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From Local to Global: How Foreign Acquisitions Reshape Job Mobility

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Abstract

This paper explores the impact of experience in foreign-owned firms on worker mobility, with a focus on Swedish companies acquired by foreign multinationals. We posit that international experience, by imparting knowledge about foreign operations, enhances an employee's appeal to multinational enterprises (MNEs). By matching acquired firms with comparable control firms and using a stacked difference-in-differences estimation methodology, we observe a significant impact of foreign acquisitions on job mobility. Our results indicate that foreign acquisitions raise the likelihood of a job switch to an MNE by 3.6 percentage points, while concurrently decreasing the probability of a switch to a local firm by around 4 percentage points. Furthermore, the post-acquisition wage growth rate is significantly higher for workers transitioning to MNEs compared to those remaining at the acquired firms, suggesting a steeper wage growth trajectory for employees moving to MNEs. Additional analyses reveal that the positive effect on mobility to MNEs is linked to learning opportunities stemming from increased trade linkages within multinational production networks and the implementation of advanced technologies after an acquisition.

Keywords: Multinational firms; Foreign acquisitions; Job mobility; International experience

JEL Codes: F16; F66; J60

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1 Introduction

The labor market is a dynamic and vibrant landscape, teeming with movement as workers navigate between firms. Job mobility serves as a critical catalyst for wage growth and higher lifetime earnings (Topel and Ward, 1992; Rubinstein and Weiss, 2006; Altonji, Smith, and Vidangos, 2013). Increased wages often accompany job transitions when workers move from low-productivity to high-productivity firms or from low-paying to high-paying firms (Haltiwanger, Hyatt, and McEntarfer, 2018; Abowd, McKinney, and Zhao, 2018; Moscarini and Postel-Vinay, 2018). Thus, for many workers, job mobility plays an essential role in the process of building a rewarding career.

In this paper, we study job mobility following the event of a domestic firm acquired by a foreign multinational enterprise. Multinational enterprises (MNEs) play a dominating role in global production and trade. To carry out international business, MNEs are heavily dependent on employees who are skilled in areas such as advanced technology, international logistics, supply chain management, foreign laws and regulations, and international marketing and finance. Consequently, employees in MNEs enjoy enhanced opportunities to acquire and refine skills in these domains. Thus, we expect workers at the acquired firm to gain international skills after the acquisition. We explore how the newly gained international experience shapes the mobility path of workers and examine the wage implications of such mobility. We are especially interested in whether foreign acquisitions lead to more upward mobility, i.e., moving toward more rewarding positions in other MNEs that place a high value on those skills.

Our paper departs from the vast literature that focuses on the effect of foreign acquisitions on employment and wages in acquired firms, e.g. McGuckin and Nguyen (2001), Heyman, Sjöholm, and Gustavsson Tingvall (2007, 2011), Huttunen (2007), Lehto and Böckerman (2008), Bandick and Görg (2010), Ouimet and Zarutskie (2020), among others. We contribute to this literature by being, to our knowledge, the first that looks at the impact of foreign acquisitions on the career paths of workers. Our focus on job mobility following foreign acquisitions has important implications for foreign investment policies. If foreign acquisitions lead to greater upward mobility and higher wage growth, then policies that facilitate acquisitions can potentially bring broad benefits to the labor market. Another broad benefit from foreign investment is the spillover of technology from MNEs to other firms. Worker mobility, in particular, has been recognized as an important channel for

facilitating these technology spillovers (e.g. Görg and Strobl, 2005; Balsvik, 2011; Poole, 2013). However, because worker mobility is treated as exogenous in those studies, it remains unclear whether technology spillovers stem from the knowledge and skills that workers have gained from working at MNEs, or simply because workers hired by MNEs are already more skilled. Our study can shed light on whether there is a causal effect of the multinational experience on worker mobility.

Unraveling the causal effect of multinational employment on mobility is challenging. A primary complication arises from self-selection: more qualified workers tend to gravitate towards better firms, which often are MNEs. Thus, comparing workers already employed in MNEs with those at local firms could bias our results, overstating the influence of multinational experience on mobility. To deal with this endogeneity problem, we use foreign acquisitions as an internationalization shock and compare the mobility of workers at local firms that are eventually acquired by foreign investors and the mobility of workers at local firms that are never acquired. Our empirical investigation of job mobility hinges upon matched worker-firm data from Sweden, spanning from 1996 to 2015. Central to identifying the causal effect on mobility of working for a multinational firm are foreign acquisitions of Swedish local firms that took place between 1998 and 2013. These events offer a window into the dynamics of labor movement, allowing us to trace the trajectories of both firms and workers before and after an acquisition.

However, there is another layer of complexity: acquisitions are not random events. The choice of a firm for acquisition depends on various characteristics of the target firm. To control for the non-random feature of acquisitions and to deal with the selection problem, we employ propensity score matching. This approach enables us to form a control group of Swedish local firms (“control firms”) that closely mirrored the characteristics of the acquired firms (“treated firms”) one year before the acquisition. We then follow the mobility of both workers who were employed in treated firms in the year when an acquisition occurred (“treated workers”) and workers in control firms (“control workers”) over the same period. Because foreign acquisitions take place in different years, we utilize a stacked difference-in-differences method to estimate the effect of foreign acquisitions on job mobility. Specifically, we compare the mobility difference between treated and control workers before and after acquisitions, controlling for worker fixed effects, time-varying worker and firm characteristics, and year-fixed effects. These controls account for the impact of worker and firm heterogeneity, and business cycles on mobility.

Our empirical analysis uncovers a rich set of new findings about worker mobility between firms. Our baseline estimates indicate that, compared to control workers, treated workers are more likely to transition to other MNEs after acquisitions and less likely to move to local firms (i.e., Swedish owned firms with no foreign affiliates). We observe a 3.6 percentage point increase in mobility to other MNEs following an acquisition, implying that out of 100 treated workers, 3-4 more workers would move to other MNEs as a result of treatment. Importantly, we discern a temporal pattern in this mobility effect: the surge in mobility does not materialize until the third year after treatment and continues to rise until the sixth year when it stabilizes at 7 percentage points. This pattern suggests that employees at acquired firms need time to develop the skills that improve their mobility within the MNE landscape. In contrast, our estimates reveal a negative impact of foreign acquisitions on mobility to local firms, with a decrease of around 4 percentage points. These findings hold consistently across various alternative specifications and robustness checks. Furthermore, our analysis does not reveal a significant impact of foreign acquisitions on mobility for high-wage firms, suggesting that international experience does not universally enhance mobility to high-wage firms. Rather, the effect appears to be confined to MNEs, which place a higher value on specific skills required by international business practices.

If workers gain new skills following acquisitions and the newly gained skills are valuable to other firms, their movement to new employers should be associated with higher wage growth. To examine this, we estimate the wage growth of treated workers by comparing movers and stayers before and after an acquisition, conditional on worker fixed effects, time-varying firm and worker characteristics, and year fixed effects. Our findings indicate that pre-acquisition wage growth rates are similar for movers and stayers. However, a significant divergence appears post-acquisition: movers experience markedly higher wage growth than stayers. For example, compared with stayers, workers who moved to another MNE have a higher cumulative wage growth rate by about 10 percentage points over a 10-year period, where the base year is one year before acquisitions. This wage growth gap translates into a 1 percentage point difference in the annualized wage growth rate. Further, we find a more modest post-acquisition wage growth premium for movers to local firms: compared with stayers, movers to local firms have a higher cumulative wage growth rate by around 7 percentage points in the first year after acquisitions, but this wage growth gap shrinks over time and becomes insignificant after the third year of acquisitions. Extra wage gains enjoyed by movers

to MNEs provide further evidence that the skills gained at foreign-acquired firms are more valuable to multinational firms. The newly acquired skills not only lead to higher mobility to MNEs, but also contribute to higher wage growth after moving to other firms.

Next, we examine mobility patterns in more detail to uncover heterogeneity in the effects of foreign acquisitions across workers in different occupations and across firms acquired by different source countries. We find that compared with control workers, treated workers in both high-skilled occupations (managers and professionals) and low-skilled occupations (e.g., operators) experience an increase in mobility to MNEs across all years following acquisitions, and the effect is slightly stronger for high-skilled occupations. Furthermore, we find that for most source countries, higher mobility to MNEs largely reflects higher mobility to foreign MNEs, and the treatment effect is particularly pronounced for movement to MNEs from the same source country as the acquiring firm, the majority of which is due to transitions to other MNEs outside the same business group.

To investigate further, we study several channels for workers to obtain new skills after foreign acquisitions. Foreign acquisitions can help integrate the acquired firms into multinational production networks and spur changes in trade, technology, and management practices. As acquired firms undergo these post-acquisition changes, workers at acquired firms will have opportunities to gain new skills, and these skills may be highly demanded by other multinationals and the most useful within specific multinational networks. This helps explain the higher mobility to other MNEs, especially those from the same source country.

Our analyses reveal that compared with control firms, acquired firms see a significant increase in the total amount of imports (but not exports) and the share of trade with the source country of foreign investors after acquisitions, which provides direct evidence for the integration of acquired firms into multinational production networks of foreign acquirers. In addition, at the firms that see tighter trade linkages with the source country of foreign acquirers, workers tend to have higher mobility to other multinationals originating from the same source country. This pattern is especially strong for Germany, Nordic countries, and the EU-15 country group. These results suggest that workers at firms acquired by Germany, Nordic countries, or the EU countries as a whole may gain more market-specific international skills after acquisitions, thus increasing the chance of moving to other multinationals with owners from the same source country (market) as that of the foreign acquirers.

We also find that workers at firms acquired by higher-tech countries (relative to Sweden) have a significantly higher mobility to MNEs and especially to the MNEs from higher-tech countries. This pattern suggests that workers may accumulate more skills at firms acquired by technologically more advanced countries and thus have a better chance of moving to other MNEs that use similarly advanced technologies. Further, using firm-level data on post-acquisition IT expenditures, we find that the positive effect of foreign acquisitions on mobility to MNEs stems from the higher mobility of workers at acquired firms that had higher IT expenditures following acquisitions.

Overall, we find strong evidence for the impact of acquisitions on job mobility through trade and technology. On the other hand, we find that foreign acquisitions have significantly positive effects on mobility to MNEs, no matter whether the source countries of foreign acquirers have high- or low-quality management practices.

To summarize, our analysis suggests that foreign acquisitions provide unique opportunities for treated workers to gain international experience, making it easier for them, especially those in highly skilled occupations, to move to other MNEs and push them onto a steeper wage growth trajectory. The overall effect underscores the role foreign acquisitions play in shaping career trajectories and wage growth dynamics, particularly within the MNE context.

1.1 Related literature

Our paper contributes to several strands of literature on globalization and labor markets. First, the literature on job mobility and job ladders reveals systematic patterns of worker movements between firms of different productivity levels and wage premiums and provides evidence for wage gains as workers move up the job ladder. Topel and Ward (1992) find that job change is a critical component of workers' movement toward a stable employment relationship of mature careers. Wage gains at job changes account for at least a third of early-career wage growth. Abowd et al. (2018) show that working for high-paying firms leads to higher earnings and increases the probability of upward mobility. Haltiwanger, Hyatt, Kahn, and McEntarfer (2018) provide strong evidence of a firm wage ladder that is highly procyclical. Haltiwanger et al. (2018) find that even though more educated workers are more likely to work with more productive firms, job-to-job moves disproportionately reallocate less educated workers up the job ladder (from low-productivity to high-productivity). Our paper contributes to this strand of literature by looking at job ladders based on the global

engagement of firms, specifically, worker movements from local firms to MNEs. Our study highlights the role of international experience in facilitating worker transition to more rewarding jobs within MNEs. On the other hand, we find no evidence for higher mobility from treated firms to high-wage firms, suggesting that the skills earned at foreign acquired firms are more valuable to other MNEs rather than to high-wage firms in general. Furthermore, we find that compared with workers who remained at acquired firms, movers enjoy extra wage gains post-acquisition, and the gains are larger for movers to MNEs and smaller for movers to local firms.

Second, the literature on human capital, job search, and wage dynamics shows that human capital accumulation and job search are two of the main driving forces of observed earnings/experience profile (e.g. Rubinstein and Weiss, 2006; Altonji, Smith, and Vidangos, 2013; Bagger, Fontaine, Postel-Vinay, and Robin, 2014). In this literature, work experience and tenure are often used to proxy the accumulation of specific human capital. One approach to testing the specific human capital model uses plant closure as an exogenous shock and examines (un)employment and wages dynamics of workers following plant closure (e.g. Ruhm, 1991; Jacobson, LaLonde, and Sullivan, 1993; Kletzer, 1998; Couch and Placzek, 2010). Our approach of studying the career paths of workers following foreign acquisitions is a novelty of the paper. We find that human capital in the form of international skills that are accumulated at one’s current employer can have significant effects on future job mobility and subsequent wage growth. The revealed pattern of mobility also suggests that to some degree international skills are specific to certain markets, and thus most transferable to the firms that operate in similar markets. Thus, our paper contributes to this literature by uncovering the interplay of human capital and job mobility in shaping wage growth trajectories.

Third, our paper is related to Mion and Opromolla (2014), Mion, Opromolla, and Ottaviano (2022), and Mion, Opromolla, and Sforza (2024). Mion et al. (2022) find that the experience-wage profile is much steeper in internationally active firms (importers, exporters, or multinationals) than in other firms, especially for managers as opposed to blue-collar workers. The higher lifetime wage income for managers stems from the stronger accumulation of experience gained from internationally active firms and from wage increases when switching to other firms. Mion and Opromolla (2014) and Mion et al. (2024) show that export experience acquired by managers in previous firms leads to better export performance in their current firm and a higher wage premium for the manager. They also find that export knowledge is market specific. Our paper confirms these findings

in that international experience is market specific and human capital is portable to other firms that operate in the same market. However, our work differs in that we are mainly interested in the role of international experience in shaping the career paths of workers. Our work exploits foreign acquisitions as an internationalization shock and addresses the causality of mobility using ownership changes. We also show that increased mobility after acquisitions plays an important role in steepening the wage growth trajectory for workers who have gained international experience at the acquired firms and later moved to other MNEs.

Lastly, our paper contributes to the literature on foreign acquisitions and labor market outcomes, where most papers study how employment and wages in the acquired firms are affected. For example, Heyman et al. (2007, 2011) find that foreign takeovers of Swedish firms have a small effect on wages at the target firms. Using plant-level data for the entire US manufacturing sector for 1977-87, McGuckin and Nguyen (2001) find that ownership changes typically increase the number of jobs and wages and improve the probability of plant survival. Lehto and Böckerman (2008) find negative employment effects of foreign acquisitions for Finnish manufacturing plants, but not in services. Using data on Swedish manufacturing plants, Bandick and Görg (2010) find positive employment growth effects for exporters in vertical takeovers. Ouimet and Zarutskie (2020) present evidence that high-value (“skilled”) employees are relatively more likely to be retained following acquisitions with an objective of acquiring and retaining the target firm’s employees. Unlike these papers, we are interested in the impact of foreign acquisitions on mobility and the subsequent impact on wage dynamics of workers who moved to other firms. Focusing on movers rather than stayers at the acquired firms sheds new light on the effect of acquisitions on labor market outcomes.

The remainder of the paper is organized as follows. Section 2 describes our data and empirical strategy. Section 3 presents empirical results on the effect of acquisitions on worker mobility. Section 4 studies the implications for wage growth. Section 5 reveals the heterogeneous effect of acquisitions on mobility. Section 6 explores the mechanisms of mobility, and Section 7 concludes.

2 Data and empirical strategy

As previously mentioned, our empirical analysis examines how experience from working at a foreign-owned firm affects job mobility. Our main concern is a selection effect: workers employed by

a foreign multinational firm could differ from workers employed by Swedish local firms. A simple comparison of mobility differences between workers in foreign multinationals and Swedish local firms would lead to biased estimates. We address this possible selection bias by looking at workers who are initially employed in Swedish local firms, but some of them experience an internationalization shock when their firm gets acquired by a foreign owner. That is, we study the impact on job mobility of ownership changes due to foreign acquisitions. Our sample of workers is created in two steps. Firstly, we match acquired firms with similar non-acquired firms. Secondly, we compare all workers employed in the acquired firm at the time of an acquisition (treated workers) with all workers employed at the same time in the control firms (control workers). We use our sample of treated and control workers to examine the effect of foreign acquisitions by applying a stacked difference-in-differences model. A more detailed description of our approach follows below.

2.1 Data

Our analysis relies on several comprehensive register-based datasets from Statistics Sweden (SCB), covering the universe of Swedish firms and individuals. The data sets are linkable through unique firm and individual identifiers, which allow us to follow both firms and workers over time and observe changes in ownership, employment, and job mobility. The analysis covers the period from 1996 to 2015 and is based on all firms with at least 10 employees.

Firm-level information is drawn from several administrative registers that together cover the entire Swedish private sector. The primary source is the financial statistics database, which includes detailed firm-level information on all Swedish firms in the private sector. In addition, we use the Regional Labor Market Statistics (RAMS), which provides plant-level information on the composition of the workforce with respect to educational level and demographics. Plant-level data are aggregated at the firm level.

A key feature of the data is the detailed information on firm ownership. A firm is classified as foreign-owned if more than 50 percent of its equity is owned by foreign shareholders. We define a foreign acquisition as a change from Swedish to foreign majority ownership. To identify the nationality of foreign owners, we merge the SCB registers with data from the Swedish Agency for Economic and Regional Growth (Tillväxtanalys). These data provide information, consistent with OECD and Eurostat definitions, on the source country of the main foreign owner. This enables us

to distinguish foreign multinationals operating in Sweden by their country of origin and to examine heterogeneity across acquirers.

Worker characteristics come from the Longitudinal Integration Database for Health Insurance and Labor Market Studies (LISA), which covers all individuals aged 16 or older who reside in Sweden. LISA integrates information from multiple administrative registers, including tax files and population registers, and provides annual data on education, labor market status, earnings, gender, age, and demographic variables. Because the register is population-wide, individuals exit the data only through emigration or death. For non-employed individuals, we observe whether they are enrolled in education, unemployed, on sick leave, taking early retirement, or on parental leave.

We use acquisitions between 1998 and 2013 to allow us to follow firms and workers at least two years before and after the acquisitions took place. This leaves us with 2,681 acquisitions between 1998 and 2013. Figure 1 shows the distribution of acquisitions over time. The yearly number of foreign acquisitions of local firms ranges from 91 in 2013 to 362 in 2001. The number of acquisitions is relatively stable over time with the exception of the dot-com boom in 2000 and 2001. Appendix Table A1 shows the top ten source countries that have the largest number of acquisitions of local firms and the largest number of employees in the acquired firms. Norway is the largest source country based on the number of acquisitions, whereas the U.S. is the largest source country in terms of employment.

We restrict our sample to firms that only experience one foreign acquisition and exclude all firms that go from, for instance, Swedish to foreign ownership and then back to Swedish. Moreover, we include all workers aged 22 to 62 years at the time of acquisitions in our regressions, which means that we can follow them a few years before and after this change of ownership. In the following analysis, we compare workers in firms acquired by foreign owners with workers in firms remaining Swedish-owned.

2.2 Empirical model

We use our groups of treated and control firms to deal with selection issues. Treated firms are local firms, i.e., Swedish-owned firms without affiliates abroad, that were acquired by foreign multinationals. Control firms are local firms with similar characteristics to their matched treated firms before acquisitions. More details of how we construct the group of control firms are given in the

next section. Because foreign acquisitions took place in different years, we use a stacked difference-in-differences (DiD) model where the mobility of workers in the years before and after an acquisition is compared to the mobility of workers in our control firms over the same period.

Specifically, we estimate the following equation for outcome Y of worker i at event time j and in calendar year t :

$$Y_{ijt} = b_0 AFTER_j + b_1 TREATED_i + b_2 DiD_{ij} + \gamma_i + X_{it}\delta + X_{f(i),t}\theta + \omega_t + \varepsilon_{ijt}, \quad (1)$$

where the event time j tracks the lags and leads relative to treatment, with $j = 0$ indicating the year of an acquisition. We measure mobility as a binary variable, Y_{ijt} , which takes the value one if the worker moves to another firm and zero otherwise. To examine the pattern of mobility, we run separate regressions where mobility is to MNEs or to local firms. $AFTER_j$ takes the value one in the year of an acquisition and all years after, which accounts for conditions that can change over time for every worker, whether treated or not. $TREATED_i$ takes the value one for workers who are employed by firms that are acquired at some point, which accounts for the fixed differences between workers at the acquired firms and those at the control firms. DiD_{ij} (i.e., $AFTER_j \times TREATED_i$) is our variable of main interest and takes the value one for treated workers in the year of an acquisition and all subsequent years. That is, it indicates workers at treated firms for the period when the acquisition would matter for mobility. Its coefficient b_2 is the estimate of the causal effect of acquisitions on the mobility of treated workers, which contrasts the post-minus-pre-acquisition change of mobility between treated and control workers. γ_i represents worker fixed effects to capture unobserved worker ability and other characteristics that may affect mobility. Note that worker fixed effects subsume both $TREATED_i$ and industry fixed effects. X_{it} are time-varying worker characteristics, including experience squared and years of schooling. $X_{f(i),t}$ are time-varying firm characteristics, including log employment, the share of skilled workers, and capital intensity. ω_t are year fixed effects to control for business cycles and other macroeconomic factors that may affect mobility for all workers. All other factors that may affect worker mobility are captured in the error term ε_{ijt} . Because interclass correlations between workers can result in too small standard errors (Moulton, 1986), we cluster our standard errors at the firm and year level in all estimations.

We also estimate an event study version of (1) to examine the dynamic effects of foreign acqui-

sitions on mobility:

$$Y_{ijt} = \sum_{j=-4}^9 \beta_j^0 T_j + b_1 TREATED_i + \sum_{j=-4}^9 \beta_j (T_j \times TREATED_i) + \gamma_i + X_{it} \delta + X_{f(i),t} \theta + \omega_t + \varepsilon_{ijt}, \quad (2)$$

where T_j are event time fixed effects that capture the over-time change of mobility by all workers, other explanatory variables are defined as above, and ε_{ijt} is the error term. Our main interest is in the coefficient β_j for the interaction between T_j and $TREATED_i$, which indicates the effect of acquisitions on the mobility of treated workers at event time j . In our estimations, the year before acquisitions ($j = -1$) is omitted as a reference year, and observations for the year of acquisitions are dropped because all variables are fixed (standardized) at the time of acquisitions and we cannot define movers or stayers at that time. We examine the effect of acquisitions on mobility by comparing β_j up to nine years after acquisitions with the years before acquisitions. Like in equation (1), $TREATED_i$ will be dropped from the regressions when worker fixed effects are included.

The DiD estimations examine the causal effect of foreign acquisitions on job mobility under two assumptions: the parallel trends assumption and the stable unit value treatment assumption. The parallel trends assumption requires that workers in treated and control firms have parallel trends in job mobility in the absence of foreign acquisitions. Past shocks (before acquisitions) should have affected treated and control workers in the same way. Failing to account for shocks that affect the two groups of workers differently can bias our estimates. However, counterfactual outcomes, what would have happened with job mobility in the absence of an acquisition, are not observable. Thus, we follow the usual practice and examine the trends in job mobility before acquisitions. Our DiD estimations are unbiased if the parallel trends assumption is not violated. Note that the parallel trends assumption is considerably weaker than assuming random treatment, i.e., that foreign acquisitions of Swedish firms are random.

We want to select control firms as similar as possible to the treated firms, which makes it tempting to choose firms from the same industry and/or region. However, the stable unit value treatment assumption (SUTVA) requires that workers in control firms are not affected by acquisitions of treated firms. Such an effect could be present if, for instance, the control and treated firms are close competitors in small markets with few firms. Then, a foreign acquisition might improve the performance of the treated firms and force control firms to make changes, including changes

to the workforce. To satisfy SUTVA, our set of control firms is selected from the population of Swedish firms and not restricted to firms within the same region or industry as the treated ones, which implies that it is highly unlikely that acquisitions of treated firms affect workers in control firms (Olsson and Tåg, 2025). As will be detailed below, although our baseline approach does not restrict treated and control firms to the same industry, we account for the impact of industry-specific shocks by including industry-year fixed effects when estimating the propensity score for treatment. We also carry out robustness checks by matching firms within the same industry-year.

We note that in our setting, treatment (an acquisition) happens in different years. It is plausible that treatments are heterogeneous over time; for instance, the effect of acquisitions on mobility might depend on business cycles. To address staggered treatments and treatment effect heterogeneity, we use a stacked difference-in-differences model combined with matching, as recommended in the recent literature (Baker, Larcker, and Wang, 2022; Roth, Saint’ Anna, Bilinski, and Poe, 2023). This approach has the advantage of avoiding biases that can appear in two-way fixed effects (TWFE) DiD models. Specifically, TWFE models in a staggered DiD context average treatment effects over different time periods and groups. However, if the treatment effect varies over time or across units (which is often the case in staggered treatments), this averaging can lead to biased estimates.¹

Thus, we follow the recent literature and take the following steps. Firstly, we adopt a stacked regression design similar to Cengiz, Dube, Lindner, and Zipperer (2019). We start by normalising the time of acquisitions to $j = 0$, which is equivalent to a setting where all acquisitions happen at the same time. We then follow the workers present at $j = 0$ in treatment and control firms. Secondly, to avoid biased estimates, we exclude firms from the control group if they are later acquired by foreign owners. Hence, our control firms are never treated at any point. Thirdly, in all regressions, we restrict the sample to individuals whose treatment or control status is clearly defined at the time of the acquisition. We remove individuals who are associated with more than one acquisition event and exclude workers who switch between treated and control firms. These restrictions guarantee a unique and well-defined exposure status for each worker and avoid contamination of treatment

¹For example, Goodman-Bacon (2021) shows that the estimate of a TWFE model is a weighted average of all possible 2x2 DiD-estimates created by the staggered timing of treatment. If treatment effects vary over time, comparing later-treated with early-treated can produce biased estimates. The reason is that the weights of each of the 2x2 DiD-estimates depend on the sample size of each subsample, so units treated towards the middle of the event window will have more weight than units treated earlier or later.

timing, an important requirement in event-study and difference-in-differences designs.² Moreover, we do not restrict workers to be in the same control firm pre-acquisition.

Baker et al. (2022) and Roth et al. (2023) also show that estimates can be affected by including time-varying covariates. They, therefore, argue that results both with and without control variables should be provided to give a better understanding of what is driving the results, a recommendation that we follow in our analysis below.

2.3 Constructing the control group

As stated above, the process by which firms are targeted for acquisition is not random. To address this inherent selection bias, we employ a propensity score matching method to establish a control group composed of local firms, Swedish-owned firms without affiliates abroad, that parallel the acquired firms’ characteristics one year before acquisitions. Specifically, we construct the control group through a one-to-one matching process, done without replacement. Thus, each control firm uniquely corresponds to one treated firm, ensuring an equal number of firms in both the control and treated groups within the same year. The matching process is conducted using the nearest neighbor algorithm. Hence, control firms are selected so that their characteristics are similar to those of the treated firms one year prior to the acquisition event.³ To maintain the integrity of the control group, we exclude any firms that are acquired in subsequent years.

To estimate the propensity score for treatment, we use a logit model in which the dependent variable is the probability of firm f being acquired in year t :

$$Pr(acquisition_{ft} = 1|X, \phi) = F(\lambda_1 X_{f,t-1} + \lambda_2 X_{f,t-2} + \lambda_3 X_{f,\Delta t-1} + \phi_{st}) \quad (3)$$

where $F(\cdot)$ is the cumulative logistic distribution function, $X_{f,t-1}$ and $X_{f,t-2}$ are vectors of firm characteristics at $t - 1$ and $t - 2$ that could affect the probability of an acquisition. We include

²Recent contributions to the difference-in-differences literature show that violations of stable treatment timing or contamination of control groups can generate biased estimates. In particular, allowing individuals to be exposed to multiple treatment events or to switch between treated and control firms risks creating “forbidden” comparisons in which already-treated units serve as controls for other treated units (Callaway and Sant’Anna, 2021; De Chaisemartin and d’Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). Ensuring that each worker has a unique acquisition event and a stable treatment–control status therefore improves identification and the credibility of the resulting event-study estimates.

³For example, we start with all firms treated in 1998 and find control firms that match their 1997 characteristics. Then we look at all firms treated in 1999 and find control firms that match their 1998 characteristics, and so on. We also ensure that the firms that serve as control firms in, say, 1998, do not end up as control firms in, say, 1999.

eight different firm characteristics as explanatory variables: profits, sales, value added, the share of low-skilled workers (i.e., those with at most 9 years of compulsory schooling), the share of high-skilled workers (i.e., those with at least undergraduate college education), capital per employee, profit over sales, and log firm size (i.e., log employment). We also include the difference in the same variables between the year before and the year of acquisitions (vector $X_{f\Delta t-1} = X_{f,t} - X_{f,t-1}$), and industry-year fixed effects ϕ_{st} .⁴ It is worth mentioning that although our matching does not restrict treated and control firms to the same industry, the industry-year fixed effects included in the probability model ensure that the estimated propensity scores capture the impact of industry-specific shocks on the likelihood of being acquired. That is, treated firms are matched to control firms with similar acquisition probabilities conditional on industry affiliation and year. Appendix Figure A1 provides information on the number of firms over the pre- and post-acquisition periods.

The results of our matching are shown in Figure 2, which displays differences in key firm characteristics between treated and control firms, both for the matched and unmatched samples. Values close to zero indicate minimal differences, whereas higher values indicate larger differences. Because acquired (treated) firms tend to be larger, have higher sales, employ a more skilled workforce, and exhibit higher productivity than non-acquired firms, we observe relatively large differences in the unmatched sample. In contrast, the matching procedure reduces these differences between acquired and non-acquired firms significantly.

Further evidence of the quality of our matching is presented in Table 1, which reports summary statistics for pre-acquisition firm characteristics in the matched sample over the period 1998–2013. Again, the matching substantially reduces the initial differences between acquired and non-acquired firms, and no statistically significant differences remain, except for the one-year change in sales and the share of low-skilled workers. We have also compared the characteristics of workers at treated and control firms one year before the acquisitions. As shown in Appendix Table A2, we find very little differences in log wage, gender, age, education, and experience between workers at treated and control firms.⁵ Industry and occupation compositions are also similar between treated and

⁴Industries are defined using the two-digit level of the Swedish Standard Industrial Classification (SNI2002). We use a slightly aggregated version of these two-digit codes, where a few closely related industries are combined.

⁵The log wage, share of women, age, education level, and years of experience are 7.76, 0.36, 42.01, 3.63, and 23.43, respectively, in treated firms, and are 7.82, 0.35, 42.39, 3.92, and 23.38, respectively, in control firms. The schooling variable at the worker level ranges from 1 (if the highest level of education is elementary school) to 7 (if the highest level of education is a doctoral degree).

control workers. Compared to the universe of Swedish workers, workers in our sample are slightly older and more experienced. To sum up, our matching successfully removes differences in observable firm characteristics between acquired and non-acquired firms before acquisitions. Our matching also produces a sample of treated and control workers who have similar observable characteristics. Note that in the following worker-level regressions, we always control for worker fixed effects and time-varying worker characteristics to account for the impact of worker heterogeneity on job mobility and wage growth.

As robustness checks, we also match treated and control firms within the same industry-year. However, besides concerns about SUTVA violations, matching within industry-year cells shrinks the pool of potential control units per cell. We observe that many industry-year cells have few or no suitable control firms, resulting in no valid matches for some treated firms.

2.4 Descriptive statistics on job mobility and wage growth

We continue by presenting descriptive statistics about job mobility and wage growth for all the workers who are employed at the treated or control firms in the year of an acquisition (event time $j = 0$). Figure 3 displays the share of those workers who are employed in the same firm up to five years before the takeover and remaining in the same firm up to five years after the takeover. We see that the magnitude of mobility is notably similar between workers from treated and control firms, and the overall mobility of workers is high. For instance, around 75 percent of the workers in the year of a takeover worked in the same firm one year before ($j = -1$). Five years before the takeover, the figure was only around 25 percent. Similarly, around 77 percent of those workers remained in the same firm after one year and only 35 percent after five years.⁶

Although the overall magnitude of mobility is similar between workers from treated and control firms, the pattern of mobility in terms of destination firm types differs substantially between them. Table 2 looks at the cumulative share of workers who move to MNEs or local firms before and after an acquisition, where the denominator is the total number of workers who are employed at a treated or control firm in the year of the acquisition. Columns 1-2 show that 4.7 percent of workers

⁶High worker mobility observed in our data can be attributed to Swedish labor market policies that encourage workers to move for work and to find a better match between their skills and job demands, coupled with generous unemployment benefits and worker training programs. We also calculated the hiring and separation rates. Again, the figures are similar in treated and control firms.

in treated firms and 3.7 percent of workers in control firms have moved to an MNE one year after the acquisition ($j = 1$). The cumulative share in treated and control firms increases to 14.2 and 11.8 percent, respectively, up to five years after the acquisition ($j = 5$). For the pre-acquisition period, 4.2 percent of workers in treated firms and 3.9 percent of workers in control firms moved to an MNE one year before the acquisition ($j = -1$). The cumulative share in treated and control firms is 8.5 and 7.6 percent, respectively, up to five years before the acquisition ($j = -5$). A more informative way of looking at these numbers is to take the difference between treated and control firms. The results are shown in column 3. We find that this difference in mobility to MNEs between treated and control firms widens after the acquisition.

Looking at movers to local firms in columns 4 and 5, we see that the cumulative shares up to five years after the acquisition are 19.7 and 21.7 percent, respectively, for workers from treated and control firms who have moved between the first and the fifth years after the acquisition.⁷ Column 6 reports the cumulative share difference between treated and control firms. We observe that the difference is very small before acquisitions, but after acquisitions a smaller share of workers at acquired firms move to local firms, compared to those at control firms. Therefore, Table 2 reveals a clear pattern of transitions where a relatively high share of workers from treated firms move to MNEs after acquisitions. In the following analysis, we will confirm this difference in mobility between treated and control workers by estimating a stacked difference-in-differences model that accounts for common trends and a rich set of worker and firm characteristics.⁸

We also note that foreign acquisitions could bring a new owner who forces workers to move in order to achieve higher efficiency. This would be a very different cause of mobility than the learning-based one we have proposed. We find some comfort in Figure 3, which reveals a very similar magnitude of mobility between workers from treated and control firms, implying that in our sample foreign takeovers do not appear to induce more layoffs at treated firms. This aligns well with the observation previously made that foreign acquisitions tend to target better-performing Swedish

⁷More workers who change employers end up in local firms, which is unsurprising considering that this is the largest group of firms.

⁸Table 2 also suggests that the ratio between the number of workers moving to MNEs and the number of workers moving to local firms increases over time for control firms. This increasing ratio reflects broader labor market trends in Sweden during our sample period, with growing MNE presence that creates more opportunities for workers to transition to multinational firms. This secular trend affects both treated and control workers, but importantly, our DiD design differences out this common trend. Pre-acquisition mobility patterns at $j = -1$ when the propensity score matching is based on show no significant differences in these ratios between treated and control firms, confirming that the matching procedure does not seem to select control firms based on differential mobility patterns.

local firms (i.e., firms with higher sales, a more skilled labor force, and higher productivity), thus reducing the need for downsizing.⁹

Table 3 takes this issue one step further by examining wage growth separately for stayers, movers to MNEs, and movers to local firms, where all the workers are employed at a treated or control firm in the year of an acquisition and all wages are relative to the year prior to the acquisition. The figures for movers up to event time $j = 5$ include all workers who have moved between the first and fifth years after an acquisition. We find that stayers at treated firms enjoy a 16 percent wage increase over a five-year period, compared with a six percent increase in control firms over the same period. More importantly, the wage increase is highest for treated workers moving to MNEs, which suggests that workers leaving a firm after an acquisition are not pushed out but rather attracted by higher wages at other MNEs, presumably due to the experience gained from working in an acquired firm.

Furthermore, the patterns revealed in Table 3 suggest important worker selection in the timing of mobility. While movers to MNEs from treated firms show consistently higher wage growth that widens over time (reaching a 14 percentage point premium at year 9), the pattern for movers to local firms is more complex. Early movers to local firms (years 1 to 3) from treated firms experience higher wage growth than their control counterparts (e.g., 9 vs 4 percent at year 1), but this advantage reverses by year 5 and persists through year 9, when control movers show substantially higher growth than their treatment counterparts (27 vs 17 percent). This reversal likely reflects worker compositional changes at acquired firms: workers who have gained valuable international experience increasingly select into MNEs over time, as shown below. This results in a more negatively selected pool of workers transitioning from treated to local firms in later periods. Control firms do not experience this compositional shift, thus explaining the reversal in relative wage patterns seen in columns 5-6. The diverging trajectories – growing wage premia for MNE movers but growing disadvantages for local firm movers – support our hypothesis that foreign acquisitions facilitate the accumulation of MNE-specific human capital that is most valued by multinational firms.

In summary, the above descriptive statistics suggest that treated workers exhibit higher mobility

⁹The literature that studies the effect of foreign acquisitions on employment has little consensus regarding their effects. For instance, see Lehto and Böckerman (2008), Bandick and Görg (2010) and McGuckin and Nguyen (2001).

to MNEs compared with control workers and that treated workers who move to another MNE after acquisitions experience the fastest wage growth following their move.

3 The effect of acquisitions on job mobility

In this section, we investigate whether the higher mobility to MNEs by treated workers is caused by foreign acquisitions. To this end, we estimate equations (1) and (2) and present the baseline estimates of the effect of acquisitions on job mobility. We then study the robustness of the baseline estimates.

3.1 The baseline estimates

Table 4A presents the difference-in-differences estimates of equation (1). The estimated coefficient for DiD_{ij} measures the average treatment effect, which is the mobility difference between workers in treated and control firms for the entire post-acquisition period, compared with the difference for the whole pre-acquisition period. Columns 1 and 2 examine the average effect of treatment (foreign acquisitions) on workers' mobility to MNEs, and columns 3 and 4 to local firms. We include estimations both with and without firm and worker characteristics. It is clear that the estimated treatment effects remain unchanged when controlling for firm and worker characteristics.

Foreign acquisitions increase the likelihood of moving to another MNE by 3.6 percentage points, which implies that out of 100 treated workers, 3-4 more workers would move to MNEs due to the treatment. In contrast, there is a large negative effect of acquisitions on the likelihood of moving to local firms. More precisely, workers in firms acquired by foreign owners are approximately 4 percentage points less likely to move to a local firm, compared with workers who work in Swedish local firms that are never foreign acquired.

Looking at the control variables, we see that workers in larger firms are more likely to transition to MNEs and less likely to move to local firms. A 10 percent increase in firm size is associated with a half percentage point increase in the probability of moving to MNEs and more than 1 percentage point decrease in the probability of moving to local firms. Workers in firms with higher skill intensity are also less likely to move to local firms.

In Figure 4, we look at the dynamic effect of foreign acquisitions on mobility to MNEs (panel

A) and to local firms (panel B). This figure shows the stacked difference-in-differences estimates of β_j 's using equation (2), relative to one year before foreign acquisitions, and with 95 percent confidence intervals. Panel A displays that the point estimates in the pre-treatment period are not statistically different from zero, implying that the treatment and control groups have similar trends over the four years preceding foreign acquisitions. The first two years after acquisitions do not see any significant treatment effect. The effect of acquisitions on mobility starts to increase in the third year. It continues to rise until the sixth year when it stabilizes at 7 percentage points, suggesting that treated workers are 7 percentage points more likely than control workers to move to an MNE in the sixth year post-acquisition. Figure 4, hence, reveals that foreign acquisitions raise mobility to MNEs three years after the acquisitions, which supports the hypothesis that it takes time before a worker can gain valuable skills from working in a foreign MNE, and these skills increase mobility to other MNEs.

The treatment effect on mobility to local firms is seen in panel B. As above, the pre-treatment estimates are close to zero, suggesting that the parallel trends assumption holds. However, sharply differing from panel A, there is an immediate and negative effect of acquisitions on mobility already in year one when workers in treated firms are around 3 percentage points less likely than workers in control firms to move to a local firm, compared with the mobility difference in the first year before acquisitions. The negative effect becomes stronger and varies between 5 and 7 percentage points in the following years. However, standard deviations are large, and some of the point estimates are statistically insignificant.

We also divide the dynamic effect of acquisitions into six periods. The pre-acquisition periods include years 3-4 (pre-medium-run) and years 1-2 (pre-short-run) before an acquisition. The post-acquisition periods include years 1-2 (short-run), years 3-4 (medium-run), years 5-6 (long-run), and years 7-9 (longest-run) after an acquisition. In the regressions, the pre-short-run period is omitted as the years of comparison. As shown in Appendix Table A3, the treatment effect is very small and statistically insignificant in the short run, but increases to around 4 percentage points in the medium run and 6 percentage points in the long run, and reaches 8 percentage points in the longest run. These estimates confirm the pattern displayed in Figure 4, panel A, suggesting that it takes time for workers to gain skills, and the effect of acquisitions becomes stronger after workers have gained sufficient experience. In contrast, the effect is negative for movements to local firms. More

importantly, the effect is about the same in the short run as in the long run, which is consistent with the idea that movement to local firms is less likely to be driven by international skills accumulated after acquisitions.

3.2 Robustness to alternative constructions of the control group

A crucial step in our identification strategy is the selection of control firms that closely resemble the treated firms, so that the same shock would have a similar impact on both control and treated firms. In our baseline approach, we do not restrict control and treated firms to the same industry because of potential SUTVA violations. However, there is a concern that industry-specific shocks could affect control and treated firms differently when they are from different industries. To assess the sensitivity of our results to this issue, we consider alternative propensity score matching (PSM) specifications that constrain matching to occur within industry-year cells.

Columns 1-2 of Table 4B report the results when industries are grouped into three broad industry categories based on the SNI2002 classification: (i) manufacturing and resource-based industries; (ii) business and market-oriented services; and (iii) construction and social/personal services. This classification allows treated firms to be matched to control firms operating in similarly aggregated industry environments. Columns 3–4 display the estimates when industries are classified based on the first digit of the SNI2002 classification. To avoid overly narrow cells, we recode these one-digit categories into a set of industry groups based on their economic similarity and size in the data. This provides a more disaggregated grouping relative to the three-sector aggregation, while still ensuring that each treated firm can be matched to an appropriate control within its industry-year.

The results in Table 4B confirm our main finding that foreign acquisitions increase mobility to MNEs and reduce mobility to local firms. On the other hand, we notice that matching within industry-year cells has resulted in no valid matches for some treated firms, leading to the omission of a small share of treated firms from our analysis. Thus, the estimated treatment effect is for the subset of treated firms that are in the “matchable” cells.

It is worth pointing out that our baseline PSM specification includes industry-year fixed effects when estimating the propensity score for treatment. Industry-year fixed effects absorb cross-industry differences in acquisition propensities and, in combination with a rich set of firm characteristics, make treated and control firms comparable conditional on industry affiliation and year. This

approach allows us to match treated firms with control firms from a larger common pool within the same year. Thus, it has the benefits of preserving treated units, maintaining sufficient overlap between treated and control firms, and ensuring stable matches and more precise estimates.¹⁰ Moreover, the similarity of the estimated treatment effect shown in Table 4A and columns 1-4 of Table 4B suggests that our baseline estimates sufficiently account for the impact of industry-specific shocks on firm selection and worker mobility.

To further address the concern about the selection of control firms, we adopt the entropy balancing method developed by Hainmueller (2012) and used by, for instance, Basri, Felix, Hanna, and Olken (2021) and Conconi, Leone, Magerman, and Thomas (2025). Our entropy balancing algorithm minimizes the entropy distance metric: $\min \sum h(\omega_{ft}) = \sum \omega_{ft} \cdot \log(\omega_{ft}/q_{ft})$ subject to balance constraints and normalization, where t indicates the acquisition year, ω_{ft} are the entropy balancing weights for control firms and q_{ft} are the uniform base weights. The weights ω_{ft} are generated to exactly balance the first moments (means) of the following covariates between treatment and control groups: log employment, total sales, sales per worker, capital stock, and value added per worker for the year before acquisitions. That is, the weighted mean of each covariate in the control group must equal the mean of that same covariate in the treatment group. Our covariates capture the key dimensions of firm heterogeneity that could confound the acquisition effect. Furthermore, firms acquired in the same year are part of the same cohort. We construct a reweighted control group for each cohort of firms acquired in a given year. So each cohort gets separate balancing weights to address the staggered nature of acquisitions, allowing us to avoid the problematic comparisons that can arise in TWFE models with staggered treatment.

Differing from Basri et al. (2021) and Conconi et al. (2025), treatment in our study occurs at the firm level, but the unit of analysis is at the worker level. We propagate the firm-level weights to workers to maintain the integrity of the treatment assignment mechanism. We also maintain original weights for workers who move to other firms to preserve the intent-to-treat interpretation and avoid post-treatment conditioning. Workers at acquired firms receive a weight of 1. As stated in Hainmueller (2012), the entropy balancing weights can be passed to regression models and additional covariates can be included to remove bias that may arise from remaining differences

¹⁰ An additional benefit of the baseline PSM approach is that we can account for the cross-industry differences at a more disaggregated level. Here, industries are defined at the two-digit level of the SNI2002 classification.

between the treatment group and the reweighted control group. So we estimate equation (1) using the entropy balancing weights. We also replace the worker fixed effects with worker-cohort fixed effects. As in the baseline approach, we cluster standard errors at the firm-year level to account for correlation between workers. The results are shown in columns 5-6 of Table 4B. Again, we find that acquisitions significantly increase mobility to MNEs and reduce mobility to local firms.

In Appendix Figure A2 we also report the event study estimates when the control group is constructed using the above alternative PSM specifications (panels A and B) or using the entropy balancing method (panel C). We observe similar dynamic patterns that are seen from the baseline estimates. For example, panel C shows that the treatment effect on movement to MNEs becomes significant in the third year after acquisitions and steadily increases until the sixth year and stabilizes afterward. Remarkably, we obtain similar results even when the control group is constructed in a way drastically different from our baseline PSM approach. Note that these mobility regressions are very data intensive because all individuals across all years are included and serve as controls with varying weights in different cohorts. These regressions have nearly 100 million observations, as compared to our baseline regressions involving around 3 million observations that are based on workers in treated firms and their 1:1 twin control firms.

Overall, the results in Table 4B and Appendix Figure A2 confirm our main finding that foreign acquisitions significantly increase mobility to MNEs but reduce mobility to local firms. To save space, in the following we report only the results based on the control group that is constructed using our baseline PSM approach.

3.3 Additional robustness checks

We continue our analysis with several alternative specifications to examine the robustness of our baseline results. We start by including firm age as an additional control to capture one of the main determinants of firm growth. As shown in Appendix Table A4, the estimated acquisition effect is identical to our baseline estimates. We also find that workers at older acquired firms are more likely to move to MNEs and less likely to move to local firms. Furthermore, because of missing information on firm age, adding firm age reduces our sample of firms and workers.

The baseline estimations examined the effect of acquisitions on job mobility over a four-year period before acquisitions and nine years after. In Appendix Table A5 we have experimented with

re-estimating equation (1) using different periods. For comparison, column 1 lists the baseline estimates from columns 2 and 4 of Table 4A. The other columns report the results when we have a shorter pre-acquisition period (starting from two or three years before an acquisition) and/or a shorter post-acquisition period (ending with six years after an acquisition). The results are robust: foreign acquisitions increase job mobility to MNEs and decrease job mobility to local firms. The effect on mobility to MNEs remains similar in size to the previously shown results: the treatment increases job mobility to MNEs by 2.8-4.0 percentage points. In contrast, foreign acquisitions decrease mobility to local firms by 5.0-7.8 percentage points in the different specifications. Finally, we have also estimated various specifications above with our event study approach. The results shown in Appendix Figures A3 and A4 confirm the patterns in Appendix Table A5. Importantly, those figures also show that the pre-treatment estimates are insignificantly different from zero, and thus the parallel trends assumption likely holds.

The analysis shows that workers in treated firms are more likely to move to other MNEs. We have argued that this is because MNEs have a similar demand for worker skills. An alternative hypothesis would be that workers in treated firms have the ability to move to “good” firms with high wages, irrespective of these firms’ international engagement. If so, the observed increase in mobility to MNEs would merely reflect that MNEs tend to be high-wage employers.

We examine this alternative hypothesis in Appendix Table A6 where destination firms are divided based on wages. More specifically, we examine mobility to the ten percent of firms with the highest wages (column 1), to the 20 percent with the highest wages (column 2), and to the 33 percent with the highest wages (column 3). Across all specifications, we find no statistically significant effect of treatment on mobility to high-wage firms. That is, the difference in mobility to high-wage firms between treated and control workers does not change significantly after acquisitions. This evidence suggests that foreign acquisitions do not increase mobility to “good firms” in general; (i.e., high-paying local firms or MNEs); instead, the effect is restricted to mobility to MNEs that are interested in hiring workers with international experience due to their needs for specific skills.

As another robustness check, we add industry-year fixed effects to our baseline specification in equation (1). The results are displayed in Appendix Table A7, columns 2 and 6. For comparison, columns 1 and 5 report the baseline estimates carried over from columns 2 and 4 of Table 4A. We find that the estimated treatment effect is little changed after adding industry-year fixed effects.

We also ran regressions by replacing worker fixed effects with firm fixed effects. The results are shown in columns 3 and 7. We can see that the results with firm fixed effects are also consistent with earlier results, although there is a larger change in the magnitude of the treatment effect. However, for our worker-level analysis, we find it less straightforward to interpret the results with firm fixed effects because we analyze job mobility and want to allow workers to switch between firms. In columns 4 and 8, we exclude worker fixed effects from the baseline specification. We find similar results with and without worker fixed effects.

Finally, we have looked at the aggregate effect of foreign acquisitions on labor market outcomes. We find no treatment effect on overall mobility, unemployment, or exiting the labor force. But foreign acquisitions appear to slightly reduce the incentive to take early retirements or social benefits.

4 Implications for wage growth

Foreign acquisitions have been found to increase mobility to MNEs. In this section we continue our analysis by examining the implications of internationalization and mobility for wage growth. We would expect to see a positive wage effect if workers gain new skills post-acquisition and their international experience is valued by new employers. A positive wage effect also supports the hypothesis that international experience gained from working at the acquired firm can help workers climb job ladders and move onto a higher wage growth trajectory. The wage growth associated with the transition from one job to another has important implications for lifetime earnings (Topel and Ward, 1992; Rubinstein and Weiss, 2006; Altonji et al., 2013).

To estimate the effect of acquisitions and mobility on wages, we look at the sample of all workers who are employed in treated firms at the time of acquisitions and compare movers with stayers in acquired firms (treated firms). Specifically, we estimate the following wage equation:

$$w_{ijt} = a_0 AFTER_j + a_1 Mover_{ij} + a_2 AFTER_j \times Mover_{ij} + \gamma_i + X_{it}\delta + X_{f(i),t}\theta + \omega_t + v_{ijt}, \quad (4)$$

where w_{ijt} is the log wage of worker i at event time j and in calendar year t ; $AFTER_j$ indicates the year of an acquisition and all subsequent years; $Mover_{ij}$ is equal to one if worker i is at another

firm in event year j , and zero otherwise; γ_i denotes worker fixed effects to capture unobserved worker ability that may affect wage growth; X_{it} and $X_{f(i),t}$ are time-varying worker and firm characteristics, respectively; ω_t are year fixed effects to capture macroeconomic factors that may affect wage growth for all workers; and v_{ijt} is the error term. Because movers to MNEs may differ from movers to local firms, we run separate regressions for these two types of movers. Stayers are always the comparison group.

Our main interest is in the coefficient a_2 , which represents the wage gains associated with moving to another firm after acquisitions, compared with those before acquisitions. With stayers as the comparison group, we interpret these gains as the extra wage growth by movers compared to what workers would have earned had they chosen to stay at the acquired firms after acquisitions.

We also look at the dynamic wage effect by modifying equation (4) as follows:

$$w_{ijt} = \sum_{j=-4}^9 \alpha_j^0 T_j + a_1 Mover_{ij} + \sum_{j=-4}^9 \alpha_j (T_j \times Mover_{ij}) + \gamma_i + X_{it}\delta + X_{f(i),t}\theta + \omega_t + v_{ijt}, \quad (5)$$

where T_j represents event time dummies with the year before an acquisition ($j = -1$) omitted as the reference year, α_j^0 captures the wage growth by stayers at event time j relative to $j = -1$, and α_j represents the additional wage growth by movers, compared with stayers at event time j . All the other explanatory variables are defined as above.

Table 5 reports the estimates of equation (4). The first two columns compare wage growth by movers to MNEs with wage growth by stayers in treated firms. In column 2 where control variables are included, the post-acquisition difference in wage growth between movers to MNEs and stayers is around 9.5 percentage points higher than the pre-acquisition difference. Moreover, the coefficient of movers is negative, suggesting that movers could be relatively underpaid in their original firm, which is why they have an incentive to move after gaining skills. Table 5 also shows that more educated workers get higher wages. Finally, more capital-intensive and skill-intensive firms pay higher wages.

Columns 3-4 report the results for movers to local firms, compared with stayers in acquired firms. We observe additional wage gains by movers to local firms after acquisitions, which is consistent with the finding by Balsvik (2011) that as workers with MNE experience move to local firms, they generate knowledge externalities to local firms and earn higher wages after moving. It also reflects

the indirect impact of foreign multinationals on wages and employment at local domestically owned firms (Setzler and Tintelnot, 2021). However, we also note that the magnitude of wage gains is smaller than that for movers to MNEs, which provides support to the conjecture that international experience gained at the acquired firms is more valuable for other MNEs.

Figure 5 displays the estimates of α_j in equation (5). Panel A shows that before acquisitions, the wage growth is not significantly different between movers to MNEs and stayers. However, one year after acquisitions, the additional wage growth for movers to MNEs is about 10 percentage points and the effect is rather stable at this level up until 9 years after acquisitions. Hence, the figure suggests that wages increased quite substantially for workers who leave their jobs in acquired firms and move to MNEs.

Panel B shows the estimates of α_j for movers to local firms. We find that one year after acquisitions, the wage growth for movers to local firms is 7 percentage points higher than that for stayers. But this wage growth gap between movers to local firms and stayers starts to shrink over time and becomes insignificant after the third year of acquisitions.

To illustrate the different wage growth trajectories for stayers, movers to MNEs, and movers to local firms, Figure 6 plots the cumulative wage growth rates for these three groups of treated workers, using the estimates of α_j^0 for stayers and $\alpha_j^0 + \alpha_j$ for movers, with $j = -1$ as the base year. As in Figure 5, the difference in wage growth between stayers and movers is small before acquisitions. After acquisitions, however, their wage paths diverge and movers to MNEs exhibit the steepest wage growth. When translated into annualized growth rates over the post-acquisition period, the wage premium for movers to MNEs is clearly higher than for both stayers and movers to local firms. With $j = -1$ as the base year, the cumulative wage rates translate into an annualized wage growth rate of 6.4 percent for movers to MNEs, 5.3 percent for movers to local firms, and 5.5 percent for stayers over 10 years. The persistent disparity in annualized wage growth rates implies divergent lifetime earnings for the three different types of workers after acquisitions.

In our analysis we look at workers who undergo two switches: one when their firm is acquired and a second one when they move out of their acquired firm into other firms. To study whether the above results are robust to any tenure restrictions, we estimate equation (5) by restricting workers to stay in the two firms for at least two years. We find that the wage growth results are almost unchanged. We also study whether the wage growth is persistent over a longer period by focusing

on acquisitions that have 12 or 15 years post-acquisition. Again, our estimates are very similar to what is shown in Figure 5.

In summary, we find that foreign acquisitions increase the probability of moving to MNEs and substantially raise the wage growth rate for movers to MNEs. On the other hand, acquisitions reduce the likelihood of moving to local firms and boost the wage growth for movers to local firms only temporarily. Therefore, acquisitions appear to shift the career path more toward MNEs and push workers onto a much steeper wage growth trajectory.

These results provide new insights into the wage effects of foreign acquisitions. While previous studies have primarily focused on stayers, our analysis reveals the wage effect on movers in comparison to stayers. To connect with the existing literature, we also explore whether foreign acquisitions influence average firm wages. We estimate equation (2) by replacing the dependent variable with log firm wages. We find that the estimated coefficients on the interactions between event time fixed effects and the treatment indicator are statistically insignificant for all years. Thus, we find no effect of foreign acquisitions on average firm wages at the acquired firms after an acquisition, which is consistent with the previous finding of a small or no wage premium in MNEs once one controls for firm and worker characteristics (e.g. Heyman et al., 2007).

5 Heterogeneous effects on mobility

In the above, we provided evidence that foreign acquisitions increase mobility to MNEs and widen the chances of climbing up the job ladder. We now delve deeper into the mobility pattern to uncover the differences between workers in different occupations and among firms acquired by different source countries, thus shedding light on the role of worker skills and source countries of foreign acquirers in shaping the mobility of workers at acquired firms.

5.1 Worker occupations

Foreign acquisitions can bring opportunities for workers to acquire new skills and international experience. For instance, when newly acquired firms reorganize their production to serve new markets, operators can learn new production methods while managers and professionals can be exposed to cross-border operations. With more skills and international experience, all types of

workers have an incentive to move to other firms for better pay. On the other hand, we expect foreign acquisitions to offer more learning opportunities to managers and professionals because they are more involved in the decision-making process. Thus, the experience of working in an acquired firm might be of higher value for more skilled occupations compared with less skilled occupations (Mion et al., 2022), suggesting that workers in higher-skilled occupations would have higher mobility to other MNEs that value international experience more.

We look at this in more detail in Figure 7, where we examine mobility separately for high-skilled occupations (managers, professionals, technicians) and low-skilled occupations (operators and clerks). The results show that there is no significant pre-acquisition difference between treated and control workers for both high- and low-skilled occupations. However, all types of workers see higher mobility to MNEs after acquisitions.¹¹ For high-skilled occupations, the positive effect starts to appear statistically significant three years after an acquisition, and peaks at nearly 10 percentage points nine years after the acquisition. In contrast, for low-skilled occupations, the effect plateaus at 5 percentage points in the 6th year post-acquisition. Thus, the effect seems stronger for high-skilled occupations than for low-skilled occupations, although the differences are not statistically significant. On the other hand, for both high- and low-skilled occupations, movements to local firms are insignificantly different between treated and control workers.

Occupations are closely linked to education levels. Managers, professionals and technicians require a higher level of education, compared to operators and clerks. Thus, we further examine mobility separately for low-, middle-, and high-education groups. The low-education group includes workers with at most 9 years of compulsory schooling. The middle-education group includes workers with 2-4 years of upper secondary school education. And the high-education group includes workers with at least an undergraduate college education. We find that the treatment effect on mobility to MNEs is significantly positive for all education groups. As shown in Appendix Figure A5, for all education groups, it takes several years for the treatment effect to turn significantly positive. It also appears that workers in the low-education group take a little longer to gain skills and move to other MNEs. On the other hand, the treatment effect on mobility to local firms is negative for all education groups and the effect becomes statistically insignificant after several years post-

¹¹Mion et al. (2022) find that blue-collar workers get a wage increase when they move to a new firm from firms where they have gained international experience, which suggests that low-skilled occupations learn valuable skills in internationalized firms.

acquisition.

To summarize, we find that workers in different skill levels see a significantly positive effect on mobility to other MNEs after foreign acquisitions. We also find that the effect seems slightly stronger for workers in high-skilled occupations and those with higher levels of education because those workers are typically more involved in international operations and thus gain more skills after acquisitions.

5.2 Source countries of foreign acquirers

Because source countries differ in their market networks, technology advantages and management practices, acquisitions by different foreign acquirers may provide workers with different opportunities to obtain international experience, which may further affect the pattern of job mobility. To examine this, we exploit the information on the source countries of foreign acquirers. In Table 6A we study the impact of foreign acquisitions on mobility separately by the source countries of foreign investors. The top investors – ranked by the number of acquisitions – come from Norway, Denmark, the US, the UK, Germany, Finland, and France. We also look at source-country groups: EU-15 countries, Nordic countries (excluding Sweden), tax havens (the Netherlands, Luxembourg, and Switzerland), and English-speaking countries.¹² The table reports the estimated coefficient on DiD_{ij} from equation (1) where the regressions are run separately by source countries.

Panel A shows that foreign acquisitions tend to have a significantly positive effect on mobility to other MNEs. For example, column 1 reports that acquisitions by Norway raise the mobility of treated workers to other MNEs by about 9 percentage points. However, there are exceptions, including Denmark, Germany, and France.

In the above, worker movements to MNEs include mobility to both foreign and Swedish MNEs. In panel B, we instead focus on mobility to foreign MNEs (excluding mobility to Swedish MNEs). To this end, we re-estimate equation (1) with the dependent variable as a binary variable that takes the value one if a worker moves to a foreign MNE, and zero otherwise. The results suggest that foreign acquisitions significantly increase mobility to foreign MNEs in most cases. Further, we compare panels A and B to assess the effect of foreign acquisition on mobility to Swedish MNEs. For

¹²EU-15 countries include investors from Belgium, Denmark, Germany, Finland, France, Greece, Ireland, Italy, Luxembourg, the Netherlands, Austria, Portugal, Spain, and the UK (which was a member of the EU during our studied period). English-speaking countries include the UK, the US, Ireland, Australia, and New Zealand.

example, in column 3, the estimated coefficient for the US is 0.052 in panel B, which is larger than the estimate of 0.037 in panel A, suggesting that acquisitions by US investors significantly increase the mobility to non-Swedish MNEs while reducing the mobility to Swedish MNEs by 1.5 percentage points (0.037–0.052). A similar pattern holds for acquisitions by Germany, Denmark, Finland, and the EU-15 group. For Norway and tax-haven countries, the estimated coefficients in panels A and B are very similar, suggesting that acquisitions by those countries significantly increase mobility to foreign MNEs while not affecting mobility to Swedish MNEs. Overall, a comparison of panels A and B indicates that higher mobility to MNEs largely reflects higher mobility to foreign MNEs.¹³

In panel C, we study the movement to foreign MNEs with owners from the same source country as those of foreign acquirers. In all cases, the treatment effect is significantly positive, implying that foreign acquisitions significantly increase the probability of transitioning to other MNEs with owners from the same source country. We further compare the estimates in panels B and C to gauge the effect of acquisitions on mobility to foreign MNEs other than those from the same source country. The comparison reveals some variations across source countries. Column 5 shows that acquisitions by Germany increase mobility to German-owned MNEs by 8.1 percentage points while reducing mobility to other foreign MNEs by 4.4 percentage points (0.037–0.081). Acquisitions by France have a similar effect which is strongly skewed toward French-owned MNEs. Acquisitions by Norway, Denmark, the US, and Finland also significantly increase the mobility to MNEs from the same source country, although the effect is not as skewed as that for Germany and France. In contrast to these countries, acquisitions by the UK increase mobility to UK-owned firms by 1.1 percentage points and increase mobility to other foreign MNEs by 3.6 percentage points (0.047–0.011).

A potential threat to identifying the acquisition effect involves multi-firm business groups. If a worker switches from an affiliate of the parent firm (the firm newly acquired) to another affiliate of the same parent firm, this job switch would mechanically lead to an increase in the probability of moving to an MNE of the same country of origin. To address this concern, we utilize information on business groups and separate the mobility to MNEs from the same source country into two categories: mobility to firms outside the same business group and mobility to firms within the same business group. The results are reported in panels B and C of Table 6B. Panel A carries over

¹³Appendix Table A8 shows that foreign acquisitions increase mobility to foreign MNEs, but have no, or even a small negative, effect on mobility to Swedish MNEs. In addition, this pattern remains unchanged when we look at larger destination firms with more than 500 or 1,000 employees.

the estimates shown in panel C of Table 6A. A comparison of the estimates in panels A-C suggests that mobility to firms outside the same business group accounts for the majority of the transitions to other MNEs from the same source country. For Denmark and France, mobility to MNEs from the same country of origin is almost entirely a transition to firms outside the same business group.

In panel D, we exclude firms that are part of multi-firm business groups. The estimated effect of acquisitions on mobility to other MNEs from the same source country remains significantly positive, except for the U.S. acquisitions.

In panel E, we restrict the sample to the set of treated workers and let the control group be the set of workers whose firms are acquired by firms from other countries. For example, in column 1, control workers include those at firms acquired by non-Norwegian companies. We find that the estimated acquisition effect is very close to that reported in panel A.

To dig deeper into worker movement from firms acquired by top investors, Table 7 separates destination firms based on their country of ownership. We find that acquisitions by Germany reduce mobility to most of the other foreign MNEs.¹⁴ In contrast, acquisitions by the UK raise mobility to MNEs owned by most of the other top investors. A similar pattern is seen for US acquisitions, although the positive effect on mobility to other foreign MNEs is relatively small compared to mobility to other US-owned firms. In addition, acquisitions by Norway and Denmark increase mobility to other Nordic-owned MNEs.

Overall, Tables 6-7 suggest that compared with control workers, workers at acquired firms are significantly more likely to move to foreign MNEs and MNEs with owners from the same source country in particular (except for UK-acquired firms). Furthermore, the effect of foreign acquisitions on mobility to other MNEs from the same country of origin is mostly reflected in mobility to firms outside the same business group rather than within it. These results suggest that the skills and experience gained at acquired firms following foreign takeovers differ from those accumulated in Swedish owned firms, leading to divergent mobility paths. Furthermore, some of the skills and experience gained after foreign acquisitions may be specific to the home markets of foreign investors, leading to higher mobility to other MNEs from the same source country of foreign acquirers.

¹⁴Based on the estimates in columns 5 and 8, we can infer that acquisitions by German investors reduce the mobility to non-German owned MNEs from the EU-15 area by 3.1 percentage points (0.050–0.081).

6 Mechanisms

In the above, we provided evidence of a positive effect of foreign acquisitions on upward mobility. We argued that the higher mobility to other MNEs is likely driven by international experience accumulated at the acquired firms and that these new skills increase the appeal of treated workers to other MNEs that demand similar skills. In this section, we explore the channels through which workers may acquire skills following acquisitions.

Existing studies have documented important firm-level changes following acquisitions, including the reorganization of firm operations (Bastos, Monteiro, and Straume, 2018), adoption of advanced technologies (Gardberg, Heyman, and Tåg, 2023; Ma, Ouimet, and Simintzi, 2025), and adoption of new management practices (Bloom and Van Reenen, 2007, 2010; Heyman, Norbäck, and Hammarberg, 2019). It is also well-documented that multinational firms are large importers and exporters and that most of them have extensive global production networks (e.g. Bernard, Jensen, and Schott, 2009). The recent study by Conconi et al. (2025) further demonstrates that foreign acquisitions increase the trade participation of acquired firms through MNE networks. Such characteristics and changes may explain the increased mobility following an acquisition.

Therefore, in the following, we consider the roles that access to multinational production networks, advanced technologies, and high-quality management practices can play in determining worker mobility. As acquired firms undergo changes to their production and trade, technology, and management practices, workers at acquired firms can gain new skills, making them more attractive to other multinationals, especially within specific multinational production networks. Thus, the probability of moving to other MNEs should increase, with the increase being strongest for movement to MNEs from the same source country.

6.1 Multinational production networks

One possible explanation for the mobility effect is that the acquired Swedish firms may be integrated into the multinational production network of foreign acquirers after acquisitions. Consequently, workers at the acquired firms have the opportunity to acquire international skills, some of which may be country or market specific. Hence, this could explain the pattern of large mobility to firms from the same countries.

To provide preliminary evidence for multinational production networks through international trade, in Table 8 we report the difference-in-differences estimates of the effect of acquisitions on firm-level trade flows based on the following equation:

$$Y_{fjt} = \rho_0 AFTER_j + \rho_1 TREATED_f + \rho_2 DiD_{fj} + \eta_f + X_{ft}\theta + \omega_t + \nu_{fjt}, \quad (6)$$

where Y_{fjt} represents the value of firm trade (export and/or import) for firm f at event time j and in calendar year t , $AFTER_j$ takes the value one in the year of an acquisition and all years after, and $TREATED_f$ takes the value one for acquired firms. DiD_{fj} (i.e., $AFTER_j \times TREATED_f$) is our variable of main interest, and its coefficient captures the effect of acquisitions on firm-level trade flows. η_f represents firm fixed effects to capture unobserved firm characteristics that may affect firm trade. Note that the firm fixed effects subsume $TREATED_f$ and industry fixed effects. We also control for time-varying firm characteristics (X_{ft}), including log employment, the share of skilled workers, and capital intensity. ω_t represents year fixed effects. ν_{fjt} is the error term.

Table 8 reports that the total amount of trade increases significantly in acquired firms after acquisitions. The increase in trade is driven by imports, while the amount of exports is not significantly affected by acquisitions. These patterns suggest that the acquired Swedish firms increase their use of imported intermediates following acquisitions and become integrated into the multinational production networks of foreign acquirers.¹⁵

To dig deeper, in Table 9 we look at firm-level trade shares separated by the source countries of foreign acquirers. Again, we focus on the top foreign investors. Panel A shows the difference-in-differences estimates of equation (6) where the dependent variable is the value of firm trade with a specific source country as a share of total firm trade. We find that acquisitions have a significantly positive effect on trade shares for most source countries; that is, acquired firms have significantly adjusted their trade composition by trading more with the source country of foreign investors following acquisitions, compared with control firms.¹⁶ This provides further evidence for the integration of acquired Swedish firms into multinational production networks of foreign acquirers. The increase in trade shares is especially strong for foreign acquirers from Nordic countries, Germany, and the EU-15 area. For example, panel A, column 1 shows that firms acquired by Norwegian

¹⁵We obtained similar results when the control firms are weighted by the entropy balancing weights.

¹⁶Our data do not have information on whether the trade is within multinational firms or at arms-length.

investors experience an increase in the value of trade with Norway as a share of total firm trade by 6.8 percentage points after acquisitions, compared to control firms. These results are consistent with the finding by Conconi et al. (2025) that foreign acquisitions generate MNE network effects through which multinational ownership increases the trade participation of acquired firms.

In contrast, column 10 in panel A shows no significant effect of acquisitions on trade shares for investment from tax havens. Note that in the case of acquisitions by tax haven countries, the nationality of foreign investors is unclear – some of the firms might be from Sweden or any other country. In this case, acquisitions could be driven by tax avoidance rather than by building up production networks. Thus, no significant effect on trade shares for source countries as tax havens is consistent with our expectation.

Panels B-F report estimates on trade with countries other than the source country as a share of total firm trade for firms acquired by Norway, Denmark, the USA, the UK, and Germany, respectively. In contrast to trade with the source country of foreign acquirers, there is little evidence of increased trade with any other countries after acquisitions. Almost all estimates are statistically insignificant.

The pattern of trade in Table 9 aligns well with the pattern of mobility shown in Tables 6-7. Overall, we see that in the firms that experience stronger trade linkages with the source country of foreign acquirers, workers there tend to have higher mobility to other multinationals originating from the same source country. For instance, at German-acquired firms, the share of trade with Germany increases significantly following acquisitions compared to control firms (3.9 percentage points in panel A, column 5 of Table 9), and the mobility of workers to other German-owned MNEs becomes significantly higher after acquisitions compared with control workers (8.1 percentage points in panel C, column 5 of Table 6A). Similarly, we see strong trade linkages among Nordic countries and high mobility among Nordic MNEs. The same patterns hold for the groups of EU-15 countries and English-speaking countries. These results suggest that workers at firms acquired by Germany, Nordic countries, the EU-15 country group, or the English-speaking country group may gain market-specific international skills after acquisitions, thus increasing the chance of moving to other multinationals from the same source country (market) as that of the foreign acquirers.

6.2 Technology adoption

Belonging to a multinational network not only facilitates access to additional sources of intermediate inputs but also access to new technologies. Studies by Bastos et al. (2018), Gardberg et al. (2023), and Ma et al. (2025) provide strong evidence for increased technology adoption after acquisitions. Acquisitions by countries with more sophisticated technologies expect to introduce larger changes to production technology at acquired firms, thereby providing more opportunities for workers to enhance their skills and leading to higher mobility to other MNEs that use similar technologies. At the same time, it is also possible that technology adoption shifts the demand for certain job tasks (Nilsson Hakkala, Heyman, and Sjöholm, 2014), leading to more mobility overall. Existing studies have focused on the impact of technology adoption on acquired firms and workers who have remained at the acquired firms. In contrast, our analysis focuses on the effect on job mobility and workers who have chosen to move to other firms.

To study the channel through technology adoption, we match annual industry-country level measures of technology sophistication, including expenses on computing and communications equipment (ICT) and expenses on computer software and database, with acquiring firms' industry affiliation and source countries. Annual information on ICT, software and database capital stock, and total capital stock at the country and industry level is drawn from the EU KLEMS 2019 database for all EU countries, the US, the UK, and Japan.

Our annual industry-country level measures of technology sophistication include ICT to total capital, and software capital to total capital. For each industry, we define higher-tech countries as those with technology expenditures (as a share of total capital stock) above Sweden's level, and lower-tech countries as those below Sweden's level. So a country can be higher-tech in some industries but lower-tech in others, implying that the measures help capture the comparative advantage in advanced technologies relative to Sweden. Based on this definition, about 20 percent of country-industry pairs are higher-tech in our sample.

Then we separately estimate the effect of acquisitions on job mobility by higher-tech and lower-tech source countries. Panel A of Table 10A shows that mobility to MNEs is significantly higher for workers at firms acquired by higher-tech countries. By contrast, the effect of acquisitions by

lower-tech countries is relatively small.¹⁷

In panel B, we focus on mobility to foreign MNEs. Based on the ICT measure, we find that acquisitions by higher-tech countries increase mobility to MNEs more than acquisitions by lower-tech countries. However, based on the software and database measure, the mobility effect does not differ significantly between acquisitions by higher-tech and those by lower-tech countries. We also compare panels A and B to assess the effect on mobility to Swedish MNEs. For instance, columns 3-4 show that acquisitions by higher-tech countries increase mobility to Swedish MNEs by 0.8 percentage points (0.056–0.048) while acquisitions by lower-tech countries reduce the mobility to Swedish MNEs by 2.9 percentage points (0.011–0.040). We note that in terms of technology, Swedish MNEs are more similar to MNEs with owners from countries with advanced technologies. Thus, this result provides a potential explanation for the weak effect of foreign acquisitions on mobility to Swedish MNEs.

In panels C and D, we further break down destination firms into those owned by higher-tech countries (“higher-tech MNEs”) and those owned by lower-tech countries (“lower-tech MNEs”), respectively. Suppose firms acquired by higher-tech countries adopt new advanced technologies, thereby providing opportunities for workers to acquire new skills. In that case, workers at those firms should expect a better chance of moving to other MNEs that use similar advanced technologies. Our expectation is borne out by the results shown in panel C. Based on the ICT measure, we find that mobility to higher-tech MNEs is 2.8 percentage points higher for workers at firms acquired by higher-tech countries, compared to 0.8 percentage points higher for workers at firms acquired by lower-tech countries (see columns 1-2). We obtain similar results when using software and database expenditures to measure technology sophistication (see columns 3-4). These results suggest that workers may gain more skills at firms acquired by countries with more advanced technologies, thus having higher mobility to other MNEs that use similar advanced technologies.

On the other hand, panel D shows that acquisitions by lower-tech countries have a larger effect on mobility to lower-tech MNEs, compared to acquisitions by higher-tech countries. This difference is larger for technology measured by software and database capital.

To strengthen the evidence for the channel of technology adoption, we use survey data for

¹⁷We also use an alternative definition of higher-tech vs lower-tech country-industry pairs based on the sample median. We find similar results that acquisitions from higher-tech countries increase mobility to other MNEs, but there is no significant effect of acquisitions by lower-tech countries.

firm-level expenditures on data and software in Swedish firms for the period 2009-2015.¹⁸ Note that out of 2,681 acquisitions in our sample, only about 400 firms were acquired after 2009. Thus, although the survey data provide us with firm-level information on IT expenditures, we have no information for most acquired firms in the pre-acquisition period. Instead, we categorize firms into higher- or lower-tech groups based on their post-acquisition IT expenditures. The results are shown in Table 10B. In panel A, higher-tech firms are those with expenditures on software or data above the industry median, and lower-tech firms are those with expenditures below the industry median. As displayed in columns 1 and 3, workers at acquired firms that spend relatively more on software or data are significantly more likely to move to MNEs, compared to workers at local firms. In contrast, columns 2 and 4 show that workers at acquired firms with lower IT expenditures are not different from workers at local firms in their mobility to MNEs. In panel B, we define higher-tech firms as those with positive spending on software or data, and lower-tech firms as those without any IT expenditures. The mobility pattern stays the same. Therefore, we find strong evidence that the positive effect of foreign acquisitions on mobility to MNEs is due to the higher mobility of workers at acquired firms that made more IT investments following acquisitions.

6.3 Management practices

Studies by Bloom and Van Reenen (2007), Bloom, Lemos, Sadun, Scur, and Van Reenen (2014), and Bloom, Sadun, and Van Reenen (2012, 2016) document significant cross-country differences in the quality of management practices and show that it accounts for about a quarter of cross-country and within-country TFP gaps. Like technology, new management practices can be introduced to acquired firms following foreign acquisitions. As shown in Heyman et al. (2019), differences in global management practices are an important determinant of productivity among foreign affiliates in Sweden. While existing studies focus on the impact of management practices on firm productivity, we are interested in the impact on worker mobility.

To study whether management practices may influence the mobility of workers at acquired firms, we construct an index of the quality of management practices for MNEs with headquarters in the source countries of foreign acquirers in our sample. Specifically, to extract source country

¹⁸The survey data are collected by Statistics Sweden from a sample of just over 4,500 firms. All firms with 200 or more employees are included in the survey. The survey also includes a representative sample of firms with 10 or more employees. Firms that have not had any IT expenditure are required to respond to ensure the quality of the survey.

differences in management practices among MNEs, we follow Heyman et al. (2019) and use global data on management practices from the World Management Survey (WMS) to estimate average differences in global management practices for MNEs originating from the source countries that are engaged in foreign acquisitions over our sample period, controlling for host country, year, and industry fixed effects. The WMS is based on randomly drawn samples of mid-sized firms in multiple industries. The questionnaire comprises 18 questions regarding management practices in three areas: monitoring, targets, and incentives/people.¹⁹ MNEs include both parent firms and their foreign affiliates. The variation in the quality of management practices in the foreign operations of MNEs stems from the institutions or economic conditions in the source countries of the MNEs, and from the need to overcome the cultural or geographical distances between the source country and the various host countries in which the MNEs invest.

We construct two measures of the quality of management practices based on two different sets of firms included in the estimation of management practice differences across source countries: *Management1* is based on all multinational firms in the WMS, and *Management2* excludes parent firms, i.e., the part of the MNEs located in the source country. Source countries with high-quality management practices are those with a measure above the sample median or above the 75th percentile of the measure. Source countries with low-quality management practices are those with a measure below the sample median or below the 25th percentile of the measure.

Table 11 shows that foreign acquisitions have significantly positive effects on mobility to MNEs (panel A) and to foreign MNEs (panel B), no matter whether the source countries of foreign acquirers have high- or low-quality management practices. However, the positive effect turns out to be smaller for acquisitions by countries with high-quality management practices. One possible explanation for this result is that firms acquired by countries with high-quality management practices see larger productivity gains (see Heyman et al., 2019) and provide more incentives for the best workers to stay (better management practices), thus attenuating the incentive to move to other MNEs.

¹⁹Monitoring focuses on how well companies observe their internal activities and how well they use this information for continuous improvement. Targets investigate whether companies establish correct targets, track correct outcomes, and take correct actions if the targets and outcomes are inconsistent. Incentives/people considers whether organizations promote and reward employees based on performance and prioritized hiring while attempting to retain their best workers.

7 Concluding remarks

We leveraged the foreign acquisitions of Swedish firms as a lens to examine the internationalization shock, and subsequently, its causal effect on job mobility. In doing so, we highlighted the potential for employees within these acquired firms to gain valuable international experience post-acquisition. In an attempt to account for the non-random nature of acquisitions, we matched foreign acquired firms with never-acquired Swedish local firms using one-to-one propensity score matching. These non-acquired domestic firms served as our control group.

We employed a stacked difference-in-differences estimation methodology, facilitating a comparison of mobility in the treated and control firms both before and after the acquisitions. Our findings suggest that international experience, accrued in the wake of foreign acquisitions, bolsters the likelihood of employees transitioning to another MNE by 3.6 percentage points. Conversely, it diminishes the probability of shifting to local firms by about 4 percentage points.

Interestingly, we observed that the impact of acquisitions on mobility does not occur immediately but instead unfolds gradually. It becomes significant in the third year following an acquisition and stabilizes thereafter. This supports the assertion that workers require time to cultivate international skills which enhance their employability and facilitate transitions to more lucrative positions.

Furthermore, we analyzed the effect of acquisitions on wage growth for those who moved to new jobs. Compared with stayers at acquired firms, movers to other MNEs experienced a steeper wage growth trajectory: the annualized wage growth rate is about 5.5 percent for stayers and 6.4 percent for movers to MNEs over a 10-year period. On the contrary, movers to local firms experienced a more modest wage growth, compared with those who remained at the acquired firms.

In light of our findings, we argue that foreign acquisitions not only have a transformational effect on the acquired firm's operations but also on the career trajectories and earnings potential of their employees. These impacts of acquisitions on job mobility and wage growth have been largely overlooked in the existing literature, underscoring the significance of our study.

Our results also have potential policy implications. If the acquisition of domestic firms by foreign multinationals increases job mobility and wage growth, then policies that facilitate these acquisitions could have broader benefits for the labor market. Furthermore, our study sheds light on the timing of these effects. With the significant impact on mobility occurring from the third year

post-acquisition, there is clear evidence of a ‘learning period’ for employees to adapt and develop skills in an international business context. This finding could guide future research into how and when workers derive the most benefits from international exposure.

In conclusion, our study not only advances our understanding of the implications of foreign acquisitions for job mobility and wage growth, but it also sets the stage for further investigations into the timing, mechanisms, and broader implications of these effects. We hope our research will inspire further inquiry into these important aspects of international economics and labor mobility.

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Figures

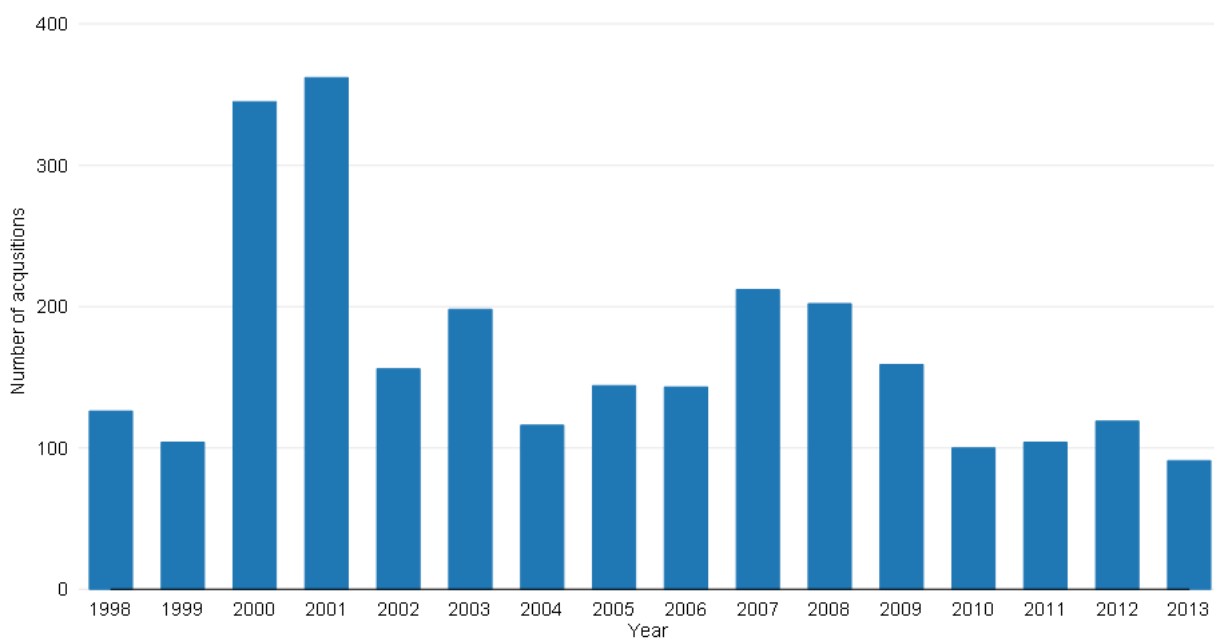


Figure 1: Annual number of foreign acquisitions of Swedish local firms (1998-2013).



Figure 2: Absolute differences in treated and control firms in matched and unmatched samples.

Note: The X-axis shows the absolute value of the standardized percentage bias for the matched and unmatched firms. This is calculated by storing the means and variances of lags one year, lags two years, and one-year differences for treated and control firms in the matched and unmatched samples, with all years pooled. Then the absolute values of standardized percentage bias are calculated according to the formula in Austin (2009). Treated firms are Swedish local firms that are acquired by foreign owners over the period 1998-2013. Control firms are Swedish local firms that are never acquired by foreign owners over the same period. Low-skilled workers are those with at most 9 years of compulsory schooling. High-skilled workers are those with at least undergraduate college education. Log firm size is measured by log employment.

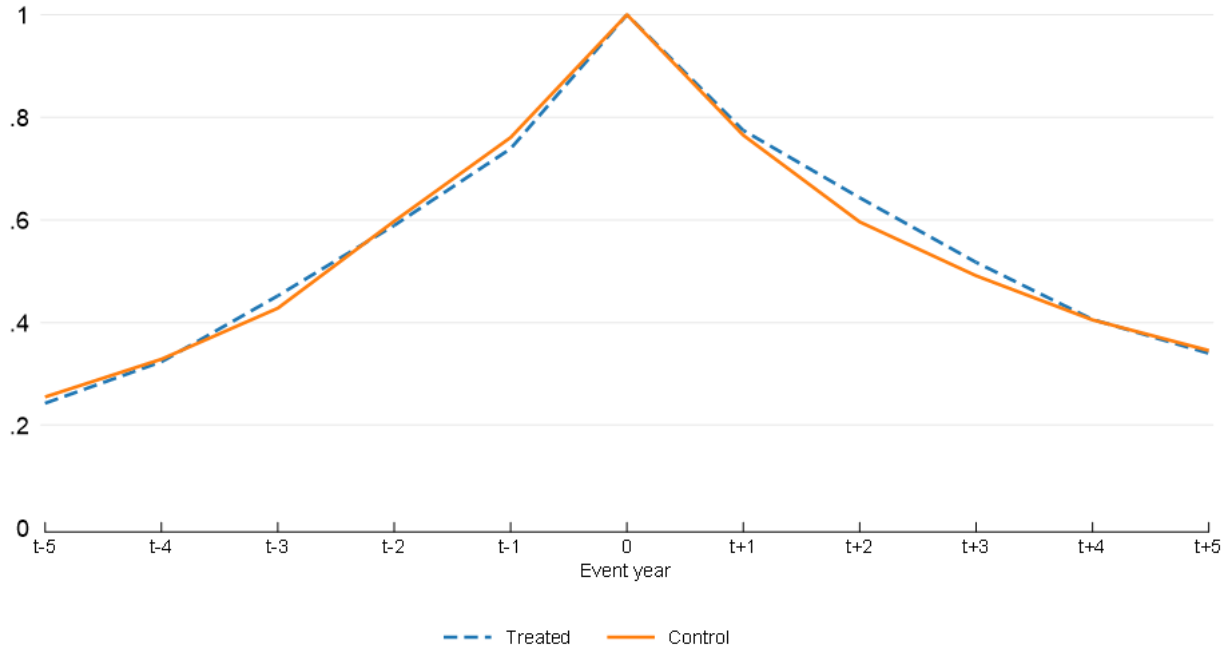


Figure 3: Job mobility in treated and control firms (1998–2013)

Note: This figure displays the descriptive statistics about job mobility for workers who are present at a treated or control firm in the year of an acquisition (event time = 0). It shows the share of those workers who are employed in the same firm up to five years before the acquisition and remaining in the same firm up to five years after the acquisition. For instance, around 75 percent of the workers in the year of a takeover worked in the same firm one year before (event time = -1). Treated firms are Swedish local firms that are acquired by foreign owners over the period 1998-2013. Control firms are Swedish local firms that are never acquired by foreign owners over the same period.

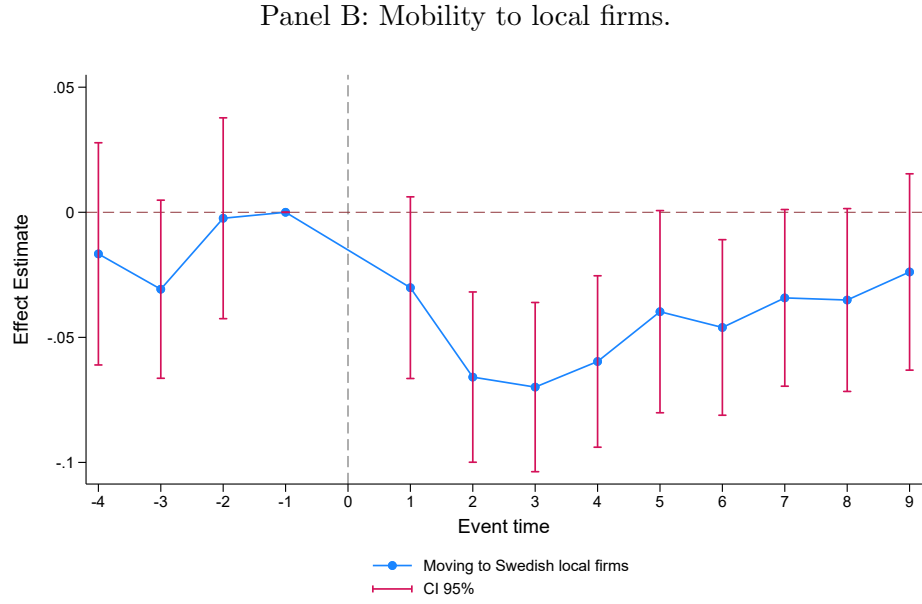
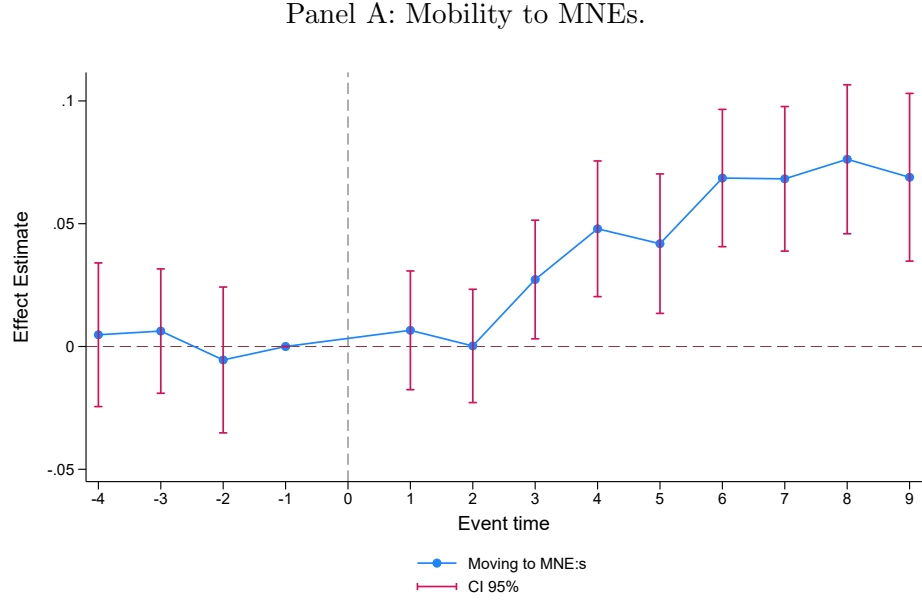
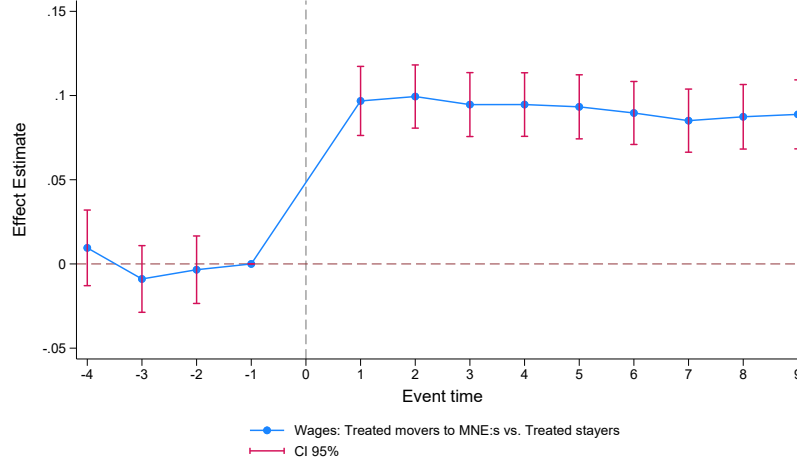


Figure 4: Event study. The effect of foreign acquisitions on job mobility.

Note: The figures show the yearly difference-in-differences estimates of β_j using equation (2), relative to one year before foreign acquisitions (event time $j = -1$). The vertical bars show the 95% confidence intervals. Controls for worker fixed effects, time-varying worker and firm characteristics, and year fixed effects are included. For details on equation (2), see Section 2.2.

Panel A. Comparison between movers to MNEs with stayers at acquired firms



Panel B. Comparison between movers to local firms with stayers at acquired firms

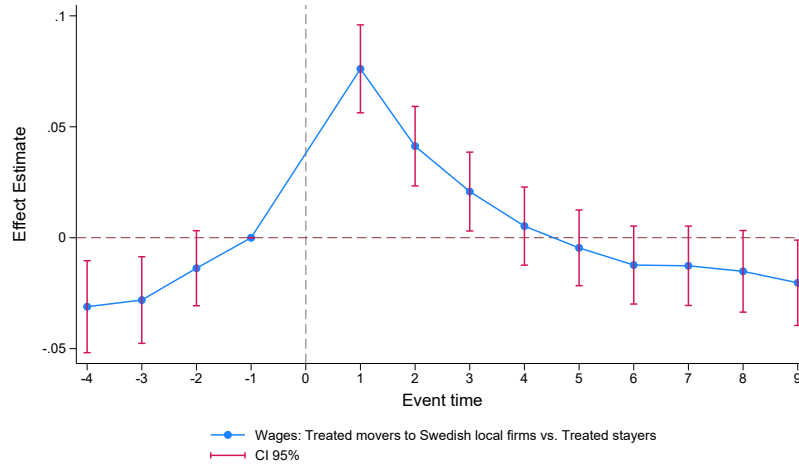


Figure 5: Event study. The wage effect of moving to other firms.

Note: The figures show the yearly difference-in-differences estimates of α_j using equation (5), relative to one year before foreign acquisitions (event time $j = -1$). Panel A compares treated movers to MNEs with treated stayers in their wage growth. Panel B compares treated movers to local firms with treated stayers in their wage growth. The treated workers are those who are present in treated firms at the time of foreign acquisitions. The vertical bars show the 95% confidence intervals. Controls for worker fixed effects, time-varying worker and firm characteristics, and year fixed effects are included. For details on equation (5), see Section 4.

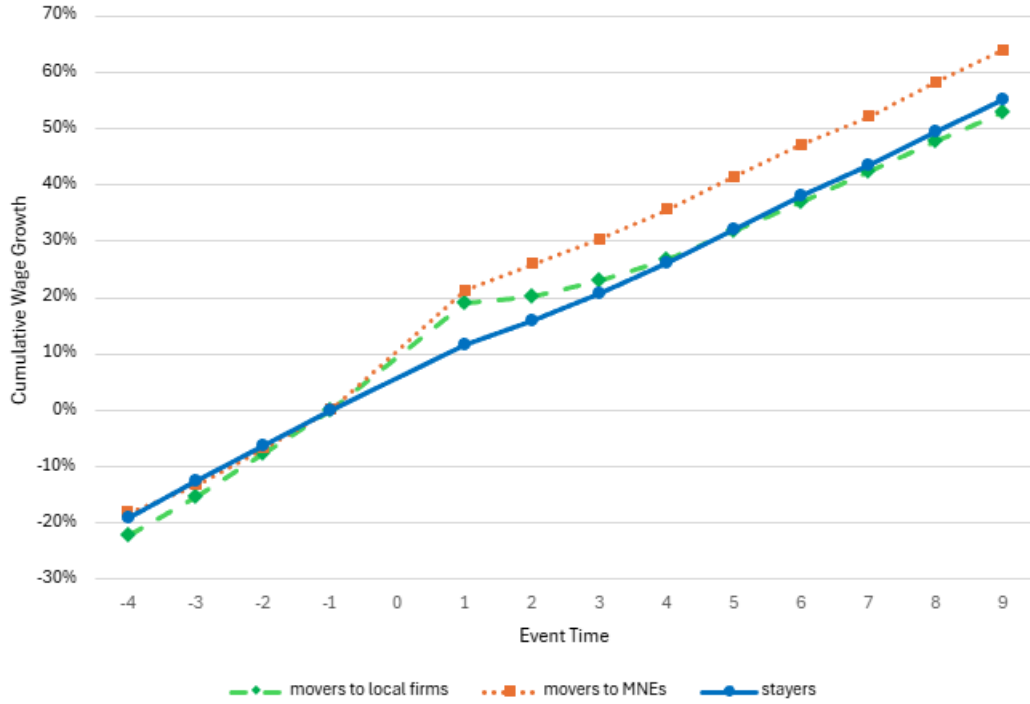
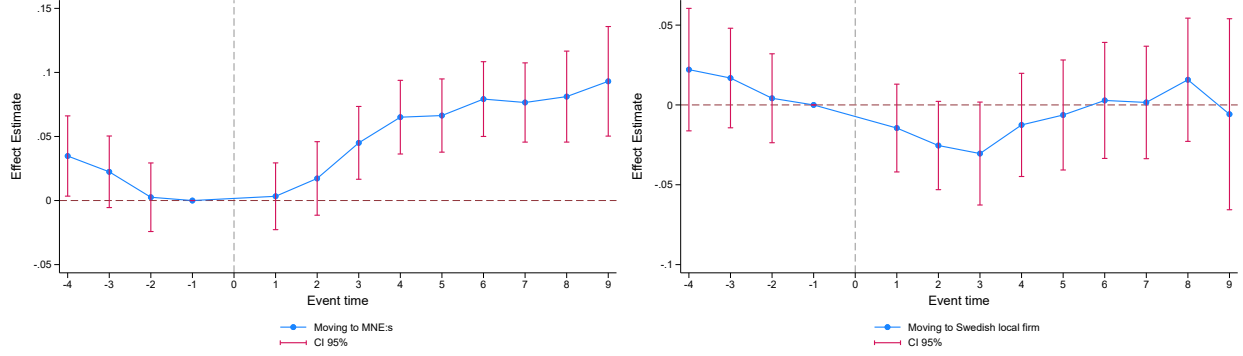


Figure 6: The cumulative wage growth for treated workers.

Note: This figure plots the cumulative wage growth rates for treated workers, with one year before foreign acquisitions (event time $j = -1$) as the base year. Treated workers are those who are present in acquired firms at the time of acquisitions. The wage growth rates are computed using the equation (5) estimates of α_j^0 for stayers and $\alpha_j^0 + \alpha_j$ for movers. Controls for worker fixed effects, time-varying worker and firm characteristics, and year fixed effects are included. For details on equation (5), see Section 4.

Panel A. Mobility of high-skilled occupations (managers, professionals, technicians)



Panel B. Mobility of low-skilled occupations (clerks and operators)

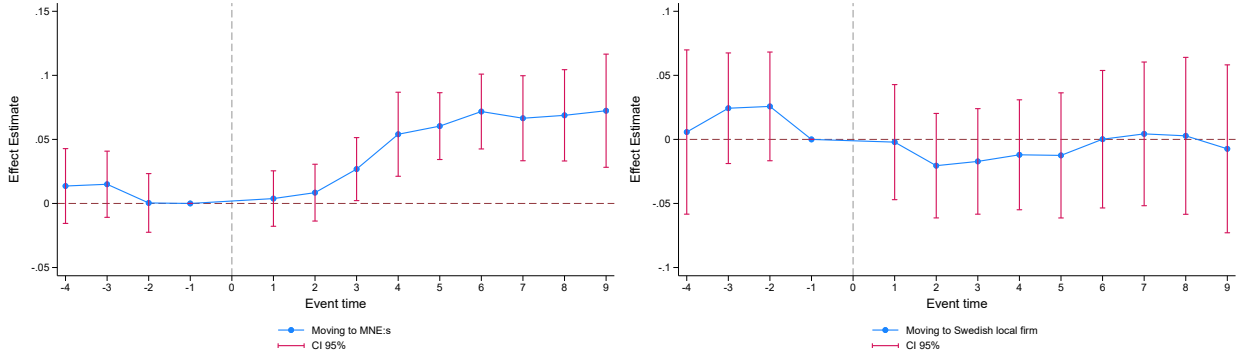


Figure 7: Event study. The effect of foreign acquisitions on job mobility of different occupations.

Note: The figures show the yearly difference-in-differences estimates of β_j using equation (2), relative to one year before foreign acquisitions (event time $j = -1$). The figures are separated by high-skilled occupations (panel A) and low-skilled occupations (panel B). The vertical bars show the 95% confidence intervals. Controls for worker fixed effects, time-varying worker and firm characteristics, and year fixed effects are included. For details on equation (2), see Section 2.2.

Tables

Table 1: Summary statistics for matched sample (pooled 1998–2013)

	Treated firms	Control firms	Difference in means	p-value
<i>One year lag</i>				
Capital intensity	0.270	0.307	-0.037	0.861
Log employment	3.347	3.334	0.013	0.254
Profits	4,559	4,277	282	0.394
Profits/Sales	-2.545	0.018	-2.564	0.838
Sales	124,074	124,892	-818	0.523
Share of high-skilled workers	0.354	0.362	-0.008	0.949
Share of low-skilled workers	0.137	0.137	0.001	0.423
Value added	34,836	34,514	323	0.463
<i>Two years lag</i>				
Capital intensity	0.287	0.339	-0.052	0.874
Log employment	3.212	3.205	0.007	0.352
Profits	3,865	10,369	-6,503	0.877
Profits/Sales	-0.159	0.072	-0.231	0.934
Sales	114,555	113,325	1,229	0.463
Share of high-skilled workers	0.350	0.358	-0.008	0.931
Share of low-skilled workers	0.142	0.143	-0.001	0.569
Value added	31,545	31,873	-328	0.541
<i>One year difference</i>				
Capital intensity	0.007	-0.005	0.012	0.229
Log employment	0.083	0.085	-0.002	0.583
Profits	-247	4,361	-4,609	0.904
Profits/Sales	2.527	-0.024	2.550	0.162
Sales	10,534	6,971	3,562	0.035
Share of high-skilled workers	0.003	0.003	0.000	0.443
Share of low-skilled workers	-0.004	-0.005	0.002	0.068
Value added	2,933	3,878	-944	0.729

Note: This table reports the mean statistics of firm characteristics for treated and control firms, with all years pooled. Treated firms are Swedish local firms that are acquired by foreign owners over the period 1998–2013. Control firms are Swedish local firms that are never acquired by foreign owners over the same period. Capital intensity is defined as capital stocks per employee. High-skilled workers are those with at least undergraduate college education. Low-skilled workers are those with at most 9 years of compulsory schooling. Capital stocks, profits, sales, and value added are in 1,000 SEK. See Section 2.3 for the details about how the group of control firms is constructed.

Table 2: The share of workers who leave to different firm types (1998-2013)

	To MNEs			To local firms		
	Treated (1)	Control (2)	Treated – Control (3)	Treated (4)	Control (5)	Treated – Control (6)
$j = -5$	0.085	0.076	0.009	0.197	0.195	0.002
$j = -3$	0.085	0.072	0.013	0.185	0.188	-0.003
$j = -1$	0.042	0.039	0.003	0.095	0.089	0.006
$j = 1$	0.047	0.037	0.010	0.074	0.098	-0.024
$j = 3$	0.113	0.093	0.020	0.152	0.209	-0.057
$j = 5$	0.142	0.118	0.024	0.197	0.217	-0.020

Note: This table presents the cumulative share of workers who move to MNEs or local firms before and after an acquisition, where the denominator is the total number of workers who are employed in a treated or control firm at the time of takeovers ($j=0$). Treated firms are Swedish local firms that are acquired by foreign owners over the period 1998-2013. Control firms are Swedish local firms that are never acquired by foreign owners over the same period. In the pre-acquisition period, movers at event time j include all workers who have moved to other firms between the j th year and the first year before the acquisition. In the post-acquisition period, movers at event time j include all workers who have moved to other firms between the first and the j th years after the acquisition. Destination firm types include MNEs (columns 1-3) and local firms (columns 4-6). MNEs consist of both foreign-owned firms and Swedish MNEs. Local firms are Swedish firms with no foreign affiliates. Columns 3 and 6 report the cumulative share difference between treated and control firms for movement to MNEs and to local firms, respectively.

Table 3: Wage growth for stayers, movers to MNEs, and movers to local firms (in percent)

	Stayers		Movers to MNEs		Movers to local firms	
	Treated	Control	Treated	Control	Treated	Control
	(1)	(2)	(3)	(4)	(5)	(6)
$j = 1$	-1	2	16	15	9	4
$j = 3$	13	6	18	17	10	-4
$j = 5$	16	6	25	17	12	16
$j = 9$	29	23	37	23	17	27

Note: This table presents the cumulative wage growth rate separately for stayers, movers to MNEs and movers to local firms, where all the workers are employed at a treated or control firm in the year of an acquisition ($j = 0$) and all the wages are relative to the year before the acquisition. Treated firms are Swedish local firms that are acquired by foreign owners over the period 1998-2013. Control firms are Swedish local firms that are never acquired by foreign owners over the same period. Stayers at event time j are workers who remain employed at the same firm from the year of an acquisition to the j th year post-acquisition. Movers at event time j include all the workers who have moved to other firms between the first and the j th years post-acquisition. Destination firm types include MNEs and local firms, where MNEs consist of both foreign-owned firms and Swedish MNEs, and local firms are Swedish firms with no foreign affiliates.

Table 4A: Estimating the effect of foreign acquisitions on job mobility

	To MNEs		To local firms	
	(1)	(2)	(3)	(4)
After	-0.085*** (0.006)	-0.095*** (0.007)	-0.088*** (0.015)	-0.069*** (0.010)
DiD	0.037*** (0.006)	0.036*** (0.008)	-0.041*** (0.015)	-0.038*** (0.011)
Experience square		-0.000*** (0.000)		0.000* (0.000)
Years of schooling		-0.000 (0.001)		0.004** (0.002)
Log employment		0.054*** (0.002)		-0.121*** (0.002)
Share of high-skilled		-0.005 (0.010)		-0.213*** (0.007)
Capital intensity		0.000 (0.000)		0.000 (0.000)
Observations	3,134,949	3,134,949	3,134,949	3,134,949
R-squared	0.033	0.096	0.031	0.234
Worker FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Note: This table reports the results from estimating equation (1). The sample period covers up to four years before an acquisition and up to nine years after the acquisition. The dependent variable takes the value one if workers in a treated or control firm move to MNEs (columns 1-2) or to local firms (columns 3-4), and zero otherwise. *After* takes the value one in the year of an acquisition and all years after. *DiD* is $After \times Treated$, where *Treated* takes the value one for workers who are employed by firms that are acquired at some point and the value zero for workers in control firms. Experience square and Years of schooling are time-varying worker characteristics. Log employment, Share of high-skilled workers, and Capital intensity are time-varying firm characteristics. All regressions control for worker fixed effects and year fixed effects. For details on equation (1), see Section 2.2. In parentheses are standard errors clustered at the firm-year level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4B: Robustness to alternative constructions of the control group

	PSM within industry-year cells				Entropy balancing	
	Broad industry categories		1-digit SNI2002		To MNEs (5)	To local firms (6)
	To MNEs (1)	To local firms (2)	To MNEs (3)	To local firms (4)		
DiD	0.016** (0.008)	-0.030*** (0.010)	0.041*** (0.007)	0.021 (0.014)	0.020*** (0.003)	-0.036*** (0.009)
Observations	2,733,029	2,733,029	2,859,683	2,859,683	96,362,371	96,362,371
R-squared	0.123	0.198	0.104	0.237	0.464	0.623
Control variables	YES	YES	YES	YES	YES	YES
Worker FE	YES	YES	YES	YES		
Worker-Cohort FE					YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Note: This table examines the robustness of the baseline estimates of equation (1) to alternative constructions of the control group. In columns 1-4, we adopt an alternative propensity score matching (PSM) procedure by restricting treatment and control firms to be in the same industry-year. In columns 1-2, industries are grouped into three broad industry categories based on the SNI2002 (Swedish Standard Industrial Classification, 2002): (i) manufacturing and resource-based industries; (ii) business and market-oriented services; and (iii) construction and social/personal services. In columns 3-4, industries are classified based on the first digit of the SNI2002 classification. To avoid overly narrow cells, we recode these one-digit categories into a set of industry groups based on their economic similarity and size in the data. In columns 5-6, we use the entropy balancing method to construct a reweighted control group for each cohort of firms acquired in a given year. The sample period covers up to four years before an acquisition and up to nine years after the acquisition. The dependent variable takes the value one if workers in a treated or control firm move to MNEs (columns 1, 3, 5) or to local firms (columns 2, 4, 6), and zero otherwise. *DiD* is $After \times Treated$, where *After* takes the value one in the year of an acquisition and all years after, and *Treated* takes the value one for workers who are employed by firms that are acquired at some point and the value zero for workers in control firms. All regressions control for time-varying worker and firm characteristics and year fixed effects. See Table 4A for the included control variables. Worker fixed effects are included in columns 1-4. Worker-cohort fixed effects are included in columns 5-6. For more details, see Section 3.2. In parentheses are standard errors clustered at the firm-year level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Estimating the effect of mobility on wages for treated workers

	Movers to MNEs compared to treated stayers		Movers to local firms compared to treated stayers	
	(1)	(2)	(3)	(4)
After	0.013*** (0.003)	0.003 (0.003)	0.017*** (0.003)	0.006 (0.004)
Mover	-0.114*** (0.005)	-0.076*** (0.005)	-0.188*** (0.006)	-0.091*** (0.005)
DiD	0.164*** (0.006)	0.095*** (0.005)	0.096*** (0.006)	0.030*** (0.005)
Experience square		-0.001*** (0.000)		-0.001*** (0.000)
Years of schooling		0.053*** (0.004)		0.066*** (0.004)
Log employment		0.001 (0.001)		0.036*** (0.001)
Share of high-skilled		0.114*** (0.009)		0.114*** (0.009)
Capital intensity		0.000*** (0.000)		0.001*** (0.000)
Observations	1,268,225	1,268,225	1,372,722	1,372,722
R-squared	0.048	0.066	0.035	0.058
Worker FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Note: This table reports the results from estimating equation (4). The sample period covers up to four years before an acquisition and up to nine years after the acquisition. Columns 1-2 compare the wage growth of treated movers to MNEs with that of treated stayers. Columns 3-4 compare the wage growth of treated movers to local firms with that of treated stayers. Treated workers are those who are present in acquired firms at the time of acquisitions. *After* takes the value one in the year of an acquisition and all years after. *Mover* takes the value one if a treated worker moves to another firm in an event year. *DiD* is $After \times Mover$. All specifications control for worker fixed effects, time-varying worker and firm characteristics, and year fixed effects. Female, Experience, Experience square and Years of schooling are worker characteristics. Log employment, Share of high-skilled workers, and Capital intensity are firm characteristics. For details on equation (4), see Section 4. In parentheses are standard errors clustered at the firm-year level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6A: The effect of foreign acquisitions on job mobility to MNEs, separately by the source country of foreign acquirers

	Norway (1)	Denmark (2)	US (3)	UK (4)	Germany (5)	Finland (6)	France (7)	EU-15 (8)	Nordic (9)	Tax havens (10)	English speaking (11)
<i>Panel A: Move to MNEs</i>											
DiD	0.088*** (0.013)	-0.001 (0.017)	0.037** (0.017)	0.058*** (0.015)	-0.041 (0.026)	0.045* (0.023)	0.001 (0.016)	0.015 (0.011)	0.051*** (0.011)	0.053*** (0.009)	0.049*** (0.011)
Observations	383 237	312 262	398 178	330 273	369 194	199 321	117 884	1 758 330	894 820	448 437	744 157
R-squared	0.156	0.092	0.077	0.092	0.110	0.258	0.132	0.108	0.144	0.106	0.083
<i>Panel B: Move to foreign MNEs</i>											
DiD	0.094*** (0.012)	0.022* (0.012)	0.052*** (0.009)	0.047*** (0.010)	0.037** (0.015)	0.097*** (0.013)	0.010 (0.011)	0.038*** (0.006)	0.073*** (0.008)	0.045*** (0.006)	0.048*** (0.007)
Observations	383 237	312 262	398 178	330 273	369 194	199 321	117 884	1 758 330	894 820	448 437	744 157
R-squared	0.097	0.056	0.044	0.060	0.072	0.140	0.077	0.061	0.081	0.059	0.050
<i>Panel C: Move to MNEs from the same source country</i>											
DiD	0.067*** (0.008)	0.016** (0.007)	0.035*** (0.007)	0.011*** (0.002)	0.081*** (0.010)	0.074*** (0.008)	0.042*** (0.006)	0.037*** (0.005)	0.059*** (0.005)	0.032*** (0.004)	0.026*** (0.004)
Observations	383 237	312 262	398 178	330 273	369 194	199 321	117 884	1 758 330	894 820	448 437	744 157
R-squared	0.040	0.010	0.025	0.008	0.063	0.137	0.049	0.041	0.035	0.021	0.023

Note: This table reports the estimated difference-in-differences coefficients from estimating equation (1) separately by the source country of foreign acquirers. The sample period covers up to four years before an acquisition and up to nine years after the acquisition. The dependent variable is mobility to MNEs (both Swedish- and foreign-owned) in panel A and mobility to foreign-owned MNEs in panel B. Panel C shows results where the dependent variable is mobility to an MNE from the same country as the source country of foreign acquirers. In column 8, EU-15 countries include Belgium, Denmark, Germany, Finland, France, Greece, Ireland, Italy, Luxembourg, the Netherlands, Austria, Portugal, Spain, and the UK. In column 9, Nordic countries exclude Sweden. In column 10, tax havens include the Netherlands, Luxembourg, and Switzerland. In column 11, English-speaking countries include the UK, the US, Ireland, Australia, and New Zealand. All specifications control for worker fixed effects, time-varying worker and firm characteristics, and year fixed effects. See Table 4A for the included control variables. In parentheses are standard errors clustered at the firm-year level. *** p<0.01, ** p<0.05, * p<0.1.

Table 6B: The effect of foreign acquisitions on job mobility to MNEs from the same source country

	Norway	Denmark	US	UK	Germany	Finland	France	EU-15	Nordic	Tax havens	English speaking
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
<i>Panel A: Move to MNEs from the same source country (Baseline)</i>											
DiD	0.067*** (0.008)	0.016** (0.007)	0.035*** (0.007)	0.011*** (0.002)	0.081*** (0.010)	0.074*** (0.008)	0.042*** (0.006)	0.037*** (0.005)	0.059*** (0.005)	0.032*** (0.004)	0.026*** (0.004)
Observations	383 237	312 262	398 178	330 273	369 194	199 321	117 884	1 758 330	894 820	448 437	744 157
R-squared	0.040	0.010	0.025	0.008	0.063	0.137	0.049	0.041	0.035	0.021	0.023
<i>Panel B: Move to firms outside the same business group</i>											
DiD	0.036*** (0.004)	0.015*** (0.006)	0.021*** (0.004)	0.008*** (0.001)	0.059*** (0.009)	0.065*** (0.007)	0.040*** (0.006)	0.030*** (0.005)	0.044*** (0.004)	0.022*** (0.003)	0.017*** (0.003)
Observations	383 237	312 262	398 178	330 273	369 194	199 321	117 884	1 758 330	894 820	448 437	744 157
R-squared	0.022	0.011	0.019	0.005	0.045	0.135	0.047	0.037	0.029	0.020	0.020
<i>Panel C: Move to firms in the same business group</i>											
DiD	0.030*** (0.007)	0.001 (0.004)	0.015** (0.006)	0.003* (0.002)	0.022*** (0.004)	0.009*** (0.002)	0.002 (0.002)	0.007*** (0.001)	0.015*** (0.004)	0.010*** (0.003)	0.009*** (0.003)
Observations	362 525	290 219	375 336	307 902	353 819	189 762	111 634	1 656 604	842 506	420 692	697 642
R-squared	0.028	0.007	0.011	0.007	0.025	0.008	0.006	0.005	0.009	0.008	0.006
<i>Panel D: Excluding multi-firm groups</i>											
DiD	0.034*** (0.005)	0.026*** (0.009)	0.008 (0.007)	0.016*** (0.004)	0.013** (0.006)	0.055*** (0.012)	0.058*** (0.022)	0.026*** (0.004)	0.039*** (0.005)	0.013*** (0.003)	0.019*** (0.004)
Observations	123 290	95 679	95 331	87 611	125 401	62 227	32 605	545 654	281 196	149 044	187 809
R-squared	0.047	0.032	0.014	0.022	0.008	0.033	0.114	0.023	0.029	0.008	0.017
<i>Panel E: Alternative control workers</i>											
DiD	0.064*** (0.009)	0.012* (0.006)	0.034*** (0.006)	0.013*** (0.003)	0.093*** (0.010)	0.083*** (0.010)	0.043*** (0.006)	0.034*** (0.006)	0.054*** (0.006)	0.026*** (0.004)	0.026*** (0.004)
Observations	1 509 662	1 509 662	1 509 662	1 509 662	1 509 662	1 509 662	1 509 662	1 509 662	1 509 662	1 509 662	1 509 662
R-squared	0.018	0.004	0.015	0.005	0.033	0.025	0.011	0.052	0.024	0.019	0.018

Note: This table reports the estimated difference-in-differences coefficients from estimating equation (1) separately by the source country of foreign acquirers. The sample period covers up to four years before an acquisition and up to nine years after the acquisition. In Panel A, the dependent variable is mobility to an MNE from the same country as the source country of foreign acquirers. The estimates are carried over from Panel C of Table 6A. Panels B and C separate mobility to MNEs from the same source country into mobility to firms outside the same business group and within the same group, respectively. Panel D excludes firms that are part of multi-firm business groups. Panel E restricts the sample to treated workers and uses those whose firms are acquired by foreign owners from other countries as controls. In column 8, EU-15 countries include Belgium, Denmark, Germany, Finland, France, Greece, Ireland, Italy, Luxembourg, the Netherlands, Austria, Portugal, Spain, and the UK. In column 9, Nordic countries exclude Sweden. In column 10, tax havens include the Netherlands, Luxembourg, and Switzerland. In column 11, English-speaking countries include the UK, the US, Ireland, Australia, and New Zealand. All specifications control for worker fixed effects, time-varying worker and firm characteristics, and year fixed effects. See Table 4A for the included control variables. In parentheses are standard errors clustered at the firm-year level. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: The effect of foreign acquisitions on job mobility to foreign MNEs, separately by the source country of the top five foreign investors

<i>Move to:</i>	Norway (1)	Denmark (2)	US (3)	UK (4)	Germany (5)	Finland (6)	France (7)	EU-15 (8)	Nordic (9)	Tax havens (10)	English speaking (11)
<i>Panel A: Acquisitions by Norway</i>											
DiD	0.067*** (0.008)	0.002** (0.001)	0.001 (0.001)	-0.000 (0.001)	0.001 (0.001)	0.002 (0.001)	0.001** (0.001)	0.027*** (0.009)	0.070*** (0.008)	0.020** (0.009)	0.001 (0.002)
R-squared	0.040	0.002	0.011	0.004	0.005	0.006	0.004	0.061	0.041	0.056	0.014
<i>Panel B: Acquisitions by Denmark</i>											
DiD	0.001 (0.001)	0.016** (0.007)	0.002 (0.002)	-0.001 (0.003)	0.003** (0.001)	0.007* (0.004)	-0.007** (0.003)	0.023** (0.010)	0.024*** (0.008)	0.006*** (0.002)	0.001 (0.004)
R-squared	0.001	0.010	0.008	0.005	0.003	0.012	0.005	0.031	0.016	0.013	0.011
<i>Panel C: Acquisitions by the US</i>											
DiD	0.003*** (0.001)	0.004*** (0.001)	0.035*** (0.007)	0.002 (0.001)	0.004*** (0.001)	-0.000 (0.001)	0.002** (0.001)	0.008* (0.005)	0.007*** (0.002)	-0.004 (0.003)	0.038*** (0.007)
R-squared	0.003	0.002	0.025	0.004	0.004	0.003	0.003	0.018	0.006	0.008	0.028
<i>Panel D: Acquisitions by the UK</i>											
DiD	0.002** (0.001)	0.007** (0.003)	0.008** (0.003)	0.011*** (0.002)	0.004*** (0.001)	-0.002** (0.001)	-0.002 (0.001)	0.038*** (0.008)	0.007** (0.003)	0.011*** (0.004)	0.019*** (0.004)
R-squared	0.003	0.005	0.017	0.008	0.007	0.003	0.004	0.038	0.007	0.017	0.021
<i>Panel E: Acquisitions by Germany</i>											
DiD	-0.002 (0.001)	0.000 (0.001)	-0.005*** (0.002)	-0.003*** (0.001)	0.081*** (0.010)	0.002 (0.001)	-0.013*** (0.004)	0.050*** (0.013)	0.001 (0.002)	-0.019*** (0.005)	-0.009*** (0.002)
R-squared	0.004	0.003	0.008	0.003	0.063	0.002	0.017	0.061	0.006	0.018	0.009

Note: This table reports the estimated difference-in-differences coefficients from estimating equation (1) separately by the source country of the top five foreign investors and by worker movements to foreign MNEs with different countries of ownership. The sample period covers up to four years before an acquisition and up to nine years after the acquisition. Panels A-E display results for acquisitions by Norway, Denmark, the US, the UK, and Germany, respectively. In column 8, EU-15 countries include Belgium, Denmark, Germany, Finland, France, Greece, Ireland, Italy, Luxembourg, the Netherlands, Austria, Portugal, Spain, and the UK. In column 9, Nordic countries include Sweden. In column 10, tax havens include the Netherlands, Luxembourg, and Switzerland. In column 11, English-speaking countries include the UK, the US, Ireland, Australia, and New Zealand. All specifications control for worker fixed effects, time-varying worker and firm characteristics, and year fixed effects. See Table 4A for the included control variables. In parentheses are standard errors clustered at the firm-year level. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: The effect of foreign acquisitions on trade at the firm-level

	Total trade (1)	Export (2)	Import (3)
DiD	5,155* (2,671)	281 (1,134)	4,873** (2,193)
Observations	57,113	57,113	57,113
R-squared	0.014	0.015	0.007
Control variables	YES	YES	YES
Firm FE	YES	YES	YES
Year FE	YES	YES	YES

Note: This table reports the estimated difference-in-differences coefficients from equation (6). The sample period covers up to four years before an acquisition and up to nine years after the acquisition. The dependent variable is the total value of trade, the value of exports, and the value of imports in columns 1–3, respectively. All specifications control for firm fixed effects, time-varying firm characteristics, and year fixed effects. For details on equation (6), see Section 6.1. In parentheses are standard errors clustered at the firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9: The effect of foreign acquisitions on trade at the firm-level, separately by source countries of acquirers and by trade destination countries

	Norway	Denmark	US	UK	Germany	Finland	France	EU-15	Nordic	Tax havens	English speaking
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
<i>Panel A: Dep. Var. = trade with source country as share of total firm trade</i>											
DiD	0.068*** (0.017)	0.063*** (0.014)	0.040* (0.021)	0.016* (0.008)	0.039** (0.016)	0.021 (0.015)	0.026 (0.016)	0.015* (0.008)	0.026*** (0.009)	0.010 (0.012)	0.029** (0.014)
Observations	8 984	6 436	5 397	4 956	4 610	4 521	1 890	27 740	29 474	5 017	10 981
R-squared	0.013	0.033	0.012	0.008	0.022	0.014	0.023	0.026	0.008	0.007	0.008
<i>Panel B: Acquisition by Norway (Dep. Var. = trade with specific countries as share of total trade)</i>											
DiD	0.068*** (0.017)	0.009 (0.006)	-0.022** (0.010)	0.004 (0.003)	0.005 (0.005)	-0.002 (0.005)	-0.003 (0.002)	0.018 (0.012)	0.075*** (0.018)	0.006 (0.006)	-0.022** (0.011)
<i>Panel C: Acquisition by Denmark (Dep. Var. = trade with specific countries as share of total trade)</i>											
DiD	-0.012 (0.019)	0.063*** (0.014)	-0.015 (0.013)	-0.010* (0.005)	-0.009 (0.007)	-0.014** (0.007)	0.001 (0.004)	0.021 (0.018)	0.037* (0.021)	0.003 (0.008)	-0.028** (0.014)
<i>Panel D: Acquisition by the US (Dep. Var. = trade with specific countries as share of total trade)</i>											
DiD	0.008 (0.019)	0.003 (0.006)	0.040* (0.021)	0.009 (0.007)	-0.004 (0.009)	-0.008 (0.006)	-0.003 (0.003)	-0.002 (0.018)	0.003 (0.020)	0.004 (0.011)	0.046** (0.022)
<i>Panel E: Acquisition by the UK (Dep. Var. = trade with specific countries as share of total trade)</i>											
DiD	0.008 (0.022)	0.004 (0.007)	0.002 (0.017)	0.016* (0.008)	-0.008 (0.009)	-0.006 (0.007)	-0.005 (0.003)	-0.003 (0.018)	0.006 (0.022)	-0.016 (0.011)	0.018 (0.019)
<i>Panel F: Acquisition by Germany (Dep. Var. = trade with specific countries as share of total trade)</i>											
DiD	-0.008 (0.020)	-0.013 (0.010)	0.007 (0.016)	-0.003 (0.006)	0.039** (0.016)	0.004 (0.007)	-0.003 (0.007)	0.015 (0.023)	-0.017 (0.022)	0.005 (0.012)	0.005 (0.017)

Note: This table reports the estimated difference-in-differences coefficients from estimating equation (6) on firm-level trade shares separately by source countries of foreign acquirers and trade destination countries. The sample period covers up to four years before an acquisition and up to nine years after the acquisition. The dependent variable in Panel A is the share of trade with the same country as the source country of foreign acquirers. Panels B-F show regression results for the top five foreign investors. Each column reports the results for trade with a specific country. For instance, the estimate of 0.009 in Panel B, column 2 shows the impact of acquisitions from Norway on trade with Denmark as a share of total firm trade. In column 8, EU-15 countries include Belgium, Denmark, Germany, Finland, France, Greece, Ireland, Italy, Luxembourg, the Netherlands, Austria, Portugal, Spain, and the UK. In column 9, Nordic countries exclude Sweden. In column 10, tax havens include the Netherlands, Luxembourg, and Switzerland. In column 11, English-speaking countries include the UK, the US, Ireland, Australia, and New Zealand. All specifications control for firm fixed effects, time-varying firm characteristics, and year fixed effects. For details on equation (6), see Section 6.1. In parentheses are standard errors clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Table 10A: The effect of foreign acquisitions on job mobility to MNEs, separately by higher- and lower-tech source countries

	ICT capital / Total capital		Software capital / Total capital	
	Higher tech	Lower tech	Higher tech	Lower tech
	(1)	(2)	(3)	(4)
<i>Panel A: Move to MNEs</i>				
DiD	0.054*** (0.011)	0.012 (0.012)	0.056*** (0.020)	0.011 (0.011)
Observations	312,299	1,524,770	337,319	1,495,518
R-squared	0.153	0.097	0.096	0.110
<i>Panel B: Move to foreign MNEs</i>				
DiD	0.046*** (0.008)	0.041*** (0.007)	0.048*** (0.012)	0.040*** (0.007)
Observations	312,299	1,524,770	337,319	1,495,518
R-squared	0.078	0.054	0.056	0.059
<i>Panel C: Move to higher-tech MNEs</i>				
DiD	0.028*** (0.005)	0.008*** (0.002)	0.031*** (0.008)	0.003 (0.003)
Observations	312,299	1,524,770	337,319	1,495,518
R-squared	0.033	0.013	0.033	0.010
<i>Panel D: Move to lower-tech MNEs</i>				
DiD	0.018*** (0.006)	0.031*** (0.005)	0.011** (0.005)	0.035*** (0.004)
Observations	312,299	1,524,770	337,319	1,495,518
R-squared	0.022	0.028	0.017	0.033

Note: This table reports the estimated difference-in-differences coefficients from estimating equation (1) separately by source countries with different technology sophistication. The measures of technology sophistication include ICT capital (expenses on computing and communications equipment) to total capital in columns 1-2, and software capital (expenses on computer software and database) to total capital in columns 3-4. For each industry, higher-tech (lower-tech) countries are those with technology expenditure shares above (below) Sweden's level. Higher-tech MNEs are those owned by higher-tech countries, and lower-tech MNEs are those owned by lower-tech countries. The sample period covers up to four years before an acquisition and up to nine years after the acquisition. All specifications include worker fixed effects, time-varying worker and firm characteristics, and year fixed effects. See Table 4A for the included control variables. In parentheses are standard errors clustered at the firm-year level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 10B: The effect of foreign acquisitions on job mobility to MNEs, separately by higher- and lower-tech source firms

	Software expenditures		Data expenditures	
	Higher-tech (1)	Lower-tech (2)	Higher-tech (3)	Lower-tech (4)
<i>Panel A: Higher-tech > median, lower-tech < median</i>				
DiD	0.020** (0.010)	0.013 (0.012)	0.032*** (0.010)	-0.013 (0.015)
Observations	567,340	179,414	566,735	181,021
R-squared	0.026	0.122	0.027	0.111
Control variables	YES	YES	YES	YES
Worker FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
<i>Panel B: Higher-tech > 0, lower-tech = 0</i>				
DiD	0.017** (0.009)	0.010 (0.017)	0.022*** (0.008)	-0.014 (0.027)
Observations	676,699	70,055	676,850	70,906
R-squared	0.035	0.119	0.034	0.132
Control variables	YES	YES	YES	YES
Worker FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

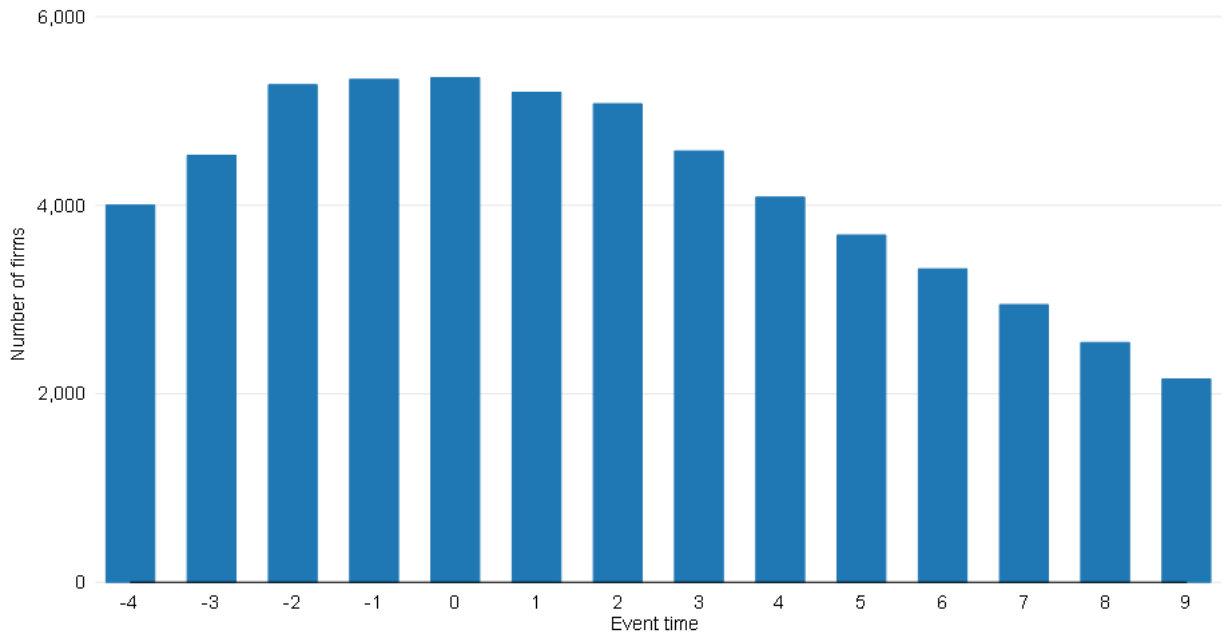
Note: This table reports the estimated difference-in-differences coefficients from estimating equation (1) separately by source firms with different levels of post-acquisition IT expenditures. In panel A, higher-tech (lower-tech) firms are those with expenditures on software or data above (below) the industry median. In panel B, higher-tech firms are those with positive spending on software or data, and lower-tech firms are those without any IT expenditures. The sample period covers up to four years before an acquisition and up to nine years after the acquisition. All specifications include worker fixed effects, time-varying worker and firm characteristics, and year fixed effects. See Table 4A for the included control variables. In parentheses are standard errors clustered at the firm-year level. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: The effect of foreign acquisitions on mobility to MNEs by high- and low-quality management practices

	<i>Management1</i>				<i>Management2</i>			
	> median (1)	< median (2)	> p(75) (3)	< p(25) (4)	> median (5)	< median (6)	> p(75) (7)	< p(25) (8)
<i>Panel A: Move to MNEs</i>								
DiD	0.010 (0.011)	0.066*** (0.009)	0.035*** (0.010)	0.074*** (0.011)	0.009 (0.011)	0.066*** (0.009)	0.025*** (0.011)	0.074*** (0.011)
Observations	1,611,800	1,090,778	761,221	711,878	1,611,544	1,091,034	641,768	712,701
R-squared	0.089	0.141	0.088	0.171	0.088	0.142	0.090	0.171
<i>Panel B: Move to foreign MNEs</i>								
DiD	0.030*** (0.006)	0.074*** (0.006)	0.036*** (0.006)	0.092*** (0.008)	0.030*** (0.006)	0.074*** (0.006)	0.030*** (0.006)	0.092*** (0.008)
Observations	1,611,800	1,090,778	761,221	711,878	1,611,544	1,091,034	641,768	712,701
R-squared	0.051	0.078	0.046	0.093	0.051	0.078	0.047	0.093

Note: This table reports the estimated difference-in-differences coefficients from estimating equation (1) separately by acquisitions from source countries with management practices at different quality levels (high vs. low). Two measures of management practices are calculated, based on data from the World Management Survey (WMS). *Management1* in columns 1-4 is based on all MNEs in the WMS, and *Management2* in columns 5-8 excludes parent firms, i.e., the part of the MNEs located in the source country. Source countries with high (low)-quality management practices are those with a measure above (below) the sample median or above the 75th percentile (below the 25th percentile) of the measure. Each column shows DiD estimates of the effect of acquisitions on job mobility to MNEs separately by source countries with high- or low-quality management practices. The sample period covers up to four years before an acquisition and up to nine years after the acquisition. All specifications include worker fixed effects, time-varying worker and firm characteristics, and year fixed effects. See Table 4A for the included control variables. In parentheses are standard errors clustered at the firm-year level. *** p<0.01, ** p<0.05, * p<0.1.

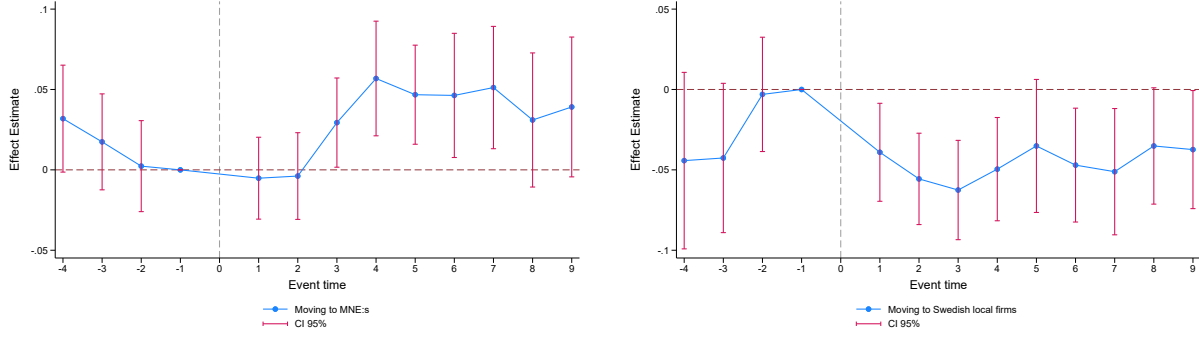
Appendix



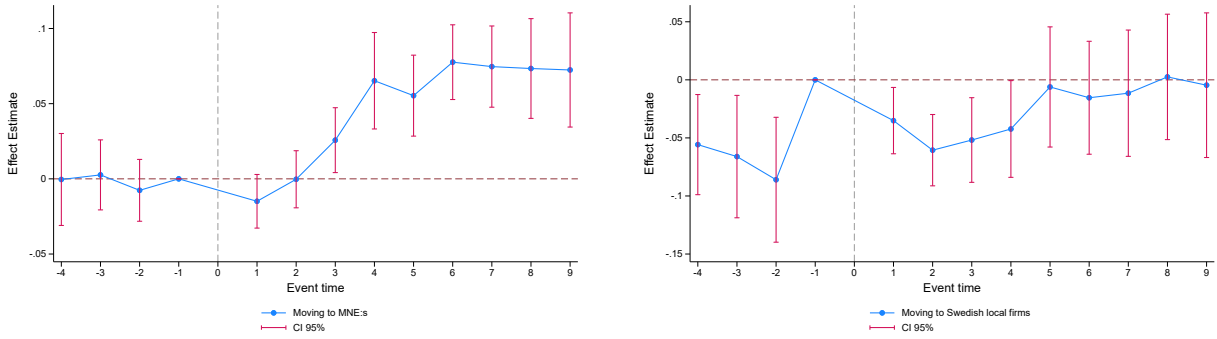
Appendix Figure A1. The number of firms before and after acquisitions.

Note: This figure shows the total number of acquired firms and their twin control firms based on the one-to-one propensity score matching procedure. For details on the matching method, see Section 2.3.

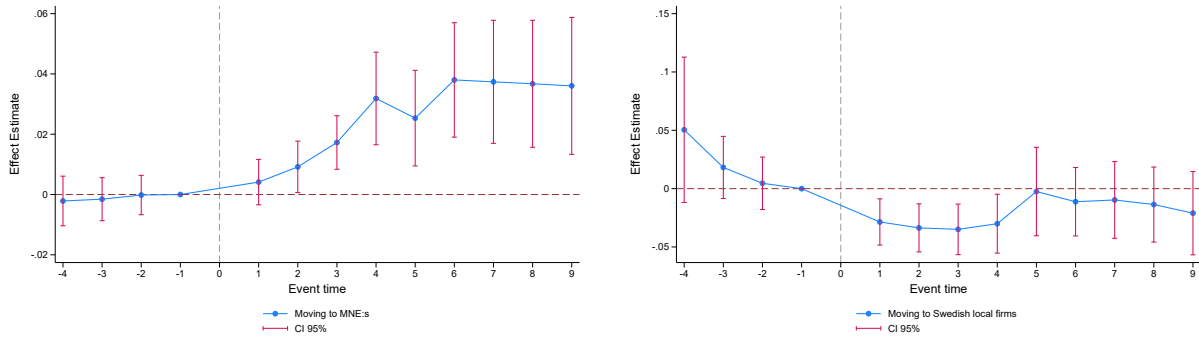
Panel A: PSM within industry-year (industries are grouped into three broad categories of SNI2002).



Panel B: PSM within industry-year (industries are at the 1-digit level of SNI2002).

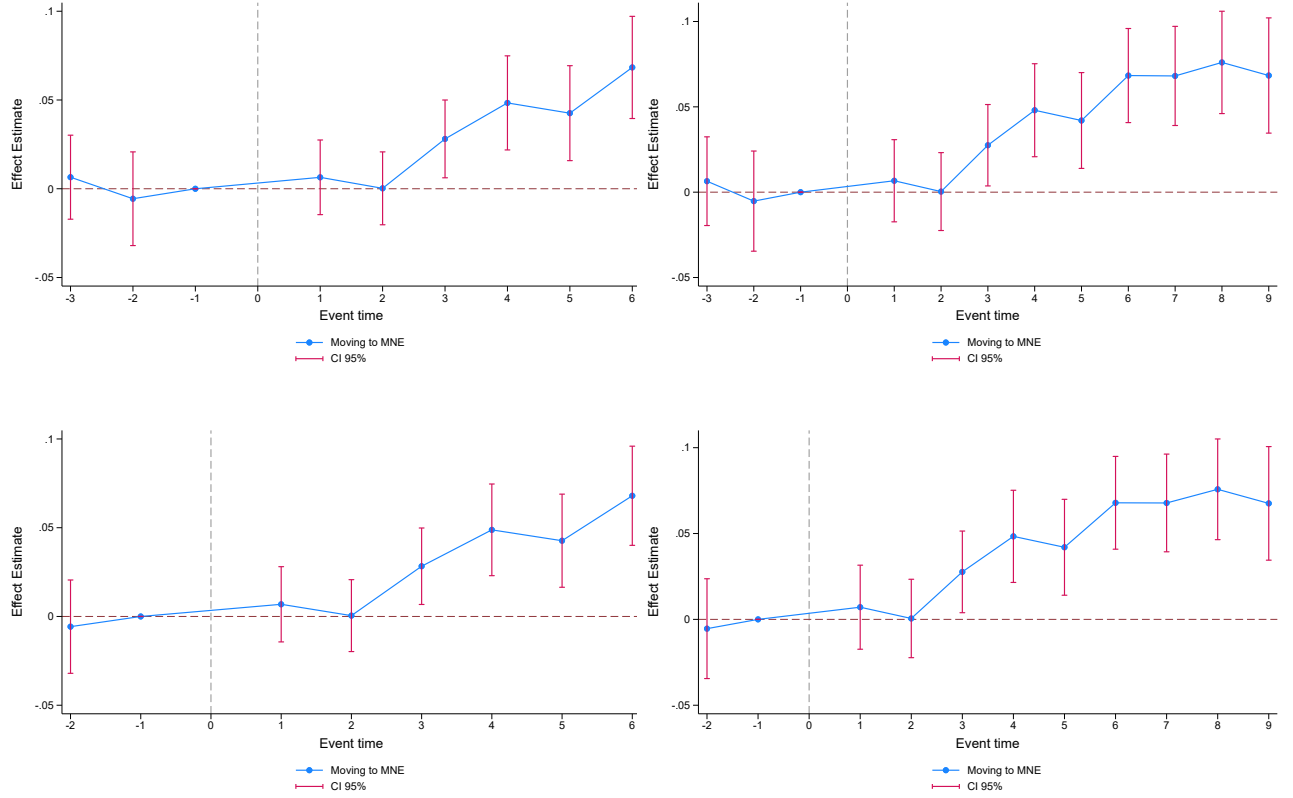


Panel C: Entropy balancing for constructing the reweighted control group.



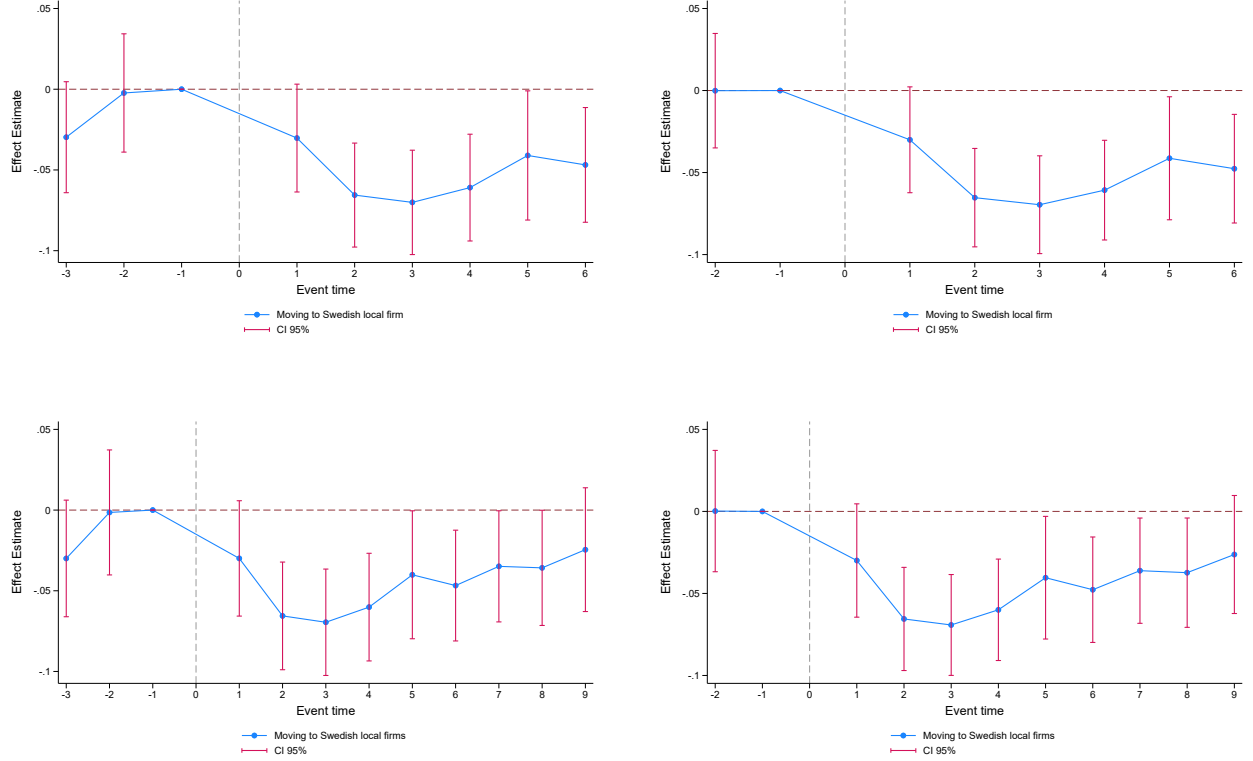
Appendix Figure A2: Event Study. The robustness to alternative constructions of the control group.

Note: The figures show the yearly difference-in-differences estimates of β_j using equation (2), relative to one year before foreign acquisitions (event time $j = -1$). Panels A-B display the estimates based on the control group constructed using alternative propensity score matching specifications that restrict treated and control firms to be in the same industry-year cells. Panel C displays the estimates based on the reweighted control group constructed using the entropy balancing method. The vertical bars show the 95% confidence intervals. Controls for worker fixed effects, time-varying worker and firm characteristics, and year fixed effects are included. For details on equation (2), see Section 2.2. Also see Section 3.2 for more details on the alternative constructions of the control group.



Appendix Figure A3: Event study. The effect of foreign acquisitions on mobility to MNEs. Robustness to different sample periods.

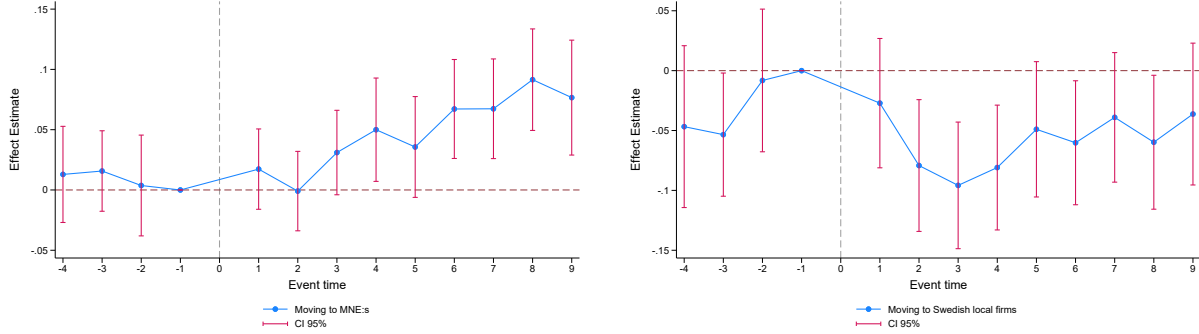
Note: The figures show the yearly difference-in-differences estimates of β_j using equation (2), relative to one year before foreign acquisitions (event time $j = -1$). Each figure displays estimates based on different sample periods. The vertical bars show the 95% confidence intervals. Controls for worker fixed effects, time-varying worker and firm characteristics, and year fixed effects are included. For details on equation (2), see Section 2.2.



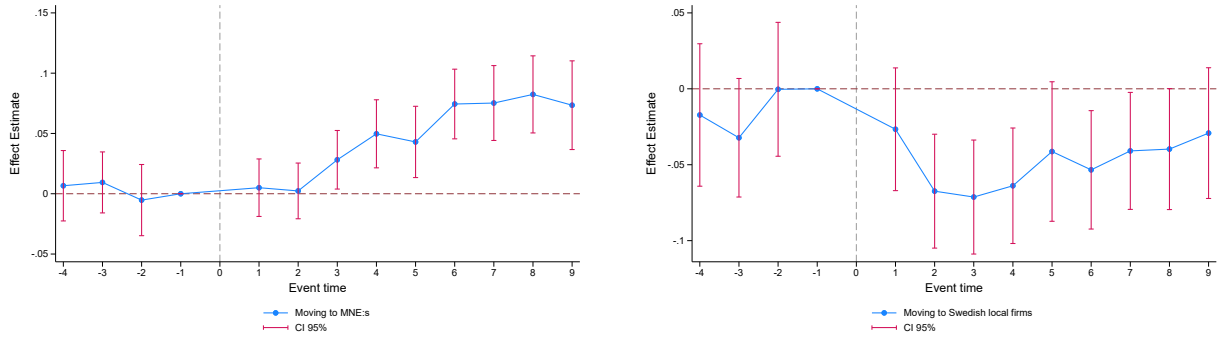
Appendix Figure A4: Event study. The effect of foreign acquisitions on mobility to local firms. Robustness to different sample periods.

Note: The figures show the yearly difference-in-differences estimates of β_j using equation (2), relative to one year before foreign acquisitions (event time $j = -1$). Each panel displays estimates based on different sample periods. Vertical bars show 95% confidence intervals. Controls for worker fixed effects, time-varying worker and firm characteristics, and year fixed effects are included. For details on equation (2), see Section 2.2.

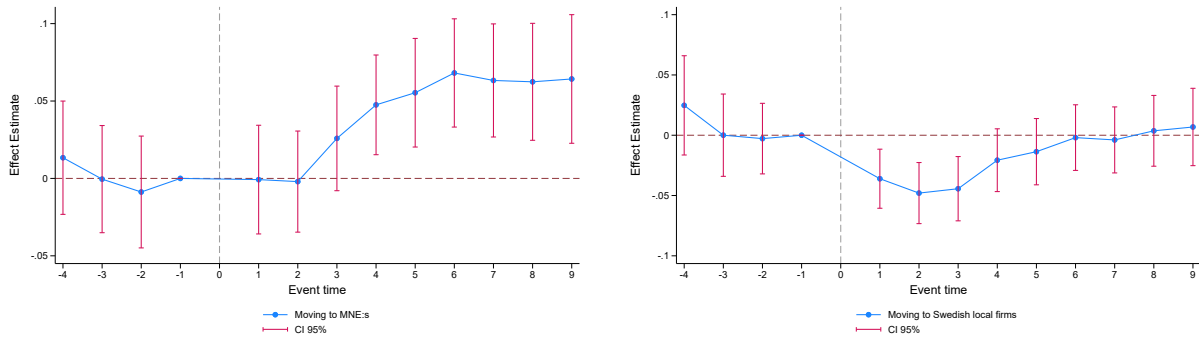
Panel A: Low-education group.



Panel B: Middle-education group.



Panel C: High-education group.



Appendix Figure A5: Event study. The effect of foreign acquisitions on mobility by education groups.

Note: The figures show the yearly difference-in-differences estimates of β_j using equation (2), relative to one year before foreign acquisitions (event time $j = -1$). The low-education group includes workers with at most 9 years of compulsory schooling. The middle-education group includes workers with 2-4 years of upper secondary school education. The high-education group includes workers with at least an undergraduate college education. The vertical bars show the 95% confidence intervals. Controls for worker fixed effects, time-varying worker and firm characteristics, and year fixed effects are included. For details on equation (2), see Section 2.2.

Appendix Table A1: Top ten source countries of foreign acquisitions

	Number of acquisitions	Share of acquired firms (%)	Share of workers in acquired firms (%)
Norway	859	19.4	14.8
Denmark	610	13.8	13.0
U.S.	512	11.5	15.1
Netherlands	506	11.4	9.1
U.K.	446	10.1	12.8
Germany	425	9.6	14.3
Finland	423	9.5	7.6
Luxembourg	334	7.5	5.8
France	172	3.9	4.3
Switzerland	147	3.3	3.0

Note: This table reports the top ten source countries of foreign acquisitions that have the largest number of acquisitions and the largest number of employees in the acquired firms during 1998-2013.

Appendix Table A2: Summary statistics for treated and control workers and for the universe of Swedish workers

	Treated workers			Control workers			Universe of Swedish workers		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Log wage	7.76	0.63	1,601,773	7.82	0.62	1,529,704	7.60	0.72	49,112,221
Woman	0.36	0.48	1,603,593	0.35	0.48	1,531,356	0.36	0.48	49,546,245
Age	42.01	10.81	1,603,593	42.39	10.72	1,531,356	40.62	12.81	49,546,245
Schooling	3.63	1.32	1,603,593	3.92	1.41	1,531,356	3.66	1.34	49,273,646
Experience	23.43	11.40	1,603,593	23.38	11.33	1,531,356	22.03	13.33	49,546,245
Primary & Construction	0.11	0.32	1,603,593	0.09	0.29	1,531,356	0.10	0.31	48,781,859
Manufacturing	0.22	0.41	1,603,593	0.24	0.43	1,531,356	0.28	0.45	48,781,859
Service	0.67	0.47	1,603,593	0.66	0.47	1,531,356	0.62	0.49	48,781,859
Managers	0.09	0.28	1,050,523	0.08	0.27	989,174	0.08	0.27	26,611,910
Professionals	0.13	0.33	1,050,523	0.21	0.41	989,174	0.13	0.33	26,611,910
Technicians	0.18	0.39	1,050,523	0.20	0.40	989,174	0.16	0.37	26,611,910

Note: This table reports the summary statistics for treated and control workers in the baseline regression sample, and for the universe of Swedish workers. See Section 2.3 for more details about the baseline PSM specification. The schooling variable ranges from 1 (if the highest level of education is elementary school) to 7 (if the highest level of education is a doctoral degree).

Appendix Table A3: The effect of foreign acquisitions on job mobility in the short-, medium-, and long run

	To MNEs		To local firms	
	(1)	(2)	(3)	(4)
DiD pre-medium	0.012 (0.008)	0.007 (0.010)	-0.036* (0.020)	-0.025* (0.015)
DiD short-run	0.008 (0.007)	0.006 (0.009)	-0.053*** (0.020)	-0.047*** (0.014)
DiD medium-run	0.043*** (0.009)	0.040*** (0.010)	-0.073*** (0.019)	-0.064*** (0.013)
DiD long-run	0.061*** (0.009)	0.057*** (0.011)	-0.052*** (0.020)	-0.042*** (0.015)
DiD longest-run	0.076*** (0.009)	0.073*** (0.011)	-0.039** (0.020)	-0.031** (0.013)
Observations	3,134,949	3,134,949	3,134,949	3,134,949
R-squared	0.051	0.118	0.068	0.261
Control variables	NO	YES	NO	YES
Worker FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Note: This table reports the estimates of the difference-in-differences coefficients from estimating equation (2) for six sub-periods. The sample period covers up to four years before an acquisition and up to nine years after the acquisition. *DiD pre-medium* measures years 3 and 4 before treatment. Years 1 and 2 before treatment are omitted as years of comparison. *DiD short-run* examines the treatment effect in years 1 and 2 post-acquisition, *DiD medium-run* in years 3 and 4, *DiD long-run* in years 5 and 6, and *DiD longest-run* in years 7, 8, and 9. All specifications include worker fixed effects, time-varying worker and firm characteristics, and year fixed effects. See Table 4A for the included control variables. In parentheses are standard errors clustered at the firm-year level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A4: Estimating the effect of foreign acquisitions on job mobility. Adding firm age as an additional firm control

	To MNEs		To local firms	
	(1)	(2)	(3)	(4)
After	-0.087*** (0.006)	-0.095*** (0.007)	-0.077*** (0.016)	-0.061*** (0.011)
DiD	0.036*** (0.006)	0.036*** (0.008)	-0.039** (0.017)	-0.039*** (0.012)
Experience square		-0.000*** (0.000)		0.000** (0.000)
Years of schooling		-0.000 (0.001)		0.002 (0.002)
Log employment		0.045*** (0.002)		-0.111*** (0.002)
Share of high-skilled		0.004 (0.011)		-0.235*** (0.008)
Capital intensity		0.000 (0.000)		0.000 (0.000)
Firm age	0.009*** (0.000)	0.004*** (0.001)	-0.019*** (0.001)	-0.006*** (0.001)
Observations	2,936,197	2,936,197	2,936,197	2,936,197
R-squared	0.060	0.097	0.094	0.238
Worker FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Note: This table examines the robustness of the baseline estimates of equation (1) by adding firm age as an additional control for firm characteristics. The sample period covers up to four years before an acquisition and up to nine years after the acquisition. The dependent variable takes the value one if workers in a treated or control firm move to MNEs (columns 1–2) or to local firms (columns 3–4), and zero otherwise. *After* takes the value one in the year of an acquisition and all years after. *DiD* is $After \times Treated$, where *Treated* takes the value one for workers who are employed by firms that are acquired at some point and the value zero for workers in control firms. For details on equation (1), see Section 2.2. In parentheses are standard errors clustered at the firm–year level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A5: The effect of foreign acquisitions on job mobility for different sample periods

	-4 to +9 (1)	-3 to +6 (2)	-2 to +6 (3)	-3 to +9 (4)	-2 to +9 (5)
<i>Panel A: Move to MNEs</i>					
DiD	0.036*** (0.008)	0.028*** (0.007)	0.031*** (0.008)	0.037*** (0.008)	0.040*** (0.008)
Observations	3,134,949	2,391,603	2,145,867	2,923,741	2,678,005
R-squared	0.096	0.083	0.101	0.109	0.124
<i>Panel B: Move to local firms</i>					
DiD	-0.038*** (0.011)	-0.045*** (0.011)	-0.053*** (0.011)	-0.040*** (0.011)	-0.048*** (0.011)
Observations	3,134,949	2,391,603	2,145,867	2,923,741	2,678,005
R-squared	0.234	0.233	0.253	0.250	0.268

Note: This table reports the estimates of difference-in-differences coefficients from estimating equation (1) for different sample periods. For comparison, column 1 lists the baseline estimates from Table 4A, columns 2 and 4. All specifications include worker fixed effects, time-varying worker and firm characteristics, and year fixed effects. See Table 4A for the included control variables. In parentheses are standard errors clustered at the firm-year level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A6: The effect of foreign acquisitions on mobility to high-wage firms

	Top-10%	Top-20%	Top-33%
	(1)	(2)	(3)
DiD	-0.002 (0.003)	-0.006 (0.005)	-0.004 (0.011)
Observations	3,134,949	3,134,949	3,134,949
R-squared	0.032	0.041	0.047
Control variables	YES	YES	YES
Worker FE	YES	YES	YES
Year FE	YES	YES	YES

Note: This table reports the estimates of the difference-in-differences coefficients from estimating equation (1) with mobility to high-wage firms as the dependent variable. High-wage firms are defined as the top ten percent of firms with the highest wages in column 1, the top 20 percent in column 2, and the top 33 percent in column 3. The sample period covers up to four years before an acquisition and up to nine years after the acquisition. All specifications include worker fixed effects, time-varying worker and firm characteristics, and year fixed effects. See Table 4A for the included control variables. In parentheses are standard errors clustered at the firm-year level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A7: Estimating the effect of foreign acquisitions on job mobility. Robustness to alternative specifications

	To MNEs				To local firms			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated			-0.050*** (0.003)	0.019*** (0.005)			0.034*** (0.003)	-0.015 (0.010)
After	-0.095*** (0.007)	-0.091*** (0.006)	-0.035*** (0.004)	0.023*** (0.005)	-0.069*** (0.010)	-0.071*** (0.008)	0.022*** (0.004)	0.052*** (0.011)
DiD	0.036*** (0.008)	0.030*** (0.006)	0.020*** (0.003)	0.040*** (0.008)	-0.038*** (0.011)	-0.037*** (0.009)	-0.012*** (0.003)	-0.050*** (0.013)
Observations	3,134,949	3,134,949	3,134,949	2,957,840	3,134,949	3,134,949	3,134,949	2,957,840
R-squared	0.096	0.491	0.032	0.043	0.234	0.569	0.004	0.045
Control variables	YES	YES	YES	YES	YES	YES	YES	YES
Worker FE	YES	YES	NO	NO	YES	YES	NO	NO
Industry-year FE	NO	YES	NO	NO	NO	YES	NO	NO
Firm FE	NO	NO	YES	NO	NO	NO	YES	NO
Year FE	YES	YES	YES	YES	YES	YES	YES	YES

Note: This table examines the robustness of baseline estimates of equation (1) to alternative specifications. The dependent variable takes the value one if workers in a treated or control firm move to MNEs (columns 1–4) or to local firms (columns 5–8), and zero otherwise. *Treated* takes the value one for workers who are employed by firms that are acquired at some point and zero for workers in control firms. *After* takes the value one in the year of an acquisition and all years after. *DiD* is *After* $\times$ *Treated*. The sample period covers up to four years before an acquisition and up to nine years after the acquisition. Columns 1 and 5 report the baseline estimates carried over from columns 2 and 4 of Table 4A. Columns 2 and 6 add industry-year fixed effects. Columns 3 and 7 replace worker fixed effects with firm fixed effects. Columns 4 and 8 exclude worker fixed effects from equation (1). All specifications include time-varying worker and firm characteristics and year fixed effects. See Table 4A for the included control variables. In parentheses are standard errors clustered at the firm-year level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A8: The effect of foreign acquisitions on job mobility to foreign and Swedish MNEs

	All firms (1)	>500 employees (2)	>1000 employees (3)
<i>Panel A: Move to foreign MNEs</i>			
DiD	0.045*** (0.005)	0.037*** (0.008)	0.023*** (0.007)
Observations	3,134,949	1,169,830	857,364
R-squared	0.055	0.034	0.021
<i>Panel B: Move to Swedish MNEs</i>			
DiD	-0.009* (0.005)	-0.007 (0.013)	0.001 (0.010)
Observations	3,134,949	1,169,830	857,364
R-squared	0.042	0.043	0.036

Note: This table reports the estimated difference-in-differences coefficients from estimating equation (1), where the dependent variable is the mobility to foreign and Swedish MNEs, respectively, in Panels A and B. The sample period covers up to four years before an acquisition and up to nine years after the acquisition. All specifications include worker fixed effects, time-varying worker and firm characteristics, and year fixed effects. See Table 4A for the included control variables. In parentheses are standard errors clustered at the firm-year level. *** p<0.01, ** p<0.05, * p<0.1.