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Abstract

We use linked employer-employee data to estimate the effect of foreign direct investment (FDI) on workers' job stability. We are the first to consider firm-internal job transitions. Specifically, we examine the impact of FDI on the individual likelihood to up- or downgrade to occupations with more or less analytical and interactive tasks. To do so, we propose an iterative matching procedure that generates a homogeneous sample of firms with equal probabilities of investing. Based on this sample, we use proportional hazard models to retrieve dynamic effects on workers. We find that FDI increases the likelihood of up- and downgrades by 25 and 37 percent, respectively. These effects increase with the share of non-routine and interactive tasks and become measurable two years after the investment. FDI does not increase the hazard of the separation of workers and firms. Instead, there is a temporal lock-in effect after the investment. Our results highlight the importance of firm-internal restructuring as a channel for adjusting to altered labor demand in response to FDI.

Zusammenfassung

Wir untersuchen die Auswirkungen von ausländischen Direktinvestitionen (FDI) auf die Beschäftigungsstabilität von Arbeitnehmerinnen und Arbeitnehmern mittels Sozialversicherungsdaten. Erstmalig berücksichtigen wir hierbei unternehmensinterne Jobwechsel und untersuchen die Effekte von FDI auf die individuelle Wahrscheinlichkeit von *Up-* oder *Downgrades* hin zu Berufen mit mehr oder weniger analytischen und interaktiven Tätigkeiten. Zu diesem Zweck entwickeln wir ein iteratives Matching-Verfahren, welches einen homogenen Datensatz von Firmen mit gleichen Investitionswahrscheinlichkeiten erzeugt und berechnen dynamische Effekte mit Proportional Harzardmodellen. Unsere Ergebnisse zeigen, dass FDI die Wahrscheinlichkeit von Up- und Downgrades um 25 bzw. 37 Prozent erhöht. Diese Effekte nehmen mit dem Anteil an nicht-routine und interaktiven Tätigkeiten zu und werden zwei Jahre nach der Investition messbar. FDI erhöht nicht die Wahrscheinlichkeit von Trennungen von Beschäftigten und Unternehmen. Stattdessen führt FDI zu einem temporären Lock-in-Effekt. Unsere Befunde belegen die Bedeutung von firmeninternen Umstrukturierungen als Reaktion auf eine veränderte Arbeitsnachfrage durch FDI.

JEL

F16, F23, F66, J23, J62

Keywords

FDI, job stability, multinational firms, tasks

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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. The empirical results on the employment and wage effects of FDI are ambiguous and can neither fully support nor reject these fears (see Crinò, 2009 and Hummels/Munch/Xiang, 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 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/Ekholm/Muendler, 2013; Nilsson Hakkala/Heyman/Sjöholm, 2014; Laffineur/Mouhoud, 2015). Moreover, if FDI is accompanied by the 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 downgrade 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.

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

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

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.

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/Melitz/Yeaple (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 a novel 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 the ability-driven sorting of workers (e.g., Card/Heining/Kline, 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 25 percent. Simultaneously, the hazard of downgrading to less-complex jobs increases by 37 percent. 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 high chance of remaining shortly after the investment, but this lock-in effect disappears after a few 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

[2] In the majority of datasets on FDI, small firms with low investment volumes are under-represented 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.

firms do when choosing who to upgrade, downgrade or dismiss. Independent of FDI, firms upgrade more productive workers and dismiss or downgrade less productive workers. Additionally, we find that upgrades lead to wage increases, whereas downgrades lead to wage reductions.

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/Ekholm/Muendler, 2013; Laffineur/Mouhoud, 2015; Laffineur/Gazaniol, 2019). 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/Baumgarten/Stiebale (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/Baumgarten/Stiebale (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, 2019, Ebenstein et al., 2014 for the US; Munch, 2010 for Denmark; Egger/Pfaffermayr/Weber, 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 separately within the borders of the firm. Baumgarten (2015) finds that offshoring—measured by an occupation-specific exposure to imported intermediates—is not associated with greater occupational instability on average. However, he also finds that offshoring decreases the risk of occupational switches for highly non-routine jobs in Germany. 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 (2019). 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 service offshoring to China and India increases job upgrades by 6 and job downgrades by 7 percent. In contrast to our paper, these papers examine the impact of offshoring in general, not FDI in particular. 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.

Additionally, the literature provides initial evidence that internationalization affects firms' workforce compositions. In a recent study, Laffineur (2019) investigates whether FDI leads to organizational changes. Following a common strand of the theoretical trade literature (e.g., Caliendo/Rossi-Hansberg, 2012), the paper assesses firms' organization through a knowledge-based hierarchy model. Laffineur shows that FDI raises the number of workers with management tasks and reduces the number of workers with production tasks. To take over skill-intensive management and supervisory tasks, workers might need additional training. Hogrefe/Wrona (2015) provide initial empirical evidence for this argument and demonstrate that offshoring spurs on-the-job training. If FDI also initializes on-the-job training, we expect to see a positive effect on within-firm occupational upgrades.

The remainder of the paper is structured as follows. The next section explains our identification strategy. Section 3 describes the data. Section 4 reports our results and discusses the implications. Section 5 summarizes several robustness exercises, and Section 6 concludes.

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/Melitz/Yeaple (2004) show, only certain types of firms are likely to invest abroad. Thus, in a first step, we use 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 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/Jean/Mayer, 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 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

[3] Propensity score matching has previously been used in the FDI context by a wide range of studies, e.g., Bronzini (2015), Crinò (2010) for Italy, Hijzen/Jean/Mayer (2011) for France, Debaere/Lee/Lee (2010) for Korea, Barba Navaretti/Castellani/Disdier (2010) for France and Italy, Becker/Muendler (2008) and Kleinert/Toubal (2007) for Germany, Hijzen/Inui/Todo (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.

[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 A.2.

only the best match for a *domestic firm* over the whole observation period (matches MNE A: ~~$\mathbf{a}_{\mathbf{2004}}$~~; matches MNE B: $\mathbf{a}_{\mathbf{2006}}$). Then, we update the list of potential matches for *MNEs* and move up second-ranked matches if necessary (matches MNE A: $\mathbf{b}_{\mathbf{2004}}$, 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 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.

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:

$$\log h_e(t|x_{ijtyro}) = h_0(t) + \gamma I(\mathrm{FDI}_j) + x_{ijt}\beta_1 + x_{ijt}t\beta_2 + \tau_y + \omega_r + \theta_o + u_{ijtyro}. \tag{2.1}$$

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, $I(\mathrm{FDI})$ 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 an-

[6] Our research question is a typical application for proportional hazard models. Compared to linear probability models and logit or probit models, proportional hazard models offer several advantages when dealing with event history data. For instance, they are robust to deviations from the normality assumption and censored events, and they allow us to include time-varying covariates. Especially when analyzing up- and downgrades, censoring is prominent in our data and we thus prefer proportional hazard models. Furthermore, proportional hazard models allow us to investigate how the effects of FDI change over time.

alytical 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. For instance, worker performance plausibly increases the likelihood of occupational upgrades, while it reduces the risk of occupational downgrades or separations. Furthermore, a firm might want to shrink or grow its domestic plants after FDI and might simultaneously plan to perform more- or less-complex tasks. Importantly, the objective of the firm distinctly alters the likelihood of each event for each individual. Consider a firm that follows the classical factor-seeking motive of FDI (see, e.g., Helpman, 1984; Markusen, 2002) and aims to reduce labor costs by relocating offshorable tasks to a foreign plant. The firm attempts to shrink, which raises the hazard of separations. Additionally, the firm requires more complex supervisory and management tasks, which increases the likelihood of upgrades. Downgrades are not affected. However, due to interwoven production processes, fragmentation is often more complex. Therefore, offshoring certain production stages can affect tasks in the firm's up- and downstream processes. Such changes in the firms' task structure might lead some incumbent workers to take over new tasks, which can result in occupational up- and downgrades in all areas of the firm. 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 (2.1)) 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:

$$\log h_e(t|x_{ijtyro}) = h_0(t) + \gamma_0 I(\mathsf{FDI}_j) + \gamma_1 I(\mathsf{FDI}_j)t + x_{ijt}\beta_1 + x_{ijt}t\beta_2 + \tau_y + \omega_r + \theta_o + u_{ijtyro}, \quad (2.2)$$

where $I(\mathsf{FDI}_j)t$ is the interaction of the investment dummy and time since the investment. The remainder of Equation (2.2) is identical to Equation (2.1). Because treatment is assigned to firms (not workers), we cluster standard errors at the firm level in both models (see Abadie et al., 2017).

3 Data and descriptive statistics

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 include the entire universe of German firms with affiliates in the Czech Republic, according to the Czech commercial register for 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 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

7 Refer to Hecht/Litzel/Schäffler (2013) for details on the ReLOC dataset.
8 Hecht et al. (2013) show in their survey of 459 firms of the ReLOC dataset that almost 70 percent 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.

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 main sample to workers who, at the time of the (pseudo) investment, had worked at their firm for at least two years. We correct inconsistent information on individual education following Fitzenberger/Osikominu/Völter (2005). Furthermore, the wages of approximately 10 percent 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/Ludsteck/Schönberg (2009) and Card/Heining/Kline (2013).

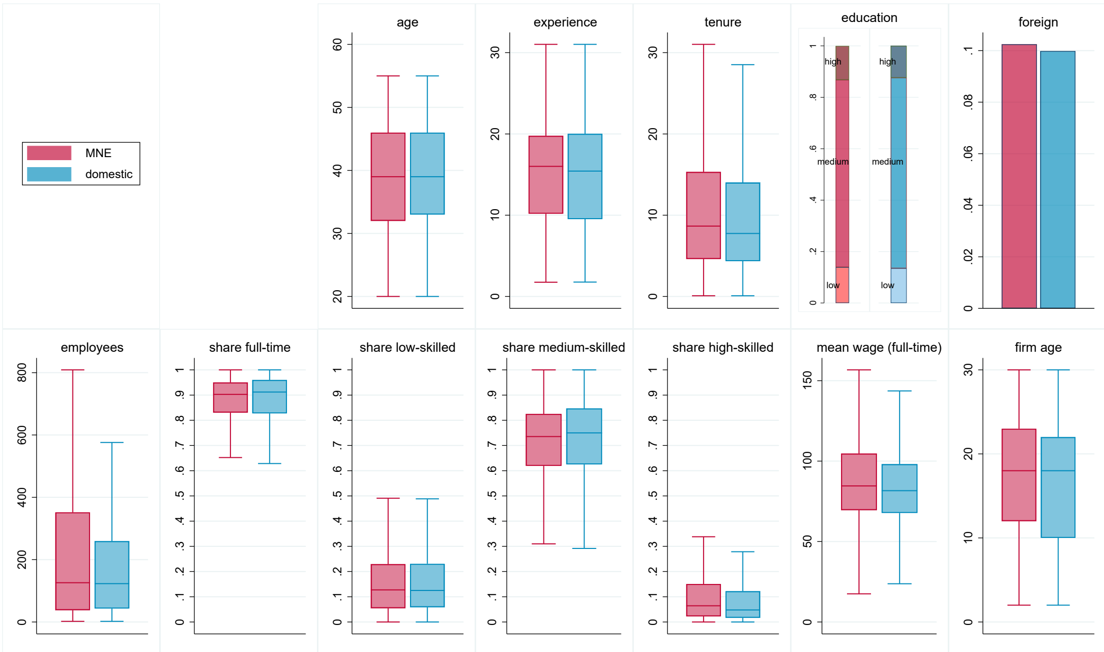
3.2 Descriptive statistics

Figure 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 tenure are almost identical. Additionally, the compositions 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 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 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 to our data include, e.g., upgrades from locksmiths to technicians or metal workers to warehouse 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 locksmiths to metal connectors. Both examples lead to a less-complex task set. Appendix A.4 lists the most frequent up- and downgrades. Figure 2 shows that for the majority of workers, an occupational switch changes the complexity of their jobs by up to 40 percentage points. Of all up- and downgrades, 60 percent 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 3 illustrates the cumulative hazards of separations and

Figure 1: Worker and firm characteristics after matching

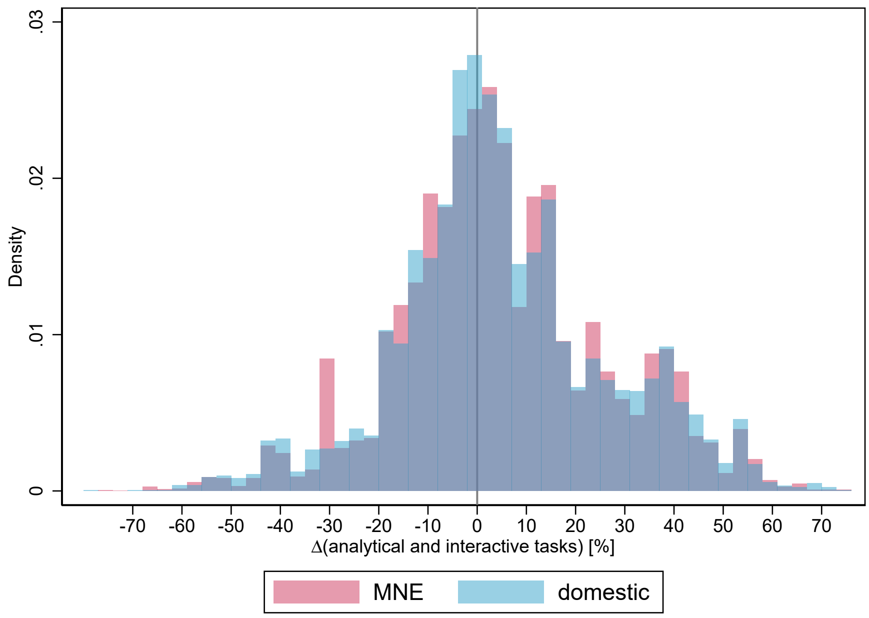


Notes: The figure shows box plots and bar charts of various worker (first row) and firm (second row) variables. The horizontal line in the middle of a box represents the median. The edges of a box indicate the first and the third quartiles. The range of the whiskers illustrates minima and maxima, limited 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.

up- and downgrades. The cumulative hazard indicates the probability of an event within a given timeframe. The upper-left panel of Figure 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 percent in MNEs. In domestic firms, it is only 4 percent. The development of the risk of downgrades is similar. However, the hazard of a downgrade is lower than the hazard of an upgrade. Figure 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.

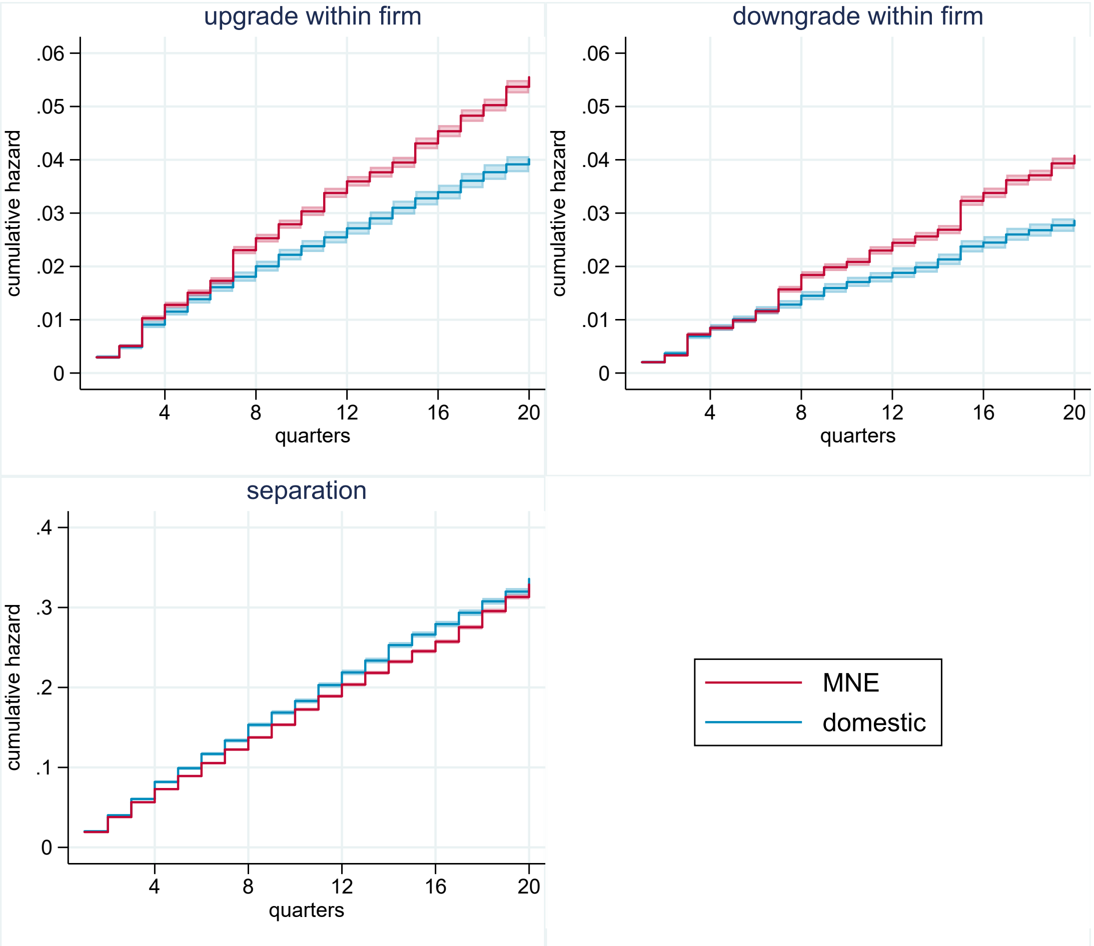
In summary, Figure 3 suggests that most of the adjustments over the course of FDI take place within the firm. Although the described hazards only provide descriptive evidence, they mirror well our multivariate findings that follow in the next sections.

Figure 2: 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.

Figure 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 percent (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.

4 Results

4.1 Main results

This section presents estimates of the impact of FDI into the Czech Republic on the job stability of workers in the investing firms in Germany. We distinguish between effects on the likelihood of separations of workers and firms, internal occupational switches in general, upgrades into more-complex jobs and downgrades into less-complex jobs within the firm. Table 1 summarizes the main results. Columns 1, 3, 5 and 7 show the time-independent impacts of FDI on the hazard of separation, any occupational switch within the firm, and up- and downgrades, respectively (see Equation (2.1)). Columns 2, 4, 6 and 8 provide the results from a dynamic specification. Here, the FDI indicator is interacted with the quarters since the investment (see Equation (2.2)). The table denotes the effects as hazard ratios, which have the same interpretation as odds ratios.

Table 1: Effects of FDI on the hazard ratios of separations, occupation switches, up- and downgrades

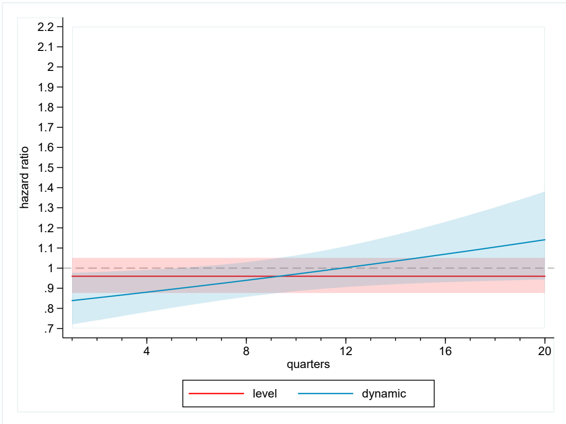
	separation		occupation switch		upgrade		downgrade	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FDI	0.9597	0.8252**	1.3005**	1.0141	1.2479**	1.0160	1.3726**	1.0071
	(0.0445)	(0.0694)	(0.1405)	(0.1820)	(0.1239)	(0.1793)	(0.1920)	(0.2053)
FDI × quarter		1.0163**		1.0282**		1.0235*		1.0347*
		(0.0079)		(0.0140)		(0.0131)		(0.0191)
Subjects	413,194	413,194	413,194	413,194	413,194	413,194	413,194	413,194
Events	112,382	112,382	29,716	29,716	17,226	17,226	12,490	12,490

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 0.8252 indicates that FDI reduces the individual risk of separation by 17.48 percent 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 A.5).
Source: ReLOC, IEB and BHP, own calculations.

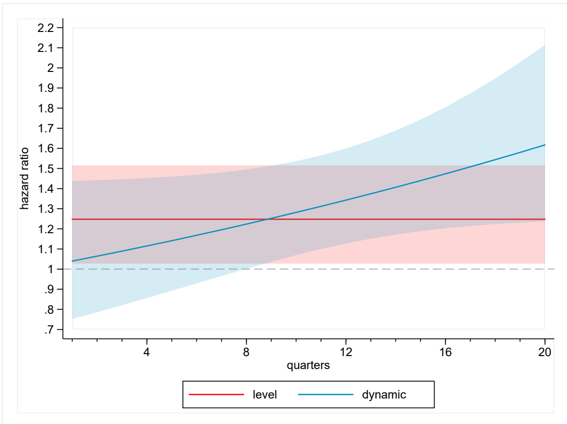
As Column 1 indicates, we find no effect of FDI on separations in the static model. In contrast, the hazard ratios of 1.30, 1.25 and 1.37 imply that FDI increases the likelihood of any occupational switch within the firm by 30 percent, the likelihood of a job upgrade by 25 percent, and the likelihood of a downgrade by 37 percent. Reassuringly, the likelihood of any occupational switch is identical to the weighted average of the likelihood of an up- or downgrade. Table 1 further shows that the absolute number of up- and downgrades in our data is much lower than the number of separations. 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. Separations do not seem to be an important adjustment channel.

The static model provides average hazard ratios over the five-year period after 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 1, we illustrate the time-varying impact of FDI on job stability in Figure 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 percent 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]

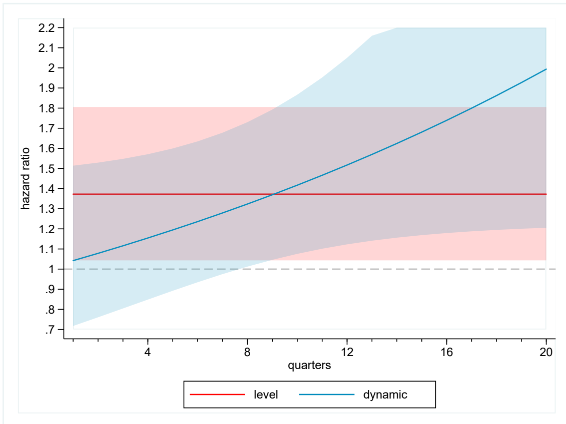
Figure 4: Dynamic effects of FDI on the hazard ratios of separations and up- and downgrades



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 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 $\times$ quarter in Table 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 of 0.8252 for separation indicates that FDI reduces the individual risk of separation by 17.48 percent in the quarter of investment.
Sources: ReLOC, IEB and BHP, own calculations.

While time-invariant hazard ratios indicate no effect of FDI on workers' separation rates, more

[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 $1.016 \times 1.024^8 = 1.23$. 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 1 do not suffice to infer the significance of the effects.

flexible time-variant estimates imply a short lock-in effect immediately after the investment. Specifically, the hazard of separation is 17 percent lower in MNEs in the quarter of the investment. It increases by 1.6 percent 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 five quarters after the investment.

Panels B and C of Figure 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 2.4 percent every quarter due to FDI. The risk of downgrading to a less-complex job increases by 3.5 percent 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, 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.

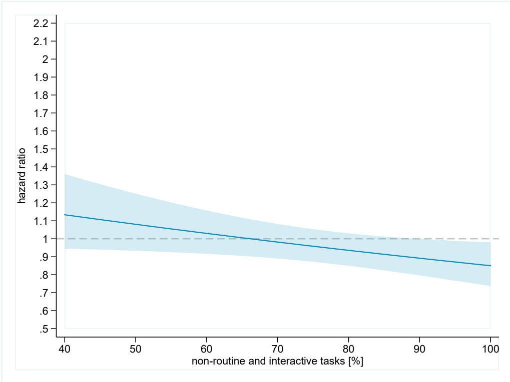
4.2 Job stability and tasks

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.

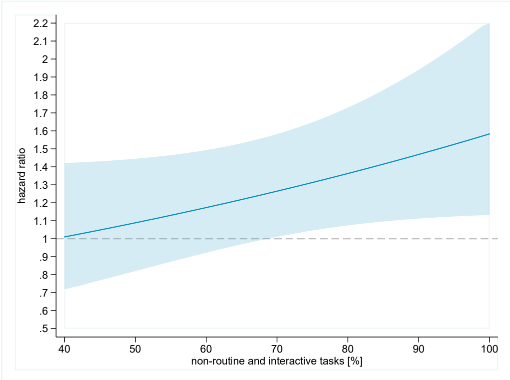
Figure 5 illuminates the impact of FDI into the Czech Republic on job stability in German firms depending on the initial offshorability of jobs. For ease of interpretation, the share of non-routine and interactive tasks increases from left to right. Thus, more easily offshorable jobs are on the left 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 A.6 in the Appendix). Note that the x-axis scale ranges from 40 to 100 percent because there are practically no occupations comprising less than 40 percent non-routine and interactive tasks (see Figure A.1 in the Appendix).

As can be seen from Panel A of Figure 5, the likelihood to separate from the firm increases with the offshorability of occupations (right to left). However, while FDI significantly reduces the hazard of separation for workers in highly non-routine and interactive jobs, FDI does not

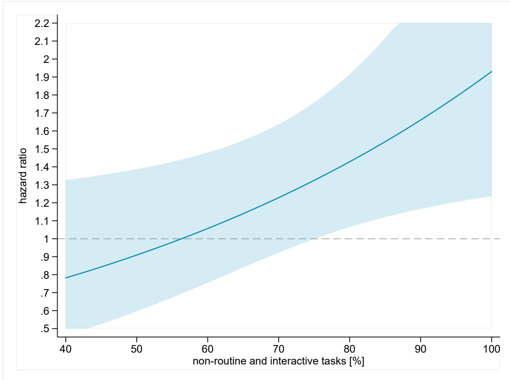
Figure 5: Effects of FDI on the hazard ratios of separations and up- and downgrades depending on the share of non-routine and interactive tasks



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 A.6 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 A.1 in the Appendix shows, the share of non-routine and interactive tasks ranges between 40 and 100 percent in the data. The range of non-routine and interactive tasks in Figure 5 is restricted accordingly.
Sources: ReLOC, IEB and BHP, own calculations.

significantly affect the risk of separations for workers in offshorable occupations. The former finding is in line with our theoretical expectations. 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.[11] This argumentation might explain why we find no effect of FDI on the separation rate for employees with routine and non-interactive jobs.

[11] Unfortunately, our data do not allow us to investigate the productivity of firms. Thus, we cannot buttress the argument of Grossman/Rossi-Hansberg (2008). The only proxy for productivity in our data is firm size. However, after the investment, the number of employees in MNEs does not grow faster than that in domestic firms.

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. First, switching occupations requires adaptation. 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.

Second, occupational upgrades requires further training and are therefore more expensive than downgrades, which merely require a reduction in tasks. Thus, in some cases, it might be 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 metal connectors. Metal connectors 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.

4.3 Job stability and unobserved worker productivity

In this section, we shed further light on the mechanisms of separations, upgrades and downgrades 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 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 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 in the exact middle of the rankings, the effects are close to our baseline estimates (e.g, for upgrades, $1.3699 \times 0.9985^{50} = 1.27 \approx 1.25$). 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 38 percent, respectively. Both effects are statistically significant.

Table 2: Effects of FDI on the hazard ratios of separations and up- and downgrades depending on unobserved worker productivity

	separation	upgrade	downgrade
	(1)	(2)	(3)
FDI	0.9441	1.3699**	1.2961
	(0.0479)	(0.1785)	(0.2079)
Wage rank	0.9962***	1.0087***	0.9913***
	(0.0004)	(0.0009)	(0.0013)
FDI $\times$ wage rank	1.0002	0.9985	1.0013
	(0.0005)	(0.0011)	(0.0017)
Subjects	406,108	406,108	406,108
Events	109,400	17,027	12,363

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 downgraded 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 the hazard of downgrades by 0.87 percent. The likelihood of promotions increases by 0.87 percent. However, these effects do 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, and workers with higher productivity face a higher likelihood of upgrades, independent of FDI.

4.4 Job stability and wages

When investigating job switches, the question arises of whether up- and downgrades are accompanied by wage changes. We therefore analyze the following Mincer-type wage equation:

$$\log w_{ijt} = I(\text{up}_i)\eta_1 + I(\text{down}_i)\eta_2 + I(\text{FDI}_j)I(\text{up}_i)\theta_1 + I(\text{FDI}_j)I(\text{down}_i)\theta_2 + x_{it}\beta + \mu_i + \tau_y + u_{ijt}. \tag{4.1}$$

Here, $I(\text{FDI}_j)$, $I(\text{up}_i)$ and $I(\text{down}_i)$ are indicators for investment, upgrades and downgrades, respectively. x_{it} includes basic worker controls, μ_i are worker fixed effects, τ_y is a series of year dummies, and u_{ijt} is the error term. We are mainly interested in the general effects of up- and downgrades on wages (η_1 and η_2) and whether FDI amplifies wage changes due to up- and downgrades (θ_1 and θ_2).

To estimate the model, we use the same data as in our main analysis. Thus, we compare workers in firms with equal probabilities to conduct FDI who were already employed two years before the (pseudo) investment. To not confuse the effects of up- and downgrades with the effects of other events, we further restrict the sample to the four quarters after the first up- or downgrade. Additionally, we compare only the wages of workers who remained in their initial firm.

Table 3 summarizes the results of the wage regression. The first two columns show estimates without worker fixed effects, which allows us to include an investment dummy. The estimates suggest that workers in MNEs earn 2.3 percent more than comparable workers in domestic firms. In line with our expectations, the estimates also imply that upgrades raise wages, whereas downgrades depress wages. However, as we illustrate in the previous subsection, more productive workers are more likely to upgrade, and less productive workers are more likely to downgrade. Thus, the results in columns 1 and 2 potentially suffer from selection bias. Columns 3 and 4 address this problem by including worker fixed effects.

After controlling for unobserved individual heterogeneity, the results confirm that up- and downgrades lead to wage increases and decreases, respectively. Workers who switch to a more complex job earn about 0.9 percent more compared to workers who remain in their initial job. On the contrary, the earnings of workers who switch to a less-complex job decrease between 1.6 and 2.6 percent. Wage reductions due to downgrades are less intense for workers in MNEs than for workers in domestic firms. This finding might reflect the positive impact of the MNE-wage premium, which might also benefit downgraded workers.

Table 3: Effects of FDI and up- and downgrades on wages

	(1)	(2)	(3)	(4)
FDI	0.0227***	0.0225***		
	(0.0006)	(0.0006)		
Upgrade	0.0830***	0.0685***	0.0087***	0.0088**
	(0.0025)	(0.0049)	(0.0019)	(0.0037)
Downgrade	-0.0632***	-0.0678***	-0.0177***	-0.0261***
	(0.0027)	(0.0056)	(0.0020)	(0.0043)
FDI $\times$ upgrade		0.0197***		-0.0001
		(0.0058)		(0.0043)
FDI $\times$ downgrade		0.0060		0.0106**
		(0.0064)		(0.0048)
Worker fixed effects	No	No	Yes	Yes
Observations	1,865,330	1,865,330	1,865,330	1,865,330

Notes: The table summarizes wage effects of up- and downgrades. Additional control variables are: age, experience, tenure and their squares, and year dummies. Columns 1 and 2 additionally include a foreign dummy, skill dummies, firm age and a dummy if the firm existed in 1975, as well as occupation, industry, and state dummies. Estimates are based on a matched sample of MNEs and domestic firms. Cluster robust standard errors at the firm level (in parentheses). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.
Source: ReLOC, IEB and BHP, own calculations.

4.5 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 percent 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/Baumgarten/Stiebale (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. Empirical evidence on the 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.

[12] In their paper, separation rates comprise both transitions to other firms and to non-employment. When Bachmann/Baumgarten/Stiebale (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.

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 no genuine need for separations. We show that instead of separations, MNEs adjust their workforce internally through upgrades and downgrades. For the average worker, the likelihood of upgrading to a more-complex job increases by 25 percent due to FDI. The likelihood of downgrading to a less-complex job increases by 37 percent. 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 percent. For the same group of workers, the probability of downgrading to less-complex jobs increases by between 30 and 90 percent. 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 occupational downgrades through FDI is in line with our expectation that MNEs might avoid the costs of dismissals by downgrading 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 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 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, which would require costly training.

There is no comparable study in the FDI literature on the 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/Ekholm/Muendler (2013) and Nilsson Hakkala/Heyman/Sjöholm (2014) find evidence of a shift in tasks in German and Swedish MNEs. In contrast to our results for FDI to the Czech Republic, Becker/Ekholm/Muendler (2013) do not find significant effects on the workforce composition of FDI to CEEC.

Overall, our analysis of German investments in the Czech Republic provide unique evidence that firms restructure their labor forces internally over the course of FDI. Some incumbent workers are upgraded to more complex occupations, while others are downgraded. Although a downgrade is not a positive occupational change per se, it might be a more minor career disruption than dismissal. 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.

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.

5.1 Non-competing risks

In Section 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 4 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.

Table 4: Effect of FDI on separations: competing vs. non-competing risks model

	baseline	no competing risks
	(1)	(2)
FDI	0.9597	0.9555
	(0.0445)	(0.0441)
Upgrade		0.7638***
		(0.0290)
Downgrade		0.9512
		(0.0473)
Subjects	413,194	413,194
Events	112,382	116,786

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.

Moreover, we control for preceding up- and downgrades within the firm in Column 2 of the same table. Independent of FDI, an occupational upgrade reduces the hazard of a separation by 24 percent. This finding is in line with the expectation that only *good* workers receive upgrades and are therefore less likely to be dismissed. The robustness exercise further indicates that past downgrades do not influence separations.

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 $e \in$ {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 estimate the likelihood of all events (against the baseline outcome *no event*). Table 5 summarizes the results.

Table 5: 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) (4)		
	separation (1)	upgrade (2)	downgrade (3)	separation	upgrade	downgrade
FDI	0.9730	1.2755**	1.4510**	0.9874	1.3261**	1.4171**
	(0.0547)	(0.1165)	(0.2153)	0.0573	0.1327	0.2143
N	413,194	412,834	413,133		413,194	
Log lik.	-230090.95	-66398.63	-51870.49		-342978.67	
Chi-squared	3773.52	3525.13	4046.33		6465.46	

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.

Overall, the estimates of the separate logit models and the multinomial logit model are in line with our main findings. Although the computed odds ratios are somewhat larger than in the proportional hazard models, the effects have a similar order of magnitude. As argued in Section 2 proportional hazard models are robust to censoring of events, which is common in our data. We therefore prefer hazard models to logit models. Furthermore, hazard models allow us to explicitly model the time structure of the impact of FDI.

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 metalworking to mechanics increases the share of complex tasks by only five percentage points and thus might not be considered a significant upgrade. As a robustness exercise, we therefore define *significant* up- and downgrades as job switches with changes in task complexity of at least ten percentage points. In Figure 2, these switches are in the bins to the left of -10 percent and in the bins to the right of +10 percent. The estimates for significant up- and downgrades in Panel A of Figure 6 are comparable to our baseline results (Figure 4). The effects on significant upgrades appear to be even more precisely estimated than effects on all upgrades. 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 common definition of offshorable tasks in the trade literature. As Panel B Figure 6 indicates, adding the manual dimension to our task measure does not affect the results.

Finally, inspired by Liu/Trefler (2019), 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 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 C of Figure 6 visualizes the corresponding 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 greater up- and downgrades within the firm holds independent of the exact definition of up- and downgrades. See Table A.7 in the appendix section for the corresponding coefficient estimates of Figure 6.

5.4 Additional robustness checks

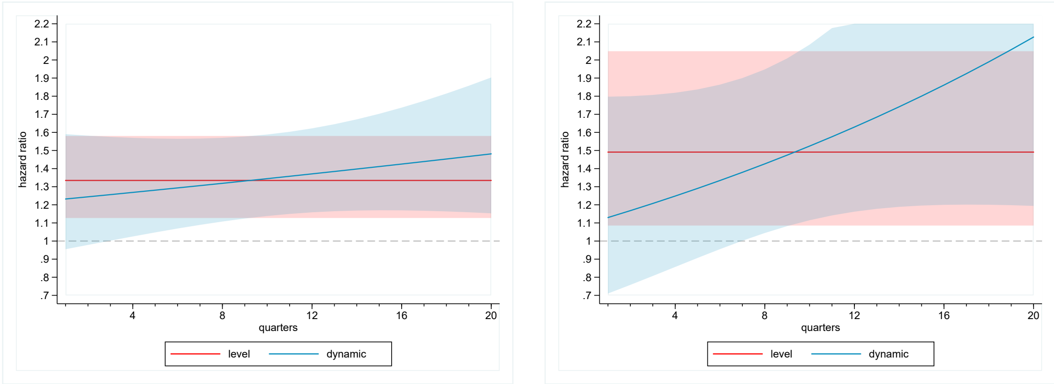
In an additional robustness check, we test whether our main findings are driven by small firms. To ensure that the investment decision is independent of the individual worker, we exclude small firms with fewer than 50 employees in Panel B of Table A.8 in the Appendix. The results point in the same direction, 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 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 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 slightly larger for up- and downgrades. 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.

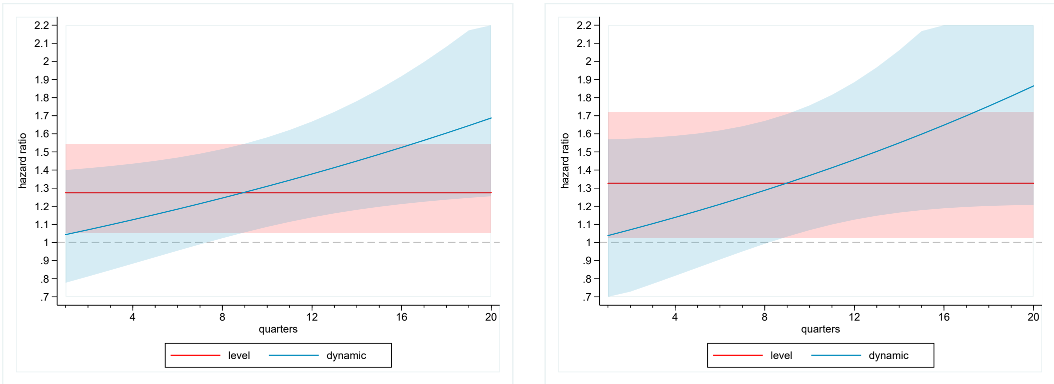
To identify the causal effects of FDI on job stability, we restrict our sample to workers who were already employed two years prior to the (pseudo) investment. This restriction ensures that individuals do not self-select into future MNEs. However, it also removes 20 percent of workers from our sample, 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 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.

Figure 6: Effects of FDI on the hazard ratios of up- and downgrades (alternative definitions)

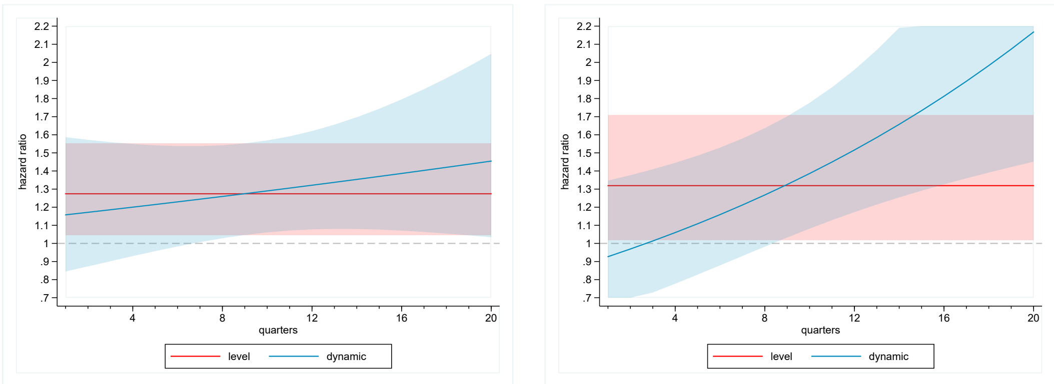
upgrades downgrades



PANEL A: Significant up- and downgrades



PANEL B: All non-routine tasks



PANEL C: Median wages

Notes: The figures provide a graphical representation of the hazard ratios and 95% confidence intervals of the estimated effects of FDI on alternative definitions of up- and downgrades. The results are obtained from the Cox regressions presented in Table A.7. 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. Panel A classifies upgrades (downgrades) as job switches with at least a ten-percentage-point increase (decrease) in analytical and interactive tasks. Panel B 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.

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. In particular, 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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Appendix

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 $\tilde{m}_{t.} \in \mathbf{P}_t$ with smallest $\Delta_{t.}$
  - add match $\tilde{m}_{t.}$ to $\mathbf{M}$
- **for** *each control firm* c
  - find best match $\tilde{m}_{.c} \in \mathbf{M}$ with smallest $\Delta_{.c}$
  - drop other matches $m_{.c} \neq \tilde{m}_{.c}$ from $\mathbf{M}$
- **for** *each treatment firm* t
  - **if** *match* $\tilde{m}_{t.} \notin \mathbf{M}$
    - drop match $\tilde{m}_{t.}$ from $\mathbf{P}_t$

drop matches $m_{..}$ with $\Delta_{..} < [0.2 \times sd(logit(\mathrm{PS}))]$ from $\mathbf{M}$

A.2 Matching results

Table 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.

Table 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.

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, 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 previously made adjustments to FDI. 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). 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 that nearest neighbors are not too far away, we calculate the optimal caliper width as recommended by Austin (2011b).[13]

Table 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 (1^{st} match) and Algorithm 1 (2^{nd} match) reduce the standardized differences of almost all variables (expect for the log number of employees and employment growth) and lead to variance ratios closer to one. The results indicate that matching substantially improves the balancing of firm characteristics.

Table A.2: Balancing test results after matching

	standardized mean differences			variance ratios		
	raw	1^{st} match	2^{nd} match	raw	1^{st} match	2^{nd} 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. “1^{st} 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. “2^{nd} match” presents the results after applying our iterative matching Algorithm (**??**). 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.

Further, Table 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.

[13] 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 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.

A.3 Most frequent up- and downgrades

In our empirical analysis, we define occupational up- and downgrades as internal job switches that lead to an in- or decrease of complex tasks. Table A.4 lists the most frequent up- and downgrades in our data. The most common upgrades are from locksmiths to technicians, electricians to technicians and assemblers and metal workers to warehouse managers. The former two upgrades entail a huge increase of task complexity: around one-third. The latter upgrade increases the complexity of tasks by ten percent.

The most frequent downgrades are from electricians to assemblers and metal workers, locksmiths to metal connectors and locksmiths to assemblers and metal workers. The first downgrade lowers the complexity of tasks by 29 percent. The latter downgrade decreases the task complexity by 18 percent. Overall, the most frequent up- and downgrades are accompanied by significant in- or decreases in task complexity. A few exceptions entail only small changes in the share of complex tasks (e.g., assistants to assemblers and metal workers). In the robustness section, we show that our results hold for all up- and downgrades as well as for up- and downgrades that lead to changes in task complexity of at least 10 percentage points.

Table A.4: Most frequent up- and downgrades

Old occupation	new occupation	N	△ complex tasks
	Most frequent upgrades		
Locksmiths	Technicians	541	34.54
Electricians	Technicians	475	30.80
Assemblers and Metal workers	Warehouse managers, Stores, transport workers	388	10.36
Office specialists, Office auxiliary workers	Management consultants, Organisors, Chartered accountants	345	15.96
Technicians	Engineers	295	14.37
Office specialists, Office auxiliary workers	Wholesale and retail trade	275	15.56
Assemblers and Metal workers	Machinists and related occupations	266	5.23
Wholesale and retail trade	Management consultants, Organisors, Chartered accountants	259	10.83
Warehouse managers, Stores, transport workers	Office specialists, Office auxiliary workers	242	25.38
Surface transport occupations	Warehouse managers, Stores, transport workers	240	3.28
	Most frequent downgrades		
Electricians	Assemblers and Metal workers	532	-29.29
Locksmiths	Metal connectors	280	-18.09
Locksmiths	Assemblers and Metal workers	262	-18.41
Mechanics	Locksmiths	245	-3.11
Engineers	Technicians	237	-23.22
Electricians	Locksmiths	208	-10.88
Assistants	Assemblers and Metal workers	196	-4.50
Warehouse managers, Stores, transport workers	Assemblers and Metal workers	178	-14.47
Wholesale and retail trade	Office specialists, Office auxiliary workers	164	-15.56
Machinists and related occupations	Assemblers and Metal workers	155	-9.44

A.4 Main results of Cox regression

Table A.5: Effects of FDI on the hazard ratios of separations and up- and downgrades (full table)

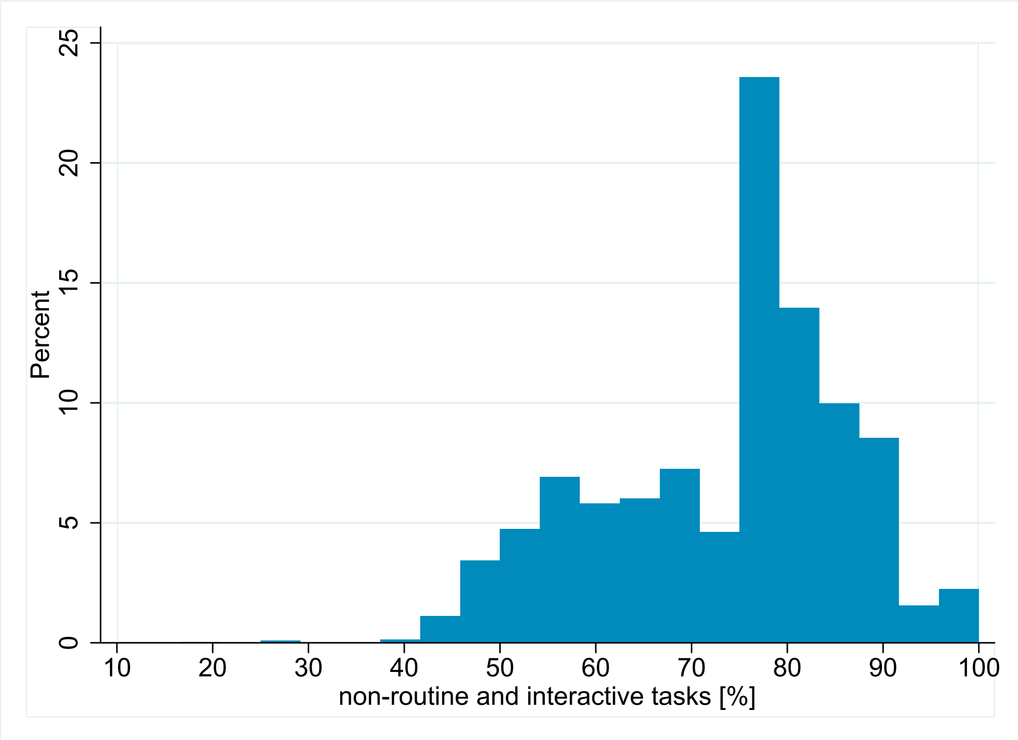
	separation		upgrade		downgrade	
FDI	0.9597	0.8252**	1.2479**	1.0160	1.3726**	1.0071
	(0.0445)	(0.0694)	(0.1239)	(0.1793)	(0.1920)	(0.2053)
FDI × quarter		1.0163**		1.0235*		1.0347*
		(0.0079)		(0.0131)		(0.0191)
Age	0.7850***	0.7848***	0.9970	0.9966	0.9659	0.9657
	(0.0148)	(0.0147)	(0.0314)	(0.0316)	(0.0518)	(0.0527)
Age squared	1.0031***	1.0031***	0.9998	0.9998	1.0004	1.0004
	(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	1.2077***	1.2082***	0.8880	0.8900	1.2510	1.2439
	(0.0851)	(0.0832)	(0.1250)	(0.1240)	(0.1892)	(0.1848)
Medium skilled	0.9964	0.9971	1.3721***	1.3714***	0.9484	0.9483
	(0.0433)	(0.0433)	(0.1104)	(0.1116)	(0.0985)	(0.1003)
High skilled	1.2293***	1.2368***	2.8456***	2.8773***	0.5296***	0.5315***
	(0.0849)	(0.0874)	(0.5666)	(0.5708)	(0.1241)	(0.1256)
Firm age	1.0259**	1.0264**	1.0668***	1.0674***	1.0291	1.0306
	(0.0109)	(0.0108)	(0.0261)	(0.0258)	(0.0356)	(0.0353)
Dummy firm < 1975	0.7259	0.7414	0.5229**	0.5467**	0.5261*	0.5619
	(0.1628)	(0.1660)	(0.1356)	(0.1410)	(0.1986)	(0.2130)
Interaction with quarters since treatment:						
Age	1.0052***	1.0052***	0.9936**	0.9936**	0.9980	0.9981
	(0.0011)	(0.0011)	(0.0030)	(0.0030)	(0.0051)	(0.0052)
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	1.0028	1.0027	0.9902	0.9900	1.0116	1.0123
	(0.0072)	(0.0070)	(0.0125)	(0.0124)	(0.0128)	(0.0124)
Medium skilled	0.9995	0.9995	0.9906	0.9906	0.9634**	0.9633**
	(0.0044)	(0.0044)	(0.0072)	(0.0072)	(0.0162)	(0.0161)
High skilled	0.9880*	0.9874*	0.9852	0.9839	0.9548	0.9544
	(0.0063)	(0.0065)	(0.0198)	(0.0197)	(0.0287)	(0.0288)
Firm age	0.9994	0.9994	1.0005	1.0004	1.0039	1.0038
	(0.0010)	(0.0010)	(0.0021)	(0.0021)	(0.0031)	(0.0031)
Dummy firm < 1975	0.9963	0.9940	0.9959	0.9908	0.9940	0.9868
	(0.0224)	(0.0223)	(0.0204)	(0.0209)	(0.0309)	(0.0314)
Subjects	413,194	413,194	413,194	413,194	413,194	413,194
Events	112,382	112,382	17,226	17,226	12,490	12,490

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 0.8252 indicates that FDI reduces the individual risk of separation by 17.48 percent in the quarter of investment. Estimates are based on a matched sample of MNEs and domestic firms.
Source: ReLOC, IEB and BHP, own calculations.

A.5 Additional material on job stability and tasks

A.5.1 Distribution of non-routine and interactive tasks

Figure 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 percent. Only 0.1 percent of workers are in occupations with less than 40 percent non-routine and interactive tasks.
Source: ReLOC, IEB and BHP, own calculations.

A.5.2 Job stability by initial share of non-routine and interactive tasks

Table A.6: 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	1.3736*	1.9799	0.7486	0.9966	0.4283	0.2289
	(0.2406)	(1.4013)	(0.2429)	(1.1752)	(0.2214)	(0.6254)
Non-routine & interactive	1.0082***	1.0037	0.9804***	0.9576	1.0069	1.0485
	(0.0016)	(0.0146)	(0.0027)	(0.0275)	(0.0048)	(0.0548)
FDI $\times$ non-routine	0.9952**	0.9847	1.0075*	0.9991	1.0152**	1.0321
& interactive	(0.0022)	(0.0198)	(0.0043)	(0.0365)	(0.0068)	(0.0756)
FDI $\times$ (non-routine		1.0001		1.0001		0.9999
& interactive)squared		(0.0001)		(0.0003)		(0.0005)
Subjects	413,104	413,104	413,104	413,104	413,104	413,104
Events	112,347	112,347	17,207	17,207	12,489	12,489

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.

A.6 Alternative definitions of job up- and downgrades

Table A.7 accompanies Figure 6 and summarizes effects of FDI on alternative definitions of occupational up- and downgrades. Overall, the effects are similar as those found with our preferred definition of up- and downgrades and therefore support our main findings.

Table A.7: 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	1.2479**	1.0160	1.3726**	1.0071
	(0.1239)	(0.1793)	(0.1920)	(0.2053)
FDI × quarter		1.0235*		1.0347*
		(0.0131)		(0.0191)
Subjects	413,194	413,194	413,194	413,194
Events	17,226	17,226	12,490	12,490
Panel B: Significant up- and downgrades with at least 10 percentage points changes:				
FDI	1.3347***	1.2204	1.4912**	1.0927
	(0.1150)	(0.1685)	(0.2416)	(0.2775)
FDI × quarter		1.0097		1.0339
		(0.0103)		(0.0231)
Subjects	413,194	413,194	413,194	413,194
Events	11,473	11,473	6,571	6,571
Panel C: All non-routine tasks:				
FDI	1.2745**	1.0173	1.3273**	1.0066
	(0.1250)	(0.1624)	(0.1759)	(0.2273)
FDI × quarter		1.0256**		1.0313*
		(0.0126)		(0.0190)
Subjects	413,194	413,194	413,194	413,194
Events	15,199	15,199	14,517	14,517
Panel D: Median wages:				
FDI	1.2742**	1.1440	1.3190**	0.8863
	(0.1287)	(0.1969)	(0.1746)	(0.1799)
FDI × quarter		1.0121		1.0457***
		(0.0143)		(0.0167)
Subjects	413,194	413,194	413,194	413,194
Events	15,123	15,123	14,593	14,593

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 D 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.

A.7 Additional robustness checks

Table A.8 summarizes several robustness exercises. 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 the table. In Panel C, we assign random investment dates to domestic firms, and in Panel D, we do not restrict our data to workers with at least two years tenure. All the described exercises lead to similar results as our main estimates.

Table 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	0.9597	0.8252**	1.2479**	1.0160	1.3726**	1.0071
	(0.0445)	(0.0694)	(0.1239)	(0.1793)	(0.1920)	(0.2053)
FDI $\times$ quarter		1.0163**		1.0235*		1.0347*
		(0.0079)		(0.0131)		(0.0191)
Subjects	413,194	413,194	413,194	413,194	413,194	413,194
Events	112,382	112,382	17,226	17,226	12,490	12,490
Panel B: Without small firms (>50 employees):						
FDI	0.9544	0.8186**	1.2513**	1.0209	1.3722**	1.0033
	(0.0454)	(0.0704)	(0.1258)	(0.1822)	(0.1950)	(0.2081)
FDI $\times$ quarter		1.0166**		1.0233*		1.0350*
		(0.0081)		(0.0132)		(0.0193)
Subjects	406,577	406,577	406,577	406,577	406,577	406,577
Events	109,875	109,875	17,048	17,048	12,344	12,344
Panel C: Random starts (plus minus 4 quarters):						
FDI	0.9522	0.8706*	1.3362***	1.3161*	1.3965**	1.2197
	(0.0442)	(0.0637)	(0.1266)	(0.2044)	(0.1948)	(0.2188)
FDI $\times$ quarter		1.0098		1.0017		1.0151
		(0.0062)		(0.0105)		(0.0159)
Subjects	284,935	284,935	284,935	284,935	284,935	284,935
Events	78,467	78,467	12,074	12,074	8,588	8,588
Panel D: No restriction to workers' tenure:						
FDI	0.9339	0.8114**	1.2723***	1.0782	1.3742**	1.0465
	(0.0505)	(0.0773)	(0.1180)	(0.1739)	(0.1823)	(0.1955)
FDI $\times$ quarter		1.0169**		1.0192		1.0312*
		(0.0079)		(0.0121)		(0.0174)
Subjects	531,045	531,045	531,045	531,045	531,045	531,045
Events	173,333	173,333	21,322	21,322	15,138	15,138

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.

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