skilled workers



Panel C: Medium-skilled workers



Panel D: High-skilled workers

Notes: The figures show DiD estimates from Equation (1) with annual earnings as the outcome variable. Annual earnings are scaled by the individual value in the base year. Dashed vertical lines mark the base year -2, and solid lines mark the year of investment. The dots indicate the estimated effects of FDI on the outcome variable according to $\beta_k - \gamma_k$ (see Table 3.A.6 in the Appendix). The estimate depicted for year -5 bins the effects up to five years before the investment, and year five bins the effects five years after the investment and onwards. Whiskers illustrate 95% confidence intervals. They are cut at -0.1 and 0.1.
Sources: ReLOC, IEB, and BHP, own calculations.

with the descriptive evidence provided in Panel B of Figure 3.3 and plausible, given that workers can have zero earnings in years of unemployment.

On average, treated workers earn 35,510.35 Euros in the base year. Their predicted earnings loss of 0.11% corresponds to a loss of 39.06 Euros in real annual earnings. According to the model, their relative loss in the absence of FDI would have been just as high as in the control group, namely 1.44% or 511.35 Euros. Thus, the average effect of FDI on annual earnings amounts to a 472.29 Euros in the year of investment. This effect is statistically significant, yet quite small given the average annual income of approximately 35,000 Euros.

In contrast to the control group, workers in the treatment group do not have any significant decrease in earnings in the years right after the base year (see Column 1 in Table 3.A.6). In the year of investment and the two following years, they are significantly better off than the control group. The estimated effects within this period range from 0.01 percentage points to 3.81 percentage points (lower and upper bound of the 95% confidence interval two years after the investment). In terms of real annual earnings, the positive effect of FDI amounts to a minimum 3.55 Euros and a maximum 1,352.95 Euros. Thus, the spread of the estimated effect is high. Nevertheless, the effect in year two is weakly significant, which explains the very low value for the lower bound. On average, the predicted gain in earnings is 678.25 Euros in year two after the investment.

The estimates' rather large confidence intervals also result from the design of the cohort sample and the nature of event study designs. The cohort is fixed in the base year and consists of workers who are employed either at a treatment or at a control firm. After the base year, various factors that influence earnings may come into play—for example, workers may switch firms and occupations, become unemployed, or receive further degrees. The impact of such changes on earnings may be greater than the effect of FDI by the initial firm. However, these various events can themselves be a result of FDI, and therefore I do not control for them. Consequently, the heterogeneity of workers' careers increases the deviation in estimated earnings and affects the estimated impact of FDI. Moreover, as time goes by after the investment, the other events become more likely to dilute the original effect on earnings. Consequently, confidence intervals increase with distance to the base year. For example, a worker may switch from a domestic firm to an MNE, or between MNEs. If these switches are systematically different between workers in the treatment group and workers in the control group, they may be one explanation why the original effect of FDI loses impact over time. In a robustness test in Section 3.5, I check whether switches to other employers within the MNE sample influence the results.

The remaining panels of Figure 3.4 show the effects of FDI on workers of different skill levels (as of the base year).[23] On the one hand, if German firms invest in the Czech Republic to save labor costs and replace labor-intensive low-skilled jobs by foreign production and these investments require more high-skilled workers who

[23]Skill groups are defined as follows: Low-skilled workers are workers that do not hold any occupational degree, medium-skilled workers hold occupational degrees, and high-skilled workers hold university degrees.

manage and coordinate the MNEs, one would expect to find different effects for different skill groups. On the other hand, if FDI makes firms more productive, positive effects independent of workers' skill levels are likewise conceivable.

The graphs in Panels B and D reveal that FDI does not affect the annual earnings of low- and high-skilled workers. Their earnings effects remain insignificant throughout.[24] Compared to the control group, they appear to neither benefit nor incur disadvantages from the investment decision of their initial employer. By contrast, the findings in Panel C indicate that medium-skilled workers have positive earnings effects from FDI. The effects are significant in the year of investment and three subsequent years. As medium-skilled workers are the largest group in the data, it seems that the positive effects for this group drive the results for all workers depicted in Panel A. Moreover, the estimated effects for medium-skilled workers range from 1.63 to 2.55 percentage points and are of comparable size as the effects for the full sample.

The results thus far do not reveal any negative effects of FDI on annual earnings on average, although annual earnings capture earnings losses due to unemployment or job changes. Instead, I find a positive effect on earnings of medium-skilled workers. In the following, I further examine whether the effects on annual earnings are driven by changes in the employment margin or changes in average daily wages.

3.4.2 Effects on days in employment and daily wages

Figure 3.5 presents the effects of FDI on days in employment for the entire cohort of workers and for the three skill groups. Panel A shows that by and large, FDI does not affect the number of days in employment. The effect is positive and significant only in the year of investment, where workers from MNEs are an extra 3.3 days in employment compared to workers in the control group. However, this is only one significant effect in the observed period rather than some general trend. Whereas Panel B and D show that FDI does not affect the number of days that low- and high-skilled workers are employed per year, Panel C shows that FDI has a significant positive effect on the days in employment of medium-skilled workers in the year of

[24]For high-skilled workers, the effect is significant in year -3. Again, I do not consider this single significant estimation to be a major problem. Additionally, the earnings and wage estimations for high-skilled workers entail greater uncertainty because a larger share of their earnings is based on imputed wages.

investment and the year after. The estimates for these years indicate that relative to the control group, FDI increases the number of days in employment by 3.7 and 4.7 days, respectively.

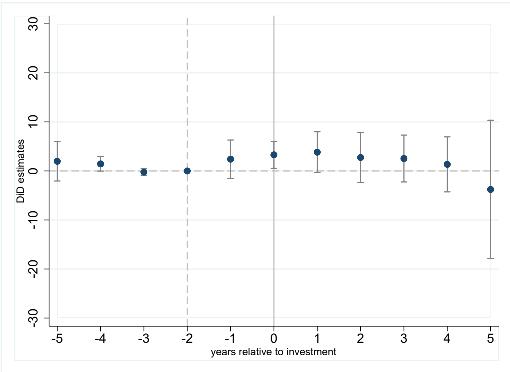
Note that according to the results in Table 3.A.7 in the Appendix, the days in employment decrease over time on average over all workers, independent of FDI. This is plausible given that effects are estimated relative to the base year. In the base year, workers are in employment close to 365 days on average due to the sampling requirements. In later years, their employment may decrease due to periods of unemployment.

In the absence of any significant negative effects on the days in employment of the skills groups, I conclude that FDI does not substantially increase workers' unemployment risk. Rather, medium-skilled workers benefit from their firms' decision to invest. The positive impact of FDI on their number of days in employment is one explanation why FDI raises annual earnings shortly after the investment. Another potential explanation is that FDI has a positive effect on their daily wages. I investigate this possibility in the following analyses.

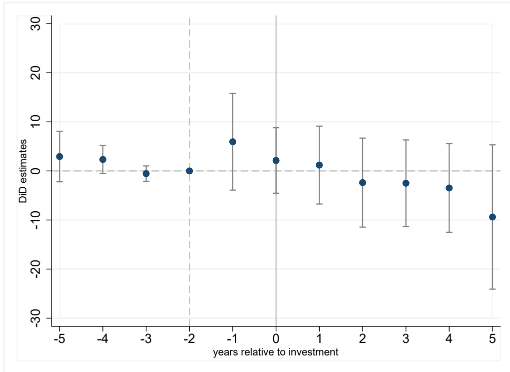
Figure 3.6 illustrates how FDI affects log average daily wages. In contrast to earnings, the average daily wage measure is conditional on employment. I calculate average daily wages across all employment spells within a year, irrespective of whether the job is full- or part-time. Because I cannot observe the hours worked per day in the data, this calculation comes with the disadvantage that changes in my daily wage measure may result from changes in hourly wages or changes in hours worked per day. However, its key advantage is that it accounts for any employment opportunities after the investment. If I restricted the analyses to full-time employment spells, results would possibly suffer from selection bias.

The results of the log daily wage estimations are provided in Table 3.A.8 in the Appendix. The within-worker estimates reveal a decline in average daily wages independent of FDI. In both the treatment and control group, low- and medium-skilled workers have falling average daily wages relative to the base year, whereas the estimates for high-skilled workers are not significant. Given that sampling required workers to be employed full-time in the initial firm in the base year, the decline in average wages also mirrors switches from full- to part-time employment. Besides a reduction in the hours worked, the decline in average wages can also result from a

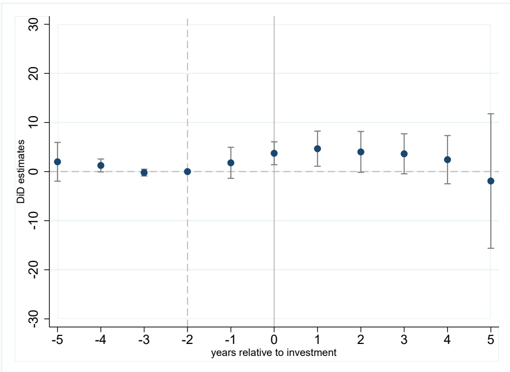
Figure 3.5: Effects of FDI on days in employment



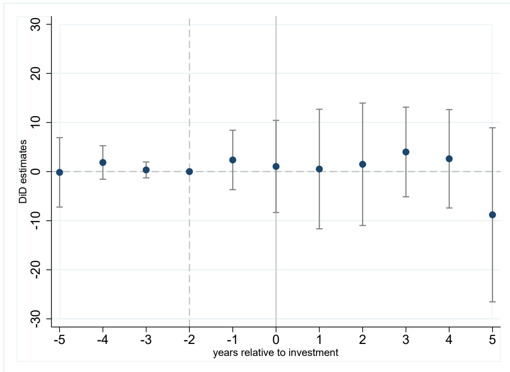
Panel A: All workers



Panel B: Low-skilled workers



Panel C: Medium-skilled workers



Panel D: High-skilled workers

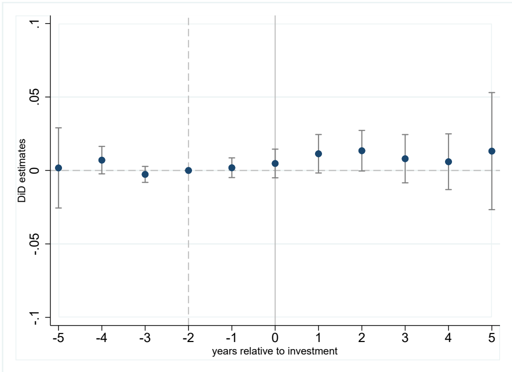
Notes: The figures show DiD estimates from Equation (1) with annual days in employment as the outcome variable. Dashed vertical lines mark the base year -2, and solid lines mark the year of investment. The dots indicate the estimated effects of FDI on the outcome variable according to $\beta_k - \gamma_k$ (see Table 3.A.7 in the Appendix). The estimate depicted for year -5 bins the effects up to five years before the investment, and year five bins the effects five years after the investment and onwards. Whiskers illustrate 95% confidence intervals. They are cut at -30 and 30.
Sources: ReLOC, IEB, and BHP, own calculations.

decline of hourly wages.[25] Moreover, the negative trend in average wages of low- and medium-skilled workers shows that the decline in annual earnings is not solely driven by unemployment periods.

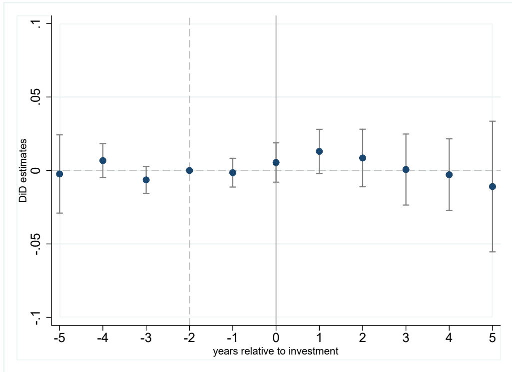
Turning to the question how FDI affects individual daily wages, Figure 3.6 reveals that overall, FDI does not have any substantial effect on the average daily wages of

[25]Real wage losses in Germany are well-documented. For example, Dustmann *et al.* (2009, 2014) show that workers in the lower percentiles of the wage distribution have incurred particularly severe real wage losses since the end of the 1990s.

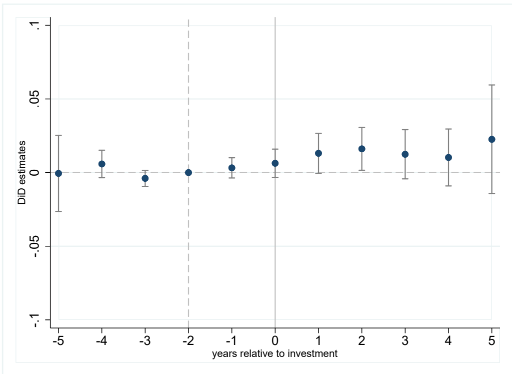
Figure 3.6: Effects of FDI on average daily wages



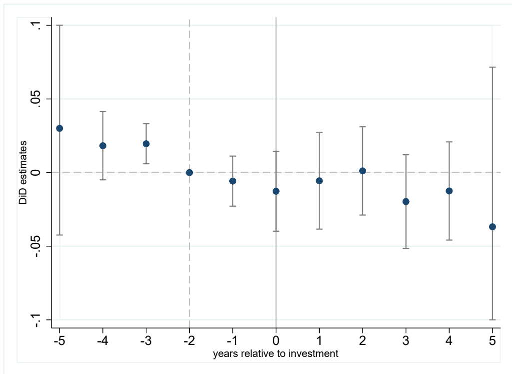
Panel A: All workers



Panel B: Low-skilled workers



Panel C: Medium-skilled workers



Panel D: High-skilled workers

Notes: The figures show DiD estimates from Equation (1) with log average daily wages per year as the outcome variable (conditional on employment). Dashed vertical lines mark the base year -2, and solid lines mark the year of investment. The dots indicate the estimated effects of FDI on the outcome variable according to $\beta_k - \gamma_k$ (see Table 3.A.8 in the Appendix). The estimate depicted for year -5 bins the effects up to five years before the investment, and year five bins the effects five years after the investment and onwards. Whiskers illustrate 95% confidence intervals. They are cut at -0.1 and 0.1.
Sources: ReLOC, IEB, and BHP, own calculations.

workers. Effects are statistically insignificant for workers of all skill groups.[26] This result is somewhat surprising against the descriptive evidence that suggests that workers in the treatment group have a larger increase in log average wages than workers in the control group (see Figure 3.3). However, the event study analysis clearly demonstrates that these differences in wage trends are not attributable to the FDI event.

All in all, the results show that the effects of FDI on workers' careers are minor.

[26]There are single significant observations for medium-skilled workers in year two and high-skilled workers in year -3 only.

There is some evidence that FDI affects the earnings of medium-skilled workers positively. Moreover, the improvement in earnings of medium-skilled workers mainly appears to result from a positive effect of FDI on their days in employment. As the outcome variables were aggregated over all employers, the previous analyses cannot show whether these results are driven by employment and wage effects at the investing firm. In the next subsection, I therefore take a closer look at days in employment and daily wages at the initial firm.

3.4.3 Effects on days in employment and daily wages at the initial employer

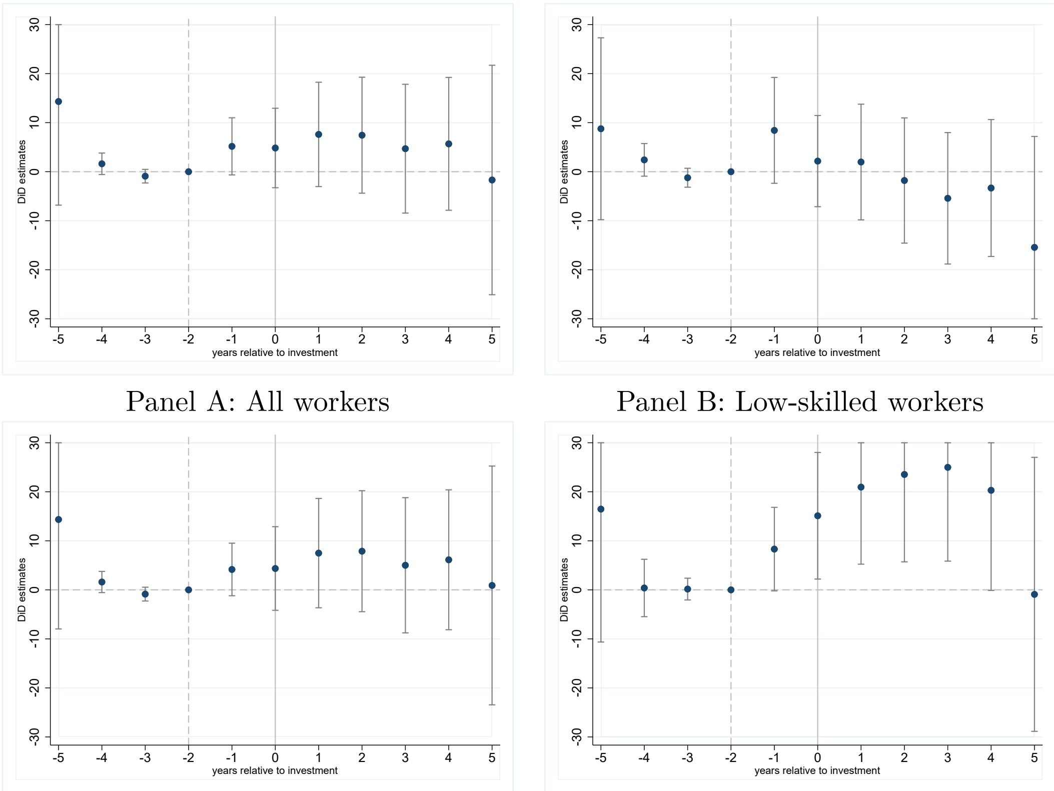
In the following, I focus on the effect of FDI on the employment and daily wages of workers at their initial firm.[27] For example, if workers separate from MNEs but quickly find new jobs, this would explain why I do not find any negative effects of FDI on days in employment overall. To better detect separations, I consider days in employment at the initial firm in the following.

Figure 3.7 presents the effects of FDI on annual days in employment at the initial firm. The days in employment are now zero for all days a person works at another firm or is unemployed. Whereas Panel A shows that FDI has no effect on the days in employment at the initial firm on average, Panels B to D reveal that effects differ by skill group. The insignificant effect for the entire cohort mainly comes from low- and medium-skilled workers, whose days in employment are not affected by the investment decision of their employer. High-skilled workers, on the other hand, experience an increase in days in employment at their firms following the investment relative to the workers at the control firms. The effect becomes significant in the year of investment and continuously increases for three years. Starting at an estimated increase of 15.1 days in year 0, the employment effect keeps growing until reaching its peak at 25.0 days three years after the investment.

Regarding the effects of FDI on average daily wages at the initial firm, Panel A in Figure 3.8 shows that workers benefit one and two years after the investment if they stay at the MNE. Compared to stayers at domestic firms, FDI increases average daily wages of stayers at MNEs by 1.09 percentage points one year after the investment

[27]By conditioning on employment at the initial firm, I can refrain from showing the results for annual earnings, which are measured independent of being in employment.

Figure 3.7: Effects of FDI on days in employment at the initial firm



Panel A: All workers

Panel B: Low-skilled workers

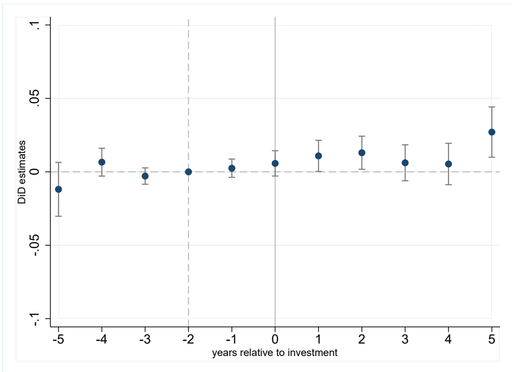
Panel C: Medium-skilled workers

Panel D: High-skilled workers

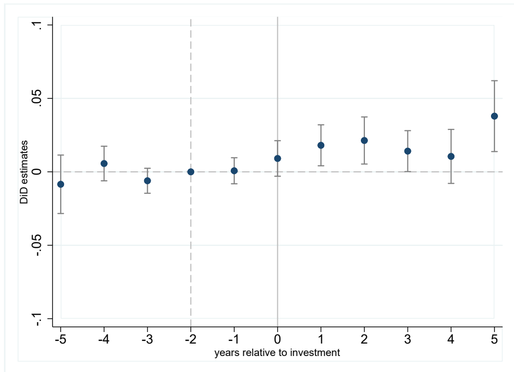
Notes: The figures show DiD estimates from Equation (1) with annual days in employment at the initial firm as the outcome variable. Dashed vertical lines mark the base year -2, and solid lines mark the year of investment. The dots indicate the estimated effects of FDI on the outcome variable according to $\beta_k - \gamma_k$ (see Table 3.A.9 in the Appendix). The estimate depicted for year -5 bins the effects up to five years before the investment, and year five bins the effects five years after the investment and onwards. Whiskers illustrate 95% confidence intervals. They are cut at -30 and 30.
Sources: ReLOC, IEB, and BHP, own calculations.

and 1.30 percentage points two years after the investment. Thereafter, the effect is insignificant. With respect to skill group differences, low-skilled workers experience a wage increase due to FDI. Compared to stayers at domestic firms, their average daily wages increase by 1.81 percentage points in year one after the investment, 2.13 percentage points in year two, and 1.41 percentage points in year three. Medium-skilled workers likewise benefit from FDI. FDI significantly increases their average wages by 1.10 percentage points in the year after the investment, and 1.29 percentage points a year later. Effects on high-skilled workers' daily wages are insignificant in most years following the investment. The only significant effects occur three years before and after the investment. Note that the sample of high-skilled workers has more imputed wages in the data because wage-censoring is more common within this group. Wage estimations for high-skilled workers therefore come with greater uncertainty, which is also reflected by their greater confidence intervals, and less reliability. I will discuss the broader findings for daily wages along with the other results in the last part of this section.

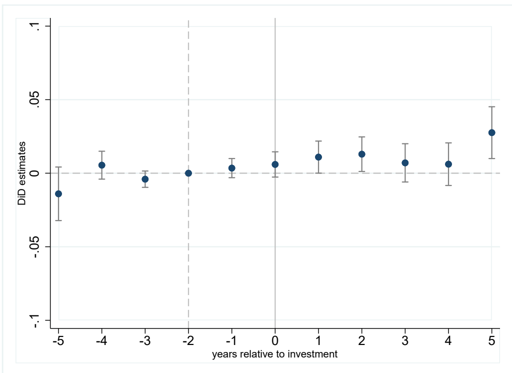
Figure 3.8: Effects of FDI on average daily wages at the initial firm



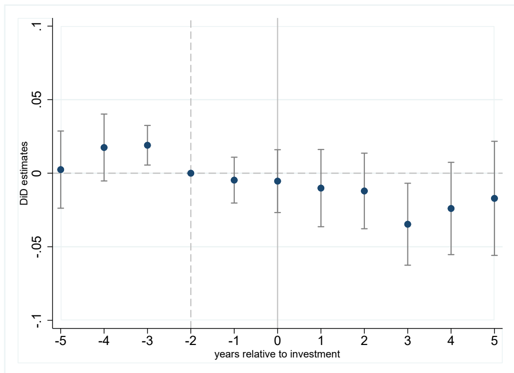
Panel A: All workers



Panel B: Low-skilled workers



Panel C: Medium-skilled workers



Panel D: High-skilled workers

Notes: The figures show DiD estimates from Equation (1) with log average daily wages per year as the outcome variable (conditional on employment at the initial firm). Dashed vertical lines mark the base year -2, and solid lines mark the year of investment. The dots indicate the estimated effects of FDI on the outcome variable according to $\beta_k - \gamma_k$ (see Table 3.A.10 in the Appendix). The estimate depicted for year -5 bins the effects up to five years before the investment, and year five bins the effects five years after the investment and onwards. Whiskers illustrate 95% confidence intervals. They are cut at -0.1 and 0.1.
Sources: ReLOC, IEB, and BHP, own calculations.

Notably, effects of FDI on wages of low- and medium-skilled stayers in Panels B and C as well as the full sample of stayers in Panel A are significant five years after the investment. These estimates should be interpreted with some caution. Within the event study design, the estimates depicted at the borders of the time window in years -5 and five also include effects that occur before or after the respective year. The administrative data only cover years between 1990 and 2010 and, thus, some individuals may not be observable beyond the effect window depending on the year of investment.[28] Notwithstanding this issue, workers who can be observed over a longer period of time in my data appear to have long-term daily wage benefits of FDI. Accordingly, when restricting the data to observations in the specific years instead of binning the effects at the endpoints, the wage effects for year five after the investment turn insignificant.[29]

3.4.4 Discussion of the empirical findings

In the following, I sum up the empirical findings and discuss their main implications. First, I do not find any negative effects of FDI on the incumbent workforce. In my baseline analyses, I consider the effects of FDI on earnings independent of workers' employers, which avoids selection bias that might occur if stayers are a positive selection of workers. Results show that FDI has a slightly positive effect on annual earnings in the years right after the investment. This effect is driven by a positive effect of FDI for medium-skilled employees, for whom I also find a positive effect of FDI on their days in employment. Further analyses reveal that this positive effect on employment does not result from more days in employment at the initial firm. Instead, the results suggest that medium-skilled workers who start out in future MNEs have better options of finding employment outside the firm than workers who start out in domestic firms. One explanation might be that medium-skilled workers hold vocational qualifications that facilitate job matching and allow them to find new jobs with other employers relatively quickly. Moreover, medium-skilled workers from MNEs may anticipate getting replaced following FDI in low-wage countries and therefore start looking for outside options of employment early on to cope with potential job losses. By contrast, workers in domestic firms,

[28] Consider, for example, that if a firm invests in year 2005, I will be able to observe its initial workforce until 2010. Because the data only cover years up to 2010, I cannot observe workers at any later point in time. Thus, the earlier an investment takes place, the longer will I be able to follow workers' subsequent outcomes. Earlier investments will therefore be overrepresented in the estimation of the effects for year five.

[29] These results are available from the author upon request. In Section 3.5, I likewise test whether the estimated effects on earnings for year -5 and five remain robust when restricting the data to observations in the specific years only.

who do not face this threat, may less proactively search for new jobs and therefore incur longer unemployment spells when losing their job than workers starting out at MNEs. However, this paper finds that FDI does not lead to fewer days in employment at MNEs for any skill group, suggesting that medium-skilled workers are not particularly exposed to such threats.

Second, I neither find any significant negative effects of FDI within the initial firm. On the contrary, stayers benefit from the investment decision of their employer. Consistent with the assumption that MNEs have higher skill requirements regarding foreign languages, communication, and supervision (see, e.g., Laffineur & Gazaniol, 2019), high-skilled workers are positively affected by FDI in terms of employment at the MNEs. The positive effect indicates that they are less likely to leave the MNEs after the investment than high-skilled workers who start out at domestic firms. Nevertheless, I find no positive effect of FDI on their average daily wages at the MNEs, which might also be due to less precise wage information for the high-skilled group.

Whereas FDI does not affect the days in employment of low- and medium-skilled workers at the initial firms, these groups benefit from FDI in terms of higher daily wages at the initial firm. The positive daily wage effect for those who stay at MNEs is in line with the literature that predicts positive effects of firms' internationalization on the domestic workforce as a result of improved productivity (see, e.g., Grossman & Rossi-Hansberg, 2008). Moreover, the effect is consistent with models that consider heterogeneous firms and labor market frictions (e.g., Amiti & Davis, 2012; Egger & Kreickemeier, 2009) and show that wages will increase if firms share additional rents from internationalization with their workforce. In contrast, the positive wage effect is incompatible with the assumption that in the face of relocation threats, employees and unions might lower their wage demands (see, e.g., Choi, 2001; Geishecker & Görg, 2008; Goeddeke *et al.*, 2018; Laffineur & Gazaniol, 2019).

All in all, the employment effects at the initial firm are in line with previous findings. As demonstrated in Chapter 2, FDI in the Czech Republic appears to lower the risk of separation for medium- and high-skilled workers while marginally increasing the risk of separation for low-skilled workers. Accordingly, I find in the paper at hand that FDI positively affects the number of days in employment at the MNEs for high-skilled workers. Although the effects for medium- and low-skilled workers are insignificant in this paper, they hint in the same direction as in Chapter 2. It is reassuring that the different estimation methods used in Chapter 2 and the paper at hand yield comparable results.

With regard to the broader literature, my results are consistent with Becker & Muendler (2008), who also show for Germany that FDI in general and FDI in CEEC in particular improves worker retention mainly among high-skilled workers. Competing results are presented by Bachmann *et al.* (2014), who find that FDI in CEEC significantly reduces employment security, especially for low-skilled workers. However, they measure FDI at the industry level and thus, their analyses are not directly comparable to analyses using measures of FDI at the firm level. Bachmann *et al.* (2014) argue that some firms may gain market shares through FDI, which may come at the expense of their domestic competitors within the same industry. My data do not extend to such industry-wide effects of FDI and therefore I cannot provide insights into competition between firms within industries. In another study that relies on the same firm-level database as the paper at hand, Schäffler & Moritz (2018) find that FDI reduces MNEs' labor demand overall, but increases their demand for high-skilled workers. In view of my finding that the employment at the initial firm is not negatively affected by FDI, it seems that the decrease in labor demand found by Schäffler & Moritz (2018) goes back to changes in hiring behavior or layoffs among workers with short tenure. Unfortunately, I cannot elaborate on the extent to which MNEs adjust their workforce by dismissing workers with short tenure or workers with other flexible arrangements, like temporary agency work, because my analyses are restricted to incumbent workers with longer tenure. Nonetheless, this matter remains an interesting field for future research.

Contrary to evidence on the effects of FDI on the employment margin, evidence on the effects of FDI on wages is scarce. In a recent study on the French labor market, Laffineur & Gazaniol (2019) show that FDI positively affects workers' hourly wages, in particular for managers and when FDI flows to low-income countries. Although my analyses yield no significant effects on high-skilled workers' daily wages, they also indicate that the high-skilled benefit from FDI with respect to their employment margin. Moreover, Laffineur & Gazaniol (2019) report negative effects of FDI on wages of workers in highly offshorable jobs. Although, I do not look at offshorability in particular, I do not find any negative effects of FDI for any skill group.

Moreover, the results of this paper can also be related to the literature on effects of offshoring. For example, Hummels *et al.* (2014) show that the wage effects of firm-level offshoring are negative for low-skilled and positive for high-skilled workers. They also find negative effects when taking the average earnings developments of cohorts employed before an offshoring shock into consideration. However, results based on offshoring data are hardly directly comparable to results based on FDI data. The former typically consider firms' imported inputs, whereas the latter typically include both vertical FDI, which resemble offshoring within the boundaries of a firm, and horizontal FDI, which aim at serving a foreign market on-site.

Although I do not find any significant negative effect of FDI on workers' days in employment at their initial firms, some workers who lose their employment at the MNEs may face earnings losses due to spells of unemployment or lower reemployment wages nonetheless. Thus, it would be particularly interesting to separately analyze the earnings trends for those who leave the MNE after FDI. However, such analyses are subject to severe selection bias, especially because one cannot easily distinguish between quits and layoffs in conventional data sets. If workers who leave MNEs are mainly laid off, they should be compared with workers that are laid off by domestic firms rather than with workers who quit. One way to cope with this problem is to compare workers who leave MNEs with workers who lose their job through mass layoffs, as has been done, for example, by Hummels *et al.* (2013) to identify effects of offshoring. Such comparisons are a promising field for future research, but require data on mass layoffs by domestic firms.

Overall, the positive results found in this paper may also reflect that German FDI in the Czech Republic is not only driven by factor-seeking motives but also by market-seeking motives.[30] In this regard, negative effects for the German workforce are even less expectable. In this context, future research could distinguish between vertical and horizontal investments. Another possibility is to examine the effects of FDI on workers' earnings using data on FDI in other destinations, for example, FDI in countries with wage levels below that of the Czech Republic.

Last, the significant effects of FDI on the workforce reported in this paper are positive. One may therefore question whether the skill groups considered in this paper represent more and less offshorable jobs. After all, the offshorability of jobs depends on the tasks carried out within them (see, e.g., Leamer & Storper, 2001; Autor *et al.*, 2003; Blinder, 2006). Because tasks are more attached to occupations

[30]See results of the ReLOC survey by Hecht *et al.* (2013a), who show that both motives are equally important for FDI in the Czech Republic.

than to skills, I also test the results by occupational groups using the classification by Blossfeld (1987). This classification distinguishes between different skill levels as well as manual, service, and administrative occupations. Small numbers of observations for some occupational classes require further grouping—for example, I subsume managers and engineers under high professionals. Using these groups instead of skill groups yields very comparable findings.[31] I do not find any negative effect for workers in unskilled manual or service jobs, which should have the greatest offshoring potential.

3.5 Robustness checks

I run several robustness checks. First, to ensure that the effect of FDI by the initial employer is not diluted by job changes to other MNEs, I exclude all workers from the analysis who work in another than the initial MNE of the ReLOC sample after the base year. This applies to only 3.01% of the sample. The graphs in Figure 3.A.1 in the Appendix reveal that when excluding these workers, the main results remain unchanged. FDI still has significant effects on annual earnings of medium-skilled workers in the year of investment until two years after. The effects also remain comparable in size. I conclude that changes of workers to other MNEs do not dilute the results, either because workers from MNEs and domestic firms do not systematically differ in their probability to find subsequent employment in another MNE, or simply because these job changes do not significantly affect earnings. However, because my data do not allow me to observe firms' investments in countries other than the Czech Republic, I can only partly control for effects on earnings from other firms' investments.

Second, I check whether the effect of FDI on annual earnings is driven by workers who do not have any spells in employment subject to social security contributions. I presumed that these workers will have zero annual earnings once they have no employment spell in the data and, thus, their annual earnings will dramatically fall after the base year. Yet, I cannot be sure that they become unemployed—they may just as well become self-employed, take a gap year, or take on a job abroad, and thereby leave the German social security system. Depending on whether the number of workers from MNEs that make such transitions exceeds that of workers

[31] Results are available from the author upon request.

from domestic firms, or vice versa, my approach will under- or overestimate the effect of FDI on earnings. Figure 3.A.2 in the Appendix shows that after dropping all workers with zero earnings, the estimation results hardly change. Compared with the baseline findings, only a few effects have lost significance. I conclude that any measurement errors that result from setting unobserved earnings to zero are negligible. Nevertheless, I prefer the baseline specification, which captures full years of unemployment with zero earnings and therefore provides a more comprehensive picture.

In the estimations, I binned the effects for all years before $k = -5$ and after $k = 5$ in the respective time-to-event dummy. In a third robustness check, I examine whether I miss out on any significant effects of FDI by binning them. Figure 3.A.3 in the Appendix shows the results that emerge if the time-to-event-dummies for year -5 and five capture the effects for these specific years only. Estimates do not change much, and there are no significant effects of FDI on annual earnings five years after the investment. This does not come unexpected, given that no effects of the baseline estimations on earnings were significant for the year before year five and after year -5, respectively. The test shows that binning the effects does not seem to influence my baseline conclusions.

3.6 Conclusion

The objective of this paper is to analyze how a firm's decision to set up or acquire affiliates abroad affects individual workers over their careers. In extension of the previous literature, I analyze developments in workers' annual earnings. Studying workers' earnings developments independent of the employer allows me to identify effects of FDI on workers within investing firms (e.g., Laffineur & Gazaniol, 2019) along with effects on workers who leave firms after FDI (see studies on the effect of FDI on job security, e.g., Becker & Muendler, 2008; Bachmann *et al.*, 2014). Thus, I can account for FDI-induced wage changes within the initial firm, transitions into unemployment as well as transitions between firms—for example, when workers are dismissed from the MNE and have to take on new jobs at other employers paying lower wages. As changes in earnings can result from both employment and wage changes, I additionally analyze FDI-induced changes in the number of days in employment and in daily wages.

To identify the effects of FDI, I use employer-employee data on German firms that invest in the Czech Republic and comparable German firms that do not invest abroad, and apply an event study DiD design. My results show that the average effect of FDI on workers' overall earnings is minor and positive at best. Further analyses reveal that on average, FDI has no negative effect on the days in employment at the MNE, which indicates that on average workers do not lose their jobs due to FDI. In fact, groups of workers will benefit from FDI in different ways. For medium-skilled workers, I find that FDI improves annual earnings and the days in employment overall. Interestingly, their duration in employment at the MNE remains unaffected by FDI. I conclude that medium-skilled workers have good opportunities of finding work outside their MNEs. By contrast, I do not find any effect of FDI on the earnings of low- and high-skilled workers overall. Nevertheless, these workers will benefit from FDI if they stay with their firm after the investment. Low- and medium-skilled workers benefit in terms of higher daily wages, and high-skilled workers benefit in terms of an extended duration in employment at the MNEs.

To sum up, the results of this paper imply that on average, workers do not incur disadvantages over their careers after exposure to FDI, as I find no negative effect of German FDI in the Czech Republic on the annual earnings of workers. These findings stand in contrast to common concern that FDI in low-wage countries leads to job and wage losses among the domestic workforce. Rather, the empirical findings reveal that the effects for workers who stay with the investing firms are positive overall.

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3.A Appendix

3.A.1 Firm-level data set

Table 3.A.1 shows characteristics of the firms in the ReLOC sample before matching. Note that not all firms from the Czech Commercial Register are found in the German administrative data. Thus, descriptives are only provided for a subset of firms from the original data set. Moreover, I drop public firms, private households, and extra-territorial organizations. Additionally, firms are required to have more than one employee in the year of treatment. I further exclude very large MNEs with more than 30,000 employees because the largest control firm has approximately 23,000 workers. Some firms did not exist two years prior to investment. Table 3.A.1 therefore includes fewer MNEs than the original data set.

Table 3.A.1: Firm characteristics of the sample before matching

	MNEs two years before FDI			MNEs two years after FDI			domestic firms		
	obs.	mean	std.	obs.	mean	std.	obs.	mean	std.
No. of employees (emp.)	1,996	383.8868	1133.8480	2,164	382.5873	1113.0740	7,767	185.5134	420.8820
Employment growth rate	1,992	0.3699	2.4167	1,870	0.3816	5.0398	7,767	0.6443	4.2290
Av. wage	1,996	88.8361	38.0267	2,164	98.4771	43.4784	7,767	82.9027	32.1573
Wage growth rate	1,992	0.0717	0.1748	1,867	0.0736	0.1796	7,767	0.0584	0.1252
Firm age	1,996	15.2169	8.5461	2,164	17.4205	9.5959	7,767	16.0055	8.8191
Av. age	1,996	38.3412	4.9081	2,164	39.4725	4.8128	7,767	39.3095	4.4908
Share of female emp.	1,996	0.3539	0.2322	2,164	0.3559	0.2239	7,767	0.3828	0.2589
Share of full-time emp.	1,996	0.8609	0.1509	2,164	0.8367	0.1639	7,767	0.7707	0.2082
Share of low skilled	1,996	0.1486	0.1396	2,164	0.1358	0.1257	7,767	0.1519	0.1280
Share of medium skilled	1,996	0.7065	0.1922	2,164	0.7007	0.1897	7,767	0.7289	0.1720
Share of high skilled	1,996	0.1304	0.1794	2,164	0.1499	0.1839	7,767	0.1019	0.1447
Share of unsk.-man. emp.	1,996	0.2197	0.2585	2,164	0.1986	0.2429	7,767	0.1786	0.2399
Share of engineers etc.	1,996	0.0303	0.0800	2,164	0.0311	0.0758	7,767	0.0226	0.0671

Notes: The table shows the means and standard deviations (std.) of various characteristics of MNEs and domestic firms in the raw data before matching. It also includes the number of firms (obs.). The table reports the values two years before the investment and two years after the investment for MNEs and averages over all years for domestic firms. "Share of unsk.-man." provides the share of unskilled-manual employees.
Source: ReLOC and BHP, own calculations.

Table 3.A.2 summarizes characteristics of the firms in the base year after matching. The data set includes 803 treatment and 803 control firms. Tables 3.A.3 and 3.A.4 show the same characteristics for the firms in the year of investment and two years after.

Table 3.A.2: Descriptive characteristics of MNEs and domestic firms in the year of matching

	MNEs			domestic firms		
	obs.	mean	std.	obs.	mean	std.
No. of employees (emp.)	803	419.7347	1239.2510	803	258.2478	821.7283
Employment growth rate	803	0.1845	0.6201	803	0.3584	4.7879
Av. wage	803	91.0190	34.2067	803	87.8429	32.3952
Wage growth rate	803	0.0650	0.1644	803	0.0589	0.1114
Firm age	803	17.0025	7.6215	803	16.8281	7.9256
Av. age	803	38.5702	4.1891	803	38.6487	4.0183
Share of female emp.	803	0.3317	0.2074	803	0.3151	0.2204
Share of trainees	803	0.0340	0.0473	803	0.0340	0.0472
Share of regular emp.	803	0.9247	0.1061	803	0.9226	0.1134
Share of full-time emp.	803	0.8693	0.1372	803	0.8689	0.1576
Share of low skilled	803	0.1558	0.1356	803	0.1584	0.1399
Share of medium skilled	803	0.7073	0.1750	803	0.7186	0.1773
Share of high skilled	803	0.1244	0.1656	803	0.1085	0.1577
Share of German emp.	803	0.9095	0.1078	803	0.9097	0.1192
Share of unsk.-man. emp.	803	0.2352	0.2601	803	0.2403	0.2638
Share of engineers etc.	803	0.0313	0.0758	803	0.0282	0.0681
Year	803	1998.1920	3.3392	803	1998.1920	3.3392

Notes: The table shows the means and standard deviations (std.) of various characteristics of the matched MNEs and domestic firms two years before investment. It also includes the number of firms (obs.). The table is restricted to firms that are part of the sample used in the event study analysis, i.e., firms with an investment date between 1995 and 2005. “Share of unsk.-man.” provides the share of unskilled-manual employees.
Sources: ReLOC and BHP, own calculations.

Table 3.A.3: Descriptive characteristics of MNEs and domestic firms in the year of investment

	MNEs			domestic firms		
	obs.	mean	std.	obs.	mean	std.
No. of employees (emp.)	803	437.8257	1216.6570	803	266.0137	832.9637
Employment growth rate	803	0.1785	0.6679	803	0.1235	0.4924
Av. wage	803	94.8081	34.5165	803	91.0212	33.8516
Wage growth rate	803	0.0495	0.1441	803	0.0390	0.0928
Firm age	803	19.0025	7.6215	803	18.8281	7.9256
Av. age	803	39.1906	4.1229	803	39.4018	3.9465
Share of female emp.	803	0.3303	0.2022	803	0.3201	0.2233
Share of trainees	803	0.0348	0.0479	803	0.0359	0.0481
Share of regular emp.	803	0.9047	0.1188	803	0.9024	0.1242
Share of full-time emp.	803	0.8520	0.1448	803	0.8458	0.1693
Share of low skilled	803	0.1504	0.1312	803	0.1567	0.1360
Share of medium skilled	803	0.7085	0.1743	803	0.7199	0.1731
Share of high skilled	803	0.1289	0.1609	803	0.1099	0.1527
Share of German emp.	803	0.9152	0.1024	803	0.9136	0.1155
Share of unsk.-man. emp.	803	0.2319	0.2569	803	0.2393	0.2628
Share of engineers etc.	803	0.0310	0.0702	803	0.0270	0.0667
Year	803	2000.1920	3.3392	803	2000.1920	3.3392

Notes: The table shows the means and standard deviations (std.) of various characteristics of the matched MNEs and domestic firms in the year of investment. It also includes the number of firms (obs.). The table is restricted to firms that are part of the sample used in the event study analysis, i.e., firms with an investment date between 1995 and 2005. "Share of unsk.-man." provides the share of unskilled-manual employees.
Sources: ReLOC and BHP, own calculations.

Table 3.A.4: Descriptive characteristics of MNEs and domestic firms two years after investment

	MNEs			domestic firms		
	obs.	mean	std.	obs.	mean	std.
No. of employees (emp.)	796	459.8003	1242.5140	800	275.9375	779.4120
Employment growth rate	796	0.0854	0.3956	800	0.2353	3.3088
Av. wage	795	101.0260	43.4846	799	94.4538	36.4622
Wage growth rate	795	0.0607	0.1468	799	0.0396	0.1020
Firm age	796	21.0515	7.5957	800	20.8350	7.9364
Av. age	796	39.9654	4.1993	800	39.9294	4.0757
Share of female emp.	796	0.3321	0.2016	800	0.3229	0.2216
Share of trainees	796	0.0391	0.0564	800	0.0386	0.0492
Share of regular emp.	796	0.8885	0.1229	800	0.8823	0.1339
Share of full-time emp.	796	0.8362	0.1480	800	0.8269	0.1745
Share of low skilled	796	0.1443	0.1214	800	0.1549	0.1293
Share of medium skilled	796	0.7059	0.1697	800	0.7186	0.1659
Share of high skilled	796	0.1390	0.1635	800	0.1129	0.1505
Share of German emp.	796	0.9209	0.0932	800	0.9171	0.1154
Share of unsk.-man. emp.	796	0.2237	0.2497	800	0.2381	0.2631
Share of engineers etc.	796	0.0334	0.0729	800	0.0263	0.0585
Year	796	2002.1920	3.3353	800	2002.1890	3.3391

Notes: The table shows the means and standard deviations (std.) of various characteristics of the matched MNEs and domestic firms two years after investment. It also includes the number of firms (obs.). The table is restricted to firms that are part of the sample used in the event study analysis, i.e., firms with an investment date between 1995 and 2005. “Share of unsk.-man.” provides the share of unskilled-manual employees.
Sources: ReLOC and BHP, own calculations.

3.A.2 Worker-level data set

Table 3.A.5 summarizes individual characteristics of the sample of workers in the matched firms two years prior to investment. The data set includes 130,972 workers of the treated group and 72,723 workers of the control group.

Table 3.A.5: Descriptive characteristics of workers at MNEs and domestic firms in the year of matching

	workers at MNEs			workers at domestic firms		
	obs.	mean	std.	obs.	mean	std.
Daily wage at matched firm	130,972	97.8436	36.9363	72,723	93.3516	37.4512
Daily wage at all firms	130,972	98.1934	37.3139	72,723	93.7361	37.6956
Annual earnings	130,972	35510.3500	13724.5600	72,723	33834.8200	13826.5200
Annual days in employment	130,972	361.2475	22.0923	72,723	360.6259	23.9305
Age	130,972	38.2711	8.1085	72,723	38.4647	8.0980
Experience	130,972	15.5415	6.3781	72,723	15.0762	6.7818
Tenure	130,972	10.2296	6.3823	72,723	9.6993	6.4776
Share of low skilled	130,972	0.1624	0.3688	72,723	0.1452	0.3523
Share of medium skilled	130,972	0.7860	0.4102	72,723	0.7961	0.4029
Share of high skilled	130,972	0.0517	0.2214	72,723	0.0587	0.2351

Notes: The table includes the sample of workers used for the event study analysis. It shows the means and standard deviations (std.) of various characteristics of workers in MNEs and domestic firms in the base year, i.e., two years prior to investment. It also includes the number of workers (obs.).
Sources: ReLOC, IEB, and BHP, own calculations.

3.A.3 Event study results

Table 3.A.6 shows the regression output of estimating Equation (1) with annual earnings as the dependent variable. All estimates are based on the workforce that was employed in either a treatment or a control firm in the base year $k = -2$. Annual earnings are scaled by workers' earnings in the base year.

The variables in Table 3.A.6 present the time-to-event dummies for the treatment and for the control group. For all years relative to investment, the treatment dummies are one if a worker worked at an MNE in the base year and zero otherwise. Analogously, for all years relative to pseudo-investment, the control dummies are one if a worker worked at a control firm in the base year and zero otherwise. Because each worker can work either in a treatment or in a control firm in the base year, the inclusion of individual fixed effects requires dropping one year for both the treatment and the control group. For each group the reference year is $k = -2$.

The coefficients in Table 3.A.6 indicate group-specific within-worker changes of the dependent variable relative to the base year and controlling for calendar year fixed effects. They do not indicate the effect of FDI on the outcome variable. The effect of FDI is given by the difference between the time-to-event estimates of the treatment and the control group (see Figure 3.4).

Note that in the model with earnings as the dependent variable, the scaling of annual earnings by the value in the base year basically works like a first-difference estimator. The reason is that the model includes a relative change to the base year at the left side of Equation (1) and also at its right side, in form of the time-to-event dummies. Thus, the inclusion of worker fixed effects is not necessary for the earnings specification. However, to be consistent with the models for the other dependent variables, I present the same model for all dependent variables (see Tables 3.A.7 to 3.A.10).

Table 3.A.6: Event study results for annual earnings

	(1) all workers	(2) low-skilled workers	(3) medium-skilled workers	(4) high-skilled workers
treatment dummies, reference category: $k=-2$				
k=-5	-0.1524^{***}	-0.1577^{***}	-0.1407^{***}	-0.2582^{***}
	(0.0168)	(0.0100)	(0.0168)	(0.0340)
k=-4	-0.0289^{***}	-0.0339^{***}	-0.0260^{***}	-0.0568^{***}
	(0.0058)	(0.0042)	(0.0062)	(0.0101)
k=-3	-0.0033	-0.0106^{**}	-0.0036	0.0164^{***}
	(0.0033)	(0.0045)	(0.0032)	(0.0053)
k=-1	-0.0019	-0.0156^{**}	0.0002	0.0122^{*}
	(0.0047)	(0.0073)	(0.0045)	(0.0071)
k=0	-0.0011	-0.0220^{***}	0.0037	-0.0016
	(0.0032)	(0.0062)	(0.0034)	(0.0109)
k=1	-0.0022	-0.0313^{***}	0.0048	-0.0056
	(0.0052)	(0.0074)	(0.0054)	(0.0153)
k=2	-0.0078	-0.0479^{***}	0.0024	-0.0185
	(0.0061)	(0.0102)	(0.0067)	(0.0140)
k=3	-0.0133^{*}	-0.0582^{***}	-0.0020	-0.0219
	(0.0073)	(0.0139)	(0.0082)	(0.0191)
k=4	-0.0185^{**}	-0.0665^{***}	-0.0056	-0.0355^{*}
	(0.0087)	(0.0168)	(0.0090)	(0.0208)
k=5	-0.0859^{***}	-0.1433^{***}	-0.0636^{***}	-0.1687^{***}
	(0.0211)	(0.0315)	(0.0191)	(0.0298)
control dummies, reference category: $k=-2$				
k=-5	-0.1536^{***}	-0.1567^{***}	-0.1418^{***}	-0.2638^{***}
	(0.0156)	(0.0112)	(0.0154)	(0.0342)
k=-4	-0.0387^{***}	-0.0444^{***}	-0.0348^{***}	-0.0767^{***}
	(0.0054)	(0.0059)	(0.0053)	(0.0149)
k=-3	0.0005	-0.0011	0.0009	-0.0045
	(0.0032)	(0.0038)	(0.0033)	(0.0083)
k=-1	-0.0104^{*}	-0.0291^{**}	-0.0085^{*}	0.0088
	(0.0055)	(0.0130)	(0.0046)	(0.0115)
k=0	-0.0144^{**}	-0.0315^{***}	-0.0126^{**}	0.0040
	(0.0061)	(0.0082)	(0.0059)	(0.0174)
k=1	-0.0217^{**}	-0.0451^{***}	-0.0189^{**}	-0.0013
	(0.0085)	(0.0097)	(0.0085)	(0.0201)
k=2	-0.0269^{***}	-0.0494^{***}	-0.0231^{**}	-0.0182
	(0.0092)	(0.0103)	(0.0095)	(0.0219)
k=3	-0.0272^{***}	-0.0552^{***}	-0.0230^{**}	-0.0081
	(0.0105)	(0.0112)	(0.0110)	(0.0249)
k=4	-0.0288^{**}	-0.0549^{***}	-0.0233^{*}	-0.0327
	(0.0118)	(0.0141)	(0.0123)	(0.0289)
k=5	-0.0945^{***}	-0.1259^{***}	-0.0837^{***}	-0.1300^{***}
	(0.0179)	(0.0221)	(0.0189)	(0.0435)
N	4,145,318	645,379	3,289,864	210,075
R^2	0.395	0.438	0.392	0.391
Adjusted R^2	0.364	0.409	0.361	0.357
Adjusted within-R^2	0.0442	0.0691	0.0475	0.0658

Notes: The regression includes individual and calendar year dummies. Robust standard errors (in parentheses) are clustered at the level of the initial firm in the base year. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Estimates are based on a matched sample of MNEs and domestic firms.
Source: ReLOC, IEB, and BHP, own calculations.

Table 3.A.7: Event study results for days in employment

	(1) all workers	(2) low-skilled workers	(3) medium-skilled workers	(4) high-skilled workers
treatment dummies, reference category: $k=-2$				
k=-5	−38.0069***	−42.9048***	−35.3208***	−57.0812***
	(5.1196)	(3.3364)	(5.1951)	(6.9084)
k=-4	−10.8861***	−11.8430***	−10.3188***	−15.8496***
	(1.0757)	(0.9368)	(1.2070)	(1.7291)
k=-3	−2.9866***	−3.4387***	−3.0856***	−0.3490
	(0.8629)	(0.9207)	(0.8428)	(1.3524)
k=-1	−5.1408***	−6.5060**	−4.3309***	−12.8109***
	(1.4936)	(2.5793)	(1.3880)	(1.4452)
k=0	−6.7264***	−11.3173***	−4.6280***	−23.2519***
	(1.2419)	(2.5866)	(1.2663)	(2.4778)
k=1	−8.8145***	−16.3044***	−5.7967***	−30.1854***
	(1.3836)	(2.7343)	(1.5033)	(3.4001)
k=2	−10.5184***	−20.8919***	−6.6670***	−34.4479***
	(1.4115)	(3.1160)	(1.5961)	(3.1194)
k=3	−10.8693***	−22.2902***	−6.7682***	−35.3899***
	(1.6208)	(3.2168)	(2.0552)	(4.3112)
k=4	−11.7914***	−23.8452***	−7.3609***	−39.5908***
	(1.7369)	(3.3895)	(2.1234)	(4.5473)
k=5	−30.6641***	−45.8244***	−24.4293***	−72.5561***
	(3.6733)	(6.7303)	(3.1224)	(4.8739)
control dummies, reference category: $k=-2$				
k=-5	−39.9773***	−45.8353***	−37.3103***	−56.9206***
	(4.3951)	(3.3313)	(4.4743)	(6.3325)
k=-4	−12.3263***	−14.1791***	−11.5638***	−17.7013***
	(1.5620)	(1.3927)	(1.5873)	(2.4774)
k=-3	−2.7558***	−2.8943***	−2.8810***	−0.6843
	(0.7032)	(0.6155)	(0.7255)	(1.2482)
k=-1	−7.5462***	−12.4440***	−6.1122***	−15.1789***
	(1.6860)	(4.3354)	(1.3447)	(2.6489)
k=0	−10.0248***	−13.4482***	−8.3555***	−24.2968***
	(1.5805)	(2.3210)	(1.5199)	(3.5285)
k=1	−12.6418***	−17.4980***	−10.4553***	−30.6988***
	(2.2643)	(2.8932)	(2.2367)	(4.1185)
k=2	−13.2641***	−18.5162***	−10.6700***	−35.9345***
	(2.6563)	(2.8800)	(2.7105)	(4.8705)
k=3	−13.4069***	−19.7904***	−10.3886***	−39.3806***
	(2.8994)	(2.8645)	(3.0381)	(5.3578)
k=4	−13.1330***	−20.3737***	−9.7877***	−42.1951***
	(3.2272)	(3.1685)	(3.4074)	(6.1687)
k=5	−26.8878***	−36.4384***	−22.5035***	−63.7504***
	(4.9717)	(3.9833)	(5.3559)	(9.0256)
N	4,145,318	645,379	3,289,864	210,075
R^2	0.414	0.471	0.393	0.437
Adjusted R^2	0.383	0.444	0.362	0.406
Adjusted within-R^2	0.0842	0.1278	0.0755	0.1002

Notes: The regression includes individual and calendar year dummies. Robust standard errors (in parentheses) are clustered at the level of the initial firm in the base year. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Estimates are based on a matched sample of MNEs and domestic firms.
Source: ReLOC, IEB, and BHP, own calculations.

Table 3.A.8: Event study results for average daily wages

	(1) all workers	(2) low-skilled workers	(3) medium-skilled workers	(4) high-skilled workers
treatment dummies, reference category: $k = -2$				
k=-5	-0.1088^{***}	-0.1107^{***}	-0.0999^{***}	-0.2165^{***}
	(0.0146)	(0.0103)	(0.0134)	(0.0543)
k=-4	-0.0084	-0.0196^{***}	-0.0048	-0.0388^{**}
	(0.0062)	(0.0050)	(0.0059)	(0.0172)
k=-3	-0.0039	-0.0137^{***}	-0.0025	-0.0094
	(0.0029)	(0.0044)	(0.0027)	(0.0076)
k=-1	-0.0063^{**}	-0.0035	-0.0068^{**}	-0.0013
	(0.0026)	(0.0032)	(0.0026)	(0.0072)
k=0	-0.0141^{***}	-0.0062	-0.0149^{***}	-0.0075
	(0.0032)	(0.0039)	(0.0031)	(0.0131)
k=1	-0.0189^{***}	-0.0104	-0.0195^{***}	-0.0083
	(0.0063)	(0.0067)	(0.0059)	(0.0199)
k=2	-0.0298^{***}	-0.0215^{**}	-0.0293^{***}	-0.0230
	(0.0087)	(0.0084)	(0.0081)	(0.0292)
k=3	-0.0438^{***}	-0.0355^{***}	-0.0425^{***}	-0.0383
	(0.0099)	(0.0094)	(0.0095)	(0.0356)
k=4	-0.0552^{***}	-0.0449^{***}	-0.0533^{***}	-0.0550
	(0.0105)	(0.0095)	(0.0104)	(0.0434)
k=5	-0.1202^{***}	-0.1006^{***}	-0.1116^{***}	-0.1977^{***}
	(0.0151)	(0.0168)	(0.0151)	(0.0404)
control dummies, reference category: $k = -2$				
k=-5	-0.1106^{***}	-0.1083^{***}	-0.0993^{***}	-0.2466^{***}
	(0.0159)	(0.0122)	(0.0142)	(0.0546)
k=-4	-0.0154^{***}	-0.0263^{***}	-0.0107^{**}	-0.0571^{***}
	(0.0055)	(0.0054)	(0.0049)	(0.0216)
k=-3	-0.0013	-0.0072^{**}	0.0014	-0.0290^{***}
	(0.0032)	(0.0032)	(0.0030)	(0.0102)
k=-1	-0.0082^{***}	-0.0020	-0.0100^{***}	0.0045
	(0.0032)	(0.0046)	(0.0030)	(0.0098)
k=0	-0.0188^{***}	-0.0116^{*}	-0.0212^{***}	0.0052
	(0.0056)	(0.0061)	(0.0054)	(0.0190)
k=1	-0.0303^{***}	-0.0234^{***}	-0.0325^{***}	-0.0028
	(0.0076)	(0.0071)	(0.0075)	(0.0267)
k=2	-0.0432^{***}	-0.0300^{***}	-0.0454^{***}	-0.0241
	(0.0094)	(0.0081)	(0.0092)	(0.0350)
k=3	-0.0518^{***}	-0.0361^{***}	-0.0550^{***}	-0.0186
	(0.0116)	(0.0099)	(0.0112)	(0.0420)
k=4	-0.0612^{***}	-0.0420^{***}	-0.0635^{***}	-0.0425
	(0.0135)	(0.0121)	(0.0130)	(0.0502)
k=5	-0.1334^{***}	-0.0897^{***}	-0.1342^{***}	-0.1609^{**}
	(0.0216)	(0.0167)	(0.0207)	(0.0803)
N	3,882,652	584,329	3,108,675	189,648
R^2	0.567	0.478	0.570	0.484
Adjusted R^2	0.543	0.448	0.547	0.451
Adjusted within-R^2	0.0691	0.0241	0.0798	0.1342

Notes: The regression includes individual and calendar year dummies. Robust standard errors (in parentheses) are clustered at the level of the initial firm in the base year. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Estimates are based on a matched sample of MNEs and domestic firms.
Source: ReLOC, IEB, and BHP, own calculations.

Table 3.A.9: Event study results for days in employment at the initial firm

	(1) all workers	(2) low-skilled workers	(3) medium-skilled workers	(4) high-skilled workers
treatment dummies, reference category: $k = -2$				
k=-5	−78.0955***	−64.9615***	−76.8759***	−132.0610***
	(9.5404)	(8.5513)	(10.1364)	(13.4201)
k=-4	2.0378	−2.9144	3.2534	−3.5781
	(2.9687)	(2.6770)	(3.1425)	(4.1849)
k=-3	5.8642***	1.9608	6.1188***	11.4015***
	(1.7286)	(1.7632)	(1.6987)	(2.7149)
k=-1	−21.6669***	−15.7173***	−21.5527***	−39.5907***
	(3.0663)	(3.5478)	(3.0150)	(3.3396)
k=0	−42.6479***	−33.3619***	−42.0427***	−74.0283***
	(4.9245)	(4.6867)	(5.1711)	(5.4121)
k=1	−60.0792***	−49.4944***	−58.8407***	−101.7465***
	(6.4796)	(6.0568)	(6.8402)	(6.9629)
k=2	−75.7387***	−63.9167***	−74.0078***	−125.1485***
	(7.1573)	(6.5679)	(7.6427)	(9.3569)
k=3	−90.7656***	−76.3540***	−89.1566***	−143.4607***
	(8.9403)	(7.7620)	(9.4689)	(14.2295)
k=4	−102.4544***	−83.6960***	−101.0004***	−164.0594***
	(9.9219)	(8.4883)	(10.5221)	(15.4125)
k=5	−165.0119***	−138.4946***	−162.6414***	−252.4925***
	(11.0027)	(13.9812)	(11.4330)	(15.2828)
control dummies, reference category: $k = -2$				
k=-5	−92.4144***	−73.7243***	−91.2258***	−148.5267***
	(11.0577)	(8.6939)	(11.6109)	(13.8255)
k=-4	0.4228	−5.3268*	1.6563	−3.9570
	(3.1376)	(3.1044)	(3.1984)	(4.7702)
k=-3	6.7772***	3.1927**	6.9895***	11.2432***
	(1.5314)	(1.5352)	(1.5546)	(2.4342)
k=-1	−26.8397***	−24.1456***	−25.7097***	−47.9000***
	(2.5645)	(4.7801)	(2.3681)	(4.1364)
k=0	−47.4816***	−35.5250***	−46.3954***	−89.1357***
	(3.9316)	(4.3484)	(3.9218)	(6.9283)
k=1	−67.6858***	−51.4707***	−66.3304***	−122.7002***
	(5.5749)	(5.9283)	(5.6101)	(8.7356)
k=2	−83.1818***	−62.1200***	−81.8907***	−148.6827***
	(6.8325)	(6.8501)	(6.9618)	(10.1934)
k=3	−95.4611***	−70.9295***	−94.1682***	−168.4483***
	(7.7896)	(7.3913)	(7.9945)	(12.2508)
k=4	−108.1357***	−80.3652***	−107.1312***	−184.3627***
	(9.1250)	(8.4615)	(9.3759)	(15.8239)
k=5	−163.3216***	−123.0631***	−163.5392***	−251.5719***
	(14.2821)	(12.8307)	(14.6188)	(22.1251)
N	4,145,318	645,379	3,289,864	210,075
R^2	0.480	0.507	0.474	0.508
Adjusted R^2	0.453	0.481	0.447	0.480
Adjusted within-R^2	0.1705	0.1888	0.1657	0.2599

Notes: The regression includes individual and calendar year dummies. Robust standard errors (in parentheses) are clustered at the level of the initial firm in the base year. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Estimates are based on a matched sample of MNEs and domestic firms.
Source: ReLOC, IEB, and BHP, own calculations.

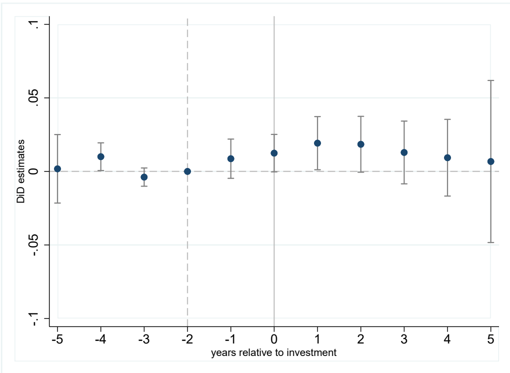
Table 3.A.10: Event study results for average daily wages at the initial firm

	(1) all workers	(2) low-skilled workers	(3) medium-skilled workers	(4) high-skilled workers
treatment dummies, reference category: $k = -2$				
k=-5	-0.0461^{***}	-0.0231^{*}	-0.0460^{***}	-0.0992^{***}
	(0.0078)	(0.0133)	(0.0075)	(0.0100)
k=-4	-0.0030	-0.0037	-0.0002	-0.0360^{***}
	(0.0042)	(0.0035)	(0.0045)	(0.0081)
k=-3	-0.0023	-0.0060^{**}	-0.0011	-0.0104^{**}
	(0.0021)	(0.0030)	(0.0020)	(0.0050)
k=-1	-0.0059^{**}	-0.0056	-0.0062^{**}	-0.0011
	(0.0026)	(0.0037)	(0.0025)	(0.0069)
k=0	-0.0112^{**}	-0.0044	-0.0133^{***}	-0.0005
	(0.0053)	(0.0079)	(0.0049)	(0.0096)
k=1	-0.0137^{**}	-0.0039	-0.0165^{***}	-0.0028
	(0.0063)	(0.0080)	(0.0059)	(0.0158)
k=2	-0.0215^{***}	-0.0098	-0.0242^{***}	-0.0197
	(0.0071)	(0.0089)	(0.0069)	(0.0162)
k=3	-0.0327^{***}	-0.0193	-0.0358^{***}	-0.0297
	(0.0097)	(0.0126)	(0.0091)	(0.0224)
k=4	-0.0412^{***}	-0.0267	-0.0442^{***}	-0.0431
	(0.0142)	(0.0175)	(0.0136)	(0.0290)
k=5	-0.0636^{***}	-0.0348	-0.0674^{***}	-0.0874^{**}
	(0.0201)	(0.0244)	(0.0190)	(0.0377)
control dummies, reference category: $k = -2$				
k=-5	-0.0342^{***}	-0.0146	-0.0320^{***}	-0.1017^{***}
	(0.0091)	(0.0141)	(0.0087)	(0.0162)
k=-4	-0.0096^{**}	-0.0094	-0.0057	-0.0534^{***}
	(0.0048)	(0.0068)	(0.0045)	(0.0123)
k=-3	0.0006	0.0001	0.0030	-0.0294^{***}
	(0.0028)	(0.0038)	(0.0028)	(0.0065)
k=-1	-0.0083^{***}	-0.0063	-0.0097^{***}	0.0036
	(0.0028)	(0.0047)	(0.0027)	(0.0068)
k=0	-0.0169^{***}	-0.0135^{*}	-0.0193^{***}	0.0049
	(0.0051)	(0.0072)	(0.0050)	(0.0115)
k=1	-0.0246^{***}	-0.0219^{**}	-0.0274^{***}	0.0073
	(0.0077)	(0.0102)	(0.0075)	(0.0150)
k=2	-0.0345^{***}	-0.0311^{**}	-0.0371^{***}	-0.0076
	(0.0089)	(0.0124)	(0.0087)	(0.0182)
k=3	-0.0388^{***}	-0.0334^{**}	-0.0428^{***}	0.0050
	(0.0104)	(0.0137)	(0.0101)	(0.0208)
k=4	-0.0466^{***}	-0.0372^{**}	-0.0503^{***}	-0.0192
	(0.0127)	(0.0172)	(0.0122)	(0.0238)
k=5	-0.0906^{***}	-0.0728^{**}	-0.0950^{***}	-0.0703^{**}
	(0.0201)	(0.0284)	(0.0191)	(0.0353)
N	3,228,094	512,561	2,575,007	140,526
R^2	0.736	0.706	0.727	0.647
Adjusted R^2	0.718	0.686	0.709	0.617
Adjusted within-R^2	0.1313	0.0755	0.1445	0.1459

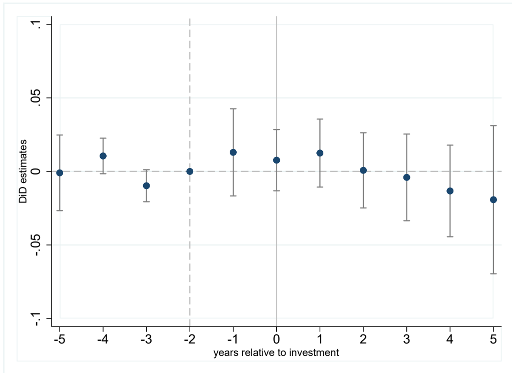
Notes: The regression includes individual and calendar year dummies. Robust standard errors (in parentheses) are clustered at the level of the initial firm in the base year. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively. Estimates are based on a matched sample of MNEs and domestic firms.
Source: ReLOC, IEB, and BHP, own calculations.

3.A.4 Robustness checks

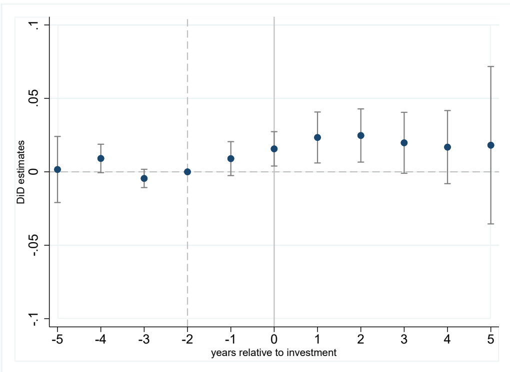
Figure 3.A.1: Effects of FDI on annual earnings (excluding workers that switch to another MNE)



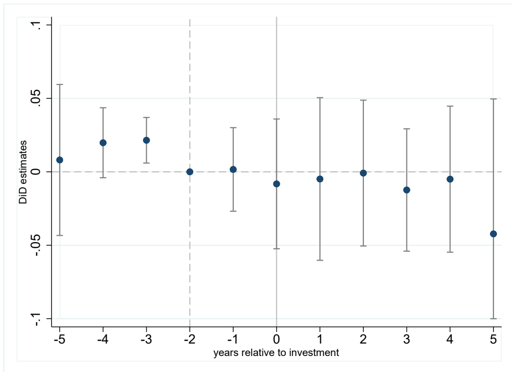
Panel A: All workers



Panel B: Low-skilled workers



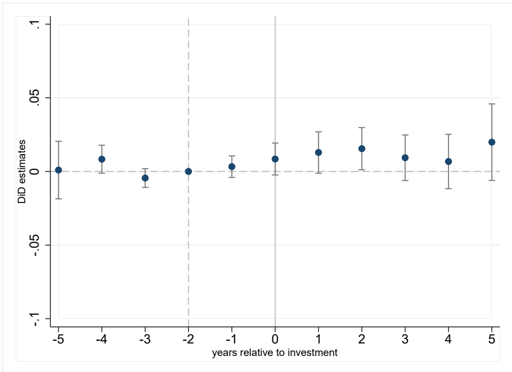
Panel C: Medium-skilled workers



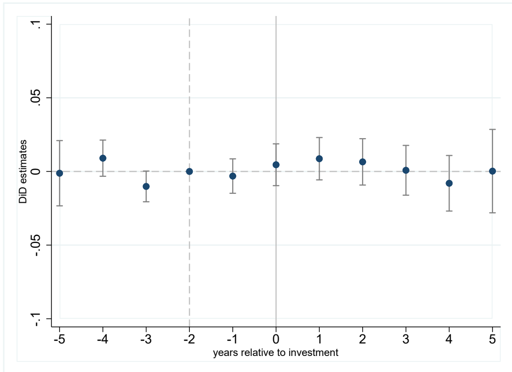
Panel D: High-skilled workers

Notes: The figures show DiD estimates from Equation (1) with annual earnings as the outcome variable. Annual earnings are scaled by the individual value in the base year. Dashed vertical lines mark the base year -2, and solid lines mark the year of investment. The dots indicate the estimated effects of FDI on the outcome variable according to $\beta_k - \gamma_k$. The estimate depicted for year -5 bins the effects up to five years before the investment, and year five bins the effects five years after the investment and onwards. Whiskers illustrate 95% confidence intervals. They are cut at -0.1 and 0.1. The sample is restricted to workers who do not work in another MNE after the base year.
Sources: ReLOC, IEB, and BHP, own calculations.

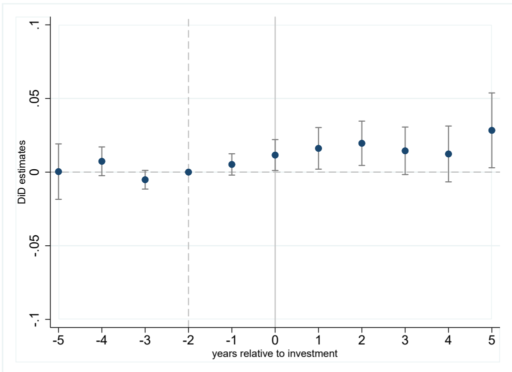
Figure 3.A.2: Effects of FDI on annual earnings (excluding workers without employment spells in later years)



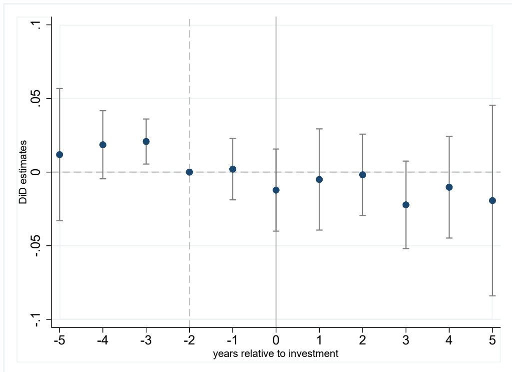
Panel A: All workers



Panel B: Low-skilled workers



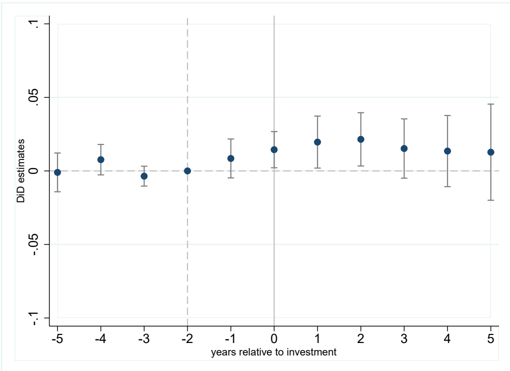
Panel C: Medium-skilled workers



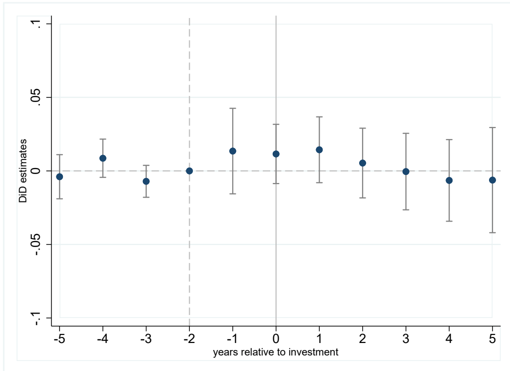
Panel D: High-skilled workers

Notes: The figures show DiD estimates from Equation (1) with annual earnings as the outcome variable. Annual earnings are scaled by the individual value in the base year. Dashed vertical lines mark the base year -2, and solid lines mark the year of investment. The dots indicate the estimated effects of FDI on the outcome variable according to $\beta_k - \gamma_k$. The estimate depicted for year -5 bins the effects up to five years before the investment, and year five bins the effects five years after the investment and onwards. Whiskers illustrate 95% confidence intervals. They are cut at -0.1 and 0.1. In the sample, workers without any employment spell subject to social security contributions within a year are dropped.
Sources: ReLOC, IEB, and BHP, own calculations.

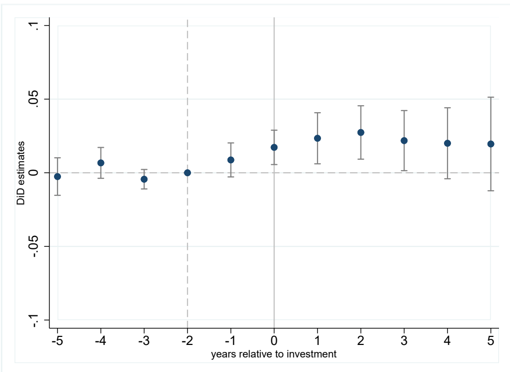
Figure 3.A.3: Effects of FDI on annual earnings (strict effect window)



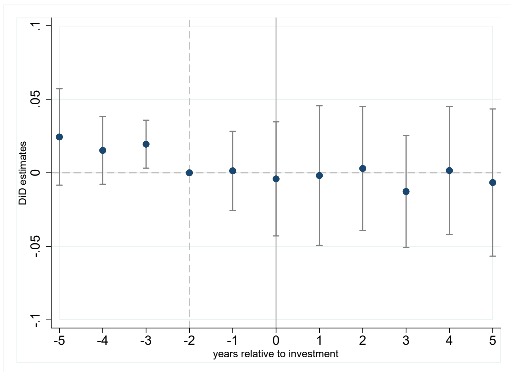
Panel A: All workers



Panel B: Low-skilled workers



Panel C: Medium-skilled workers



Panel D: High-skilled workers

Notes: The figures show DiD estimates from Equation (1) with annual earnings as the outcome variable. Annual earnings are scaled by the individual value in the base year. Dashed vertical lines mark the base year -2, and solid lines mark the year of investment. The dots indicate the estimated effects of FDI on the outcome variable according to $\beta_k - \gamma_k$. In contrast to the baseline model, effects in year -5 and five do not bin effects, but only include the estimates for $k = -5$ and $k = 5$, respectively. Whiskers illustrate 95% confidence intervals. They are cut at -0.1 and 0.1.

Sources: ReLOC, IEB, and BHP, own calculations.

Eidesstattliche Versicherung

Ich, Linda Borrs, 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.

Düsseldorf, 28. August 2019 ______________________________