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FDI and onshore job stability: Upgrades, downgrades, and separations in multinationals

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ABSTRACT

We use linked employer–employee data to estimate the impact of firms' foreign direct investment (FDI) into a low-wage country on workers' job stability in a high-wage country. We are the first to consider internal (i.e., within-firm) job transitions. Specifically, we examine the impact of German firms' FDI into the Czech Republic on the likelihood of onshore employees up- or downgrading to occupations that are more or less intensive in analytical and interactive tasks. To do so, we match firms with similar investing probabilities. We use this sample to estimate proportional hazards models to retrieve the dynamic effects on workers. We find that FDI increases the average likelihood of upgrades and downgrades by 17% and 19%, respectively. These effects are the strongest for jobs with low shares of nonroutine and interactive tasks, and they increase over time. They become substantial two years after the investment and reach 32%–46% and 35%–48%, respectively. FDI does not increase the hazard of worker–firm separations. Our results highlight the importance of internal firm restructuring, which enables firms to satisfy their altered domestic labor needs after FDI.

1. Introduction

Multinational enterprises (MNEs) are a key driver of economic integration between high- and low-wage countries and have been considered controversial in the economic literature. On the one hand, firms benefit from foreign direct investment (FDI) by saving production costs or by exploiting new markets. On the other hand, workers in the onshore departments of such firms are often concerned about their future job stability. Since the acquisition of a foreign affiliate facilitates relatively inexpensive access to the foreign labor market, onshore workers become more substitutable. Thus far, however, the empirical results regarding the effects of these international expansions on onshore workers are ambiguous and can neither fully support nor reject these fears (see Crinò, 2009 and Hummels et al., 2018 for recent surveys). It seems odd that after a firm's expansion into a low-wage country, there are ambiguous or no sizeable effects on domestic employment numbers in the MNE (e.g., Harrison and McMillan, 2011).[1] We argue that internal firm restructuring is the missing channel that can explain why adjustments after FDI lack substantial effects on firm aggregates.

Our first and main contribution is to zoom into onshore plants' internal processes and to explore workers' job stability after FDI. Using Cox proportional hazards models, we show that foreign expansion has very little effect on job stability in terms of separations

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[1] Harrison and McMillan (2011) show that the low-wage foreign affiliates of US MNEs serve as both substitutes and complements for onshore employment. Their estimated effect sizes are also very low (far below 1%). Similar results are reported by Kambayashi and Kiyota (2015) for Japanese MNEs. Furthermore, the estimates in the literature are ambiguous. While Desai et al. (2009) finds positive effects of affiliate growth on employment of parent companies in the US, Debaere et al. (2010) find negative effects for Korean MNEs after FDI.

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but has a very large effect in terms of up- and downgrades within MNEs. The rate of internal occupational switches is approximately 1.5 times higher for a worker in an MNE than in a comparable domestic firms (5–6 years after FDI). Second, we investigate the type of jobs that are particularly affected, either with respect to worker productivity or with respect to the typical share of nonroutine and interactive tasks in the occupation. After FDI, firms allocate onshore workers to either more complex task bundles (occupational upgrades) or less complex task bundles (occupational downgrades). Third, we analyze the wage differences of workers who switch occupations.

This article is the first to show that after FDI, firms meet their altered labor needs in their onshore market by internally restructuring their workforce. We are able to do this by measuring the timing of FDI, which has several advantages over the use of trade flows or the consideration of relatively costly outsourcing. Mainly, this measure enables us to detect not only the onshore reallocation effects caused by potential trade flows of goods and services within the MNEs but also changes due to exchanges that do not result in a cashflow, such as exchanges of ideas, information and other intangibles that can be easily transferred via information and communication technology (ICT). Our approach is, hence, capable of shedding light on nuanced changes to onshore labor demand after a firm shifts tasks into a low-wage labor market at low costs. Since occupational upgrades are usually perceived as beneficial for the worker whereas downgrades occur with wage losses, these demand changes also reflect several not-yet analyzed costs and benefits of globalization. The outcomes of this paper are, therefore, highly relevant for policy makers because they underline the importance of (on-the-job) training in a structurally transforming but rigid labor market.

To investigate the impact of FDI on job stability, we exploit a unique administrative micropanel dataset. 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. While the specific country pair used is a limitation to external validity, we still expect the findings to be transferable to similar trade relationships. The data also represent a compelling case of FDI flows, as Germany is the largest economy in Europe and the Czech Republic was one of its major FDI recipients during the 1990s and 2000s (see Marin, 2004, p. 4). Most of the FDI is efficiency seeking and associated with offshoring activities, as approximately 76% of the German affiliates in the Czech Republic trade inputs with their parent firms in Germany (see Marin, 2006, p.614). In contrast to the data in previous studies, our data are not selective toward large FDI and also cover small firms with low investment volumes.[2] Our data further include the complete administrative employment biographies of all workers in the investing and domestic firms. In 2008, these comprise 1.9 million workers or 6 percent of all employees subject to social security in Germany.

We utilize a three-step procedure to identify the effects of FDI on the occurrence of job upgrades and downgrades within firms as well as the separation of workers from these firms. First, we account for the selection of more productive firms into FDI as described by Helpman et al. (2004). We therefore construct a sample of MNEs and domestic firms with similar probabilities of investing, balanced firm characteristics and similar pretrends in the job stability outcome variables.[3] The identifying assumption is that in the period of analysis, noninvesting domestic firms undergo an evolution that is able to serve as the counterfactual for the treatment firms (MNEs) in the unobserved virtual state in which they had not invested abroad. Our iterative matching procedure achieves a unique one-to-one matching of MNEs and domestic firms over the entire observation period. Since we match firms precisely in the same year, we can assign the investment dates for the matched MNEs as *pseudo* investment dates for the domestic firms.[4] Since FDI is conducted based on a decision made well in advance, we match firms three years before the investment. Second, to overcome the (ability-driven) sorting of workers (e.g., Card et al., 2013) into treatment firms, we restrict our data to individuals who already worked in the firm in the year of the match. The treatment and control firms have similar probabilities of conducting FDI; similar firm characteristics (i.e., wages or wage growth); similar likelihoods for upgrades, downgrades and separations; and a significant time lag between the matching and the (pseudo) investment. Therefore, we assume that it is either impossible for workers to distinguish between the two groups of firms or they are indifferent to the choice. Third, we compare the likelihood of job upgrading and downgrading and of 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 hazards models to estimate the effect of FDI on job changes. We define job upgrades (downgrades) as job switches within a firm to occupations with a higher (lower) share of analytical and interactive tasks, which we refer to as *complex tasks*.

Our main finding is that when firms invest abroad, the likelihood that workers upgrade internally to more complex jobs increases by 17%. Simultaneously, the hazard of downgrading to less complex jobs rises by 19%. Both effects increase over time and become substantial two years after the investment. Along the distribution of nonroutine and interactive jobs, we find that it is mostly workers in the first quintile (noncomplex jobs) who are more likely to switch occupations internally. Workers in jobs with low complexity also face only slightly greater hazards of employment separation in MNEs. Altogether, we find no significant effect of FDI on separations. We further investigate whether worker productivity influences workers' job stability in the investing firms. Although workers in MNEs are considerably more likely to switch occupations, MNEs follow a pattern similar to that of domestic firms when choosing whom to upgrade, downgrade or dismiss. Independent of FDI, firms upgrade more productive workers and dismiss or downgrade less

[2] In the majority of FDI datasets, small firms with low investment volumes are underrepresented because only investments above a certain threshold need to be officially registered (see Pflüger et al., 2013). With regard to our analysis, Koerner et al. (2021) shows that only one-fourth of Czech affiliates with a German owner appear in the Microdatabase Direct Investment (MiDi) data provided by the Federal Bank of Germany, which is commonly used to study the FDI of German firms.

[3] The matching part of the empirical strategy—especially matching on the pretrends in the outcome variables—is similar to the strategies used by Schmieder et al. (2022) and Illing et al. (2021).

[4] In most statistical programs, standard commands for propensity score matching do not meet both requirements (e.g., `teffects psmatch` in Stata).

productive workers. Additionally, we find that upgraded workers earn more, whereas downgraded workers earn less, than workers without a job change.

This paper relates to several strands of the theoretical and empirical literatures on the employment effects of FDI in the source country. According to Yeaple (2003), a typical firm expansion abroad follows a complex integration strategy including both efficiency-seeking and market seeking motives. With respect to the analysis of the onshore labor market effect, we, however, place some emphasis on offshoring or vertical FDI, following Marin (2006), who demonstrates that vertical FDI characterizes most investments in the specific German–Czech case. Related theories, such as the seminal theory of task trade by Grossman and Rossi-Hansberg (2008) or the firm-level model by Egger et al. (2015), predict both positive and negative effects of vertical FDI on the employment and wages of domestic workers. Thus, determining the net effect remains an empirical question (e.g., for offshoring from Germany, see Moser et al., 2015; Eppinger, 2019; and Koerner, 2022).

Within the empirical literature, our paper is related to studies on the employment effects of FDI and those differentiating between tasks and the firms' workforce compositions. Becker et al. (2018) show that if FDI is accompanied by a global fragmentation of production chains, MNEs can also specialize the jobs performed by their domestic workers through fewer tasks per occupation. The fragmentation of the production process can, thus, lead to specialization into fewer task sets for some workers, while others might need to perform new tasks in a more complex task set. In a recent study, Laffineur (2019) investigates whether FDI leads to organizational changes. Following the theoretical trade literature such as Caliendo and Rossi-Hansberg (2012), Laffineur assesses firms' organization through a knowledge-based hierarchy model. In accordance with Cortes et al. (2021) and Koerner and Le Moigne (2021), Laffineur shows that FDI raises the number of workers engaged in management tasks and reduces the number of workers engaged in production tasks. Since an international team requires more management and coordination, the demand for complex interactive and analytical tasks also increases when firms become multinational (e.g., Becker et al., 2013 for Germany, Hakkala et al., 2014 for Sweden, or Laffineur and Mouhoud, 2015; Laffineur and Gazaniol, 2019 for France).

Our paper relates in particular to the empirical literature considering the effects of FDI on employment stability. Becker and Muendler (2008) were the first to consider the job separation rates of German MNEs. The authors estimate them to be four percentage points lower than those of domestic firms. Half of this difference can be explained by foreign employment expansions among MNEs. Bachmann et al. (2014) estimate the effects of both inward and outward FDI on employment security in Germany. The authors find that FDI, especially to the Central and Eastern European countries (CEEC), reduces employment security for low-skilled and older workers. In contrast to our paper and to Becker and Muendler (2008), Bachmann et al. (2014) use industry-level data on FDI and cannot analyze the direct effects of firm-level decisions regarding FDI.

A larger body of literature considers the job security effects of offshoring, which—in contrast to FDI—does not include horizontal integration but the trade flows with any affiliate or unaffiliated firm abroad (see, e.g., Munch, 2010 for Denmark; Egger et al., 2007 for Austria; and Geishecker, 2008, Bachmann and Braun, 2011, Görg and Görlich, 2015 for Germany). The offshoring measures, hence, result in less precise estimates of the adjustments to changed labor needs that are due to the exchange of intangibles or a different allocation of production tasks within the firm but across borders (and without measurable trade flows). Within this strand of literature, some studies have also considered occupational switches within and outside the borders of firms. Baumgarten (2015) finds that offshoring—as measured by 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 nonroutine jobs in Germany. These effects are strongest for transitions into nonemployment. The author does not distinguish between occupational upgrades and downgrades. The only other paper that considers up- and downgrades is by Liu and Trefler (2019). These authors are the first to show theoretically and empirically that offshoring services leads to more promotions and demotions in the source country. They use occupational service trade exposures akin to Ebenstein et al. (2014) and find that US service offshoring to China and India increases job upgrades by 6% and job downgrades by 7%. In contrast to these authors, who focus on exposure to service imports, we consider both the manufacturing and the service sector. Moreover, we examine the impact of FDI in particular and not offshoring overall. We believe that over the course of conducting FDI, internal firm restructuring processes play a crucial role because establishing or acquiring foreign firms entails extensive organizational changes. Conversely, offshoring does not necessarily require comparable organizational changes, e.g., if it simply replaces an unaffiliated domestic supplier with a foreign firm.

To adjust to these changes in labor needs after FDI, MNEs can rely on internal labor markets. Becker (1962) already formulated that incumbent workers possess firm-specific human capital, which represents a productivity advantage over outsiders, who, according to Greenwald (1986), might be an adverse selection of workers. Hiring internally reduces asymmetric information regarding the skills and abilities of workers (Waldman, 1984), and thus on-the-job training is often less costly than hiring outside the firm (Demougin and Siow, 1994), especially for skilled workers (Rosen, 1968). Moreover, it can be less costly for MNEs to up- or downgrade workers whose tasks become redundant over the course of FDI than to dismiss them. This might apply especially to labor markets with strict dismissal protection laws, strong works councils and unions. Thus, in addition to making adjustments along the extensive margin represented by hires and layoffs, MNEs have incentives to restructure their workforce internally after investing abroad. To take over skill-intensive management and supervisory tasks, workers may need additional training. Hogrefe and Wrona (2015) provide initial empirical evidence for this argument and demonstrate that offshoring spurs on-the-job training. If FDI also stimulates on-the-job training, we would expect to find 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 used. Section 4 reports our results and discusses their implications. Section 5 summarizes several robustness exercises, and Section 6 concludes.

2. Empirical strategy

Our aim with the empirical analysis is to identify the effect of FDI on job stability. Our approach involves three steps. First, we construct a panel dataset and match MNEs to domestic firms, which act as the counterfactual in our main analysis. Second, we address the sorting of workers into firms. Third, we use proportional hazards models to estimate the influence of FDI on the probability of employment separations and occupational up- and downgrades.

As Helpman et al. (2004) show, more productive firms are more likely to invest abroad. To account for firm selection into treatment, we first apply a propensity score matching approach. Since the anticipation of FDI could already accompany changes in firm characteristics, we match treatment and control firms three years prior to FDI. Conditional on the matching covariates, we assume that the FDI decision is random for firms with the same estimated propensity scores. Our matching procedure returns several important features for our identification strategy: the matched firms 1) feature very similar propensity scores for investing in the Czech Republic using a battery of balanced matching covariates, 2) have the same growth trends over at least four years to account for productivity, and 3) have the same pretrends in job stability (the number of separations and up- and downgrades) over at least four years such that the results of our main analysis are not driven by inherent differences between treatment and control firms. Specifically, the procedure establishes balance in each matching covariate such as the number of employees, wage bills, the number of plants, firm age, employment growth, wage growth, the number of job changes, separations, upgrades, downgrades, the share of females, the shares of three different skill groups, the share of unskilled manual occupations, the share of engineers, industries, and states.

The starting point for the matching procedure is manual enforcement of the region of support for each economic sector. This means that we prune each observation that features a matching covariate that is outside the yearly maximum or minimum for the other group by 3%.[5] For each economic sector, we estimate firm-specific investment propensities using a logit model with observable firm characteristics (see above). Within the same economic sector, these propensity scores are then used to match each MNE to the *unique* domestic firm with the most similar score (without replacement).[6] As the balancing statistics in Table A.4 show, this procedure matches comparable MNEs and domestic firms, with a balanced distribution of firm characteristics between the two groups, similar productivity-related growth variables, as well as similar pretrends for each type of job change for at least seven years prior to our sample period (see also Figure A.1).[7] The major benefits of a matched sample are that it increases internal validity and the robustness of the statistical inference (Imbens and Rubin, 2015).

After matching the treatment and control firms, we consider the onset of workers' risk of switching occupations or leaving a firm two years prior to an investment. This timing accounts for the fact that firms make FDI decisions well in advance, and the anticipation of this decision could lead to changes in the job trajectory well before the FDI is conducted. For example, a firm could separate from some workers with a limited working contract or upgrade workers through due diligence for an acquisition beforehand.

For workers at domestic firms, there is no investment date and thus no inherent interval during which to observe their risk for each event. We therefore need to assign them a pseudoinvestment date, which we obtain by matching firms in the same year and then assigning the same year of FDI as that of the matched MNE.

In the second step, we link the full employment histories of workers to the matched firm data. Although it is impossible to fully control for workers' selection into firms—especially because the dataset contains no information on productivity—we mitigate such threats to identification by restricting our data to individuals who already worked at the firm at the time of matching, i.e., three years prior to the (pseudo) investment. Given the similar propensities for FDI at the time of the matching, it is very difficult for such workers to distinguish between future MNEs and domestic firms. Moreover, it is likely that the workers are almost indifferent between employers, since treatment and control firms are on very similar growth paths (wage and employment), feature almost identical hazards regarding job stability (e.g., chances of upgrades or threat of separations), and exhibit very similar firm characteristics, such as the probability of conducting FDI, among others (e.g., mean wages and the share of high-skilled workers).

In the final step, we estimate the effects of FDI on the individual-level likelihood of switching jobs within a firm and of separating from a firm. To reap the benefits of the event history design of our data, we use (Cox, 1972) proportional hazards models to measure the effects of FDI on job stability.[8] We estimate the hazard ratios for employment separations and occupational up- and downgrades

[5] We distinguish among 4 economic sectors: agriculture, manufacturing, construction, and private services.

[6] See Appendix A.2 for details on the matching algorithm. Our choice to use unique 1:1 matching is due to the good proportion of well-balanced statistics (Austin, 2011a) and relatively high external validity (the number of control observations). To ensure that the nearest neighbors are not too different, we calculate the optimal caliper width as recommended by Austin (2011b). 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 standard deviations of the logit of the propensity score.

[7] Propensity score matching has been previously used in the FDI context in a wide range of studies, e.g., Bronzini (2015) and Crinò (2010) for Italy; Hijzen et al. (2011) for France; Debaere et al. (2010) for Korea; Barba Navaretti et al. (2010) for France and Italy; Becker and Muendler (2008), Kleinert and Toubal (2007) and Koerner et al. (2021) for Germany; Hijzen et al. (2007) for Japan; and Egger and Pfaffermayr (2003) for Austria. However, the majority of these studies consider FDI effects at the firm level, not the individual level.

[8] Our research question is a typical application for proportional hazards models. Compared to linear probability models and logit or probit models, proportional hazards 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 in the analysis of up- and downgrades, censoring is prominent in our data, and we thus prefer proportional hazards models. Furthermore, proportional hazards models allow us to investigate how the effects of FDI change over time.

in separate models and censor observations in the case of competing events, meaning that in each model, we exclude observations after they document any job change (separation, upgrade, or downgrade):[9]

$$\ln h_e(t|x_{ity}) = \ln h_{0e}(t) + \gamma I(\text{MNE}_f) + \boldsymbol{\beta_1'} \boldsymbol{x_{ift}} + \boldsymbol{\beta_2'} \boldsymbol{x_{ift}} \times t + \tau_y + \eta_s + \omega_r + \theta_o. \quad (1)$$

Here, $h_e(t|x_{ity})$ is the hazard rate for event $e \in$ {separation, upgrade, downgrade} in quarter t after the (pseudo) investment. The respective baseline hazard rate is denoted as $h_{0e}(t)$. The next term, $I(\text{MNE}_f)$, indicates whether firm f is an FDI-conducting firm (MNE) or a domestic firm. We are mainly interested in the static treatment effect γ. Further, $\boldsymbol{x_{ift}}$ is a vector of time-varying worker i characteristics such as age, age squared, experience, tenure, a binary categorical variable for German nationality, and skill dummies as well as time-varying firm f characteristics such as firm age and a dummy if the firm existed in 1975 (all interacted with the number of quarters since treatment) and indicator variables for the occupation, year, and federal state of the firm. We also include an interaction term between these characteristics and the number of quarters from the (pseudo) investment $\boldsymbol{x_{ift}} \times t$. Note that we do not include information on the dynamics of employment, wages, or the recomposition of workers, as such variables would generate a bad control problem. By introducing fixed effects τ, ω, and θ, our model also captures any unobserved heterogeneity specific to calendar year y, industry of economic activity s, state/region (*Bundesland*) r or occupation o.

In our setting, workers become at risk of separation or up- or downgrade two years prior to the (pseudo) investment, and we trace them for up to eight years. We define occupational switches within a firm as upgrades if the intensity of analytical and interactive tasks is higher in the new job than in the old one and as downgrades if the intensity of analytical and interactive tasks decreases. We summarize analytical and interactive tasks with the term *complex tasks*. Because task compositions may vary within occupations and their imprecise measurement could change the direction of switches, we compare old and new jobs using a static task measure. Employment separations occur if workers leave a firm.

Note again that we treat competing events with censoring. This means that after the occurrence of any job change (e.g., an occupational upgrade), we remove workers from the risk set for the other two events (e.g., occupational downgrades and job separations). The underlying rationale is that FDI affects the job stability of workers in the MNEs heterogeneously with respect to observable (e.g., occupations, tasks, and age) and unobservable (e.g., motivation and productivity) worker characteristics. 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's FDI distinctly alters the likelihood of each event and for each individual. Consider a firm that, after FDI, rearranges its task allocation across country borders to exploit international factor price differences (see, e.g., Helpman, 1984; Markusen, 2002). On the one hand, the firm's new allocation of labor includes reduced onshore demand for offshored tasks and could raise the hazard of separations and up- and/or downgrades for associated workers. On the other hand, it is likely that the firm's demand for complex supervisory and management tasks increases in order to cope with the organization of an international team due to the acquisition of a foreign affiliate. FDI can thus affect tasks in the firm's up- and downstream processes. Overall, such changes in the firm's task demand may lead some incumbent workers to take over new tasks, which can result in occupational up- and downgrades in all areas of the firm. The complex interplay between worker performance and firm objectives reflects various causal mechanisms that idiosyncratically influence the probabilities of separations, upgrades, and downgrades. While we regard competing events as mutually exclusive (censoring) in the baseline specification, in the robustness section, we show that the results are not sensitive to the omission of this assumption (i.e., in Table 3, where separations follow up- or downgrades).

The baseline model given by Eq. (1) captures the time-constant effects of FDI on job stability, i.e., the average effect over the eight-year interval from two years prior to the FDI to six years afterwards. However, it is possible that the effect of FDI varies over time. If, for example, workers need further training to switch occupations within a firm, we would not observe any effects of FDI immediately after investment. Thus, we estimate the influence of FDI on job stability over four two-year time intervals d_t with the following equation:

$$\log h_e(t|x_{ity}) = h_0(t) + \sum_{j=1}^{4} \gamma_j I(\text{MNE}_f) \times d_t^j + \boldsymbol{\beta_1'} \boldsymbol{x_{ift}} + \boldsymbol{\beta_2'} \boldsymbol{x_{ift}} \times t + \tau_y + \eta_s + \omega_r + \theta_o, \quad (2)$$

where $I(\text{MNE}_f) \times d_t$ is the interaction between the investment dummy and the time interval relative to the investment. The remainder of Eq. (2) is identical to Eq. (1). Because treatment is assigned to firms (and not to workers), we cluster standard errors at the firm level in both models (see Abadie et al., 2022).[10]

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*).[11] The ReLOC data include the

[9] For example, if we did not censor observations for the periods after the first event, we would consider a worker who is downgraded in one period and then separates in a subsequent period in both regressions. Such a design would overstate the true effect of FDI on the average worker, especially if only a small fraction of workers is affected multiple times.

[10] This is also why more disaggregated fixed effects at the firm or worker levels would render our treatment effect collinear.

[11] Please refer to Hecht et al. (2013b) for details on the ReLOC dataset.

entire universe of German firms with affiliates in the Czech Republic according to the Czech commercial register for 2010. ReLOC covers more than 2,800 German investors and the exact date of their investment.[12,13] To compare developments in investing firms with those in domestic firms, a control group of more than 8,100 German firms without any foreign affiliates (in any country) completes the ReLOC data.

We link the ReLOC data to two administrative microdatasets from the Institute for Employment Research (IAB). We receive 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 structures of all German establishments with at least one employee subject to social security contributions as of June 30 between 1975 and 2014.[14] The IEB includes the complete employment biographies of all individuals in the German social security system after 1975. In particular, the data provide information on occupations and employment spells with daily level precision. Because both the BHP and IEB use mandatory social security notifications for all German employers, they are highly reliable. Applying record linkage, Schäffler (2014) combines the ReLOC and BHP data. The resulting dataset groups establishments into firms and provides investment information at the firm level. The firm-level information for a region or industry is based on data regarding the largest share of workers within a firm. 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* in 1990 and ends with the most recent registered investments in the ReLOC data from 2010.

To identify occupational up- and downgrades, we extend our dataset with the occupational task structures, which we derive from the 1999 *BIBB-IAB Employment Survey* (see Hall and Tiemann, 2006). Similar to Autor et al. (2003) and Spitz-Oener (2006), for each occupation, we retrieve the share of each of five task categories—i.e., routine-manual, routine-cognitive, nonroutine-manual, analytical, and interactive activities—by using an algorithm developed by Matthes (forthcoming), which is described in Appendix A.1.

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 use only the job with the highest earnings. To ensure that we do not mistake maternity leave or retirement for job separations, we restrict the sample to male workers between the ages of 20 and 55 at the time of investment. Further, we consider only regular full-time workers for two reasons. First, we are interested only in *regular* job changes and not in, e.g., switches from part- to full-time employment or from marginal to regular employment.[15] 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, two years prior to the (pseudo) investment, had worked at their firms for at least one year (i.e., the year of matching). We correct inconsistent information on individual-level educational attainment following Fitzenberger et al. (2005). Furthermore, the wages for approximately 10% of the spells are right-censored due to the contribution assessment ceiling in Germany. We impute these records using an imputation procedure that follows Dustmann et al. (2009) and Card et al. (2013).

3.2. Descriptive statistics

Fig. 1 presents an overview of the individual and firm characteristics identified after applying our matching algorithm. The box plots and bar charts indicate that the distributions of the features of the MNEs and the domestic firms are well balanced in the quarter of the match. Although worker characteristics were not part of the matching procedure, the resulting sample features well balanced worker characteristics such as work experience, tenure and skill. Moreover, most of the firm-level characteristics of the treatment and control firms are almost equivalent in their medians and first and third quartiles. Since the dataset includes no direct measure of firm productivity, we approximate this information with firm size, growth, and survival. The figure shows that both firm groups are similar in their productivity-related characteristics such as the number of plants per firm, the number of employees, the wage bill, four-year employment growth, four-year wage growth, and firm age. Since the unmatched sample has different pretrends (see Table A.3 in the appendix), we also need to restrict the sample to comparable firms with regard to job stability. To account for such job changes (separations, upgrades and downgrades), we directly include the four-year aggregate of such switches as a matching variable. Finally, the firms are not only similar in productivity, size, and skill composition, but as Figure A.1 in the appendix shows, the firms also exhibit very similar likelihoods of any kind of job change for at least seven years before the start of our sample period in the regressions (i.e., two years prior to FDI).

The focus of this article is on occupational up- and downgrades. Fig. 2 therefore visualizes the changes in analytical and interactive tasks for workers who switch occupations within a firm. Based on these changes, we define occupational upgrades as job switches accompanied by an increase in analytical and interactive tasks (bins to the right of zero) and downgrades as job switches accompanied by a decrease in analytical and interactive tasks (bins to the left of zero). Common upgrades in our data include, e.g., upgrades from locksmiths to technicians or metal workers to warehouse managers. The former upgrade leads to a broader, less routine set of tasks; the latter upgrade increases supervisory responsibilities. Frequent downgrades include, e.g., electricians

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

[13] The dataset, however, includes no information on other FDI activities by MNEs. If such FDI has the same onshore labor market effects, these activities may overstate the true effect of German FDI in the Czech Republic. Note, however, that the effects would still be caused by FDI.

[14] Please refer to Eberle and Schmucker (2017) for details on the BHP.

[15] The inclusion of marginal or part-time employment in the analysis would generate confusion about the correct job title because we would not know whether any change in job title would be due to changes in the worker's working time.

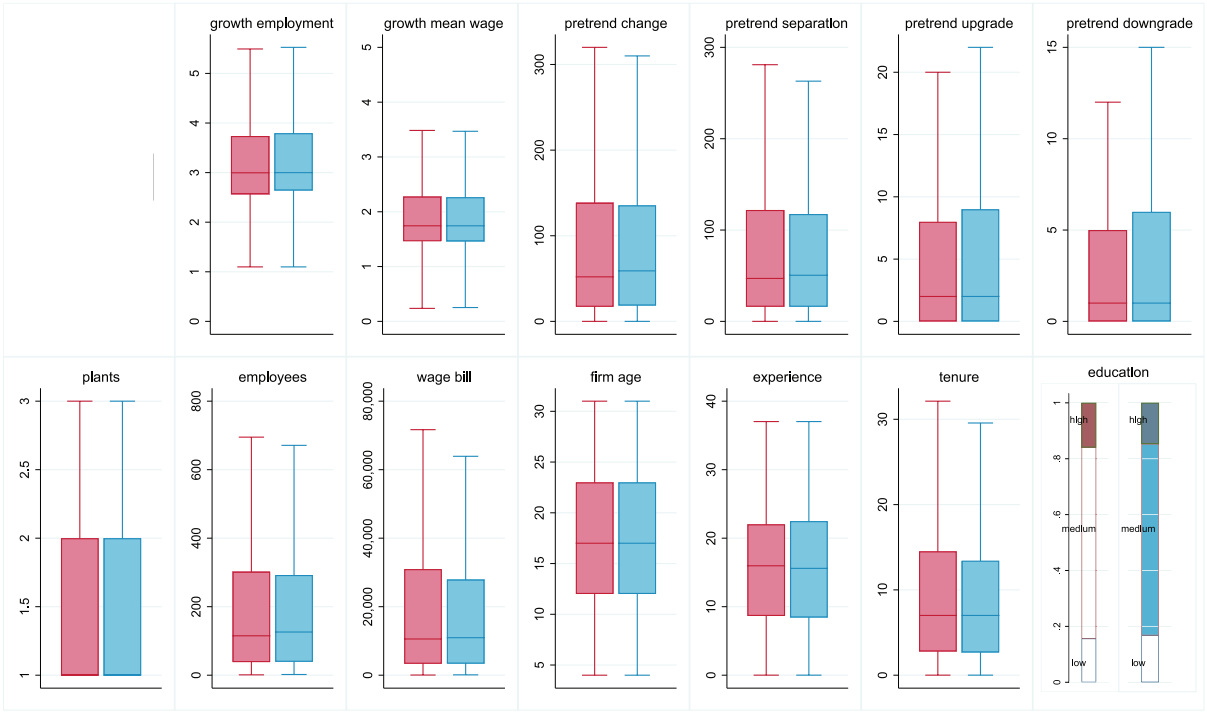

Fig. 1. Worker and firm characteristics after matching.
Notes: The figure shows box plots and bar charts for various firm and worker (experience and tenure) variables for the year of the match. The horizontal line in the middle of a box represents the median. The edges of a box indicate the first and third quartiles. The range of the whiskers illustrates minima and maxima, limited to $\frac{3}{2}$ of the first or third quartiles, respectively. For the education variable, the figure presents bar charts, which depict the shares of individuals in the corresponding group.
Source: ReLOC, IEB and BHP, own calculations.

to metal workers or locksmiths to metal connectors. Both downgrades lead to a less complex task set. Appendix A.6 lists the most frequent up- and downgrades. Fig. 2 further 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%

Having defined up- and downgrades, let us now descriptively assess their relative frequencies in MNEs and domestic firms. Fig. 3 illustrates the cumulative hazards for separations and up- and downgrades. The cumulative hazard indicates the probability of an event occurring within a given timeframe. The upper-left panel of Fig. 3 shows that the hazard of receiving a job upgrade is greater 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. Twenty-four quarters after FDI, the probability of receiving an occupational upgrade is 7.2% in MNEs, while it is 6% in domestic firms. The development of the risk of downgrades is similar, although of a lower magnitude, with cumulative hazards of approximately 5% in MNEs and 4% in domestic firms. The lower panel of Fig. 3 illustrates the risk of separation, which is higher than the likelihood of either type of occupational change within a firm. The separation rates, however, barely differ between the two types of firms. They are only slightly lower in MNEs than in domestic firms.

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

4. Results

4.1. Main results

This section presents estimates of the impact of FDI in the Czech Republic on the job stability of workers in the investing firms in Germany. We distinguish between effects on the likelihood of separations between workers and firms, upgrades into more complex jobs and downgrades into less complex jobs within firms.[16]

Fig. 4 illustrates Table A.8 in the appendix, that is, the estimates from Eqs. (1) and (2). The horizontal red lines in Panels A, B and C indicate the estimates from the static models for separations, upgrades and downgrades. The blue lines show the time-dependent

[16] We parallelized the estimations of Cox models using the ado-file `parallel` by Vega Yon and Quistorff (2019).

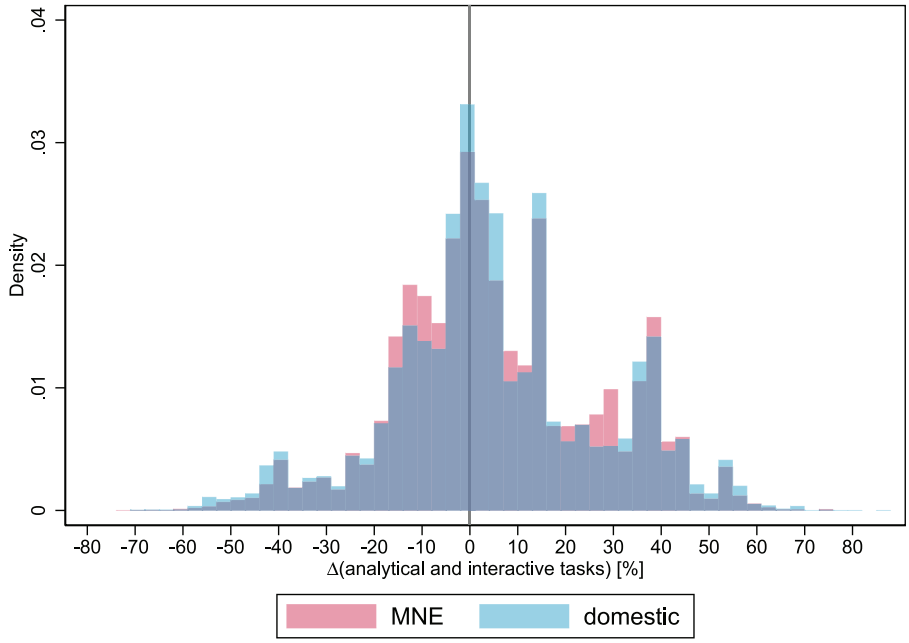

Fig. 2. Histograms of up- and downgrades in MNEs and domestic firms
Notes: The figure shows the distribution of up- and downgrades by percentage point changes in the share of analytical and interactive tasks for job switches within investing (MNE) and domestic firms. We define job upgrades (downgrades) 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 are shown to its left.
Source: ReLOC, IEB and BHP, own calculations.

hazard ratios. Shaded areas indicate 95% confidence intervals. The dashed line in each panel has an intercept of one. It serves as a reference line indicating the case where the evolution for our counterfactuals—the noninvesting domestic firms—is the same as that for the MNEs.

While time-invariant hazard ratios indicate no effect of FDI on workers' separation rates, more flexible time-variant estimates suggest instead increasing job stability after the FDI. Specifically, the hazard of separation is 3.6% higher in MNEs in the two years before FDI and roughly 3.4% lower from five to six years after investment. Over the eight-year sample period, however, the estimated coefficient is never statistically significant. We therefore conclude that there is no evidence for an increased risk of separation for the average worker.[17] The results for separations are in accordance with and add to the existing literature on the industry-level finding of no or very limited employment effects from FDI. Bachmann et al. (2014), for instance, reveal that industry-level FDI does not significantly affect individual separation rates.[18] Baumgarten (2015), moreover, finds no significant effect of offshoring on the hazard of nonemployment on average.

Panels B and C of Fig. 4 illustrate the effect of FDI on the hazard ratios for up- and downgrades. The line for the static model shows that upgrades are about 17% more likely when firms conduct FDI, while the likelihood of downgrades increases by 19%. The latter effect crosses the reference line slightly and the difference relative to the control firms is significant only at the 10%-level. Regarding the dynamic specification, both graphs show no instantaneous effect of FDI on the likelihood of job switches within a firm. Instead, the effects evolve over time and become statistically significant approximately two years after investment. In MNEs, the likelihood of upgrading (downgrading) to a more (less) complex job increases to 32% (35%) two to four years after FDI and to 46% (48%) four to six years after FDI. 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 investment but first establish a supply network. Further, it takes time to negotiate new positions with incumbent workers, and it might be necessary to retrain workers before they can fill new positions (see Hogrefe and Wrona, 2015).

Although this paper analyzes vertical and horizontal FDI, our results are in line with the theoretical predictions regarding task trade made by Egger et al. (2015) and Grossman and Rossi-Hansberg (2008). These authors argue that the positive productivity effect of offshoring could outweigh the negative effects for workers with offshorable jobs. In our analysis, this would imply that the

[17] Note, however, that using the same dataset, Koerner et al. (2021) show that the number of employees in MNEs grows more slowly than in purely domestic firms until 6 years after FDI.

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

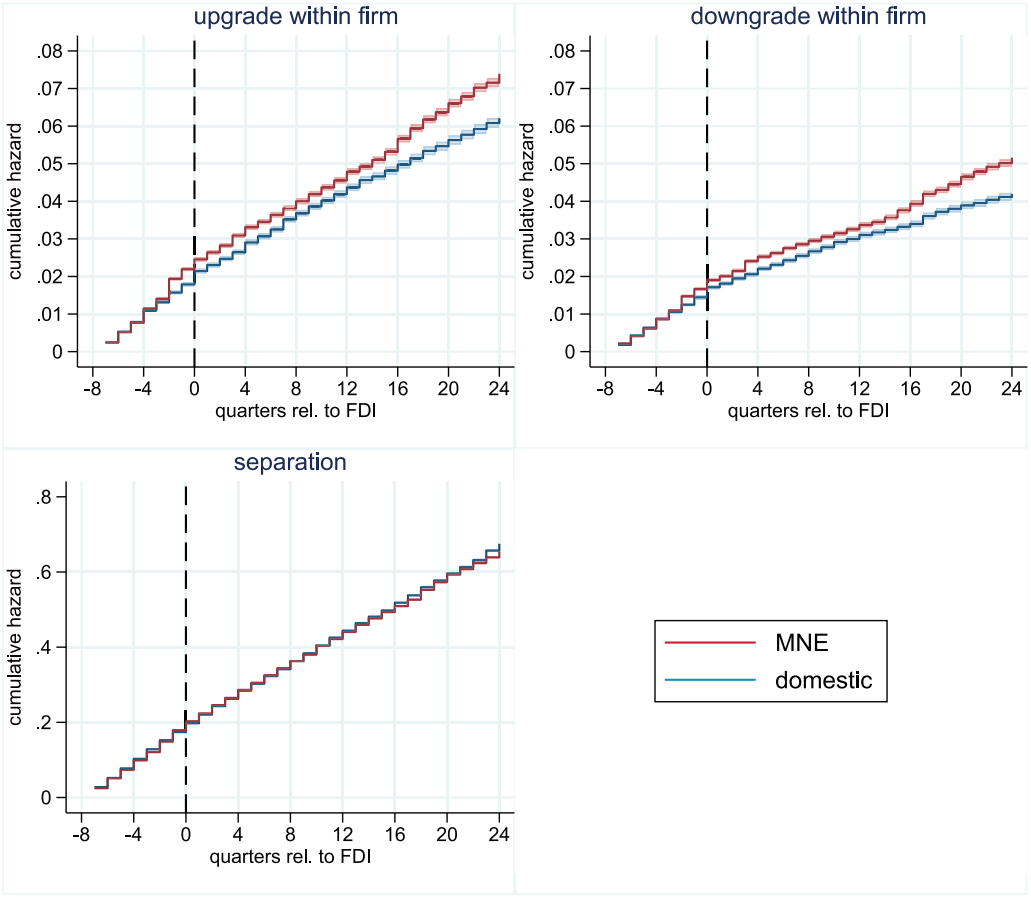

Fig. 3. Cumulative hazards for up- and downgrades and separations in MNEs and domestic firms
Notes: The figure shows the cumulative hazards for three events, *separation from the firm* as well as *internal upgrades and internal downgrades* by quarter from 8 quarters prior to 24 quarters after the (pseudo) investment. The figure distinguishes between investing firms (MNEs) and domestic firms. The light blue and light red colors indicate 95% confidence bands. The cumulative hazard indicates the probability of an event occurring within a given timeframe. For instance, the individual-level hazard of receiving an occupational upgrade 24 quarters after FDI is 7.2% in MNEs (first panel). The hazards of occupational up- and downgrades are significantly greater in MNEs than in domestic firms. By contrast, the hazard of separations is slightly greater in domestic firms. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)
Source: ReLOC, IEB and BHP, own calculations.

improved cost–benefit ratio from (foreign) affiliate production also prevents dismissals of onshore workers. Moreover, the relatively strong labor unions in Germany could raise the cost of separations such that MNEs adjust their workforces through the channel of hires (see Koerner et al., 2021).

4.2. Job stability and tasks

It is possible that not all workers in the investing firms are homogeneously affected by FDI. One strand of the literature argues, for instance, that the effects of multinational activities are substantially dependent on the tasks that are performed on the job (e.g., Blinder, 2006). In particular, scholars classify routine (Levy and Murnane, 2004), codifiable (Leamer and Storper, 2001), and noninteractive tasks (Blinder, 2006) as prone to substitution by foreign low-wage labor. These differences in the substitutability of different types of labor could also have an impact on the onshore labor demand for specific tasks. In this section, we therefore explore the heterogeneous effects of FDI depending on the task composition of the initial job. Following the literature, we analyze occupations along the task dimension using the shares of nonroutine and interactive tasks (task complexity).

Fig. 5 highlights the impact of FDI in the Czech Republic on job stability in German firms depending on the initial task complexity of jobs. For ease of interpretation, we order workers by their jobs' task profiles. On the left side of the x-axis, jobs feature low shares of nonroutine and interactive tasks, whereas this share increases toward the right. Technically, the graphs show the interaction effect between MNEs and the share of nonroutine and interactive tasks (see Table A.9 in the appendix). The latter enters the interaction with FDI in two different specifications—either as a linear index (red line) or as quintiles of the task distribution (blue line). Note that the x-axis scale ranges from 40% to 100% because there are practically no occupations composed of less than 40% nonroutine and interactive tasks (see Figure A.2 in the appendix).

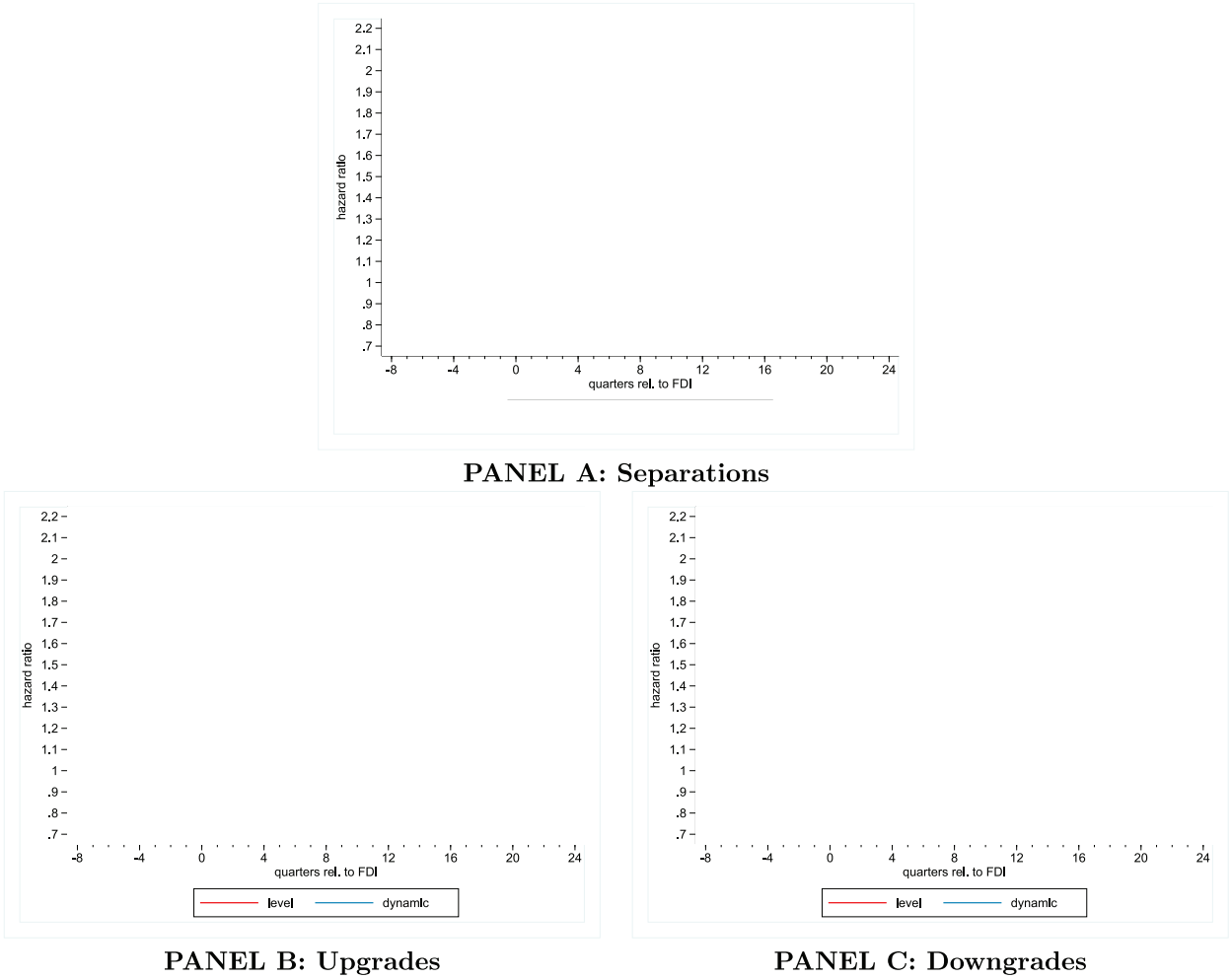

Fig. 4. Dynamic effects of FDI on the hazard ratios for separations and up- and downgrades
Notes: The figures provide a graphical representation of the hazard ratios and 95% confidence intervals for the estimated effects of FDI on separations and up- and downgrades. The results are obtained from the Cox regressions presented in Table A.8. The red lines display the level effects of FDI, i.e., the average effects from 2 years before to 6 years after investment. The blue lines show the development of the estimated hazard ratios over time. The deviation of the estimated hazard ratios from one can be interpreted as changes attributable to FDI in the probabilities of each event. For example, an estimated hazard ratio of 0.9655 for separations indicates that FDI reduces the individual-level risk of separation by 3.45% in the corresponding quarters. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)
Source: ReLOC, IEB and BHP, own calculations.

Turning to the results, Panel A in Fig. 5 shows no increase in the separation rate after FDI for each type of worker (95% confidence intervals cross the reference line). However, looking more closely (at the estimates in Table A.9 in the appendix), we observe imprecisely estimated positive coefficients for the separation rate among jobs with a lower share of nonroutine and interactive tasks (significant at the 10% level). One interpretation of these coefficients is that jobs with low levels of complex content seem to exhibit more substitutability with the foreign affiliate's production.

The results for upgrades in Panel B imply that the probability of switching positions within a firm is positive in MNEs and decreases with respect to the share of nonroutine and interactive tasks. Moreover, this linear relationship is mainly driven by jobs with a low complexity share, as the lower quintiles exhibit higher likelihoods for upgrades after FDI.

Panel C reports the results for the heterogeneity in downgrades along the task dimension. It reveals that jobs with less complex tasks also feature higher probabilities for downgrades after FDI. As in the cases of the other types of job changes, these workers have the strongest response to the changed labor demand of the investing firms.[19] Downgrades, however, feature a nonlinear relationship along the task share, and that relationship is strongest in the first and fourth quintile of the distribution. Common downgrades

[19] Table A.7 documents the plants in which the job changes occur. All numbers hint toward the interpretation that most job changes occur in the onshore production plants and not in the headquarters or other administrative offices. The type of plants were identified using occupational groups by Blossfeld (1985).

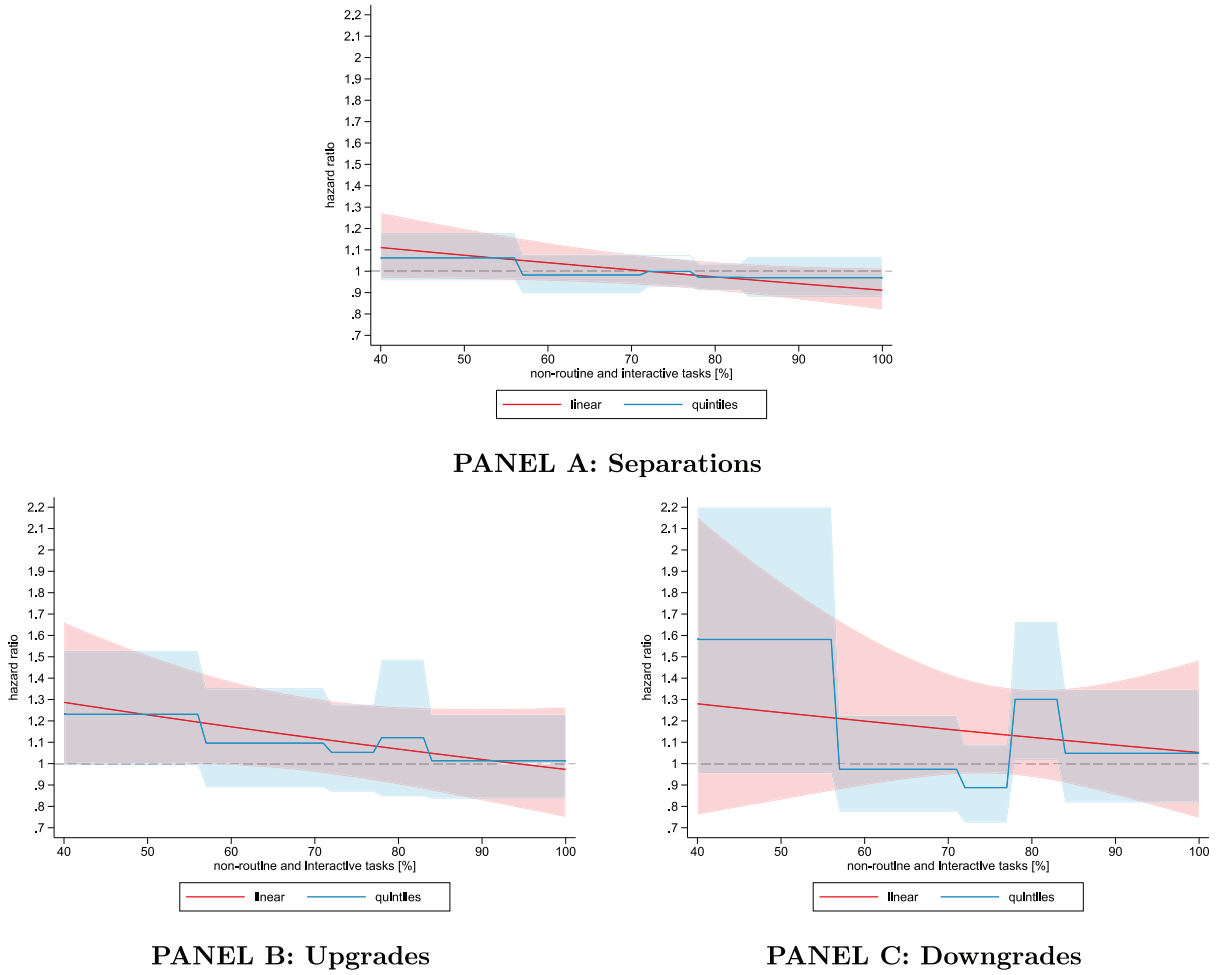

Fig. 5. Effects of FDI on the hazard ratios for separations and up- and downgrades depending on the share of nonroutine and interactive tasks.
Notes: The figures provide a graphical representation of the hazard ratios and 95% confidence intervals for the estimated effects of FDI on separations and up- and downgrades. The red lines plot these estimated hazards against the worker's initial share of complex tasks, i.e., nonroutine and interactive tasks. The results are obtained from the Cox regressions presented in Table A.9 in the appendix with an interaction between FDI and the share of nonroutine and interactive tasks. The blue lines report the estimates from a specification using the interaction between FDI and the quintiles of the task distribution instead. The estimated hazard ratios are averages over the eight-year period surrounding the time of the investment. As Figure A.2 in the appendix shows, the share of nonroutine and interactive tasks ranges between 40% and 100% in the data. The range of nonroutine and interactive tasks in Fig. 5 is restricted accordingly. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)
Source: ReLOC, IEB and BHP, own calculations.

involve, for example, transitions from locksmiths (very nonroutine) to metal connectors (see Table A.6 in the appendix). Since metal connectors typically carry out only a subset of a locksmith's tasks, this reduction in the complexity of tasks is an adjustment to the fragmentation of the production process. This effect is in accordance with Becker et al. (2018), who show that occupations in the domestic part of a firm specialize in narrower task sets while employing a broader range of different occupations.

These results are also in line with those of previous studies such as Baumgarten (2015). The author shows that industry-level imports of inputs from Eastern Europe increase the likelihood of transitions into nonemployment and occupational switches among exposed workers. This is particularly pronounced for jobs with low shares of nonroutine tasks. Additionally, our outcomes for job switches are, to some extent, comparable to the outcomes from studies on workforce composition. In line with our results, Hakkala et al. (2014) find evidence of a shift toward more complex tasks in Swedish MNEs.

4.3. Job stability and unobserved worker productivity

In this section, we shed further light on the mechanisms underlying 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 equations. We use standard controls from the labor literature, such as quadratic polynomials in age, experience,

Table 1
Effects of FDI on the hazard ratios for separations and up- and downgrades depending on unobserved worker productivity.
Source: ReLOC, IEB and BHP, own calculations.

	Separation (1)	Upgrade (2)	Downgrade (3)
MNE	0.9823	1.1712**	1.0920
	(0.0383)	(0.0944)	(0.1090)
Wage rank	0.9977***	1.0043***	0.9899***
	(0.0003)	(0.0008)	(0.0009)
MNE × wage rank	1.0005	1.0001	1.0020*
	(0.0003)	(0.0009)	(0.0011)
Subjects	450,052	450,052	450,052
Events	195,726	20,873	14,754

Notes: The table presents exponentiated coefficients (hazard ratios) and cluster robust standard errors at the match 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, industry, year, and state dummies. Estimates are based on a matched sample of MNEs and domestic firms.

and tenure; skill level; and dummies for foreign nationality, two-digit occupation and year. We then rank all workers within a firm according to their estimated wage residuals (in bins of 100). Technically, the wage residual captures the positive or negative wage premia that workers earn relative to those of workers with identical observable characteristics (e.g., the same education, work experience, and occupation). Ranking residual wages within firms additionally nullifies all time-invariant firm-specific effects on wages. Economically, the ranking of residual wages within a firm should reflect unobserved worker productivity. We expect workers with high (low) unobserved productivity to have better (lower) chances of being upgraded and to be less (more) likely to be downgraded or to leave a firm.

Table 1 presents estimates of our main specification extended with workers' positions in the wage ranking and an interaction between the ranking and the FDI indicator. Compared to the magnitudes of our baseline estimates (Table A.8), the sizes of the coefficients on FDI change somewhat. However, these changes are simply the result of the interaction between FDI and the wage ranking. For workers in the middle of the ranking, the effects are close to our baseline estimates (e.g., for upgrades, $1.1712 \times 1.0001^{50} = 1.1771 \approx 1.1729$), while at the median wage ranking, we find no effect of FDI on separations, and FDI increases the likelihood of up- and downgrades by 17.7% and 20.7%, respectively.

Our main estimates with the wage ranking indicate that the job stability of workers indeed depends on their unobserved ability. This implies that more productive workers are less likely to be dismissed or downgraded and are more likely to receive occupational upgrades. Specifically, an increase in the residual wage ranking of one (on a scale of one to 100) reduces the hazard of separations by 0.23% and the hazard of downgrades by approximately 1%. The likelihood of promotions increases by 0.43%. Note that these effects barely differ between MNEs and domestic firms.

To shed more light on the potential differences in the effect of the wage ranking between the two groups of firms, we also include an interaction term. The term shows that heterogeneity along the wage ranking does not differ between separations and upgrades. However, a high residual wage seems to protect workers against downgrades slightly less in investing firms than in domestic firms.

Overall, the three models indicate that MNEs follow patterns similar to those of domestic firms when choosing whom to upgrade, downgrade or dismiss in terms of individual productivity. This result is not surprising. Although MNEs are more likely to restructure, the shape of their restructuring is comparable to the dynamics found in domestic firms. Workers with lower productivity always face higher risks of dismissal and downgrades, and workers with higher productivity face a higher likelihood of upgrades, independent of whether the firm conducts FDI.

4.4. Job stability and wages

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

$$\log w_{it} = I(\text{up}_{it})\eta_1 + I(\text{down}_{it})\eta_2 + I(\text{MNE}_j)I(\text{up}_{it})\theta_1 + I(\text{MNE}_j)I(\text{down}_{it})\theta_2 + \boldsymbol{x_{it}}\boldsymbol{\beta} + \mu_i + \tau_y + u_{it}. \quad (3)$$

Here, $I(\text{MNE}_j)$, $I(\text{up}_{it})$ and $I(\text{down}_{it})$ are indicators for treatment firms, upgrades and downgrades, respectively. Vector $\boldsymbol{x_{it}}$ includes basic worker controls, μ_i denotes worker fixed effects, τ_y is a series of year dummies, and u_{ijt} is the error term.[20] We are interested mainly in the general wage differences experienced by up- and downgraded workers (η_1 and η_2) and in whether FDI amplifies these wage differences (θ_1 and θ_2).

We estimate the model using the sample of matched firms. We also restrict the sample to the four quarters after the first up- or downgrade to mitigate confounding effects from other events on the wage differences experienced by up- and downgraded workers. Additionally, we compare only the wages of workers who remained at their initial firms.

[20] Basic worker controls include polynomials in age, tenure, and work experience.

Table 2
FDI, up- and downgrades and wages.
Source: ReLOC, IEB and BHP, own calculations.

	(1)	(2)	(3)	(4)	(5)	(6)
MNE	0.0234***	0.0233***				
	(0.0006)	(0.0006)				
Upgrade	0.0778***	0.0818***	−0.0004	0.0002	0.0007	0.0033
	(0.0024)	(0.0039)	(0.0019)	(0.0031)	(0.0019)	(0.0032)
Downgrade	−0.0949***	−0.1120***	−0.0398***	−0.0675***	−0.0399***	−0.0687***
	(0.0029)	(0.0048)	(0.0023)	(0.0039)	(0.0023)	(0.0040)
MNE × upgrade		−0.0064		−0.0010		−0.0042
		(0.0049)		(0.0039)		(0.0040)
MNE × downgrade		0.0273***		0.0430***		0.0443***
		(0.0061)		(0.0048)		(0.0049)
Worker fixed effects	No	No	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	No	No
Firm-year fixed effects	No	No	No	No	Yes	Yes
Observations	1,972,261	1,972,261	1,972,284	1,972,284	1,971,840	1,971,840

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 match level (in parentheses). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 2 summarizes the results of the wage regression. The first two columns show estimates obtained without worker fixed effects, which allows us to include an MNE dummy, as well as firm age, and time-invariant controls for being a German native, the skill level, the occupation, industry, and state. The estimates suggest that during the observation window, workers in MNEs earn 2.3% more than comparable workers in domestic firms. In line with our expectations, we also find that upgraded workers earn more than workers without an upgrade, whereas downgraded workers have lower wages than workers without any job change. These results, however, may suffer from selection bias since—as the previous subsection shows—more productive workers are more likely to upgrade and less productive workers are more likely to downgrade. To shed more light on this concern, columns 3 and 4 include worker fixed effects, which capture any time invariant heterogeneity between workers. Note that some confounding factors such as match-specific productivity enhancements still remain. Therefore, we also include firm-year fixed effects in columns 5 and 6, which control for yet more confounders due to common shocks at the firm level (e.g., spillover effects from the workforce composition). The estimates, however, remain fairly robust to this extension.

The results from the latter specifications reveal that upgrades do not coincide with an additional real wage increase. Downgrades, by contrast, come along with wage decreases that are more pronounced in domestic firms. Workers who switch to a less complex job earn approximately 4% less than workers who remain in their initial jobs. The difference is less intense for workers in MNEs than for workers in domestic firms ($-6.75+4.3=-2.45$). We assume that the productivity effect of FDI is the reason that the adverse effect for downgraded workers is mitigated.[21]

5. Robustness checks

In this section, we perform several robustness exercises. Specifically, we assess the competing risks 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. Noncompeting risks

In Section 2, we assume that separations and up- and downgrades are mutually exclusive events during our sample period. We therefore 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 a firm. Table 3 shows the results (column 2) and re-reports the estimates from our baseline specification (column 1) for comparison. Both models yield the same results and show no effect of FDI on job separations. Hence, these results affirm that the conclusions drawn from the main specification are not driven by the assumption of competing risks.

In column 2, we control for preceding up- and downgrades within a firm. Independent of FDI, an occupational upgrade reduces the hazard of a separation by approximately 5%. By contrast, past downgrades increase the hazard of separations by 8%. The estimates affirm our expectation that *good* workers receive upgrades and, therefore, are less likely to be dismissed, whereas unproductive workers receive downgrades and are more likely to be dismissed.

[21] Since wages in Germany are sticky due to downward rigidities and not all downgraded workers would accept this occupational switch with an additional wage decrease, we assume that one of the channels left open to German firms for lowering wages in real terms is to not give wage increases (at positive inflation rates).

Table 3
Effect of FDI on separations: competing vs. noncompeting risks models.
Source: ReLOC, IEB and BHP, own calculations.

	Separation	
	Baseline	No competing risks (no censoring of events)
	(1)	(2)
MNE	1.0058	0.9989
	(0.0339)	(0.0335)
Preceding upgrade		0.9473*
		(0.0266)
Preceding downgrade		1.0795**
		(0.0349)
Subjects	460,767	460,770
Events	201,599	213,603

Notes: The table presents exponentiated coefficients (hazard ratios) and cluster robust standard errors at the match 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, industry, year, and state dummies. Estimates are based on a matched sample of MNEs and domestic firms.

Table 4
Logit estimates of the effect of FDI on separations and up- and downgrades.
Source: ReLOC, IEB and BHP, own calculations.

	Separate logit models by events			Multinomial logit model		
	Separation	Upgrade	Downgrade	(Base category: no event)		
	(1)	(2)	(3)	(4)		
				Separation	Upgrade	Downgrade
Panel A: Starting 2 years prior to FDI						
FDI	0.9751	1.1403*	1.1552	0.9848	1.1399*	1.1133
	(0.0500)	(0.0810)	(0.1015)	0.0537	0.0868	0.1032
N	456,669	456,272	434,647	456,670		
Log lik.	−275156.5	−78663.737	−59418.513	−392133.62		
Panel B: Starting at quarter of FDI						
FDI	0.9403	1.1856**	1.1957*	0.9416	1.1717*	1.1449
	(0.0510)	(0.0933)	(0.1177)	0.0540	0.0959	0.1167
N	382,948	382,590	362,344	382,959		
Log lik.	−212791.67	−53033.897	−38621.659	−296731.84		
Panel C: Starting 2 years after FDI						
FDI	0.8624*	1.3543**	1.4983***	0.8743	1.3189*	1.4371**
	(0.0698)	(0.2043)	(0.2350)	0.0734	0.1948	0.2666
N	207,076	205,683	195,231	207,087		
Log lik.	−95432.19	−20507.233	−14063.041	−129434.57		

Notes: The table presents exponentiated coefficients (odds ratios) and cluster robust standard errors at the match 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, state, and industry dummies. The multinomial logit does only include one-digit occupational dummies. Estimates are based on a matched sample of MNEs and domestic firms.

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 each individual the first event $e \in \{\}$separation, upgrade, downgrade from two years prior until six years after the (pseudo) investment. Since logit estimates ignore the chronological order of events, we shift the starting year of the sample period to increase its similarity with the design of our baseline specification. We therefore also estimate each event separately in the simple logit models. In the multinomial logit model, we go one step further and jointly estimate the likelihood of all events (against the baseline outcome *no event*). Table 4 summarizes the results.

Overall, the estimates from the separate logit models and the multinomial logit model support our main findings. Although the computed odds ratios are less precisely estimated than the results of the proportional hazards models, the estimates are similar in

value and of the same order of magnitude. As argued in Section 2, proportional hazards models are robust to the censoring of events, which is common in our data and which explains the lower statistical power of the logit models. 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 this 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 be due to measurement error and may 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. Thus, as a robustness exercise, we define *significant* up- and downgrades as job switches with changes in task complexity of at least ten percentage points. In Fig. 2, these switches are shown in the bins to the left of −10% and in the bins to the right of +10%. The estimates for significant up- and downgrades in Panel A of Fig. 6 are comparable to our baseline results (Fig. 4). For both types of significant job switches, the static measures are somewhat higher and more precisely estimated than in the baseline version. The same applies for the dynamic measure of significant upgrades. Significant downgrades, by contrast, are less precisely estimated in the dynamic specification and the timing of the effects also changes. It seems that most significant downgrades already appear within two years of the FDI event and decrease over time when more minor job switches occur (baseline).

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 according to the share of all nonroutine 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 nonroutine 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 used in the trade literature. As Panel B of Fig. 6 indicates, adding the manual dimension to our task measure does not affect the results beyond altering the precision of the dynamic effect of downgrades.

Finally, inspired by Liu and 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 (the 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 during the time frame of our analysis. We link the occupational panel to our main dataset and redefine upgrades (downgrades) as job switches within a firm to occupations with higher (lower) median wages. Panel C of Fig. 6 depicts the corresponding estimates. Both our task-based definition from the baseline model and the alternative wage-based definition of job switches generate similar results. Overall, our main finding that FDI leads to notably greater up- and downgrades within a firm holds independently of the exact definition of up- and downgrades used. See Table A.10 in the appendix for the corresponding coefficient estimates illustrated in Fig. 6.

5.4. Alternative control groups

The identification of the previous results relies on our matching strategy. The aim of the strategy is to increase internal validity, which comes at the cost of external validity. While we have thus far provided proof of robustness regarding internal validity, we now turn to robustness checks of the external validity of our results. To do so, we propose two alternative matching strategies that generate alternative control groups. In the first approach, we change the matching period to four years prior to FDI and keep the tenure restriction from the year of matching to the beginning of our observation window. Moving to earlier periods further decreases the likelihood of workers anticipating FDI into the Czech Republic. In the second approach, we match the MNEs to domestic firms that acquire a new establishment in Germany in the same year as the observed FDI (similar to Wang and Wang, 2015). This specification changes the interpretation of the estimates, as we now compare a firm's entry into a low-wage country with a new affiliate in a more similar labor market (within Germany).

We report the results in Table 5 and again list the estimated coefficients from the static models for separations, upgrades and downgrades in columns 1, 3 and 5, respectively, as well as the results from the dynamic models in columns 2, 4 and 6, respectively. Panel A shows the results after altering the year of matching. In the static specification, the estimates for the three types of job changes remain stable. The estimates for up- and downgrades even increase in magnitude and statistical significance. These changes are due to some striking differences in the timing of the effects. Relative to the alternative set of control firms, the treatment firms already exhibit relative increases in the likelihood of upgrades and downgrades in the two years prior to FDI. The likelihood of separations, by contrast, decreases around the year of FDI. This combination implies that the treatment firms rely on firm-internal labor to adjust their task composition in order to organize their foreign expansion before FDI occurs. The lion's share of the effect, however, still occurs 4 to 6 years after FDI. We now also observe dynamic effects in the separation of workers, which goes from a lock-in effect among workers around the year of FDI to higher separation rates in the subsequent years. These estimates suggest that the MNEs' adjustments to production require some years until they merge with the reallocation of task sets (i.e., occupations).

Panel B reports on the comparison of the treatment firms with domestic firms that acquire a domestic plant in the same year as the FDI. While the estimates for separations change little in terms of higher job security in MNEs, we observe drastic differences in the static models for upgrades and downgrades. Compared to the new control group, the treatment group exhibits almost no increased

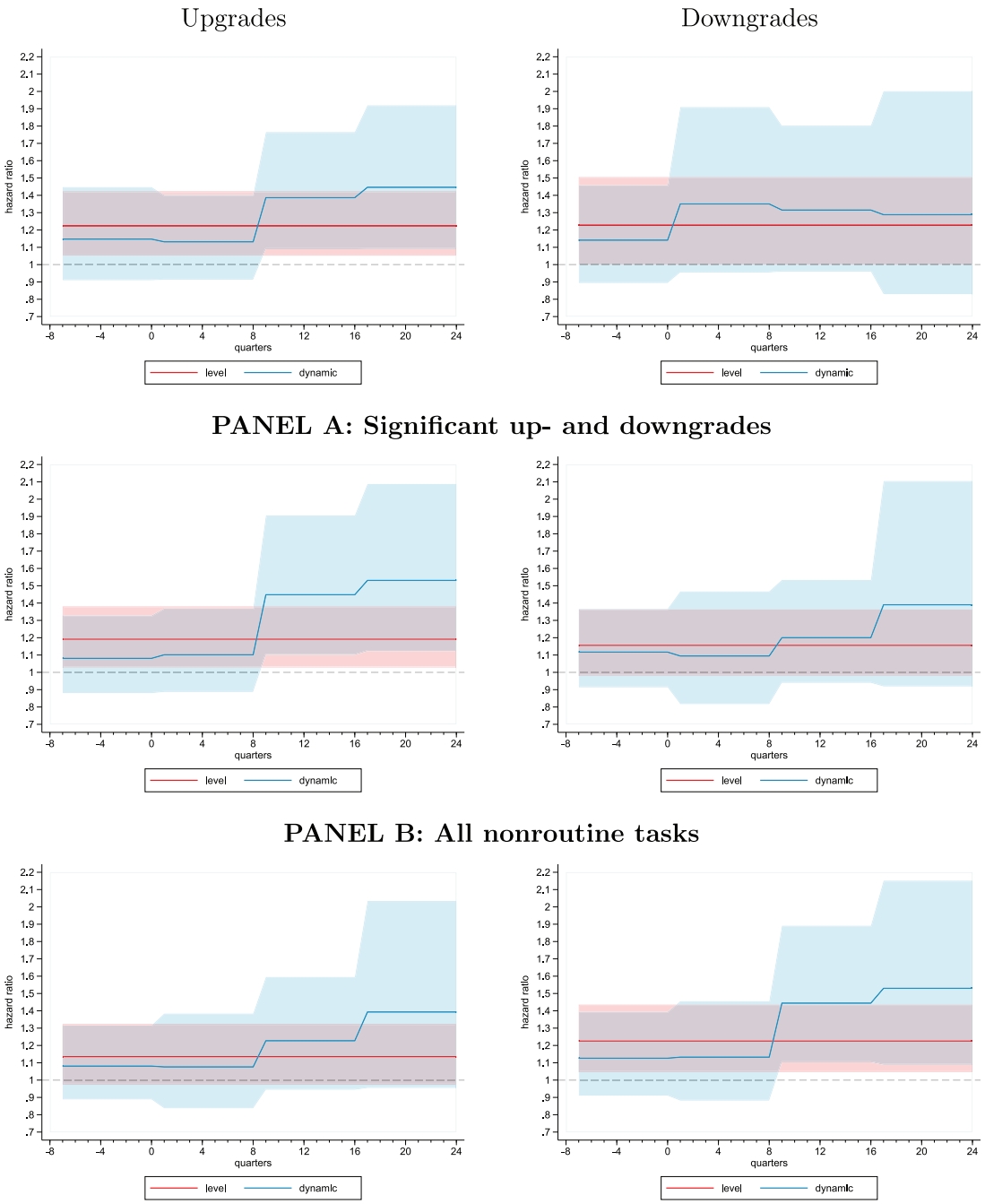

Fig. 6. Effects of FDI on the hazard ratios for up- and downgrades (Alternative definitions)
Notes: The figures provide a graphical representation of the hazard ratios and 95% confidence intervals representing the estimated effects of FDI on up- and downgrades, defined with alternative definitions. The results are obtained from the Cox regressions presented in Table A.10. The red lines display the level effects of FDI, i.e., the average effects from 2 years before to 6 years after investment. The blue lines show the development of the estimated hazard ratios over time. 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, nonroutine manual and interactive tasks. Panel C specifies job switches as upgrades (downgrades) if the occupational median wage increases (decreases) with the job switch. The control variables in all models are age, age squared, experience, tenure, a foreign dummy, skill dummies, firm age and a dummy indicating whether the firm existed in 1975 (all interacted with quarters since treatment), as well as occupation, year, industry, and state dummies. Estimates are based on the matched sample of MNEs and domestic firms. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)
Source: ReLOC, IEB and BHP, own calculations.

Table 5
Effects of FDI on the hazard ratios for separations, occupation switches, and up- and downgrades using alternative control groups.
Source: ReLOC, IEB and BHP, own calculations.

	Separation		Upgrade		Downgrade	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Matched 4 years prior to FDI						
MNE	0.9821		1.2559***		1.2385**	
	(0.0458)		(0.0857)		(0.1095)	
× up to 2 years prior to FDI		0.9533		1.2010*		1.2799***
		(0.0712)		(0.1195)		(0.1217)
× 0 to 2 years after FDI		0.8787**		1.1937		1.1128
		(0.0563)		(0.1382)		(0.2149)
× 2 to 4 years after FDI		1.0775		1.3083**		1.0315
		(0.0807)		(0.1629)		(0.2038)
× 4 to 6 years after FDI		1.1589		1.5430**		1.9808***
		(0.1259)		(0.2725)		(0.5133)
Subjects	345,869	345,869	345,869	345,869	345,869	345,869
Events	137,530	137,530	14,237	14,237	9,934	9,934
Panel B: Domestic firms acquiring plants in year of FDI						
MNE	0.9601		1.0194		0.9380	
	(0.0463)		(0.0772)		(0.0907)	
× up to 2 years prior to FDI		0.9144		0.9466		0.8976
		(0.0733)		(0.0923)		(0.1026)
× 0 to 2 years after FDI		0.9691		0.9114		0.7650
		(0.0681)		(0.1016)		(0.1346)
× 2 to 4 years after FDI		1.0568		1.2267		1.1314
		(0.0799)		(0.1768)		(0.2112)
× 4 to 6 years after FDI		0.9727		1.2472		1.4454*
		(0.0966)		(0.2569)		(0.2988)
Subjects	303,765	303,765	303,765	303,765	303,765	303,765
Events	131,695	131,695	11,925	11,925	8,275	8,271

Notes: The table presents exponentiated coefficients (hazard ratios) and cluster robust standard errors at the match 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, industry 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.

likelihood of upgrades, whereas the likelihood of downgrades becomes even lower in the MNEs. From the dynamic model, it is clear that timing plays a substantial role. Specifically, it seems that MNEs adjust their labor force with a time lag and do so less through the channel of separations. We assume that this could stem either from political pressure (and the public exposure associated with FDI) or from the higher fixed costs of the investment, which also include the risk of entering a new market.

5.5. Additional robustness checks

In an additional robustness check, we test whether our main findings are robust to the inclusion of industry-year dummies. We thereby account for yet more unobserved heterogeneity, but this also comes with very high computational costs. With slight decreases in the size and precision of the coefficients, Table A.11 confirms the robustness of the main results. The reestimation of the logit models (Table A.12), by contrast, returns somewhat higher coefficients and precision. Both tables broadly confirm the robustness of our main results.

Moreover, we want to test whether our results are driven by small firms, in which the FDI decision could depend on the individual worker. Panel B of Table A.13 in the appendix reestimates the baseline specification over a subsample that excludes small firms with fewer than 50 employees. The results point in the same direction, and deviations from the results of 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 reason, 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 pseudoinvestment dates of the domestic firms. In particular, we randomly draw pseudoinvestment 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 the MNEs. As Table A.13 in the appendix shows, this robustness exercise does not affect the results for separations. In the static model, the effects on job switches are slightly lower for upgrades and slightly larger for downgrades. However, overall, the dynamic effects on up- and downgrades are quite similar to those in the baseline results, as the effects become substantially greater beginning two years after the FDI and onward.

To identify the causal effects of FDI on job stability, the baseline specification restricts the sample to workers who were already employed at the time of matching. This restriction ensures that individuals do not self-select into future MNEs. However, it also removes approximately 15% of the workers from our sample, to whom our findings might not be applicable. To test the

generalizability of our findings to workers with less than one year of tenure at the onset of risk, we discard this restriction and re-estimate our models. The resulting estimates are almost identical to our main findings (see Table A.13 in the appendix). Although the unrestricted estimates should not be interpreted causally, they suggest that our findings also apply to more recently hired workers.

6. Conclusion

The objective of this paper is to bring together a seeming paradox between adjustment needs in response to the international activities of multinationals in lower wage countries and previous results that have revealed rather small movements in such firms' onshore employment numbers. We argue that firms use internal reorganizations of their workforce as an important adjustment channel to adapt to the changes in labor demand generated over the course of FDI. In particular, we analyze how FDI affects the job stability of workers and consider the occupational up- and downgrades of workers into 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 match their changed onshore 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 a firm, we use employer–employee data on German firms that invest in the Czech Republic and on comparable domestic firms. Although it would be desirable to have a larger dataset in terms of FDI partner countries in order to improve the external validity of the results, our data are very rich and detailed regarding the employment biographies of the workers. The data therefore also fulfill the high demands for internal validity.

Our identification strategy is based on a matched sample of treated and control firms with very similar characteristics and pretrends. Using Cox proportional hazards models, the analysis provides unique evidence that firms restructure their labor forces internally over the course of FDI. Two years after FDI, the likelihood of upgrades to more complex occupations or downgrades to less complex occupations increases for most incumbent workers in MNEs. By contrast, we do not find evidence of changes in the separation rate after FDI.

The greater opportunities to climb the career ladder through occupational upgrades in MNEs are in line with the theoretical expectation that MNEs require more administration and management tasks and are in line with our hypothesis that these firms attempt to fill these vacant complex positions internally. Moreover, the increased risk of occupational downgrades due to FDI is in line with the expectation that MNEs avoid the costs of dismissals by downgrading workers whose tasks become redundant over the course of FDI. Although a downgrade is not a positive occupational change per se, it might create a more minor career disruption than a dismissal. Besides opening up career opportunities for some workers, FDI also exerts pressure on others to adapt and keep up. Since we do not find increased separation rates, we argue that fears about globalization are also fueled by indirect substitution and job downgrades. Especially at the lower end of the job complexity distribution, workers are faced with more pressure to move along the career ladder in the desired manner (higher likelihoods for up- and downgrades).

In summary, our results imply that MNEs use internal restructuring rather than dismissals as an important adjustment channel to meet their labor demand, which changes over the course of FDI. Our findings can alleviate fears that foreign labor directly substitutes for domestic workers in MNEs. However, workers in firms that gain access to a low-wage labor market need to be more flexible and willing to take on new tasks. What these tasks are specifically and how they differ by the wage level of the destination country, or by the type of FDI, would be interesting avenues for further research. The results of this paper show that further training is indispensable for successful occupational transitions and highlight the importance of lifelong learning.

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Appendix A. Supplementary material

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.euroecorev.2022.104332.

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