IFN Working Paper No. 1453, 2022

How International Experience Helps Shape Labor Market Outcomes

Carl Davidson, Fredrik Heyman, Steven Matusz, Fredrik Sjöholm and Susan Chun Zhu

Research Institute of Industrial Economics
P.O. Box 55665
SE-102 15 Stockholm, Sweden
info@ifn.se
www.ifn.se

How International Experience Helps Shape Labor Market Outcomes*

Carl Davidson[1], Fredrik Heyman[2], Steven Matusz[3], Fredrik Sjöholm[4], and Susan Chun Zhu[5]

[1,3,5]Michigan State University

[2,4]Research Institute of Industrial Economics (IFN)

December 22, 2022

Abstract

This paper examines how experience from working in a foreign owned firm affects worker mobility. International experience can provide a worker with knowledge about foreign operations, thereby making them more attractive to other employers who are also engaged in international businesses. We follow workers in local Swedish firms where some experience an internationalization shock when their firm is acquired by a foreign multinational firm. Matching acquired firms with a group of similar control firms and applying a stacked difference-in-differences estimation approach, we find that international experience increases the likelihood of job switching to a multinational firm by around 4 percentage points and decreases the likelihood of job switching to a local firm by around 5 percentage points. Moreover, the post-acquisition wage growth rate is 10 percentage points higher for workers moving to MNEs as compared to stayers at acquired firms, leading to a steeper wage growth trajectory for movers to MNEs.

Keywords: Globalization; Multinational firms; FDI; Job mobility
JEL Codes: F16; F66; J60

*__Acknowledgements__: Fredrik Heyman and Fredrik Sjöholm gratefully acknowledge financial support from the Jan Wallander och Tom Hedelius stiftelse and the Marianne and Marcus Wallenberg Foundation. Fredrik Heyman also acknowledges financial support from the Johan och Jakob Söderbergs Stiftelse and the Torsten Söderbergs Stiftelse. We are indebted to Luca Opromolla, Martin Olsson, and Joacim Tåg for helpful discussions, and we have also benefited from the feedback provided by participants at various seminars and conferences. Marcos Demetry, Emma Gunnarsson, Gustav Häggbom and Glenn Nielsen provided excellent research assistance.

1 Introduction

A recent study by the US Bureau of Labor Statistics found that individuals born during the baby boom years of 1957-1964 held an average of 12.4 jobs during the prime age years of 18 to 54. Even though roughly half of those jobs were held from ages 18 to 24, there was significant job turnover across the entire age range. For example, for those who started a job between the ages of 35 and 44, 26% had their job end in less than a year and 61% had their job end in less than 5 years.[1] This underscores the fact that for many workers building a career is a complex, time-consuming process. As workers move up the job ladder, they acquire new skills that afford them the opportunity to compete for better, higher paying jobs. In our previous work (Davidson, Heyman, Matusz, Sjöholm, and Zhu, 2020), we developed and analyzed a trade model that incorporates a job ladder in the labor market. We argued that for many workers a stepping-stone to building a successful career involves working for an internationally engaged firm where a worker can acquire skills that are valuable to other internationally engaged firms. More precisely, such employment allows workers to gain skills that help reduce the cost of accessing and penetrating international markets.

In this paper we examine empirically whether and how the experience of working for a multinational enterprise (MNE) affects job mobility. Multinational operations involve knowledge of logistics, foreign preferences, legal systems, and other aspects of international commerce that play little or no role when serving domestic markets only. We anticipate that these sets of international skills can be obtained by working at an MNE and will be valued by other multinational firms that aim to acquire the competence necessary to compete in the global marketplace by recruiting employees with the needed skills (Balsvik, 2011; Parrotta and Pozzoli, 2012; Mion and Opromolla, 2014; Labanca, Molina, and Muendler, 2013; Mion, Opromolla, and Ottaviano, 2022). Our first objective is to provide new evidence on the causal effect of working for a multinational on job mobility.

[1] See the August 31, 2021 News Release from the Bureau of Labor Statistics (US Department of Labor) https://www.bls.gov/news.release/pdf/nlsoy.pdf.

Although our empirical question appears to be straight-forward, the results can have profound implications for the way that trade economists should evaluate the impact of globalization on workers. Many workers early in their careers may be more concerned with their future earnings than with their current wage. If working for a multinational facilitates mobility to higher-paying jobs, then globalization, which increases the number of jobs offered by MNEs, should make it easier for workers to ascend the job ladder. If globalization increases the rate at which these workers move up the job ladder while also pushing up the wages that they will earn later in life, then these workers may gain substantially in terms of lifetime earnings. Thus, the second objective of this paper is to study how job mobility can shift the wage trajectories and affect lifetime earnings. This approach departs from the existing literature that focuses on the impact of globalization on static wages.

Our empirical investigation exploits matched worker-firm data from Sweden for the period 1996-2015. To identify the causal effect of working for a multinational firm on mobility, we look at foreign acquisitions between 1998 and 2013, which allow us to follow firms and workers at least two years both before and after an acquisition taking place. One methodological problem that needs to be addressed is that workers are likely to self-select into different firm types based on their qualifications. In other words, more skilled workers are likely to work in better firms, which in turn are more likely to be multinational firms. Hence, comparing workers who are already employed by a multinational with those that are employed by strictly local firms will most likely bias our results on experience and mobility. We mitigate this endogeneity problem by comparing job mobility of workers in the Swedish firms that are eventually acquired by foreign investors with mobility of workers at the Swedish firms that remain strictly local. We intend to determine whether workers become more mobile after acquisitions, particularly in terms of moving to other multinational firms that place a similar value on international skills. However, it is not random which firms are targeted for acquisitions. To account for the non-random feature of acquisitions and to deal with the selection problem, we use propensity score matching to construct a control

group of local firms ("control firms") whose characteristics match those of acquired firms ("treated firms") one year prior to the acquisition. We then follow job mobility of both the workers that have gained international experience through foreign acquisitions ("treated workers") and workers at local control firms ("control workers").

To estimate the causal effect of foreign acquisitions on job mobility, we use a stacked difference-in-difference estimation method to compare the mobility of treated workers in the years before and after an acquisition with the mobility of control workers in the same period, conditional on observed firm and worker characteristics in the year of the acquisition and year fixed effects. Any workers who leave their firms can take jobs either in MNEs, or in local firms. Our results show that experience from working in an acquired firm increases mobility to other MNEs but decreases mobility to local firms. On average, acquisitions increase mobility to MNEs by around 4 percentage points. This effect is strong given that the share of workers who leave to MNEs is 14 percent in treated firms over the five-year period after acquisitions. Further, our estimates of the dynamic effect of acquisitions reveal that the increase 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, which supports the idea that it takes time for workers in acquired firms to gain skills that enhance their ability to move to other MNEs. We also estimate the effect of foreign acquisition on mobility to local firms and find a negative effect of around 5 percentage points.

We also examine how the effect of acquisitions on mobility differs between different occupations. High skilled occupations (managers and professionals) may be more involved in international operations such as management of global supply chains, logistics, and foreign legal systems in comparison to low skilled occupations (clerks and operators). As such, we expect the effect of foreign acquisitions on mobility to MNEs to be stronger for high skill occupations. We find that in all the years after acquisitions, high skilled occupations have higher mobility to MNEs compared to low skilled occupations. On the other hand, when we look at different occupations, movements to local firms appear to be insignificantly different between treated workers and control workers.

We then study the wage growth of treated workers by comparing movers and stayers in the years before and after an acquisition (relative to one year before the acquisition), conditional on observed firm and worker characteristics in the year of the acquisition and year fixed effects. We find that workers who moved before an acquisition have a similar wage growth rate as stayers. In contrast, workers who moved after an acquisition have a significantly higher wage growth compared to stayers. For example, workers who moved to another MNE in the third year after acquisitions have a higher wage growth rate by about 9 percentage points, as compared to stayers. This difference in wage growth increases steadily to 11 percentage points in the 9th years after acquisitions. We find a more modest post-acquisition wage growth premium for movers to local firms as compared to that for stayers at the acquired firms.

To further quantify the impact of acquisitions on treated workers, we account for the effect of acquisitions on mobility and compute the expected wage growth rate for movers in a particular year as a product of the probability of moving and the wage growth rate by movers in that year (relative to the year before acquisitions). We find that over the entire 9-year period after acquisitions, movers to MNEs have an expected wage growth rate of 7 percent, which more than double the growth rate (3 percent) if those movers had the same wage growth as stayers had and acquisitions did not increase worker mobility. This means that more than half of the expected wage growth represents the wage gains due to increased mobility and higher wages earned by movers to MNEs. In comparison, the expected wage growth rate is more modest for movers to local firms. We also study the wage growth trajectory for high and low skilled occupations separately and find that the wage growth is highest for workers in high skilled occupations who moved to MNEs after acquisitions. Overall, the results suggest that acquisitions provide an opportunity for treated workers to gain international experience, making it easier for them, especially high skilled occupations, to move to other MNEs and pushing them onto a steeper wage growth trajectory.

Our paper builds on the substantial literature on job mobility. Previous work has shown that mobility of workers between firms tends to be large and that it accounts for a substantial part

of wage increases (e.g. Topel and Ward, 1992). We also know that the return to job switching is relatively higher for workers who remain in the same industry, suggesting that the transferable skills are not completely general (Neal, 1995). Moreover, workers tend to move between firms that are similar in various characteristics. For instance, Haltiwanger, Hyatt, and McEntarfer (2015); Haltiwanger, Hyatt, Kahn, and McEntarfer (2018) find that firms of different sizes and with different levels of productivity tend to use different networks to fill their vacancies. Davidson, Heyman, Matusz, Sjöholm, and Chun Zhu (2022) provide evidence that exporting firms primarily recruit workers from other exporters while non-exporting firms tend to recruit from other non-exporters, and Andersson, Castellani, Fassio, and Jienwatcharamongkhol (2022) find a relatively high mobility of workers between MNEs in Sweden. We contribute to this strand of literature by examining if foreign acquisitions (a form of internationalization) have a causal effect on job mobility.

The relationship between firm acquisitions and labor market outcomes has received some attention, with most papers studying how employment and wages in the acquired firms are affected. For instance, Heyman, Sjöholm, and Tingvall (2007) find a very small effect of foreign acquisitions on wages at the target firms, and Olsson and Tåg (2018) find no effect on net employment after private equity buyouts of Swedish firms, but a negative effect on workers doing routine type of tasks or tasks that are easily offshorable. 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. 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. Differing from 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.

Our paper is also related to Mion et al. (2022) who find that in internationally active firms (importers, exporters, or multinationals) the experience-wage profile is much steeper 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. In contrast, our work exploits foreign acquisitions as an internationalization shock and addresses the causality of mobility using ownership changes. We use a stacked difference-in-difference approach combined with matching to estimate the treatment effects of acquisitions on job mobility and use the event regression method to uncover the dynamic effect of acquisitions on mobility. We also show that increased mobility after acquisitions plays an important role in making the wage growth trajectory steeper for workers who have gained international experience at the acquired firms and later moved to other MNEs.

The rest of the paper is organized as follows. Section 2 describes our data and empirical strategy. Section 3 presents empirical results on the effect of acquisition on worker mobility and wage growth. Section 4 concludes.

2 Empirical setup

Our empirical analysis examines how experience from working in an international firm affects job mobility. Our main concern is a selection effect: workers employed by a multinational firm could differ from workers employed by local firms. A simple comparison of mobility differences between workers in local firms and multinational firms would be biased. We address this possible selection bias by looking at workers who are initially employed in local firms but where some of them experience an internationalization shock when their firm gets acquired by a foreign owner. Hence, a foreign acquisition is our internationalization shock. We use acquisition by foreign MNEs, rather than by all MNEs including domestic ones, as our internationalization shock since foreign MNEs are the most internationalized ones in terms of export, import, offshoring, and foreign affiliates. 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 year of an acquisition with all workers employed at the same time in the control firms. We use our sample of

treated and control workers to examine the effect of international experience by applying a stacked difference-in-differences model. A more detailed description of our approach follows below.

2.1 Data

We use two Swedish data sets covering the period 1996-2015 provided by Statistics Sweden. The first one is the Swedish firm database containing detailed information on all Swedish private sector firms, including information on foreign and domestic ownership, which we use to identify foreign acquisitions of Swedish firms. The second data set includes detailed information on all Swedish individuals at the age 16 or above, from the Longitudinal Integration Database for Health Insurance and Labor Market Studies (LISA). LISA combines information from different register databases such as tax records and population registers, and includes information on age, gender, education, labor market participation, and wages. For individuals not in the labor force, we know if they are, for instance, in school, unemployed, on sick-leave, taking early retirement, or on maternity leave. Since LISA covers the universe of individuals in Sweden, a person exits our data only by emigration or dying. Unique identification codes allow us to link firms and workers and to follow both over time. We utilize this feature of the data to examine changes in ownership and job mobility.

2.2 Empirical model

We use our groups of treated and control firms to deal with selection issues. Treated firms are those that were acquired by foreign multinationals. Control firms are those that had similar characteristics to their matched treated firms prior to acquisitions. More details about how we create the group of control firms are given in the next section. To estimate the causal effect of internationalization on job mobility, we use a stacked difference-in-differences model (DiD) where mobility of workers in the years before and after an acquisition is compared to mobility of workers in our control firms.

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

$$Y_{ijt} = b_0 AFTER_j + b_1 TREATED_i + b_2 DiD_{ij} + X_i + X_f + \omega_t + \varepsilon_{ijt} \quad (1)$$

Hence, we measure mobility as a binary variable, Y_{ijt}, which takes the value one if the worker is moving to another firm. To examine the pattern of mobility, we run separate estimations where mobility is either to MNEs or to local firms. $AFTER_j$ takes the value one in the year of acquisition ($j = 0$) 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 in 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 it 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 worker mobility. Its coefficient b_2 is the estimate of the causal effect of acquisitions on worker mobility in treated firms, which contrasts the post-minus-pre-acquisition change of worker mobility in treated and control firms. X_i and X_f are observable individual- and firm characteristics that may affect worker mobility, measured at the year of acquisition. Individual characteristics include gender, experience, experience squared, and years of schooling. Firm characteristics include log firm size, 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 worker mobility for all firms. All other factors that may affect worker mobility are captured in the error term ε_{ijt}.

We also estimate a modified version of (1) to examine the dynamic effects of foreign acquisitions on worker mobility:

$$Y_{ijt} = \Sigma_{j=-4}^{9} \beta_j^0 T_j + b_1 TREATED_i + \Sigma_{j=-4}^{9} \beta_j (T_j \times TREATED_i) + X_i + X_f + \omega_t + \varepsilon_{ijt}, \quad (2)$$

where T_j are event year fixed effects that capture the change of mobility by all workers over time,

$TREATED_i$ and 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 worker mobility in treated firms in event year 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 event year j=0 and we cannot define movers or stayers at that time. We examine in our base estimations the effect of acquisitions on mobility by comparing β_j up to nine years after acquisitions with the years before acquisitions. Moreover, we distinguish in some estimations between the short-, medium-, long, and longest-run effects by examining the effects after acquisitions at event times 1-2, 3-4, 5-6, and 7-9 respectively, and the effects in the third and fourth years before acquisitions, while all these effects are relative to the mobility difference in the two years prior to acquisitions (the omitted two years of comparisons). Since interclass correlations between workers can result in too small standard errors (Moulton, 1986), we cluster our standard errors at the firm and year. Hence, the underlying assumption is that workers employed in a firm at the same time might affect each other, including the choice to move to another firm.

The DiD estimations examine the causal effect of international experience on job mobility under two conditions. The parallel trend condition 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 workers in treated and control firms in the same way. Failing to account for shocks that affect the two groups of workers differently biases our estimates. Counterfactual outcomes, what would have happened with job mobility in the absence of an acquisition, are not observable, and we follow practice and examine the trend in job mobility before acquisitions. The parallel trend assumption is considerably weaker than assuming random treatment, i.e. that acquisitions of local firms is random. Our DiD estimations are unbiased if the parallel trend assumption is not violated.

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. Our set of control firms are selected from the population of Swedish firms and are not restricted to firms within the same region or industry as the treated ones, which implies that it is presumably unlikely that acquisitions of treated firms affect workers in control firms Olsson and Tåg (2018).

Our DiD is carried out on panel data with staggered treatments, i.e. where treatment (an acquisition) happens in different years. It is plausible that treatments are heterogenous over time; the effect on mobility might for instance depend on the business cycle. Moreover, using workers who later work in firms that are treated as control units is another issue that requires special attention (e.g. Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). Baker, Larcker, and Wang (2022) thoroughly discuss the biases arising from treatment effect heterogeneity, and offer some possible solutions. We follow their suggestions and control for such heterogeneity bias by applying several different approaches.

Firstly, we use a stacked regression design which is one way to reduce the risk of a bias when estimating DiD with panel data and with staggered treatments (Cengiz, Dube, Lindner, and Zipperer, 2019). More specifically, we normalize the acquisition to $j = 0$ which is equivalent to a setting where all acquisitions happen at the same time. We then follow the workers present in $j = 0$ in treatment and control firms. As previously discussed, we do not include firms in the control group if they are later treated, which could otherwise be another possible cause of a bias.

Baker et al. (2022, p. 40) also show that estimates can be largely affected by inclusion of control variables. They therefore suggest that results both with and without such control variables should be provided to give a better understanding on what is driving the results. We follow this approach and provide estimations with and without control variables.

2.3 Creating the control group

As previously discussed, we compare workers in acquired firms (treated) with workers in firms remaining local (control). We restrict our sample to firms that only experience one acquisition and exclude all firms that go from, for instance, Swedish to foreign ownership and then back to Swedish. Moreover, we look only at foreign acquisitions of Swedish local firms and exclude acquisitions by Swedish MNEs. We include all workers aged 22-62 at the time of acquisitions in our regressions, which means that we can follow them a few years before and after this change in ownership. Finally, we use acquisitions between 1998 and 2013 to allow us to follow firms and workers at least two years before and after the acquisitions taking 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.

[Figure 1 about here]

As previously noted, it is not random which firms are targeted for acquisition. We mitigate the selection problem by a propensity score matching approach to find a control group of firms whose firm characteristics match the ones of acquired firms one year prior to the acquisition. We create the control group by matching at the firm level using one-to one matching without replacement. Each control firm is only a control firm to one treated firm and hence, the number of firms in the control group equals the number of firms in the treated group in the same year. The matching is done for each year separately and with a nearest neighborhood algorithm. Hence, control firms are selected so that their characteristics are similar to the treated firm one year prior to the year when the acquisition is taking place. We do not include firms in the control group if they are acquired in later years.[2]

[2]For example, we start with all firms treated in 2010 and find control firms that match their 2009 characteristics. Then we look at all firms treated in 2011 and find control firms that match their 2010 characteristics, and so on. We also make sure that the firms that serve as control firms in say 2010 do not end up as control firms in say 2011.

We use a binomial logit estimation to conduct our matching where the dependent variable takes the value one if a firm is acquired and zero if it is not acquired.

$$acquisition_{it} = \alpha_1 X_{it-1} + \alpha_2 X_{it-2} + \alpha_3 X_{i\Delta t-1} + \delta_s + \theta_t + \mu_{it}, \tag{3}$$

where X_{it-1} and X_{it-2} are vectors of firm characteristics at $t-1$ and $t-2$ that could affect the probability of an acquisition. We include eight different firm characteristics as explanatory variables, and also include the changes in the same variables for the two years before the acquisition (vector $X_{i\Delta t-1}$). Finally, we also include industry, δ_s, and year, θ_t, fixed effects.

The results for our matching estimation for our first year (1998) and our last year (2013) are shown in Tables A1 and A2 in the appendix. Two conclusions can be drawn from the tables. First and as can be expected, foreign acquisitions target strong firms. Treated (acquired) firms tend to be larger and more profitable, with a relatively skilled labor force and high capital intensity in comparison to control firms in the unmatched sample. Secondly, matching reduces the difference between treated and control firms significantly. There are, with few exceptions, no statistically significant differences in firm characteristics between treated and control firms in the matched sample.

A similar picture is seen in Table 1 which shows firm characteristics before the acquisitions for our pooled set of firms over the whole period 1998-2013. Again, matching substantially reduces any differences between acquired and non-acquired firms and there are no statistically significant differences in firm characteristics left, with the exceptions of the one-year difference in sales and share of low skilled workers.

[Table 1 about here]

Our final way to illustrate the reduction in bias is seen in Figure 2 which shows the difference in various variables between treated and control firms, both in the matched sample of firms and in the unmatched sample. A value close to zero means a low difference with high values indicating

large differences. Again, it is clear that matching reduces the differences in almost all variables. To sum up, our matching successfully removes differences in firm characteristics between acquired and non-acquired firms prior to the acquisitions.

[Figure 2 about here]

2.4 Job Mobility in Treated and Control Firms

Overall, mobility of workers is high. Figure 3 shows the share of workers in our sample of treated and control firms who are employed in the same firm up to five years before the takeover and remaining in the firm up to five years after the takeover. For instance, around 75 percent of the workers in the year of a takeover (event year $j = 0$), worked in the firm one year before ($j = -1$). Five years before the takeover, the figure was only around 25 percent. Similarly, around 77 percent of the workers remained in the firm after one year and 35 percent after five years.

[Figure 3 about here]

The size of mobility is notably similar between treated firms and control firms, but the type of mobility might differ. Table 2 looks at the pattern of mobility. It covers the accumulated share of workers who leave to different firm types after an acquisition. Most workers who change employers end up in local firms, which is not surprising considering that this is the largest group of firms. The first two columns show that 4.7 percent of workers in treated firms and 3.7 percent of workers in control firms have moved to an MNE one year after the acquisition. The aggregated share has increased to 14.2 and 11.8 percent respectively five years after the acquisition. Looking at movers to local firms in columns 3 and 4, we see that the shares five years after the acquisition are 19.7 and 21.7 percent respectively. Hence, there is a clear difference in the pattern of transition where a relatively high share of workers from treated firms move to MNEs and a relatively high share of workers in control firms move to local firms.

[Table 2 about here]

Foreign acquisitions could bring a new owner who forces workers to move. This would of course be a very different cause of mobility than the learning based one we have discussed above. We find some comfort in Figure 3, which suggests that this is not a major problem since the magnitude of mobility is similar in treated and control firms. Table 3 takes this issue one step further and looks at wages for workers staying in local firms, moving to an MNE or moving to another local firm. All wages are compared to the year of acquisition ($j = 0$). The figures for event year five include all workers who move between the first and fifth years after an acquisition. Acquisitions increase wages for workers who stay at the firm by 16 percent in a five-year period, compared to six percent in the control firms. More importantly, the wage increase is highest for treated workers moving to MNEs. This suggest that workers leaving a firm after an acquisition are not pushed out but rather attracted by higher wages, presumably due to the experience gained from working in an MNE.

[Table 3 about here]

The above results suggest that treated workers have higher mobility to MNEs than workers in control firms, and that treated workers who move to another MNE have the highest wage growth after they move. In the following analysis we investigate whether the higher worker mobility and higher wage growth are caused by acquisitions.

3 Econometric results

3.1 The effect on mobility

The effect of foreign acquisition on mobility to MNEs (panel A) and local firms (panel B) are shown in Figure 4. This figure shows the stacked difference-in-difference estimates of β_j's using equation (2), relative to one year prior to foreign acquisitions, and with 95% confidence intervals. Panel A shows that the point estimates in the pre-treatment period are close to zero and statistically insignificant, implying that the treatment and control groups have a similar trend four years before foreign acquisitions occur. There is no effect of treatment in the first two years after acquisitions.

The effect of acquisitions on mobility then starts to increase in the third year and 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 after acquisitions as compared to the mobility difference between treated and control workers in the first year before acquisitions. Figure 4, hence, suggests that foreign acquisitions raise worker mobility three years after acquisitions, which supports the hypothesis that it takes time before a worker has gained valuable skills from working in a foreign MNE, skills that increase mobility to other MNEs.

The effect on mobility to local firms is different as seen in Panel B. The pre-treatment estimates are relatively close to zero which suggest that the parallel trend assumption holds. Moreover, there is an immediate and negative effect on mobility already in year one when workers in treated firms are around 3 percent less likely than workers in control firms to move to a local firm, as compared to the mobility difference in the first year before acquisitions. The effect becomes stronger and varies between 5 to 7 percent in the following years. However, standard deviations are large and some of the point estimates are statistically insignificant.

[Figure 4 about here]

We continue by showing the difference-in-difference estimates of equation (1) in Table 4. The estimated coefficient for DiD measures the mobility difference between workers in treated and control firms for the whole period after acquisitions as compared to that difference for the whole period before acquisitions. Columns 1 and 2 examine the effect of treatment (foreign acquisition) on workers' mobility to MNEs, and columns 3 and 4 to local firms. We include estimations both with and without firm- and worker characteristics.

Foreign acquisitions increase the likelihood of moving to a MNE by around 4 percentage points. This effect is strong given that the share of workers who leave to MNEs is 14 percent in treated firms over the five-year period after acquisitions. There is a relatively large negative effect of acquisitions on the likelihood of moving to a local firm. More precisely, workers in firms that are acquired by

foreign owners are around 5 percentage points less likely to move a local firm, in comparison to workers who work in local firms which are not acquired.

[Table 4 about here]

Looking at the control variables, we see that females are less likely than males to move to other firms, which holds for both movement to multinational and to local firms. Experience is measured as the time since finishing the highest level of education and is therefore highly correlated with age. The coefficients on Experience show that older experienced workers are less likely to move to other firms. Highly educated workers (Schooling) are relatively likely to move to MNEs and unlikely to move to local firms, but the point estimates are small. Finally, the firm characteristics suggest that workers in large and skill intensive firms are relatively likely to move to MNEs and unlikely to move to local firms.

We continue by dividing the effect of international experience into six periods. The pre-acquisition periods are pre-medium-run including years 3-4 before the acquisition (DID pre-medium-run) and pre-short-run including years 1-2 years before the acquisition (DID pre-short-run). The post acquisitions periods are short-run including years 1-2 (DiD short-run); medium-run including years 3-4 (DiD medium-run); long-run including years 5-6 (DiD long-run), and longest run including years 7-9 (DiD longest-run). The effect is seen in Table 5 and is very small and statistically insignificant in the short run, substantially larger in the medium run (around 5%), larger in the long run (close to 7%), and even larger in the longest-run (close to 8%). 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 different than the experienced caused movement to MNEs.

[Table 5 about here]

There are reasons to believe that the pattern of mobility differs between different occupations. The experience of working in an internationalized firm might for instance be of higher value for

more skilled occupations compared to less skilled occupations (Mion et al., 2022), suggesting that skilled occupations would have higher mobility to other MNEs that value international experience more. We look at this in more detail in Figure 5 where we examine mobility separately for high skilled occupations (managers and professionals) and low skilled occupations (operators and clerks). The results show that for both high and low skilled occupations, there is no significant difference between treated and control firms prior to acquisitions. However, mobility starts to pick up three years after acquisitions and peaks in the 5th and 6th years after acquisition. As expected, the effect is stronger for high skilled occupations than for low skilled occupations. In all the years after acquisitions, high skilled occupations have higher mobility to other MNEs compared to low skilled occupations. For example, in the 5th and 6th years after acquisitions, managers and professionals in treated firms are 6.9 and 8.3 percentage points respectively more likely to move to MNEs in comparison to managers and professionals in the control group. The corresponding figures for clerks and operators are 6.1 and 6.4 percentage points respectively. Hence, movements to MNEs from treated firms are more common among managers and professionals than among operators and clerks.

On the other hand, for both high and low skilled occupations, movements to local firms appear to be insignificantly different between workers from treated firms and those from control firms.

[Figure 5 about here]

The above analysis has documented strong evidence that foreign acquisitions increase mobility to other MNEs, especially for high skilled occupations. To investigate further, we now look at the effect on mobility to Swedish and foreign MNEs separately. It is possible that foreign MNEs may place greater value on international experience gained from working at the firms that are newly acquired by foreign owners. As seen from Table 6, treatment increases mobility to foreign MNEs but not to Swedish MNEs. More specifically, column 1 shows a positive and statistically significant effect on mobility to foreign MNEs and column 2 reports an insignificant effect on mobility to Swedish MNEs. However, the parallel trend assumption was violated when we estimated the

coefficients in columns 1 and 2 (not shown), which means that differences in trends rather than the treatment might explain the results. To deal with this issue, we look at larger firms only and find no differences in mobility pre-trends for larger firms with above 500 or above 1000 employees. Moreover, the results in columns 3-6 confirm that international experience increases mobility to foreign MNEs but there is no, or even a negative, effect on mobility to Swedish MNEs, suggesting that movements to other MNEs may mostly be driven by movements to foreign MNEs. The similar mobility patterns across firms of different size also suggest that higher mobility to foreign MNEs is not due to their larger firm size.

[Table 6 about here]

3.2 Robustness

We continue our analysis with several alternative specifications to examine the robustness of our results. The previous estimations examined the effect of international experience on job mobility in the period four years before an acquisition and nine years after. In Table 7 we have experimented with re-estimating equation 1 using different time periods. with alternative periods. Columns 1 and 6 are identical to columns 2 and 4 in Table 4, and they are included for comparisons with the results when we change the time period. The other columns show the results when either 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: international experience increase job mobility to MNEs and decrease job mobility to local firms. The effect for MNEs remains similar in size to the previously shown results: treatment increases job mobility to MNE by between 3.3 and 4.7 percentage points. International experience decreases mobility to local firms by between 5 and 6 percentage points in the different specifications. Finally, we have also estimated the different specifications in Table 7 but with our event study approach. The results are shown in Figures A1 and A2 in the appendix and confirm the results in Table 7.

We also use different restrictions on our sample of workers as another robustness test. Adding

restrictions that workers are not allowed to move between control firms or between treated firms gave similar results: foreign experience has a positive effect on mobility to MNEs and the estimated effect was very similar to the ones found above.

[Table 7 about here]

The analysis above shows that workers in treated firms are more likely to move to MNEs. We have argued that this higher mobility is due to the fact that MNEs are internationally engaged firms and they value workers with international experience. 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. Hence, a large mobility to MNEs would then be explained by MNEs being good firms that pay relatively high wages. We examine this issue in Table 8 where firms are divided based on their average 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). The results show no statistically significant effect of treatment on mobility to high wage firms. Hence, we conclude that international experience does not increase mobility to good firms in general (i.e., local firms or MNEs that pay high wages); instead, the effect is restricted to mobility to MNEs that are interested in hiring workers with international experience due to their needs for the specific skills.

[Table 8 about here]

3.3 The effect on wages

We continue our analysis by examining the effect of internationalization and mobility on wages. We find no effect on average wages at the acquired firms after an acquisition, which is consistent with 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).[3]

[3]We estimate equation (2) by replacing the dependent variable with log wages. The estimated coefficients on the interactions between event year fixed effects and the treatment indicator are statistically insignificantly for all years

We next examine the wage effect of moving from acquired firms to MNEs. We would expect to see a positive wage effect if working for MNEs allows workers to gain skills that enable them to move to better jobs. No positive wage effect would make us suspect that mobility is caused by a push rather than a pull effect, in other words, workers are forced to leave after an acquisition. A positive wage effect also provides support for the hypothesis that international experience gained from working at the acquired firm can help workers climb job ladders and move onto a higher wage trajectory.

To estimate the effect of acquisition on wages, we compare movers with stayers in acquired firms (treated firms). Specifically, we estimate the following wage equation:

$$w_{ijt} = a_0 AFTER_j + a_1 Mover_{ij} + a_2 AFTER_j \times Mover_{ij} + X_i + X_f + \omega_t + v_{ijt}, \qquad (4)$$

where w_{ijt} is the log wage of worker i in event year j and calendar year t; $AFTER_j$ indicates the year of acquisition and all subsequent years; $Mover_{ij}$ is equal to one if worker i is at another MNE or a local firm in event year j, and zero otherwise; X_i and X_f represent observed worker and firm characteristics, respectively, in the year of acquisition; ω_t is year fixed effects that 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 as compared to 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 of acquisitions by modifying equation (4) as follows:

$$w_{ijt} = \Sigma_{j=-4}^{9} \gamma_j^0 T_j + a_1 Mover_{ij} + \Sigma_{j=-4}^{9} \gamma_j (T_j \times Mover_{ij}) + X_i + X_f + \omega_t + v_{ijt}, \qquad (5)$$

(not shown).

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

The estimates of γ_j are displayed in Figure 6. Panel A shows that before acquisitions, the wage growth is not different between movers and stayers. However, one year after acquisitions, the additional wage growth by movers is about 10 percent 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. Since these post-acquisition wage gains happened in the year when workers moved and a few years afterwards, the extra wage growth may be interpreted as an additional return to international experience gained after acquisitions at the treated firms.

Panel B shows the estimates of γ_j for movers to local firms. We find that one year after acquisitions, the wage growth for movers to local firms is 11 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 stabilizes at around 6 percent.

[Figure 6 about here]

Table 9 shows the estimates of (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 difference in wage growth between movers to MNEs and stayers is around 12.9 percentage points higher after acquisitions than that difference before acquisitions. Moreover, experienced and highly educated workers get comparably high wages and females comparably low wages. Finally, large firms pay low wages and capital-intensive firms with a skilled labor force pay high wages.

Columns 3-4 report the results for movers to local firms as compared to 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 externality to local firms and earn higher wages after moving. 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 is more valuable for MNEs.

[Table 9 about here]

3.4 The expected wage growth for movers

To further quantify the effect of acquisition on workers at the acquired firms, we now look at the wage growth for movers with stayers as the comparison group. Specifically, using the estimates of (2) and (5), we compute the expected wage growth associated with moving to MNEs in event year j as $\left(\beta_j^0+\beta_j\right)\left(\gamma_j^0+\gamma_j\right)$, which can be expanded into $\beta_j^0\gamma_j^0+\beta_j^0\gamma_{_j}+\beta_j\gamma_j^0+\beta_j\gamma_j$. These terms can be interpreted as follows: (i) $\beta_j^0\gamma_j^0$ is the amount of wage growth if work mobility is at the same rate as that for control firms (we call it the benchmark mobility rate) and wage growth is at the same rate as that for stayers (we call it the benchmark wage growth rate); (ii) $\beta_j^0\gamma_j$ is the amount of wage growth if mobility is at the benchmark level, but there is additional wage growth associated with moving to another MNE; (iii) $\beta_j\gamma_j^0$ is the amount of wage growth if there is an increase in mobility due to acquisition, but wage growth is at the benchmark level; and (iv) $\beta_j\gamma_j$ is the amount of wage growth if there are an increase in mobility due to acquisitions and a rise in wage growth associated with moving to another MNE. This cross term reflects the correlation between mobility and extra wage changes for movers as compared to stayers. In the case of moving to MNEs, higher mobility combined with extra wage growth for movers (a positive correlation) leads to a further increase in the expected wage growth for movers. The expected wage growth associated with moving to local firms in event year j can be computed in a similar way.[4]

In Figure 7 we draw the trajectories of expected wage growth for movers. Panel A shows that $\beta_j^0\gamma_j^0$ rises steadily after acquisition, suggesting a steady post-acquisition wage growth for stayers.

[4]In the case of moving to local firms, lower mobility and extra wage growth for movers (a negative correlation) leads to a reduction in the expected wage growth for movers.

Part of the wage growth could be associated with the returns to increased international experience by stayers after acquisition. We also observe that both $\beta_j^0\gamma_j$ and $\beta_j\gamma_j^0$ contribute markedly to the wage growth for movers to MNEs. In comparison, the cross term $\beta_j\gamma_j$ is smaller. For example, in the 9th year after acquisition, the benchmark cumulative wage growth $\beta_j^0\gamma_j^0$ is at 3 percent (relative to $j = -1$), while each of $\beta_j^0\gamma_j$, $\beta_j\gamma_j^0$, and $\beta_j\gamma_j$ contributes to the total wage growth for movers to MNEs by 1.78, 1.32, and 0.77 percent respectively, leading to a total growth rate of 7 percent. Since these estimates are obtained after controlling for various individual and firm characteristics that may affect wages (including years of experience and schooling), the wage growth reported in Figure 7 represents the additional wage gains due to increased mobility and higher wages earned by movers to MNEs. Overall, we observe that movers to MNEs have a much steeper wage growth trajectory after acquisitions.

Panel B of Figure 7 displays the wage growth trajectories for movers to local firms. We see that the cumulative expected wage growth rate is close to the benchmark rate. For example, the cumulative wage growth rate for movers to local firms is 5.3 percent nine years after acquisition, which is just slightly above the benchmark rate of 4.9 percent.

[Figure 7 about here]

Based on the cumulative expected wage growth rates, we also compute the annualized wage growth rates.[5]. Again, the year before acquisitions is the reference year. The results are displayed in Figure 8. Panel A shows the annualized rate for movers to MNEs. The benchmark annualized growth rate is based on the mobility rate in control firms and the wage growth by stayers at the acquired firms. We see that the annualized expected wage growth rate for movers to MNEs more than double the benchmark rate (if those movers had the same wage growth as stayers had and acquisitions did not affect worker mobility).

Panel B of Figure 8 shows the annualized wage growth trajectory for movers to local firms. Up

[5]Let x_j denote the cumulative expected wage growth rate in event year j. Given that the year before acquisition ($j = -1$) is the reference year, the annualized expected wage growth rate is computed as $(1 + x_j)^{1/(j+1)} - 1$

to four years after acquisition, the annualized expected wage growth lags behind the benchmark growth rate due to lower mobility to local firms after acquisition. In the 9th year after acquisitions, the annualized wage growth rate reaches 0.52 percent, slightly above the benchmark rate of 0.48 percent.

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

[Figure 8 about here]

We further explore the difference in wage growth trajectory between high and low skilled occupations. The results are shown in Figure 9. We can see that the annualized expected wage growth is the highest for movers to MNEs in high occupations. In contrast, in low occupations, movers to local firms have a higher wage growth. These patterns suggest that international experience is more relevant for high occupations and workers in those occupations benefit more from international experience by moving to MNEs.

[Figure 9 about here]

4 Concluding remarks

We used foreign acquisitions of Swedish firms as an internationalization shock and identified the causal effect of international experience on worker mobility. Workers at acquired firms may gain international experience after the acquisition shock. To adjust for the nonrandom feature of acquisitions, we paired acquired firms with never-acquired local firms based on one-to-one propensity score matching and used those local firms as the group of control firms. We applied a stacked difference-in-difference estimation approach by comparing worker mobility in treated and control

firms before and after acquisitions. We find that international experience gained after foreign acquisitions increases the likelihood of job switching to another MNE by about 4 percentage points while reducing the likelihood of job switching to local firms by about 5 percentage points. We also find that the effect of acquisitions on worker mobility is gradual, and it turns significant in the third year after treatment and remains steady afterwards, which supports the idea that it may take time for treated workers to gain international skills that increase their ability to move to better jobs.

We also studied the effect of acquisitions on wage growth for movers. We find that one year after acquisitions, the additional wage growth by movers to another MNE is about 10 percent and the effect remains stable at this level afterwards. We also find a more modest post-acquisition wage growth for movers to local firms as compared to that for stayers at the acquired firms.

Overall, acquisitions allow workers at the acquired firms to gain international experience, thus increasing the probability of moving to other MNEs and enabling movers to reach a steeper wage growth trajectory.

References

Andersson, M., D. Castellani, C. Fassio, and V. Jienwatcharamongkhol (2022). Leaving the multinational: The likelihood and nature of employee mobility from mnes. *Journal of International Business Studies*, 1–14.

Baker, A. C., D. F. Larcker, and C. C. Wang (2022). How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics 144*(2), 370–395.

Balsvik, R. (2011). Is labor mobility a channel for spillovers from multinationals? evidence from norwegian manufacturing. *The Review of Economics and Statistics 93*(1), 285–297.

Callaway, B. and P. H. Sant'Anna (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics 225*(2), 200–230.

Cengiz, D., A. Dube, A. Lindner, and B. Zipperer (2019). The effect of minimum wages on low-wage jobs. *The Quarterly Journal of Economics 134*(3), 1405–1454.

Davidson, C., F. Heyman, S. Matusz, F. Sjöholm, and S. C. Zhu (2020). Globalization, the jobs ladder and economic mobility. *European Economic Review 127*, 103444.

Davidson, C., F. Heyman, S. Matusz, F. Sjöholm, and S. Chun Zhu (2022). Globalization, recruitments, and job mobility. *Canadian Journal of Economics, forthcoming*.

De Chaisemartin, C. and X. d'Haultfoeuille (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review 110*(9), 2964–96.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics 225*(2), 254–277.

Haltiwanger, J., H. Hyatt, and E. McEntarfer (2015). Firm size, wages, and productivity. *2015 Society of Labor Economists Conference in Montreal, Canada.*.

Haltiwanger, J. C., H. R. Hyatt, L. B. Kahn, and E. McEntarfer (2018). Cyclical job ladders by firm size and firm wage. *American Economic Journal: Macroeconomics 10*(2), 52–85.

Heyman, F., F. Sjöholm, and P. G. Tingvall (2007). Is there really a foreign ownership wage premium? evidence from matched employer–employee data. *Journal of International Economics 73*(2), 355–376.

Labanca, C., D. Molina, and M.-A. Muendler (2013). Preparing to export. *University of California San Diego Working Paper*.

McGuckin, R. H. and S. V. Nguyen (2001). The impact of ownership changes: a view from labor markets. *International Journal of Industrial Organization 19*(5), 739–762.

Mion, G. and L. D. Opromolla (2014). Managers' mobility, trade performance, and wages. *Journal of International Economics 94*(1), 85–101.

Mion, G., L. D. Opromolla, and G. I. Ottaviano (2022). Dream jobs in a globalized economy: Wage dynamics and international experience. *CEPR DP15027 Working paper*.

Moulton, B. R. (1986). Random group effects and the precision of regression estimates. *Journal of econometrics 32*(3), 385–397.

Neal, D. (1995). Industry-specific human capital: Evidence from displaced workers. *Journal of Labor Economics 13*(4), 653–677.

Olsson, M. and J. Tåg (2018). What is the cost of privatization for workers? IFN Working Paper No. 1201.

Ouimet, P. and R. Zarutskie (2020). Acquiring labor. *Quarterly Journal of Finance 10*(03), 2050011.

Parrotta, P. and D. Pozzoli (2012). The effect of learning by hiring on productivity. *The RAND Journal of Economics 43*(1), 167–185.

Sun, L. and S. Abraham (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics 225*(2), 175–199.

Topel, R. H. and M. P. Ward (1992). Job mobility and the careers of young men. *The Quarterly Journal of Economics 107*(2), 439–479.

Figures

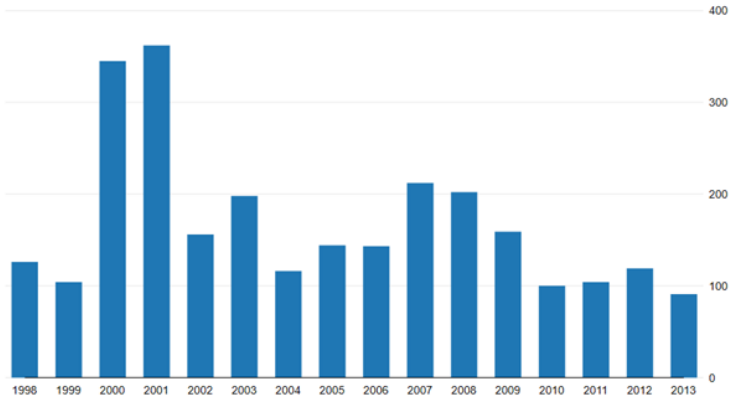


Figure 1: Annual number of foreign acquisitions of local firms (1998-2013)

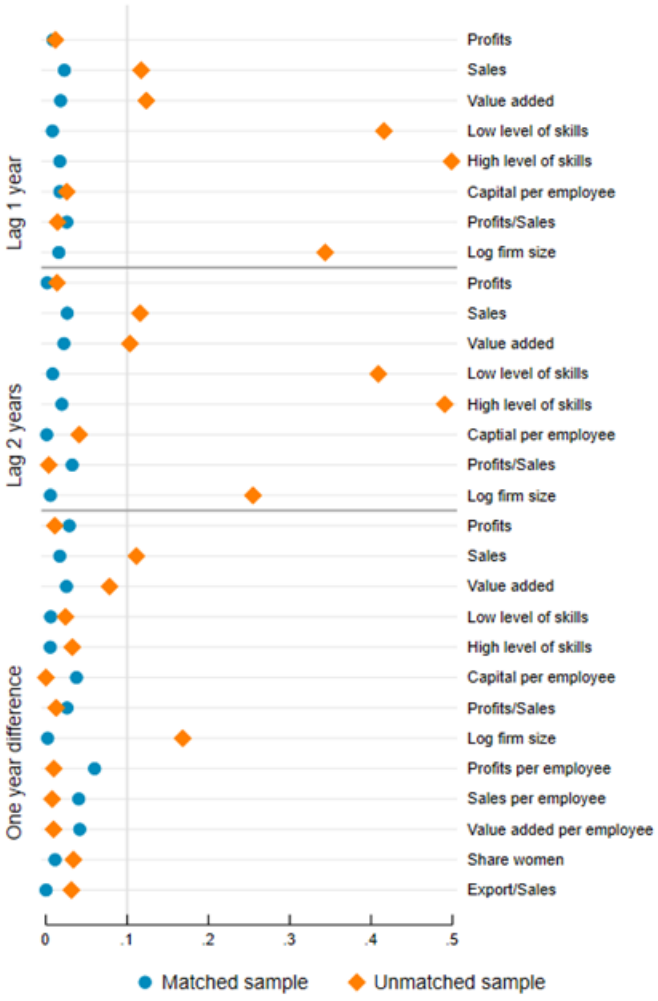


Figure 2: Absolute differences in treated and control firms in matched and unmatched samples

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 controls in the matched and unmatched samples, all years pooled. Then the absolute values of standardized percentage bias are calculated according to the formula in Austin (2009).

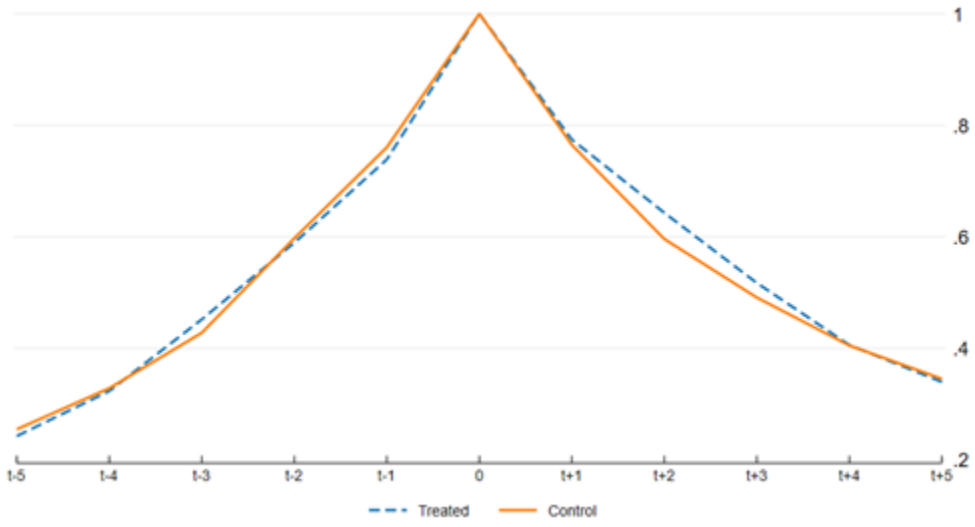
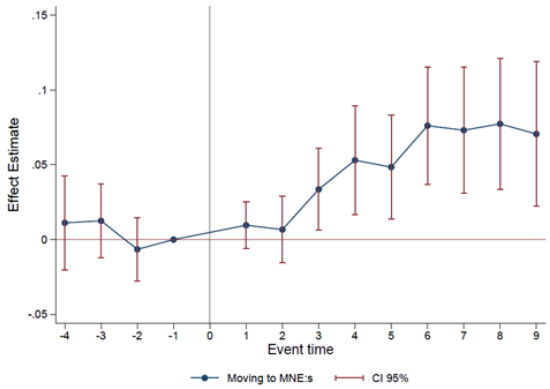


Figure 3. Job mobility in acquired and non-acquired firms (1998-2013).

Panel A: Mobility to MNEs



Panel B: Mobility to local firms

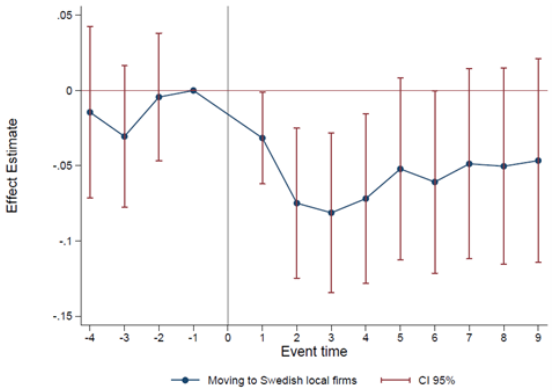
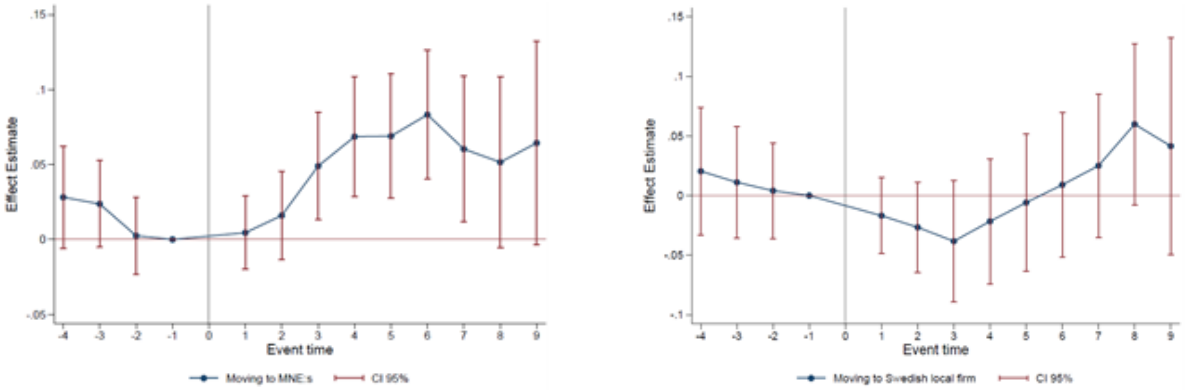


Figure 4. Event study. The effect of foreign acquisitions on job mobility.

Note: The figures show yearly difference-in-difference estimates relative to one year before the foreign acquisition (event time $t-1$). The vertical bars show the 95% confidence intervals. Control variables and fixed effects are included. For details on included variables see Table 4.

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



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

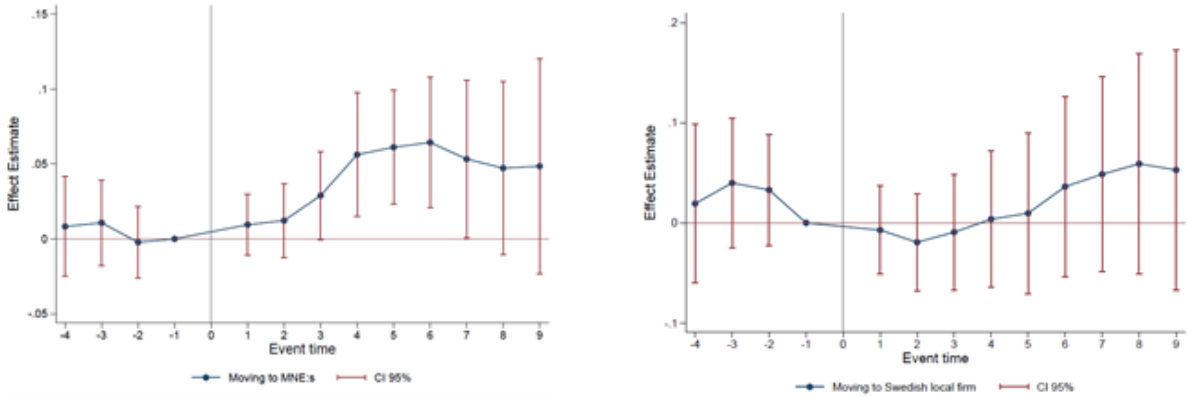
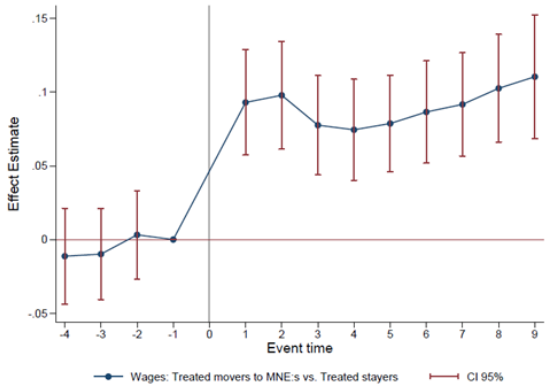


Figure 5. Event study. The effect of foreign acquisitions on job mobility of different occupations.

Note: The figures show yearly difference-in-difference estimates relative to one year before the foreign acquisition (event time $t-1$). The vertical bars show the 95% confidence intervals. Control variables and fixed effects are included. For details on included variables see Table 4.

Panel A: Comparison between movers to MNEs with stayers at an acquired firm.



Panel B: Comparison between movers to local firms with stayers at an acquired firm

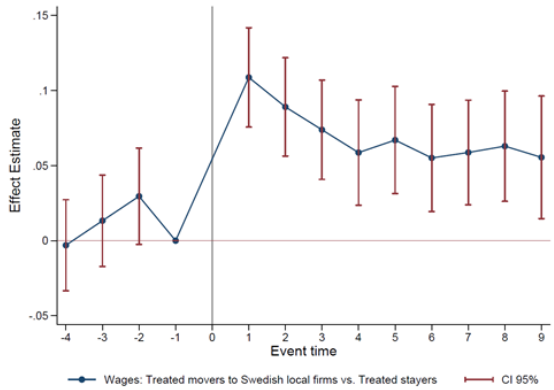


Figure 6. Event study. The wage effect of moving to other firms

Note: The figures show yearly estimates relative to the one year before the foreign acquisition (event time $j = -1$). The vertical bars show the 95% confidence intervals. Control variables and fixed effects are included. For details on included variables see Table 4.

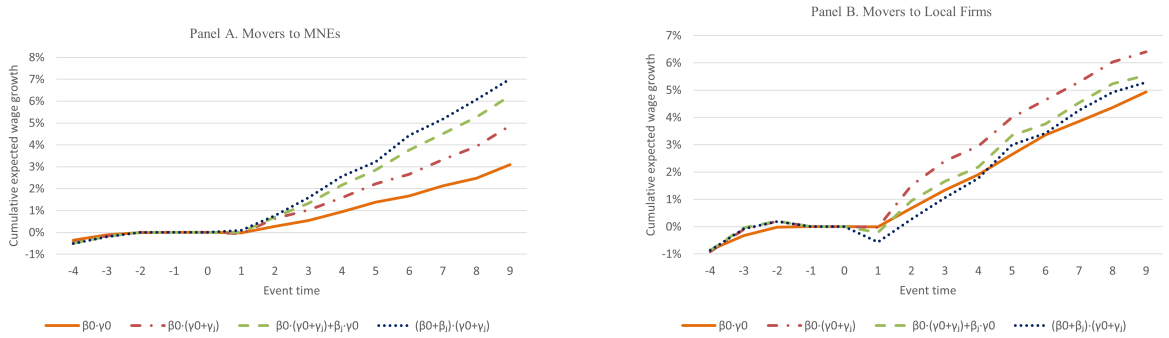


Figure 7: The Cumulative Expected Wage Growth for Movers.

Note: (i) $\beta_j^0\gamma_j^0$ is the amount of wage growth if work mobility is at the same rate as that for control firms and wage growth is at the same rate as that for stayers; (ii) $\beta_j^0\gamma_j$ is the amount of wage growth if mobility is at the benchmark level, but there is additional wage growth associated with moving to another MNE; (iii) $\beta_j\gamma_j^0$ is the amount of wage growth if there is an increase in mobility due to acquisition, but wage growth is at the benchmark level; and (iv) $\beta_j\gamma_j$ is the amount of wage growth if there are an increase in mobility due to acquisition and a rise in wage growth associated with moving to another MNE. See Section 3.3 for details.

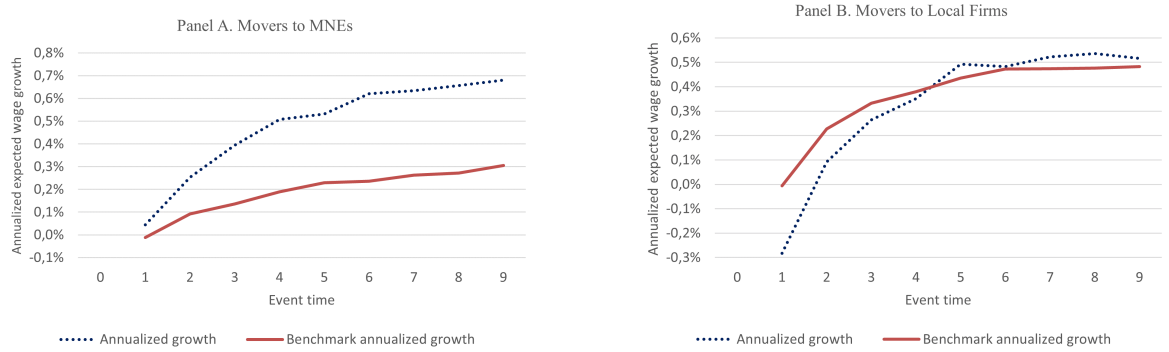
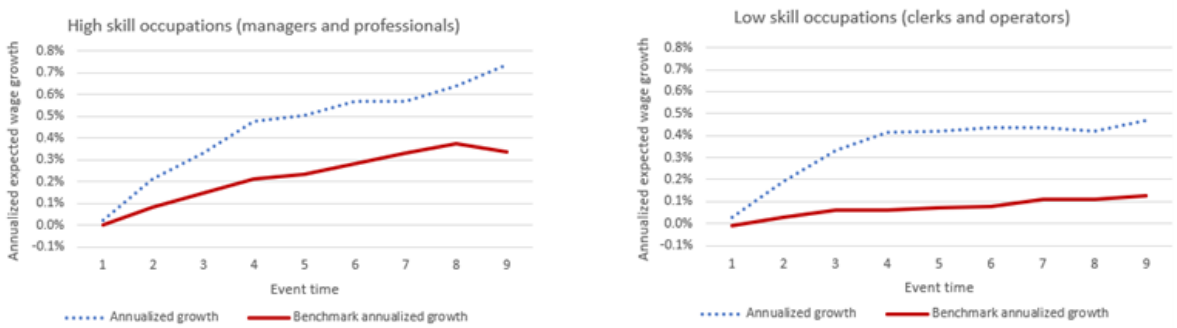


Figure 8: The Annualized Expected Wage Growth for Movers.

Note: The benchmark annualized growth rate is based on the mobility rate in control firms and the wage growth by stayers at the acquired firms. See Section 3.3 for details.

Panel A. Annualized expected wage growth for movers to MNEs



Panel B. Annualized expected wage growth for movers to local firms

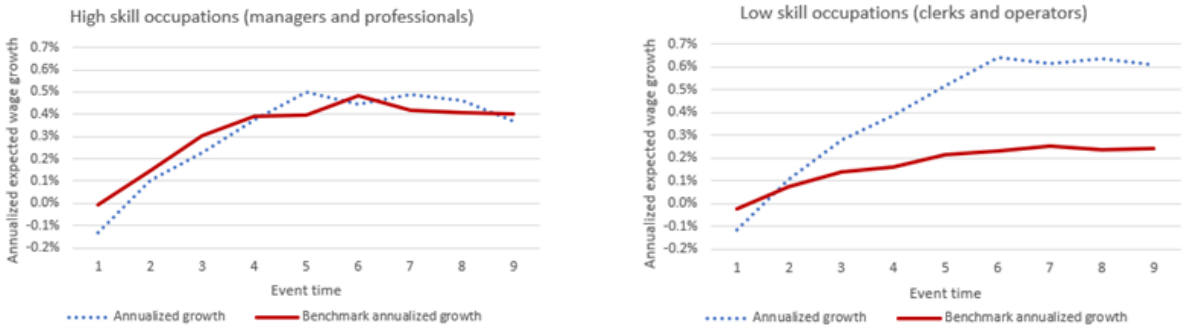


Figure 9: The Annualized Expected Wage Growth for Movers in Different Occupations.

Note: The benchmark annualized growth rate is based on the mobility rate in control firms and the wage growth by stayers at the acquired firms. See Section 3.3 for details.

Tables

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

		Treated	Control	Difference in means t-test	p-value
One year lag					
	Capital intensity	0.270	0.307	-0.037	0.861
	Log Firm size	3.347	3.334	0.013	0.254
	Profits	4559	4277	282	0.394
	Profits/Sales	-2.545	0.018	-2.564	0.838
	Sales	124074	124892	-818	0.523
	Share high-skilled employees	0.354	0.362	-0.008	0.949
	Share low-skilled employees	0.137	0.137	0.001	0.423
	Value added	34836	34514	322.500	0.463
Two years lag					
	Capital intensity	0.287	0.339	-0.052	0.874
	Log Firm size	3.212	3.205	0.007	0.352
	Profits	3865	10369	-6503	0.877
	Profits/Sales	-0.159	0.072	-0.231	0.934
	Sales	114555	113325	1229	0.463
	Share high-skilled employees	0.350	0.358	-0.008	0.931
	Share low-skilled employees	0.142	0.143	-0.001	0.569
	Value added	31545	31873	-328	0.541
One year difference					
	Capital intensity	0.007	-0.005	0.012	0.229
	Log firm size	0.083	0.085	-0.002	0.583
	Profits	-247	4361	-4609	0.904
	Profits/Sales	2.527	-0.024	2.550	0.162
	Sales	10534	6971	3562	0.035
	Share high-skilled employees	0.003	0.003	0.000	0.443
	Share low-skilled employees	-0.004	-0.005	0.002	0.068
	Value added	2933	3878	-944	0.729

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

	To MNEs		To local firms	
Event Year	Control	Treated	Control	Treated
$j = 1$	0.0374	0.0472	0.0976	0.0736
$j = 3$	0.0929	0.1129	0.2094	0.1522
$j = 5$	0.1184	0.1424	0.2172	0.1974

Note: Treated firms are acquired by foreign owners. Control firms are never acquired by foreign owners. MNEs (columns 1 and 2) consist of both Swedish MNEs and foreign owned firms. Local firms (columns 3 and 4) have no foreign affiliates.

Table 3: Wages for workers staying or leaving to other firm types (percent).

	Stayer		To MNEs		To local firms	
Event Year	Control	Treated	Control	Treated	Control	Treated
$j = 1$	2	-1	15	16	4	9
$j = 3$	6	13	17	18	-4	10
$j = 5$	6	16	17	25	16	12

Note: Treated firms are acquired by foreign owners. Control firms are never acquired by foreign owners. Stayers are workers who remain employed at the firm. Local firms have no foreign affiliates. MNEs consist of both Swedish MNEs and foreign owned firms.

Table 4: Estimating the effect of international experience on job mobility.

	To MNEs	To MNEs	To Local	To Local
Treated	0.009	0.019***	0.004	-0.015
	(0.006)	(0.005)	(0.011)	(0.010)
After	0.028***	0.023***	0.065***	0.052***
	(0.006)	(0.005)	(0.012)	(0.011)
DiD	0.041***	0.040***	-0.050***	-0.050***
	(0.008)	(0.008)	(0.014)	(0.013)
Female		-0.021***		-0.017***
		(0.002)		(0.002)
Experience		-0.004***		-0.006***
		(0.000)		(0.000)
Exp. Square		0.000***		0.000
		(0.000)		(0.000)
Schooling		0.002***		-0.007***
		(0.000)		(0.001)
(log) Size		0.003*		-0.013***
		(0.001)		(0.002)
High-skill		0.086***		-0.109***
		(0.009)		(0.012)
Capital int.		0.000		-0.000
		(0.000)		(0.000)
Observations	2,957,840	2,957,840	2,957,840	2,957,840
R-squared	0.022	0.048	0.021	0.052
Year FE	YES	YES	YES	YES
Industry FE	NO	YES	NO	YES

Note: Estimating the effect in the period up to nine years after the acquisition compared to the period up to four years before the acquisition. Standard errors are clustered at the firm-year level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: The effect of international experience on job mobility in the short-, medium-, and long run.

	To MNE	To MNE	To Local	To Local
DiD pre-medium	0.015	0.014	-0.024	-0.022
	(0.012)	(0.010)	(0.022)	(0.020)
DiD short-run	0.012	0.011	-0.051***	-0.051***
	(0.008)	(0.008)	(0.017)	(0.016)
DiD medium-run	0.047***	0.047***	-0.076***	-0.075***
	(0.013)	(0.012)	(0.022)	(0.021)
DiD long-run	0.065***	0.065***	-0.056**	-0.055**
	(0.015)	(0.014)	(0.024)	(0.023)
DiD longest-run	0.078***	0.076***	-0.046**	-0.047**
	(0.015)	(0.014)	(0.022)	(0.021)
Control variables	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	No	Yes	No	Yes

Note: DiD pre-medium measures years 3 and 4 before treatment. Years 1 and 2 before treatment are omitted as years of comparisons. DiD short-run examines the treatment effect in years 1 and 2, 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. See Table 4 for the included control variables. Robust standard errors in parentheses. Standard errors are clustered at the firm-year level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. All specifications include year fixed-effects. All specifications with firm controls also include industry fixed-effects. Standard errors are clustered by firm and year.

Table 6: The effect of international experience on job mobility to foreign and Swedish MNEs.

	Foreign	Swedish	Foreign	Swedish	Foreign	Swedish
	All firms		Above 500 employees		Above 1000 employees	
DiD	0.049***	-0.009*	0.053***	-0.024*	0.046***	-0.016
	(0.005)	(0.005)	(0.011)	(0.013)	(0.013)	(0.016)

Note: The coefficient shows the difference-in-differences coefficient from estimating equation (1) with mobility to foreign- or Swedish MNEs as dependent variable. All specifications include year fixed-effects, control variables, and industry fixed-effects. See Table 4 for the included control variables. The effect is estimated in the period up to nine years after the acquisition compared to the period up to four years before the acquisition. Standard errors are clustered at the firm-year level. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: The effect of international experience on job mobility using different time periods.

	MNE	MNE	MNE	MNE	MNE	Local	Local	Local	Local	Local
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	-4 to +9	-3 to +6	-2 to +6	-3 to +9	-2 to +9	-4 to +9	-3 to +6	-2 to +6	-3 to +9	-2 to +9
DiD	.040***	0.033***	0.038***	0.042***	0.047***	-0.050***	-0.054***	-0.061***	-0.051***	-0.059***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.013)	(0.014)	(0.014)	(0.013)	(0.013)

Note: The coefficient shows the difference-in-differences coefficient from estimating equation (1). All specifications include year fixed-effects, control variables, and industry fixed-effects. See Table 4 for the included control variables. Standard errors are clustered at the firm-year level. ***p<0.01, ** p<0.05, * p<0.1.

Table 8: The effect of international experience on job mobility to high-wage firms.

	To top-10% high wage firms	To top-20% high wage firms	To top-33% high wage firms
DiD	-0.003	-0.010	-0.014
	(0.004)	(0.006)	(0.011)

Note: The coefficient shows the difference-in-differences coefficient from estimating equation (1) with mobility to high wage firms as dependent variable. All specifications include year fixed-effects, control variables, and industry fixed-effects. See Table 4 for the included control variables. The effect is estimated in the period up to nine years after the acquisition compared to the period up to four years before the acquisition. Standard errors are clustered at the firm-year level.

Table 9: Estimating the effect of mobility on wages

		Movers to MNEs compared to stayers in treated firms		Movers to local firms compared to stayers in treated firms	
After		0.077***	0.100***	0.081***	0.101***
		(0.019)	(0.006)	(0.019)	(0.006)
Mover		0.033**	-0.031***	-0.183***	-0.188***
		(0.015)	(0.013)	(0.013)	(0.006)
After×Mover		0.090***	0.129***	0.047***	0.085***
		(0.018)	(0.017)	(0.017)	(0.007)
Female			-0.355***		-0.352***
			(0.003)		(0.003)
Experience			0.037***		0.037***
			(0.000)		(0.000)
Experience square			-0.001***		-0.001***
			(0.000)		(0.000)
Schooling			0.082***		0.078***
			(0.001)		(0.001)
(log) Size			-0.022***		-0.018***
			(0.001)		(0.001)
High-skill			0.494***		0.438***
			(0.010)		(0.010)
Capital Intensity			0.004***		0.005***
			(0.000)		(0.000)
Observations	1,206,218	1,206,218	1,298,064	1,298,064	
R-squared	0.034	0.264	0.028	0.200	

Note: Estimating the effect in the period up to nine years after the acquisition compared to the period up to six years before the acquisition. All estimations include year fixed effects and industry fixed effects. Columns 1-2 compare wage growth of movers to MNEs with that of stayers at acquired firms. Columns 3-4 compare wage growth of movers to local firms with that of stayers at acquired firms. Standard errors are clustered at the firm-year level. *** p<0.01, ** p<0.05, * p<0.1

Appendix

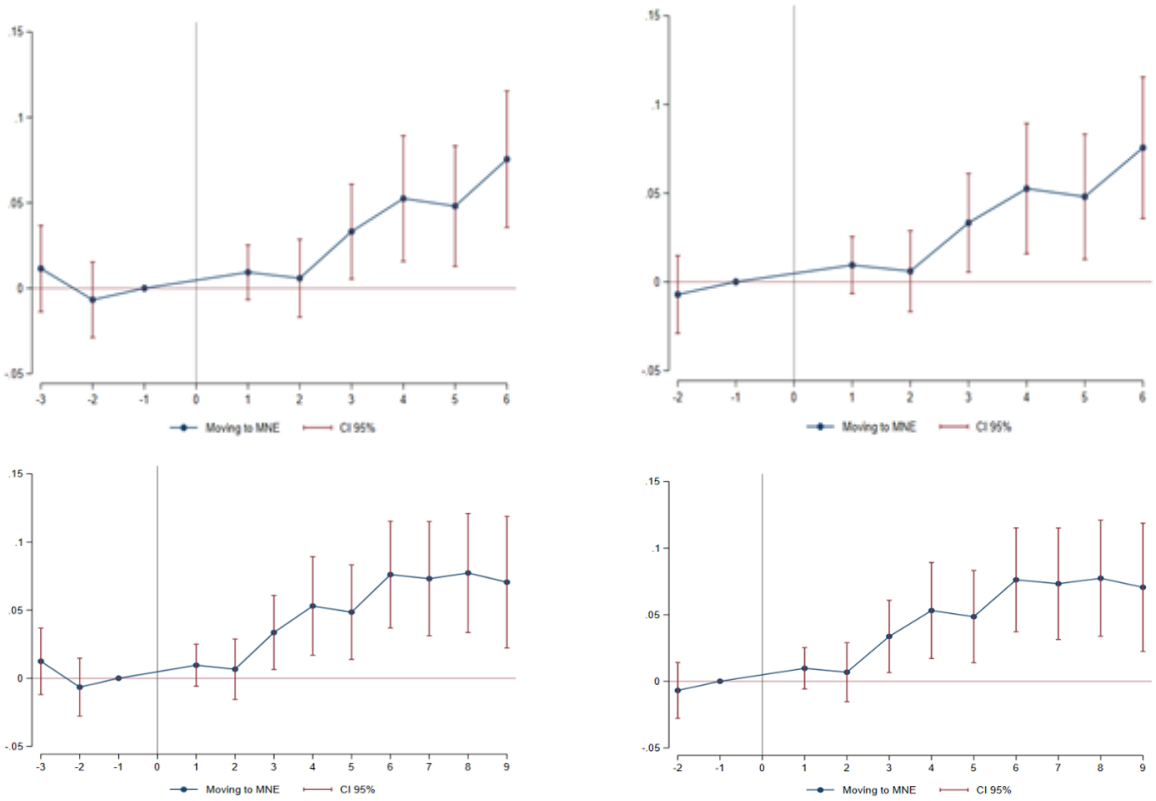


Figure A1. Event study. The effect of foreign acquisitions on job mobility to MNEs. Robustness estimations with different time periods.

Note: The figures show yearly difference-in-difference estimates relative to one year before the foreign acquisition (event time $t-1$). The vertical bars show the 95% confidence intervals. Control variables and fixed effects are included. For details on included variables see Table 4

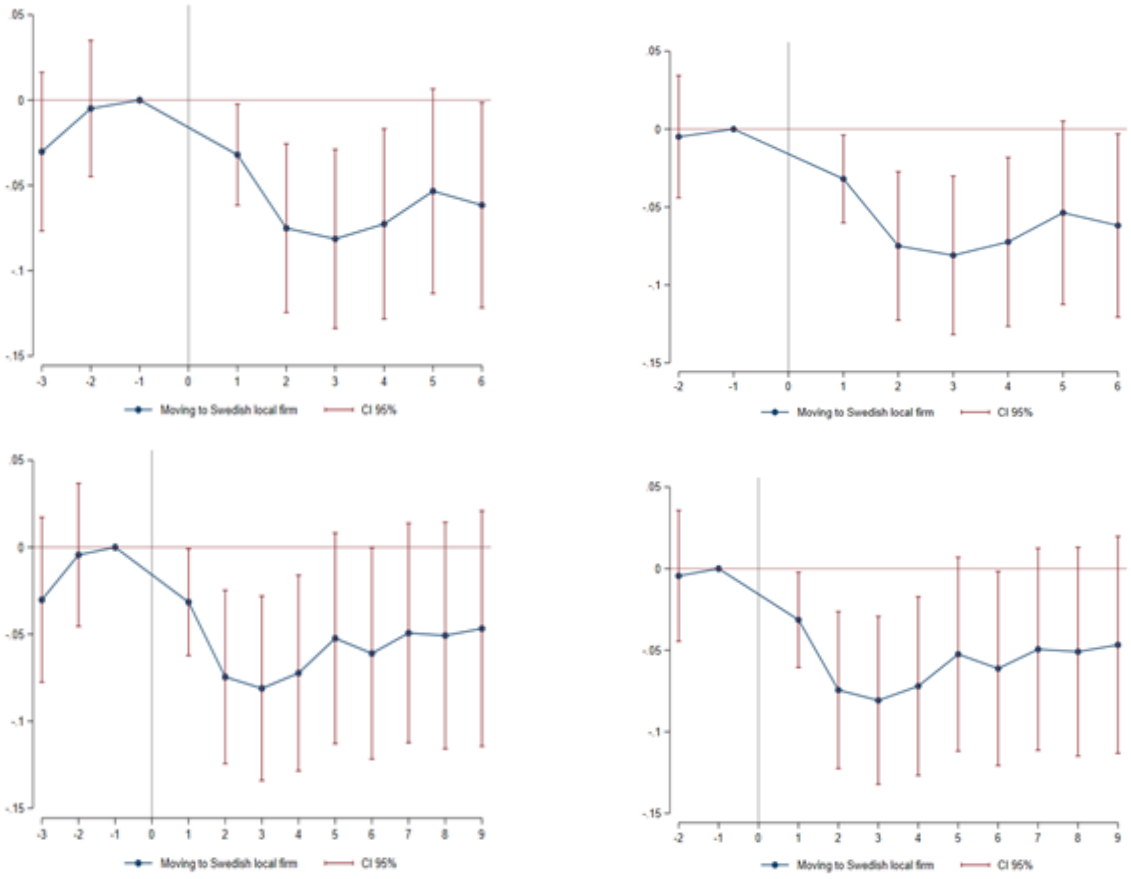


Figure A2. Event study. The effect of foreign acquisitions on job mobility to local firms. Robustness estimations with different time periods.

Note: The figures show yearly difference-in-difference estimates relative to one year before the foreign acquisition (event time $t-1$). The vertical bars show the 95% confidence intervals. Control variables and fixed effects are included. For details on included variables see Table 4.

Table A1. Indicators of covariance balancing before and after matching, 1998.

	Unmatched (U) and Matched (M)	Mean		%bias	-Δ%bias	t-test	p-value
		Treated	Control				
Lag 1 year							
Profits	U	20844	3974.9	12.5		1.37	0.170
	M	12291	2302.3	7.4	40.8	1.20	0.233
Sales	U	120 000	54161	15.4		1.43	0.152
	M	110 000	110 000	2.2	85.4	0.25	0.802
Value added	U	22603	15042	5.2		0.42	0.673
	M	22773	28609	-4.0	22.8	-0.96	0.338
Low level of skills	U	0.147	0.269	-73.1		-7.20	0.000
	M	0.147	0.1436	1.8	97.5	0.18	0.858
High level of skills	U	0.338	0.163	73.3		9.13	0.000
	M	0.338	0.314	9.9	86.4	0.71	0.479
Capital per employee	U	0.498	0.290	7.3		1.18	0.236
	M	0.185	0.156	1.0	85.8	0.64	0.524
Profits per sales	U	0.147	0.055	3.2		0.26	0.793
	M	0.090	0.021	2.5	24.0	2.09	0.037
Log firm size	U	3.330	2.786	52.6		6.75	0.000
	M	3.316	3.418	-9.9	81.2	-0.72	0.475
Lag 2 year							
Profits	U	-3879.3	1528.8	-8.3		-1.35	0.178
	M	2951.2	2014.3	1.4	82.7	0.42	0.677
Sales	U	110 000	52674	13.9		1.29	0.196
	M	110 000	93720	3.3	76.0	0.41	0.682
Value added	U	18922	14432	3.1		0.26	0.797
	M	19372	25347	-4.2	-33.1	-0.88	0.379
Low level of skills	U	0.150	0.275	-71.1		-7.00	0.000
	M	0.149	0.146	1.7	97.6	0.16	0.872
High level of skills	U	0.336	0.162	71.5		8.80	0.000
	M	0.336	0.322	5.5	92.3	0.39	0.699
Capital per employee	U	0.572	0.307	6.7		0.69	0.489
	M	0.259	0.160	2.5	62.9	1.34	0.181
Profit per sales	U	-0.040	0.036	-6.3		-0.53	0.594
	M	-0.004	-0.000	-0.3	95.2	-0.10	0.919
Log firm size	U	3.162	2.699	42.0		5.40	0.000
	M	3.149	3.264	-10.5	75.1	-0.76	0.447
One year difference							
Profits	U	-30726	101.45	-16.2		-2.72	0.006
	M	-11407	-2097	-4.9	69.8	-1.07	0.283
Sales	U	17970	5788.7	13.8		1.29	0.197
	M	18201	8724.9	10.8	22.2	1.32	0.188
Value added	U	-25.976	1313.6	-3.6		-0.39	0.695
	M	2745.4	-401.73	8.6	-134.9	1.35	0.177
Low level of skills	U	0.007	-0.004	14.3		1.38	0.167
	M	0.007	0.003	5.6	61.0	0.57	0.570

High level of skills	U	-0.009	-0.000	-11.3		-1.28	0.202
	M	-0.009	0.012	-28.4	-151.3	-2.02	0.045
Capital per employee	U	-0.045	0.014	-4.6		-0.37	0.708
	M	-0.019	0.006	-1.9	59.2	-1.02	0.308
Profit per sales	U	-0.199	0.062	-2.1		-0.17	0.868
	M	-0.064	-0.021	-0.3	83.6	-1.37	0.172
Log firm size	U	0.125	0.086	10.4		1.18	0.236
	M	0.125	0.111	3.8	63.6	0.29	0.769
Profit per employee	U	-0.248	-0.001	-13.4		-1.37	0.171
	M	-0.111	-0.158	2.6	80.9	0.38	0.707
Sales per employee	U	0.0743	0.024	2.7		0.27	0.789
	M	0.0763	0.120	-6.6	-142.0	-0.38	0.704
Value added per employee	U	-0.039	0.014	-13.6		-1.74	0.081
	M	-0.020	-0.151	33.7	-147.2	1.10	0.272
Share women	U	-0.009	0.002	-14.2		-1.54	0.125
	M	-0.009	-0.010	1.2	91.5	0.09	0.924
Export / Sales	U	-0.015	-0.001	0.8		0.15	0.884
	M	0.000	0.003	-3.2	-292.9	-0.25	0.804

Note: The matching also include industry dummy variables.

Table A2. Indicators of covariance balancing before and after matching, 2013.

	Unmatched (U) and Matched (M)	Mean		%bias	-Δ%bias	t-test	p-value
		Treated	Control				
Lag 1 year							
Profits	U	3245.6	3167.2	0.2		0.01	0.989
	M	3698.2	8.7198	8.1	-4604.2	0.70	0.483
Sales	U	110 000	60994	15.0		1.30	0.195
	M	95907	73899	6.1	59.2	0.76	0.448
Value added	U	33320	18580	20.2		1.73	0.084
	M	30125	29992	0.2	99.1	0.02	0.987
Low level of skills	U	0.112	0.156	-35.7		-3.14	0.002
	M	0.113	0.095	14.5	59.4	1.14	0.254
High level of skills	U	0.370	0.259	41.1		4.10	0.000
	M	0.367	0.409	-15.6	62.1	-0.99	0.324
Capital per employee	U	0.163	0.663	-10.7		-0.73	0.468
	M	0.163	0.118	1.0	91.0	1.00	0.318
Profits per sales	U	-0.030	-0.132	0.8		0.05	0.957
	M	-0.030	-0.017	-0.1	86.7	-0.17	0.861
Log firm size	U	3.422	3.077	39.0		4.03	0.000
	M	3.389	3.448	-6.7	82.9	-0.43	0.671
Lag 2 year							
Profits	U	3805	2994.3	2.3		0.18	0.854
	M	4995.7	9770.4	-13.4	-489.0	-0.65	0.516
Sales	U	99605	57796	13.1		1.21	0.226
	M	99832	71189	9.0	31.5	0.85	0.395
Value added	U	26809	17912	14.0		1.10	0.271
	M	26770	27110	-0.5	96.2	-0.05	0.961
Low level of skills	U	0.116	0.160	-34.7		-3.04	0.002
	M	0.116	0.103	10.4	70.0	0.79	0.432
High level of skills	U	0.367	0.255	40.5		4.10	0.000
	M	0.364	0.406	-15.1	62.7	-0.96	0.338
Capital per employee	U	0.167	0.641	-11.6		-0.79	0.430
	M	0.169	0.128	1.0	91.4	0.86	0.391
Profits per sales	U	0.057	-0.004	0.3		0.02	0.983
	M	0.072	0.127	-0.3	9.5	-0.56	0.574
Log firm size	U	3.296	3.024	30.8		3.12	0.002
	M	3.294	3.32	-4.0	87.1	-0.25	0.806
One year difference							

Profits	U	-1732.4	130.65	-6.0		-0.45	0.652
	M	-1243.4	2258.2	-11.3	-88.0	-0.82	0.411
Sales	U	42220	875.28	14.7		6.01	0.000
	M	1445.7	1887.4	-0.2	98.9	-0.10	0.918
Value added	U	7787.6	512.54	13.2		3.74	0.000
	M	73.868	256.25	-0.3	97.5	-0.10	0.921
Low level of skills	U	-0.001	-0.005	7.3		0.59	0.556
	M	-0.001	-0.004	5.3	27.7	0.58	0.564
High level of skills	U	0.0100	0.004	8.1		0.72	0.470
	M	0.0100	-0.000	13.9	-71.0	1.13	0.259
Capital per employee	U	-0.023	-0.013	-0.4		-0.03	0.977
	M	-0.023	0.004	-1.2	-179.9	-1.53	0.129
Profits per sales	U	-0.026	0.044	-0.4		-0.03	0.977
	M	-0.027	-0.006	-0.1	70.3	-0.31	0.758
Lof firm size	U	0.062	0.004	15.4		1.53	0.126
	M	0.048	0.023	6.7	56.7	0.58	0.562
Profits per employee	U	-0.077	-0.009	-4.2		-0.29	0.776
	M	-0.078	0.087	-10.1	-143.0	-1.38	0.170
Sales per employee	U	-0.057	-0.001	-4.4		-0.32	0.753
	M	-0.052	-0.024	-2.2	50.0	-0.27	0.785
Value added per employee	U	-0.048	0.010	-10.5		-0.79	0.429
	M	-0.047	-0.040	-1.4	86.9	-0.18	0.861
Share women	U	-0.007	-0.001	-7.9		-0.69	0.492
	M	-0.007	-0.005	-3.1	61.3	-0.25	0.799
Export / Sales	U	0.008	0.000	13.4		1.71	0.088
	M	0.008	0.004	6.7	49.8	0.46	0.648

Note: The matching included industry dummy variables.