Discussion paper

Do firms or workers drive the foreign acquisition wage premium?

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Abstract

Firms pay higher wages after they are acquired by a foreign owner. The post-acquisition wage premium can arise with worker changes (e.g. as the quality of employees rises) or with firm-level changes (e.g. as productivity improves). We propose a full-sample, dynamic decomposition of the wage premium for the universe of employer-employee relations in the Netherlands. After accounting for selection effects, we find a wage premium of foreign acquisition that rises from 1% to 5% in the years after acquisition. Firm-level premia account for three quarters of the premium, comprising a 3.5% wage increase. Worker-level premia are absent just after acquisition but rise over time to account for one fifth of the premium. Within firms, premia are higher for workers with a relatively high earnings capacity. Though industry variation and firm size class heterogeneity is considerable, the dominance of firm-level premia suggests that foreign acquisitions change firms beyond a workforce reshuffling.

Keywords: foreign acquisition, wage decomposition, matched employer-employee data, labor mobility, Netherlands
JEL Codes: F23, F66, J31, G34

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1 Introduction

Foreign-acquired firms pay higher wages than domestic firms. Across the developed world, estimates of the wage premium in foreign-owned firms range from 2% to 50% (Girma and Görg, 2007; Heyman et al., 2007; Huttunen, 2007; Andrews et al., 2009; Heyman et al., 2011; Hijzen et al., 2013; Earle et al., 2018). The wage premium of foreign-owned firms inspires policy makers to attract multinational firms, and it is a key indicator in the debate on how multinationals affect local labor markets. However, the source of the wage premium in foreign firms is not well understood.

Foreign-owned firms can pay higher wages because they hire higher-quality workers than domestic firms. Foreign firms' workers have higher levels of education and experience (Heyman et al., 2007; Andrews et al., 2009; Hijzen et al., 2013), and large shares of wage variation are linked to the worker rather than the firm they work for (e.g., Setzler and Tintelnot, 2021; Balsvik, 2011). If worker movement between firms explains the foreign acquisition wage premium, the anticipated benefits of foreign acquisitions are not clear. If foreign-owned firms merely herd productive workers, then the measured wage premia might reflect a transfer of productive workers from domestic to international firms, but not an improvement of overall productivity.

Foreign acquirers might also pay higher wages because they change the targeted firm. A foreign acquisition may increase the target's productivity, improve production practices and management, and promote on the job training of workers (Girma and Görg, 2007; Bircan, 2019; Koch and Smolka, 2019). Similarly, a multinational's expanding activities can increase its labor demand in the host economy and raise local wages (Kovak et al., 2021). Such firm-level transformations can benefit the local economy through higher aggregate productivity or spillovers (Haskel et al., 2007; Keller and Yeaple, 2009; Poole, 2013). However, there is less evidence that firm-level change explains multinational wage premia (e.g., Heyman et al., 2007).

We contribute to the literature by identifying the acquired-firm premium (i.e., the wage premium for the same worker in an acquired firm relative to a domestic firm) and the worker-level premia (i.e., the wage premium that arises as the worker composition of the acquired firm changes). We employ a two-way fixed effects model for wages to identify both fixed effects for workers and time-varying fixed effects for firms (Abowd et al., 1999; Engbom et al., 2022). We then estimate the causal impact of a foreign acquisition on the wage premium and its constituent components, accounting for target selection with a matching strategy.

We employ the universal matched employer-employee dataset of the Netherlands for the years 2006 to 2018, combined with the ownership status of firms. Given the economic openness of the country, the availability of highly detailed data and the relatively large number of foreign acquisitions, the Netherlands is a particularly useful case for a comprehensive empirical analysis on the wage effects of foreign acquisitions. The high detail in the data allows us to identify estimates for virtually all firms and workers, which enables precise control for selection effects in our identification strategy. Furthermore, the large number of acquisitions in our sample offers a unique and unexplored opportunity to document industry variation and firm size class heterogeneity in the firm wage adjustments after acquisition.

To rule out that the selection of firms for acquisition explains our wage premia, we use propensity scores before acquisition to match acquired firms to similar domestic firms in the same sector. Pre-acquisition size, wage variation, age and export status are used as covariates to explain the acquisition. We then identify the wage impacts (and all its constituent explanations) by comparing over 1,200 acquired firms to their matched domestic firms. For a structural comparison between the firms, we employ a difference-in-differences regression with three years of lags and leads to study pre-trends and the dynamic impact, while differencing out fixed effects for all firms, and for matched pair-year combinations.

The results show a wage premium of about 1.4% in the year of acquisition, growing up to 5% over the three years after the acquisition. Decomposing the wage premium, we find that firm-level premia rise from 1.1% to 3.6% in post-acquisition wage growth, accounting for roughly 75% of the premium. In the year of acquisition, differences in workforce composition between foreign-owned and domestic firms do not explain the acquisition premium. However, worker-level premia rise over time, explaining 0.7% in wage growth by the third year after acquisition, or about 15% of the acquisition premium. These findings contrast the majority of the literature that stresses workforce composition as the explanation for multinational wage premia. We find that the workforce composition effect on wages is explained by a higher earnings capacity of newly hired workers, with no significant changes in exiting workers and very minor changes to the rate of hiring after the acquisition. We report larger firm-level premia in medium-sized firms, and that worker mobility is more important in explaining post-acquisition wage growth in high-tech manufacturing and knowledge-intensive service sectors. Within the firm, acquisition premia increase faster for workers with a higher earnings capacity. This group of beneficiaries contains relatively many managers and professionals.

Our study adds to a growing literature that splits the multinational wage premium into firm and worker contributions. It commonly employs the AKM (Abowd et al., 1999) decomposition of wages into premia explained at the firm level and premia explained at the worker level. For the United States, Setzler and Tintelnot (2021) show that a multinational's worker composition explains two thirds of its wage premium, conditional on a fixed effect for grouped firms (Bonhomme et al., 2019). Balsvik (2011) decomposes the wages of multinational and domestic firms in Norway, reporting that worker fixed effects explain the majority of the full multinational wage premium.

Our approach differs from this literature in two important dimensions. First, we analyze the dynamic effect of a foreign acquisition, tracing the development of wage components in the years after the event. We can estimate year-on-year impacts as our approach estimates time-varying fixed effects for firms, rather than static fixed effects or grouped fixed effects. This contrasts the static premium for multinational firms estimated in other studies. Second, we employ a matching strategy to rule out that the acquisition premium is driven by target selection of the domestic firms (indeed, we find significant wage pretrends to acquisition in our unmatched sample). Relative to this literature, we find a large role for firm-level wage premia after an acquisition, while the worker composition effects are small and slow to materialize. An explanation is that after the acquisition, the firm starts to hire new workers of higher quality, but as there is no significant change in employee turnover, the average quality of workers changes slowly. The slow materialization of worker composition effects coincides with productivity results at the firm level, which show delayed improvements in the technical and labor productivity after a firm's acquisition (Chen, 2011; Fons-Rosen et al., 2021).

An earlier literature separates firm contributions to wage premia by isolating samples of workers that stay inside the firm, or separates worker composition effects by isolating subsamples of moving workers. While most studies find a positive multinational wage premium, firm-level wage premia identified from stayers are sometimes zero (Germany, Portugal, the United Kingdom, Brazil and Indonesia in Hijzen et al., 2013), negative (Heyman et al., 2007 for Sweden), or confined to highly skilled workers only (Heyman et al., 2011). A minority of studies find positive firm-level premia (Andrews et al., 2009 and Egger and Jahn, 2020 in Germany), or argue that the firm-level premia materialize only for workers that were high earners in domestic firms (Martins, 2011 for a sample of Portuguese job movers towards multinational firms, compared to stayers).

Relative to this literature, our main contribution is to provide a complete decomposition approach. In methodologies based on subsample identification, the constituent parts usually do not add up to the total estimated wage premium. In contrast, using the full employer-employee network, our decomposed wage premia add up to the total wage premium by construction and highlight firm-level premia as the main driver of the acquisition wage premium.

2 Methodological Approach

We examine the impact of a foreign acquisition on wages, and worker- and firm-level variation in wages within a difference-in-differences framework. The framework compares the development of wages in acquired firms to wage developments in firms that remain domestic.

Domestic firms are arguably not plausible counterfactuals for foreign acquired firms (had they not been acquired), as the group differ along several dimensions. Therefore, we use pre-acquisition characteristics to match acquired firms to firms that remain domestic. The propensity score matching approach is a prevalent solution to eliminate potential biases from firm target selection in difference-in-difference estimates (e.g., Huttunen, 2007; Girma and Görg, 2007; Heyman et al., 2007; Hijzen et al., 2013; Bastos et al., 2018; Orefice et al., 2019; Koch and Smolka, 2019; Egger et al., 2020). We use the difference-in-differences framework to identify post-acquisition wage premia and the worker composition and firm developments effects that contribute to it. However, the worker- and firm-level contributions cannot be observed directly. Therefore, we first employ the universal employer-employee dataset to decompose wages into wage variation attributable to the firm and to the worker (in observed and unobserved characteristics). We then match firms and employ the difference-in-differences framework to identify a causal effect of foreign acquisitions. The next subsections lay out the steps of our empirical strategy in detail.

2.1 Difference-in-differences framework

We exploit the variation in ownership status that arises from foreign acquisitions of domestic firms to identify a causal effect of foreign ownership. Our main specification is a difference-in-differences regression with three years of lags and leads that compares firm- and worker-level changes in acquired and non-acquired firms. The specification takes the form

$$y_{jmt} = \sum_{s=-3}^{3} \delta_s FA_{jms} + \omega_{mt} + \Psi_j + u_{jmt}, \tag{1}$$

where j and t index the firm and the calendar year; y_{jmt} is the firm-level outcome of interest (wages; wage variation attributable to the firm; wage variation attributable to the worker). The dummies FA_{jms} identify observations relative to the year of foreign acquisition at $s = 0$, and are zero for non-acquired firms. We drop the relative time dummy for the pre-acquisition year, so that the coefficients of foreign ownership, δ_s, capture changes in firm-level outcomes relative to the pre-acquisition year. Finally, u_{jmt} is an error term.

There are two fixed effects in the specification. The first is a time-varying fixed effect ω_{mt} for every matched pair m of acquired and non-acquired firms. The matching procedure is described in more detail below. With this fixed effect, yearly (log) wage developments in the acquired firm are estimated relative to the developments in the matched domestic firm that serves as a counterfactual non-acquired firm. The fixed effect thus prevents biases from time-varying omitted variables that both firms in the pair experience, such as local policy changes, demand fluctuations or labor market developments, from contributing to the identification of the impact of acquisition. Second, the specification contains a firm-level time-invariant fixed effect, Ψ_j, which controls for any unobserved firm-level confounders and prevents level differences between the firms from explaining the estimated wage premium effects.

2.2 Wage decomposition

To explain wages after an acquisition, we decompose the wage into a worker-specific unobserved components, firm-level pay, and observable characteristics of the worker. We use a variant of the AKM decomposition (Abowd et al., 1999) that allows firm contributions to vary by calendar year (Engbom et al., 2022). The log wage is modeled as

$$ln(w_{ijt}) = \alpha_i + X_{it}\beta + \psi_{jt} + \gamma_t + \epsilon_{ijt}, \tag{2}$$

where i, j and t index worker, firm and calendar year; $ln(w_{ijt})$ is log real hourly wage; α_i is a time-invariant worker fixed effect; ψ_{jt} is a firm fixed effect that varies by calendar year; γ_t is a calendar year fixed effect; $X_{it}\beta$ is a wage-age profile; and ϵ_{ijt} is an error term.

In the estimating equation (2), the worker fixed effects, α_i, capture time- and employer-invariant worker-level wage premia. They are often interpreted as measures of worker productivity and capture workers' observed and unobserved capacities to earn wages, such as skill. The wage-age profile $X_{it}\beta$ captures age and labor market experience-dependent developments of individual wages, through a third order polynomial that is flat at the age of 40 (Card et al., 2013). The firm fixed effects, ψ_{jt}, identify the firm-level premium that is estimated conditional on the observed and unobserved characteristics of the workforce composition. Firm fixed effects represent a wage premium that is common to all employees at a given firm in a given year: When taking up employment elsewhere, a worker loses the benefits of the previous employer's firm fixed effect and gains the benefits of the new employer's firm fixed effect.

The estimation of equation (2) forms the full wage decomposition as the components add up to the full observed wage for every worker. The firm fixed effects are estimated conditional on the fixed effects of its workforce, and the worker fixed effect is identified conditional on the employer's fixed effect. The estimation leverages both movers' and stayers' wage changes for the identification of the fixed effects (Engbom et al., 2022).

For our difference-in-differences regressions, we aggregate the worker-level wage components to the yearly firm level. At the firm level, the approach fully separates the mean of log wages (or equivalently the log of the geometric mean of wages) into firm fixed effects, the mean of the individual fixed effect of the workers employed in the firm, and the mean observed characteristics of workers as defined by the wage-age profile.

The decomposition is demanding on the frequency of mobility of workers in the dataset. Firm fixed effect estimates are conditional on worker fixed effects, and sufficiently many workers need to move to disentangle the two and avoid limited mobility bias (Andrews et al., 2008; Jochmans and Weidner, 2019; Kline et al., 2020). In Section 5.1, we explore the sensitivity of our main estimates to the degree of mobility (Jochmans and Weidner, 2019), finding that the mobility poses limited concern for our results.

2.3 Matching non-acquired firms

Every acquired firm needs to be paired to a non-acquired firm in the difference-in-differences comparison. Targets for foreign acquisitions generally differ substantially from most domestic firms in wages, wage dynamics and workforce (e.g., Almeida, 2007; Hijzen et al., 2013; Orefice et al., 2019), and we confirm substantial differences in levels and growth of employment, wages, and fixed effects between domestic and target firms in our data (see the table in Appendix B.3). As a consequence, wage change following an acquisition can be conflated with (pre-acquisition) firm development differences. While the fixed effects in our difference-in-differences estimation account for static differences between firms, they cannot address growth differences. To ensure that our identification of the wage impacts are caused by the acquisition, and not by ex-ante differences, we match acquired firms to domestic firms that are very similar before the acquisition.

For every acquired firm, we select groups of firms that could plausibly have been acquired but were not. We first divide the firms into industry-year groups. Within each industry-year group, we estimate the firms' propensity to be acquired in the next year using a group-specific logistic regression. As covariates, we use mean ln wage, ln employment, firm fixed effects, worker fixed effects and their one- and two-year growth rates; the within-firm variance of worker fixed effects; ln firm age; and ln real value of exports. Then, we match firms on propensity scores by nearest neighbour matching without replacement across firms, which produces unique pairs of matched firms. We restrict the differences between matched firms by allowing propensity score differences within matched pairs of at most 0.2 times the standard deviation of propensity scores within the industry-year group (Austin, 2011). The descriptive statistics of the matched sample are in Section 3.1.

Our matching procedure relies on the untestable conditional independence assumption, which implies that, conditional on the matching covariates, the assignment of foreign acquisitions is random between matched firms. We select the matching covariates to minimize observed differences between firms which could explain wage differences, such as firm size and exports. We also include growth rates of the wage components in the propensity score estimation to mitigate the risk of capturing spurious pretrends with our difference-in-differences coefficients. In Section 3.1, we report the balance in covariates after matching. In Section 5.3, we also discuss the consequences of using different sets of covariates for matching and of employing coarsened exact matching instead of propensity score matching.

3 Data and Sample Selection

We employ two types of administrative data of Statistics Netherlands. First, we assemble the universal matched employer-employee dataset for the years 2006 to 2018 based on information that employers send to the Dutch national employment agency (Uitvoeringsinstituut Werknemersverzekeringen). This source delivers detailed information on workers' demographics, total income and total hours worked. Different from many other matched employer-employee data, wages are not subject to censoring and with information on around 9.35 million employees and 0.77 million employers the dataset covers virtually all workers and firms in the Netherlands. High coverage is required for the identification of fixed effects in a firm-worker network structure. We focus our estimation on the subset of firm fixed effects that are connected through worker movements. The subset covers more than 99% of workers and more than 90% of firms with employees. In appendix B, we describe in detail how we compile the dataset.

Our main difference-in-differences regression focuses on the firm level. As explained, we aggregate worker-level wages and wage components to the firm level by taking yearly averages. For all firms, we add yearly information on NACE industry classification, age, real value of exports and ownership structure from Statistics Netherlands. Because this information is not available for firms from the financial sector, we remove these firms from the sample after the identification of the fixed effects.

We identify a firm as foreign owned if the ultimate owner, which controls strategic decisions, is non-Dutch. While the precise day of a foreign acquisition is unobserved in the data, we can identify the date on a yearly basis as a change of ultimate owner from Dutch in the previous year to foreign in the current year. To limit our scope to foreign acquisitions of Dutch domestic firms, we remove all firms that ever reported foreign affiliates under Dutch ownership or were ever foreign owned before the acquisition. For our difference-in-differences estimation, we select foreign acquisitions for which we observe the firm in all three years before and after the acquisition.[1] In addition, we drop acquired firms with fewer than 5 workers in these years, and we drop firms that reverted to Dutch ownership before 2018 in order to avoid estimating the consequences of divestment. In total, we identify 1,357 foreign acquisitions over the years 2009 to 2015 that meet these requirements.

3.1 The matched sample

We apply a two-step procedure for the propensity score matching. First, we select potential control firms by the same criteria as target firms. We require the firm to be neither foreign owned nor to have foreign affiliates, to employ at least five workers, to be continuously present in the data for seven years and to be in the same 2-digit

[1] The survival requirement after acquisition could introduce a sample selection bias if foreign ownership systematically decreases the probability of firm survival. Earlier research suggests no negative link between foreign ownership and firm survival (Bandick and Görg, 2010). This is confirmed in our data, as on average 88% of Dutch domestic firms (standard deviation 2.2) and 92% of foreign-owned firms (standard deviation 3.3) survive year-on-year.

NACE industries as the foreign-acquired firms. This selection procedure results in 71,681 potential control firms. Then, we sort the firms into industry-year groups (2-digit) and apply the propensity score matching procedure as explained in Section 2.3. This approach yields matches for 1,009 acquired firms in the same 2-digit industry class. For the remaining set of firms, we relax the industry requirement and match firms that are in the same 1-digit industry class, producing 260 additional matches. Limiting the estimation to matches in 2-digit industry groups has no influence on the results (see Table D2 in the appendix).

In total, we find matches for 1,269 target firms. Table B2 in the Appendix presents mean normalized differences of matching covariates between target and control firms in the matched and unmatched sample (normalized by the variation across target firms before matching) (Imbens and Wooldridge, 2009). Matching reduces the mean of these differences from 0.2599 in the unmatched sample to -0.0037 in the matched sample. All differences in the matched sample are well below the threshold of 0.25 suggested by Imbens and Wooldridge (2009), which indicates that our matching approach balances the data well. We discard all unmatched firms and balance the sample to three years before and three years after the acquisition year. Our estimation of the difference-in-differences coefficients proceeds on this balanced sample.

Relative to earlier research on the acquisition premium, our matched sample contains a large number of foreign acquisitions. The 1,269 foreign acquisitions are diverse in terms of industry and firm size. Figure A1 in the appendix shows this heterogeneity, by plotting acquisition numbers by pre-acquisition firm size class and (broad) industry. In terms of size, the average target firm employs around 45 workers (standard deviation 135) and the distribution of employment across firms is right-skewed with about half of the firms employing less than 20 workers. About 8.5% of the firms in our sample employ 100 or more workers in the pre-acquisition year.

This large variation in employment size results from the broad industry coverage of our sample. More than two-thirds of the acquisitions come from three industries (see Figure A1 in the appendix). Most acquisitions are in Wholesale and Retail Trade (509), followed by Manufacturing (218) and Professional, Scientific and Technical Activities (149). Targeted Wholesale and Retail Trade firms tend to employ fewer workers and their proportion of acquisitions shrinks from 49% to 14% across the employment size classes. The share of manufacturing acquisitions, on the other hand, rises from 12% to 32% with the size class.

4 Results

In our main set of results, we estimate what share of the post-acquisition wage premia is accounted for by changes in the wage premia of the firm itself, and what share can be explained by changes to the workforce. We also document the heterogeneity of the acquisition premium across industries and firm sizes. Within the firm, we explore the varying impact of foreign acquisitions on different segments of the workforce in terms of earnings capacity. Finally, we analyze what drives worker-level premia by looking at changes in the quantity and type of workers entering and exiting firms after an acquisition.

Table 1: Decomposition of the acquisition wage premium.

Years since acquisition	ln Wage (1)	Firm FE (2)	Worker FE (3)	Age profile (4)
$s = -3$	0.0027	0.0024	-0.0011	0.0014
	(0.0030)	(0.0022)	(0.0020)	(0.0013)
$s = -2$	0.0017	0.0019	-0.0004	0.0002
	(0.0025)	(0.0017)	(0.0016)	(0.0010)
$s = 0$	0.0140***	0.0107***	0.0024	0.0009
	(0.0025)	(0.0017)	(0.0015)	(0.0010)
$s = 1$	0.0289***	0.0203***	0.0055**	0.0032**
	(0.0031)	(0.0022)	(0.0019)	(0.0012)
$s = 2$	0.0366***	0.0264***	0.0052*	0.0051***
	(0.0034)	(0.0024)	(0.0021)	(0.0014)
$s = 3$	0.0486***	0.0354***	0.0073**	0.0059***
	(0.0039)	(0.0029)	(0.0024)	(0.0015)
Fixed-effects				
Firm ID (2,538)	✓	✓	✓	✓
Pair-year (8,883)	✓	✓	✓	✓
Observations	17,766	17,766	17,766	17,766
R^2	0.9665	0.9097	0.9740	0.9306
Pre-trends				
P-value	0.6635	0.4652	0.8477	0.4773

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Clustered standard errors (Firm ID) in parentheses. s identifies years since acquisition. Dependent variables are firm-level averages of the decomposition on (2). Estimated using difference-in-differences regression (1) on propensity score matching sample. The regressions include a fixed effect for each firm and each year of matched pairs of firms. Propensity scores are estimated within industry-year groups and using firm-level characteristics at $s = -1$; see Section 2.3 for details. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

4.1 Firm and worker contributions to the post-acquisition wage premium

Table 1 presents the results of the difference-in-differences regressions that compare wage developments in acquired firms to the counterfactual matched firms.[2] Column 1 shows the impact on the mean log wage, and Columns 2-4 show the impact of separate firm- and worker-level wage components that jointly explain the overall log wage development (as decomposed from the AKM model, see Section 2.2).[3] We cluster standard errors at the firm level to account for within-firm serial correlation of errors. In Section 5.4, we explore alternative approaches for calculating these standard errors.

Column 1 of Table 1 shows a statistically significant wage premium of around 1.41% (or 0.014 log points, $e^{0.014} \approx 1.0141$) of the acquired firm in the year of acquisition.[4] The wage premium grows over time, to 2.93%, 3.72% and 4.98% in the first, second and third year after the acquisition has taken place.

Column 2 shows the development of the firm fixed effect after acquisition. In the year of acquisition, the firm fixed effect of the acquired firm is over 1% higher than that of the non-acquired counterfactual. This difference grows over the years after acquisition in tandem with the overall wage increase as documented in Column 1. The development of the firm fixed effect is significantly different from zero for all post-acquisition years. Column 3 shows the development of the average worker fixed effect in the acquired firm. The magnitude is considerably lower than that of the firm fixed effects in Column 2, with statistically significant increases of around 0.5 to 0.7% in the years after acquisition. Finally, Column 4 shows the development of wages attributable to workers' observed characteristics, such as higher age associated with higher pay.

Through the decomposition on (2), firm fixed effects, firm-level average worker fixed effects and the age profile fully explain the wage premium. The share of the wage premium explained by firm fixed effects is largest in the acquisition year where it explains 76% ($\approx 0.0107/0.0140$). Over the following three years, growth in firm fixed effects steadily explains around 70 to 73%. By comparison, the workforce of acquired firms plays a smaller role throughout the three post-acquisition years. One year after the acquisition, when the regression coefficients become statistically significant, changes in average worker fixed effect are most important and explain 19% of the wage premium. Two and three years after the acquisition, they explain 14% and 15%. Changes in the age profile explain less of the wage premium, with 11% one year after the acquisition, 14% in the second and 12% in the third year. Changes in worker composition thus appear to be less important, while the development of firm fixed effects explains the immediate wage premium and remains its main driver throughout the following years.

The causal interpretation of the results in Table 1 assumes that, conditional on level differences, acquired firms would have developed similarly to their matched partners if they had not been acquired. To assess whether

[2]In table D1 in the appendix, we present results for the unmatched sample. This approach ignores differences in propensity scores and compares firms within 2-digit industries. Within industries the results violate the parallel trends assumption of the difference-in-differences estimator, suggesting that propensity score matching eliminates pre-trends.

[3]Figure A2 in the appendix shows the results visually.

[4]As explained in Section 3, we identify acquisitions on a year-on-year basis, whereby the unobserved exact date of acquisition lies within the acquisition year. In consequence, our estimates at $s = 0$ only partially capture the effect of acquisition.

the trends were parallel before the acquisition, we report the p-value of the joint Wald test on the pre-acquisition coefficients ($s = -3$ and $s = -2$). All p-values are higher than any conventional level, showing no signs of divergence between the acquired firm and the matched firm before the acquisition year.

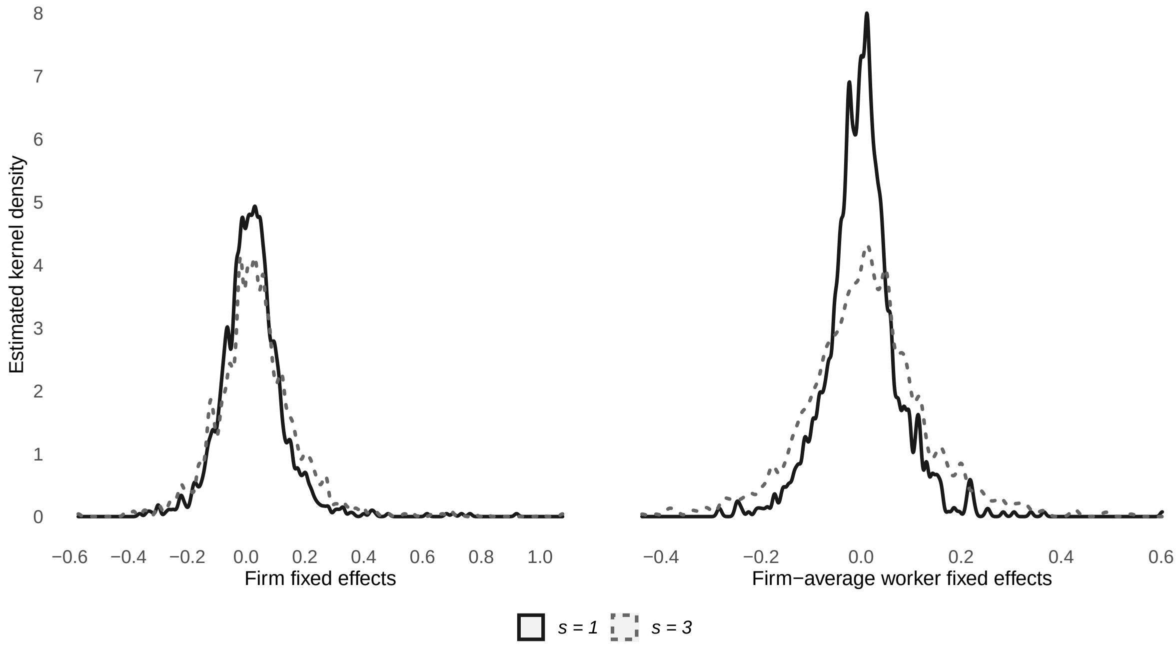
4.2 Firm size and industry heterogeneity

The estimates of firm and worker-level premia in Table 1 contain considerable heterogeneity. To visualize the distribution of post-acquisition development, Figure 1 shows the distribution of firm and worker (averaged by firm) fixed effect point estimates, one and three years after the acquisition. In the left-hand panel, the firm-level fixed effects distribution shows higher average fixed effects three years after acquisition, but also larger tails, in particular in the right tail. Similarly, the firm-averaged worker fixed effects show larger dispersion after the acquisition. If all firms and workers benefited similarly from an acquisition, the distribution would shift rightward. Instead, the increase in dispersion signals variation across workers and firms in the wage premia associated with acquisitions.

The wage impacts of a foreign acquisition may vary by the size of the acquired firm. Models of search frictions and imperfect labor markets predict that larger, more productive firms pay higher wages (e.g., Burdett and Mortensen, 1998) and employ more expensive workers (e.g., Card et al., 2018). Moreover, larger firms engaged in internationalization may screen their workers better and pay higher wages (e.g., Helpman et al., 2010). The scope for productivity improvements through transfers of technology, knowledge and management practices may also differ with the size of acquired firms. Table 2 shows the estimates of the baseline regression for firm and worker fixed effects after acquisition by size class of the firm (determined before acquisition). There is a significant positive impact of acquisitions on firm-level fixed effects for all size classes (Columns 1, 3, 5 and 7). However, the impact on firm-level fixed effects is largest in medium-sized firms, while the coefficients for firms with less than 20 and firms with more than 100 employees are lower (and less precisely estimated). A joint Wald test on the post-acquisition coefficients in a pooled regression shows a significant deviation in the coefficients for firms with 50-99 employees and large firms with over 100 employees ($\chi^2(4, 6322)$=3.5, $p < 0.01$) as well as for firms with under 20 employees ($\chi^2(4, 6322)$=2.08, $p < 0.1$). The firm-average worker fixed effect impacts also vary substantially with firm size. For firms of size 20-49 and of size 50-99, acquisition leads to significant improvements in the worker fixed effects. For small firms (under 20 employees) and large firms (over 100 employees), the coefficients are smaller and insignificant. The difference is significant for small firms ($\chi^2(4, 6322)$=2.13, $p < 0.1$) but not for large firms ($\chi^2(4, 6322)$=1.10, $p = 0.35$).

The firm's use of technology can also moderate the wage impacts of an acquisition (Syverson, 2011). Firms with superior technology and knowledge may demand different workers and pay higher wages to prevent leakage of their productivity advantage through worker turnover (e.g., Fosfuri et al., 2001). Similarly, access to domestic technologies and knowledge is probably an important motive for acquisitions in technology-intensive sectors and this might impact firm-level and worker fixed effects differently. To examine whether the premia vary with the use of technology, we run our analysis on a sample split according to Eurostat's definitions of knowledge-intensive

Figure 1: Distribution across firms of the impact estimates of a foreign acquisition on firm and worker fixed effects (kernel density of difference in differences coefficients).



Notes: The figure shows kernel density estimates of individual difference-in-differences point estimates across matched pairs of firms in the propensity score matching sample. The left panel shows the density for firm fixed effects in the decomposition on (2) as the dependent variable. The right panel shows the density of firm-level averages of worker fixed effects in the decomposition as the dependent variable. The kernel density estimation uses the Sheather and Jones (1991) method to select the bandwidth for the Gaussian kernel. s identifies years since acquisition. Propensity scores are estimated within industry-year groups and using firm-level characteristics at $s = -1$; see Section 2.3 for details.

and high-tech sectors.[5] Table 3 shows the regressions in the respective samples. For services, firm fixed effects (Columns 1 and 3) are more important in explaining the acquisition premia than worker fixed effects. For knowledge-intensive service sectors, the estimated wage premium explained by the firm fixed effect is more than twice as large as for non-knowledge intensive sectors. A Wald-test on the pooled sample shows that the difference between the sectors is statistically significant ($\chi^2(4, 5024)$=7.32, p $<$ 0.001). In knowledge-intensive service sectors, there is some evidence for a role of worker fixed effects too (Column 2), while for other service sectors the average worker fixed effect is noticeably unaffected by an acquisition. However, the difference in worker fixed effects between the sectors is statistically insignificant ($\chi^2(4, 5024)$=1.59, p = 0.17). Among manufacturing firms, firm fixed effect responses explain acquisition premia both in the low- and high-tech industries (Columns 5 and 7). The measured impact is in fact larger in low-tech industries, but the difference between the coefficients is also statistically insignificant ($\chi^2(3, 844)$ = 2, p = 0.11). In contrast to services, worker fixed effects are more important than firm fixed effects in high-tech manufacturing just after the acquisition: In the first two years, the estimates for worker fixed effects are significant and larger than the estimates for firm fixed effects.

Altogether, these results suggest that the largest improvements in pay are driven by firm-level changes, especially for firms employing less than 100 workers. However, there are significant contributions from worker-level fixed effects in firms between worker size 20 and 99, and in knowledge-intensive services and high-tech manufacturing.

4.3 Within-firm wage premia

Our results imply that after a foreign acquisition, firm-level changes explain most of the wage increase. However, the firm-level impact of the acquisition might not apply equally to all workers: foreign acquisitions can lead to higher wage growth in top layers of the firm (Bastos et al., 2018) and largely benefit managers and CEOs (Heyman et al., 2011). Indeed, the results in Section 4.2 suggest that firm-level premia may end up with specific parts of the organization. Therefore, we explore whether changes in the firm fixed effects apply differently across different groups of workers within the firm.

We estimate wage developments for groups of workers within the firm as follows. The mean residual from decomposition (2) for each firm is zero as yearly firm-level fixed effects are included. For subgroups of workers within each firm, we recover their average pay deviation relative to the overall firm as the average residual within the group. We subsequently calculate the yearly group-within-firm fixed effect as the group's deviation from the firm-level fixed effect, plus the firm-level fixed effect itself. We split workers according to their tertile of worker fixed effects within the firm, where higher tertile workers have higher earnings potentials (conditional on the age-wage profile). Within every firm, workers are roughly evenly distributed over the tertiles (but not perfectly, as worker entry and exit after the acquisition is not necessarily equal across tertiles).

[5] The classification is based on Eurostat's sectoral approach that classifies NACE industries at the 2-digit level according to the ratio of R&D expenditures to value added and the share of tertiary educated workers. For manufacturing sectors we classify high- and medium-high-technology sectors as high-technology, and low- and medium-low-technology sectors as low-technology.

Table 2: Change in firm and worker fixed effects by employment size.

	5 - 19		20 - 49		50 - 99		> 100	
Years since	Firm FE	Worker FE	Firm FE	Worker FE	Firm FE	Worker FE	Firm FE	Worker FE
acquisition	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$s = -3$	0.0033	-0.0035	-0.0004	0.0032	0.0042	-0.0018	0.0055	-0.0029
	(0.0035)	(0.0033)	(0.0035)	(0.0029)	(0.0062)	(0.0049)	(0.0073)	(0.0059)
$s = -2$	0.0012	-0.0021	0.0038	0.0016	0.0008	0.0015	0.0006	-0.0011
	(0.0027)	(0.0026)	(0.0026)	(0.0025)	(0.0048)	(0.0038)	(0.0048)	(0.0038)
$s = 0$	0.0103***	-0.0015	0.0101***	0.0057*	0.0158**	0.0090**	0.0072	0.0025
	(0.0025)	(0.0024)	(0.0030)	(0.0024)	(0.0054)	(0.0033)	(0.0053)	(0.0032)
$s = 1$	0.0185***	0.0011	0.0228***	0.0090**	0.0222***	0.0169***	0.0186*	0.0009
	(0.0032)	(0.0030)	(0.0039)	(0.0031)	(0.0059)	(0.0045)	(0.0079)	(0.0049)
$s = 2$	0.0244***	0.0018	0.0278***	0.0078*	0.0375***	0.0137*	0.0161·	0.0020
	(0.0036)	(0.0035)	(0.0042)	(0.0034)	(0.0066)	(0.0055)	(0.0089)	(0.0046)
$s = 3$	0.0313***	0.0043	0.0376***	0.0090*	0.0515***	0.0149*	0.0272*	0.0074
	(0.0041)	(0.0037)	(0.0050)	(0.0040)	(0.0081)	(0.0065)	(0.0108)	(0.0056)
Fixed-effects								
Firm ID	✓	✓	✓	✓	✓	✓	✓	✓
Pair-year	✓	✓	✓	✓	✓	✓	✓	✓
# Firm ID	1,218	1,218	786	786	318	318	216	216
# Pair-year	4,263	4,263	2,751	2,751	1,113	1,113	756	756
Observations	8,526	8,526	5,502	5,502	2,226	2,226	1,512	1,512
R^2	0.9088	0.9714	0.9022	0.9712	0.9097	0.9801	0.9322	0.9863
Pre-trends								
P-value	0.6408	0.5781	0.2089	0.5369	0.7083	0.6066	0.7334	0.8840

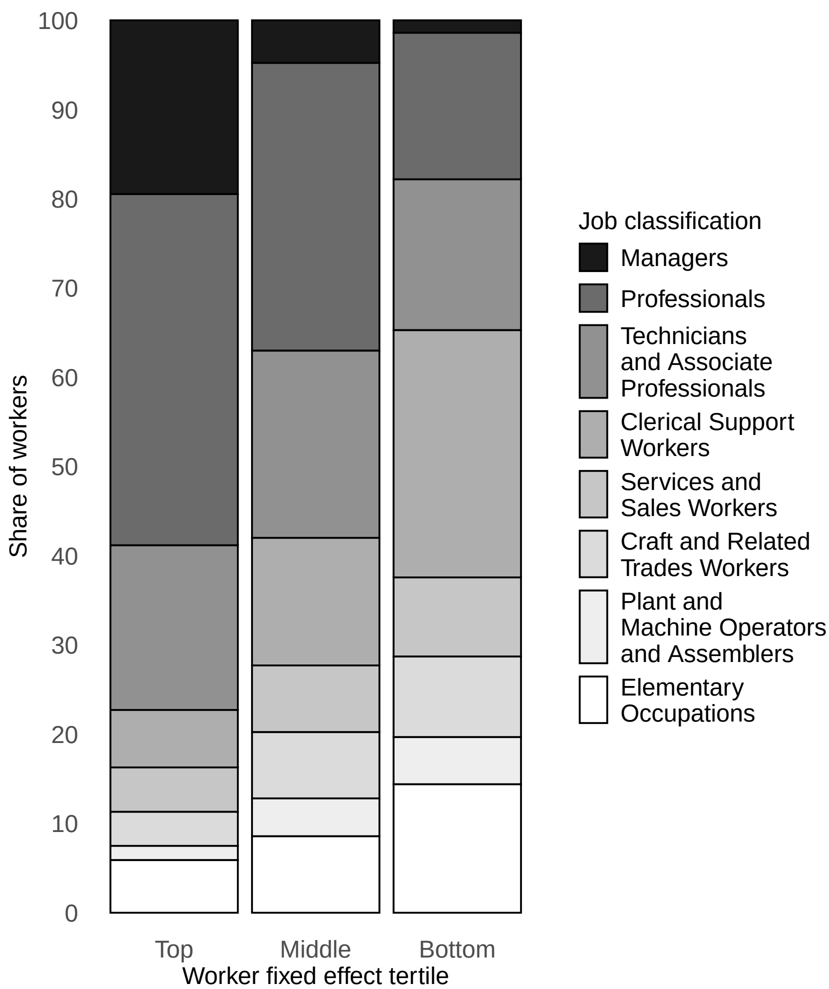
Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Clustered standard errors (Firm ID) in parentheses. s identifies years since acquisition. Firms are split up by employment size of acquired firm at $s = -1$. Dependent variables are firm fixed effects and firm-level average worker fixed effects of the decomposition on (2). Estimated using difference-in-differences regression (1) on propensity score matching sample. The regressions include a fixed effect for each firm and each year of matched pairs of firms. Propensity scores are estimated within industry-year groups and using firm-level characteristics at $s = -1$; see Section 2.3 for details.
Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

Table 3: Change in firm and worker fixed effects by industry type.

Years since acquisition	Services knowledge-intensive Firm FE (1)	Worker FE (2)	Services other Firm FE (3)	Worker FE (4)	Manufacturing high-tech Firm FE (5)	Worker FE (6)	Manufacturing low-tech Firm FE (7)	Worker FE (8)
$s=-3$	0.0080$^{\cdot}$	0.0007	-0.0024	-0.0024	0.0029	-0.0046	-0.0020	-0.0053
	(0.0043)	(0.0038)	(0.0032)	(0.0029)	(0.0058)	(0.0061)	(0.0077)	(0.0052)
$s=-2$	0.0056$^{\cdot}$	-0.0003	-0.0023	-0.0013	-0.0033	-0.0003	0.0125*	-0.0035
	(0.0034)	(0.0032)	(0.0023)	(0.0023)	(0.0048)	(0.0045)	(0.0062)	(0.0037)
$s=0$	0.0172***	-0.0007	0.0046*	0.0034$^{\cdot}$	0.0087	0.0120*	0.0083	0.0098*
	(0.0035)	(0.0027)	(0.0022)	(0.0020)	(0.0064)	(0.0057)	(0.0060)	(0.0044)
$s=1$	0.0326***	0.0076*	0.0112***	0.0046$^{\cdot}$	0.0129$^{\cdot}$	0.0145*	0.0286***	0.0046
	(0.0047)	(0.0035)	(0.0028)	(0.0027)	(0.0075)	(0.0060)	(0.0079)	(0.0062)
$s=2$	0.0355***	0.0084*	0.0185***	0.0041	0.0275***	0.0123*	0.0427***	0.0085
	(0.0052)	(0.0041)	(0.0032)	(0.0031)	(0.0080)	(0.0062)	(0.0081)	(0.0064)
$s=3$	0.0549***	0.0102*	0.0217***	0.0059$^{\cdot}$	0.0454***	0.0178*	0.0477***	0.0162*
	(0.0061)	(0.0046)	(0.0036)	(0.0035)	(0.0120)	(0.0071)	(0.0093)	(0.0066)
Fixed-effects								
Firm ID	✓	✓	✓	✓	✓	✓	✓	✓
Pair-year	✓	✓	✓	✓	✓	✓	✓	✓
# Firm ID	738	738	1,276	1,276	162	162	180	180
# Pair-year	2,583	2,583	4,466	4,466	567	567	630	630
Observations	5,166	5,166	8,932	8,932	1,134	1,134	1,260	1,260
R^2	0.9063	0.9728	0.9069	0.9684	0.8971	0.9586	0.9293	0.9680
Pre-trends								
P-value	0.1353	0.9459	0.6016	0.7093	0.4661	0.4834	0.0339	0.5593

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Clustered standard errors (Firm ID) in parentheses. s identifies years since acquisition. Firms are split up according to NACE industry at $s=-1$ using Eurostat's sectoral approach. Dependent variables are firm fixed effects and firm-level average worker fixed effects of the decomposition on (2). Estimated using difference-in-differences regression (1) on propensity score matching sample. The regressions include a fixed effect for each firm and each year of matched pairs of firms. Propensity scores are estimated within industry-year groups and using firm-level characteristics at $s=-1$; see Section 2.3 for details. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s=-3$ and $s=-2$).

Figure 2: Occupations per within-firm worker fixed effect tertile.



Notes: The figure shows the distribution of ISCO-08 job classifications within each worker tertile for 3.5% of the 331,200 workers in the matched sample. Workers are sorted in tertiles in each year according to the within-firm tertile boundaries of the within-firm worker fixed distribution in the year before acquisition. Worker fixed effects are derived from the decomposition on (2).

To visualize the worker group distribution, Figure 2 shows the ISCO occupation distribution within each tertile for workers whose occupation is observed.[6] The figure shows that managers and professionals are considerably overrepresented in the top tertile, but underrepresented in the bottom tertile (20% instead of 60%). Conversely, 60% of workers in the bottom tertile come from occupations ranked lower in the ISCO classification, such as Elementary Occupations and Crafts and Trades.

Table 4 shows the results when estimating the wage premium regression by tertile of the within-firm worker fixed effects distribution. The dependent variables are the experienced group-within-firm yearly fixed effects. The top tertile of workers experiences higher premia over their peers in non-acquired firms than workers from the bottom and middle tertile. By the third year, top tertile workers experience a wage premium of 5.2% over equivalent workers in domestic firms, while the lower and middle tertile workers see premia of 2.4% and 3.1%. For the lower tertile workers, the wage premia also materialize with a delay, as regression coefficients become statistically significant one year after the acquisition. This difference is not an artefact of the worker ordering in the data, as the groups are organized on pre-acquisition dates and no significant differences relative to counterfactual firms can be detected before the acquisition.

4.4 Hires and separations

Part of the post-acquisition wage premium arises at the acquired firm employs workers with higher individual fixed effects. In our framework, individual worker fixed effects are constant. Then, the firm-averaged worker fixed effect can change along two margins: by hiring new workers, or by separating from current workers.

The evolution of a firms' average worker fixed effect follows

$$N_t \times \alpha_t = N_{t-1} \times \alpha_{t-1} + H_t \times \alpha_t^h - S_t \times \alpha_t^s, \tag{3}$$

where N_t and N_{t-1} are the number of current workers and last year's workers, H_t is the number of newly hired workers and S_t is the number of workers separated from the firm. The terms α_t, α_t^h and α_t^s are their average fixed effects.

The year-to-year growth in the firm's average worker fixed effect, using the shares $s_t^h = \frac{H_t}{N_{t-1}+H_t-S_t} = \frac{H_t}{N_t}$ and $s_t^s = \frac{S_t}{N_{t-1}+H_t-S_t} = \frac{S_t}{N_t}$, is

$$\underbrace{\alpha_t - \alpha_{t-1}}_{\Delta \text{ worker FE}} = \underbrace{s_t^h(\alpha_t^h - \alpha_{t-1})}_{\text{hires}} - \underbrace{s_t^s(\alpha_t^s - \alpha_{t-1})}_{\text{separations}}. \tag{4}$$

The growth in the average worker fixed effect, $\alpha_t - \alpha_{t-1}$, is higher when newly hired workers have higher fixed effects than the firm's preceding average fixed effect (i.e., $(\alpha_t^h - \alpha_{t-1})$ is high) and when workers with below-average fixed effects exit the firm (i.e., $(\alpha_t^s - \alpha_{t-1})$ is low). The deviations of fixed effects of new hires and separations are weighted with their respective shares in firm employment, s_t^h and s_t^s.

[6]The source is the ISCO-08 job classification from the yearly Dutch Labor Force Survey (enquête beroepsbevolking), which shows the occupation of 3.5% of the 331,200 workers in the matched sample. We cannot calculate occupation-specific fixed effects as we do not observe occupations of all workers in the pairs of matched firms.

Table 4: Group-within-firm fixed effects per tertile of within-firm worker fixed effect distribution.

	Group-within-firm fixed effect		
Years since	Top	Middle	Bottom
acquisition	(1)	(2)	(3)
$s = -3$	0.0058$^{\cdot}$	0.0018	-0.0003
	(0.0032)	(0.0030)	(0.0028)
$s = -2$	0.0032	0.0004	0.0038$^{\cdot}$
	(0.0025)	(0.0023)	(0.0022)
$s = 0$	0.0178***	0.0101***	0.0032
	(0.0026)	(0.0022)	(0.0021)
$s = 1$	0.0337***	0.0163***	0.0118***
	(0.0032)	(0.0028)	(0.0026)
$s = 2$	0.0421***	0.0174***	0.0176***
	(0.0036)	(0.0029)	(0.0027)
$s = 3$	0.0509***	0.0311***	0.0242***
	(0.0040)	(0.0036)	(0.0031)
Fixed-effects			
Firm ID (2,538)	✓	✓	✓
Pair-year	✓	✓	✓
# Pair-year	8,883	8,883	8,882
Observations	17,715	17,661	17,681
R^2	0.8636	0.8751	0.8831
Pre-trends			
P-value	0.1899	0.8192	0.1303

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Clustered standard errors (Firm ID) in parentheses. s identifies years since acquisition. Workers are split up by boundaries of within-firm worker fixed effect distribution at $s = -1$. Dependent variables are firm fixed effects plus the group-specific mean residual of the decomposition on (2). Estimated using difference-in-differences regression (1) on propensity score matching sample. The regressions include a fixed effect for each firm and each year of matched pairs of firms. Propensity scores are estimated within industry-year groups and using firm-level characteristics at $s = -1$; see Section 2.3 for details. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

According to the decomposition in equation (4), acquired firms change their average worker fixed effect by hiring and firing, and along a quantity margin or a quality margin. Acquired firms may use hiring to increase the average worker fixed effect by hiring new workers with higher fixed effects than before (by increasing α_t^h), or by simply hiring more new workers, if new workers generally have higher fixed effects (if $\alpha_t^h - \alpha_{t-1}) > 0$ then increasing s_t^h increases the average worker fixed effect). Similarly, the firm could use separations to increase the average work fixed effect by lowering the average fixed effect of leaving workers, or, if the fixed effect of leaving worker is generally lower, by letting more workers go.

To analyze the margins by which the worker fixed effects adjust to an acquisition, we examine the impact of an acquisition on the firm-average worker fixed effect, and on the quantity and fixed effects of new hires and separations of workers. As not all firms have hires or separations for all years, we examine averages before and after acquisition and drop matched firm pairs if one of the firms in the pair has no hires or separations. Hence, the coefficient for firms post acquisition identifies the average annual impact over a four-year period in the acquired firm relative to the matched non-acquired firm.

The result in Table 5, Column 1, shows an increase in the average worker fixed effect after a firm is acquired, very close to the estimates in Table 1 (which presents a dynamic specification rather than the post-acquisition four-year average). Column 2 shows the impact of acquisition on the average worker fixed effect along the hiring margin - the product of the quantity of new hires and the average fixed effect of newly hired workers relative to workers already in the firm, as in equation (4). The margin of hires explains a change around 97% of the total change in worker fixed effects. The separations margin is very close to zero and statistically insignificant.

From equation (4), the effect of hiring in the acquired firm relative to the non-acquired firm could be due to a higher fixed effect of incoming workers, or more new hires (if the average fixed effect of new hires is generally higher than the average of the current workforce). The regressions reported in Columns 4 and 5 of Table 5 have the fixed effect of incoming workers and the share of newly hired workers in the firm as dependent variables. After an acquisition, newly hired workers have around 2% higher fixed effects. The impact of an acquisition on the share of newly hired workers in the firm is only around 0.7 percentage points. In comparison to the average share of new hires in firms before the acquisition of 24%, a 0.7 percentage point increase in the share suggests a small effect on the average worker fixed effect, implying that the entry of workers with higher fixed effect explains most of the increase in average worker fixed effects.

Table 5: Hire and separation margins.

		Components		Worker FE	Share of hires
	Δ Worker FE	Hires	Separations	of hires	in workforce
	(1)	(2)	(3)	(4)	(5)
Post Acquisition	0.00404**	0.00395**	−0.00009	0.01990***	0.00688*
	(0.00145)	(0.00122)	(0.00086)	(0.00516)	(0.00327)
Fixed-effects					
Firm ID (2,090)	✓	✓	✓	✓	✓
Pair-post (2,090)	✓	✓	✓	✓	✓
Observations	12,060	12,060	12,060	12,060	12,060
R^2	0.2522	0.3819	0.3797	0.6129	0.6248

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Clustered standard errors (Firm ID) in parentheses. Mean worker fixed effects come from the decomposition on (2). Columns 1 to 3 estimate decomposition (4). The dependent variable in Column 4 is the mean fixed effect of workers that entered firm in a given year. The dependent variable in Column 5 is the share of new hired workers in the firm's workforce in a given year. Estimated using difference-in-differences regression (1) without dynamic effects on propensity score matching sample. The regressions include fixed effects for each firm and matched-pair fixed effects that differentiate between pre-acquisition and post-acquisition years. Propensity scores are estimated within industry-year groups and using firm-level characteristics at pre-acquisition year; see Section 2.3 for details.

5 Robustness Checks

We offer robustness checks of the main decomposition estimates to four different caveats: the estimation of fixed effects in a firm-worker network structure, the alternative method of identifying firm fixed effects from a sample of non-moving workers, variations in the way firms are matched to other firms for comparison; and different ways of estimating the standard errors around our main coefficients.

5.1 Network size and limited mobility bias

If few workers move across firms, there are few observations to identify the firm-level fixed effect. The lack of moving workers leads to an incidental parameter problem, resulting in a "limited mobility bias" (Andrews et al., 2008; Jochmans and Weidner, 2019). In our firm-level decomposition, biases in the fixed effect estimates could distort the results on the relative importance of worker- and firm-level contributions to the wage premium. Moreover, as we estimate time-varying firm fixed effects instead of time-invariant ones, any limited mobility biases could be exacerbated.

To examine the scope for a limited mobility bias, we re-estimate our main model in a highly connected subset of the firm-worker network. We first identify the largest connected set, re-estimate the firm and worker fixed effects, and then re-estimate our difference-in-differences coefficients (see Appendix C for details). In the resulting network, the connectivity measures lie well within ranges accepted for our two-way fixed effects approach (Jochmans and Weidner, 2019). We report the results of the subnetwork analysis in Table C2 in the appendix. The estimates in the main network and the highly connected network are very similar. This suggests that limited mobility bias is not a large concern in our dataset. This is not a surprising finding, as the acquired firms tend to be well connected: Almost all of them are also part of the well-connected subnetwork.

5.2 Identification from stayers

In order to isolate post-acquisition firm-level wage developments from changes in the workforce, most earlier studies exploit a subsample of workers that stay within the firm. Assuming that the earnings capacity of stayers does not change with the acquisition, stayers' wages reflect firm-level premia. Our decomposition approach (2), by contrast, uses the wages of workers that stay in the firm as well as the wages of workers that move between firms to identify the firm-level premium. If stayers and movers differ systematically, the two estimates may diverge: If the firm-level premium after acquisition is higher for moving workers, the firm-level premium identified from stayers is lower than the firm-level premium experienced by the average worker.

We estimate the firm-level premium in our sample for a subsample of workers that stay within the firm, in order to examine whether sample selection on stayers or movers explains our findings. In an analysis of a sample of stayers in Table D3 in the appendix, the coefficients for stayers' average residual wage developments (adjusted for observable worker characteristics) are very similar to the developments of firm fixed effects estimated from our decomposition. Moreover, the residual wage of stayers, after additionally taking out the estimated firm fixed effects, show no statistically significant developments after the acquisition. This finding suggests that the firm-level fixed effects estimated for the sample of stayers and movers do not differ significantly from the firm-level fixed effects estimated from a subsample of stayers.

5.3 Alternative matching strategies

The covariates used to match firms on their propensity of acquisition can affect the set of firms that is matched. As a result, the set of covariates can affect the presumed counterfactual development in an acquired firm, thus changing the difference-in-differences estimates. To chart the sensitivity of our main results to the choice of matching covariates, we estimate the main decomposition with varying sets of matching covariates. Table D4 in the appendix shows three sets of results based on different sets of covariates: First, a set of only pre-acquisition wage and employment and their growth rates, firm age, and exports. Second, a set with firm and worker fixed effects, the variance of worker fixed effects, financial information (sales, value added) and the share of female workers added to the first set. Third, a set of wage, employment, and their squares, financial information (sales, sales to export ratio, the square of sales to exports) and mean age of workers.

The results in Table D4 show that using different sources of information to match firms leads to comparable estimates of the firm- and worker-level premia. This occurs despite considerable changes in the sample of firms used to estimate the difference-in-differences regression. In the smallest set, we find a pretrend in the firm-level average of worker fixed effects as they are higher in the acquired firm before acquisition.

Additionally, we examine our results when employing coarsened exact matching (Iacus et al., 2012). In contrast to propensity score matching, all the covariates used in coarsened exact matching need to be similar for firms to qualify as matches. We match within the 2-digit NACE industry on calipers of the percentile distribution. This necessarily has smaller sets of covariates. The sets vary over firm and average worker fixed effects, with firm age, employment, exports, within-firm worker fixed effect variance added; and pre-acquisition growth rates of the fixed effects. The results are in Table D5. Across the covariate sets, we consistently find higher firm-level fixed effects after acquisition, but lower or even negative developments in the worker fixed effects. This is not entirely surprising as the firm sets differ substantially by matching strategy, and the evidence in Section 4.2 suggests that the estimated changes in worker fixed effects are determined by a limited number of firms. When matching on the within-firm variance of the worker fixed effects, we find significant pretrends. Together, this suggests that the main result that firm fixed effect explain most of the acquisition premium is stable both across matching methods and sets of covariates used to match firms.

5.4 Inference

In our main results, we cluster standard errors at the firm level to account for serial correlation across the firm's observations. Our main results are robust to alternative estimation methods for the standard errors. In Table D6, we report the results of different strategies. To take into account the serial nature of switching status from non-acquired to acquired, we cluster pre- and post-acquisition observations. Additionally, we allow for a second level of clustering at the year level (across firms). Finally, we compare our estimates against a randomized assignment within the matched firm pairs with a randomization inference estimator (Barrios et al., 2012; MacKinnon et al., 2022). Across these methods, the standard errors vary and two-way clustering and randomization inference lead to higher p-values. However, across different estimates of the standard errors, no qualitatively different conclusions arise.

Our decomposition treats the point estimates of fixed effects as outcomes. The fixed effect estimates may be imprecisely estimated, however. It is computationally unfeasible to estimate the standard errors around the fixed effects in the dataset. Instead, we gauge the impact of the uncertainty of fixed effects estimates in the difference-in-differences standard errors by simulating the impact of plausible distributions of the fixed effect parameters. First, we suppose that all the fixed effect estimates from a given firm are estimates of a constant (taking the extreme stance that all within-firm variation is driven by uncertainty, and not by actual firm development). Then, we generate 9,999 new random sets of fixed effects drawn from the distribution implied by the within-firm variation. Within each set we retrieve the difference-in-differences estimates and standard errors clustered at the firm level. Finally, we use the average t-values across the sets to recover bootstrapped clustered standard errors for our estimates.

Using the within-firm standard deviation as a measure of uncertainty, the bootstrap produces standard errors slightly higher than our clustered standard errors, but leads to no qualitative change in the conclusions (see Table D7, Column 3). Magnifying the standard deviation of the distribution for bootstrap draws to two times the actual within-firm standard deviation also leads to little change in the conclusions. When the bootstrap employs a distribution with a threefold standard deviation over the actual within-firm standard deviation, the difference-in-differences estimates lose statistical significance. Altogether this suggests that the uncertainty around the fixed effects estimates has little bearing on our conclusions.

6 Concluding Remarks

We comprehensively decompose the wage premium after a foreign acquisition of a domestic firm into firm- and worker-level premia, using the universal matched employer-employee data of the Netherlands for the years 2006-2018. Our decomposition approach augments Abowd et al. (1999)'s framework with yearly firm fixed effects (Engbom et al., 2022) and models firm- and worker-level premia simultaneously, without the use of subsamples. The identified components in the acquisition wage premium are on an equivalent scale, add up to the total wage premium by definition and, as a result, allow us to document how the relative weights of workforce composition and firm-level wage premia change in the year of acquisition and the next three years. For identification, we compare 1,269 acquired firms to their matched domestic firms through a combined difference-in-differences matching strategy. We additionally explore differences in wage premia by industry, firm size, segments of the workforce and worker hiring and exit dynamics.

Our results suggest a firm-level premium as the main driver of wage increases after a foreign acquisition. After accounting for the targeting of firms for acquisition, wages in acquired firms steadily rise faster than wages in matched firms, from 1.4% in the year of acquisition up to 5% by the third year after acquisition. Our decomposition shows that the firm-level premia explain about 70-76% of the wage increase, while changes in the composition of workers materialize with a delay and explain 27-30%. This contrasts the findings of related worker-level studies of the multinational wage premium that mostly connect the wage premium to the firm's workers, rather than the firm itself. The difference might arise with our dynamic decomposition approach which simultaneously estimates wage contributions of the firm and individual workers, while leveraging both stayers' and movers' wage developments for the identification of the effects. The difference may also arise from the broad coverage of our sample: Subsample analyses suggest that changes in workforce composition are statistically significant for firms with 20 to 99 employees, while they are insignificant for smaller and larger firms. The importance of firm- and worker-level contributions also varies with the use of technology and knowledge in the firm. In high-tech manufacturing, just after the acquisition, the sorting of workers with higher earnings capacity to firms is more important in explaining the wage premium than firm-level developments. For service sectors, we find that the firm-level premia are more than twice as large in knowledge-intensive than in non-knowledge intensive sectors.

Within the firm, our results suggest that the firm-level premia do not apply equally to all workers. The acquisition wage premia in the groups of highest earnings capacity, which contain large shares of managers and professionals, are about twice as large as the premia experienced by workers with lower earnings capacity. However, all groups of workers experience positive firm-specific premia. The differential wage premia within the firm are not paired with increased layoffs of workers with low earnings capacity. Instead, the main cause of higher average worker-level premia after acquisition is the hiring of new workers with high earnings capacity.

The importance of firm-level wage premia after acquisition is consistent with but not evidence of productivity or technology improvements in acquired firms. While we leave a detailed analysis of potential productivity improvements for future research, our results shed some light on alternative explanations for the wage premium. Two other prominent explanations are that the wage premium arises as a compensation of the acquiring firm for potential later job loss, or that temporary (bonus) pay compensates employees for organizational change. These interpretations are less plausible as we find virtually no impact of acquisitions on separation rates, and the firm-level wage premia are persistent and increasing over the three years after acquisition - too long for most organizational incentives.

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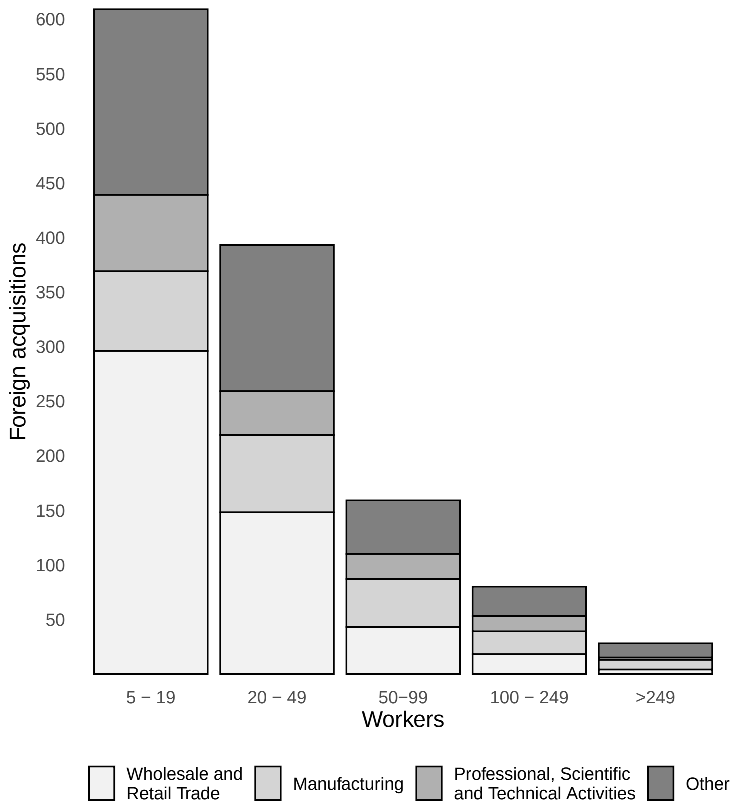
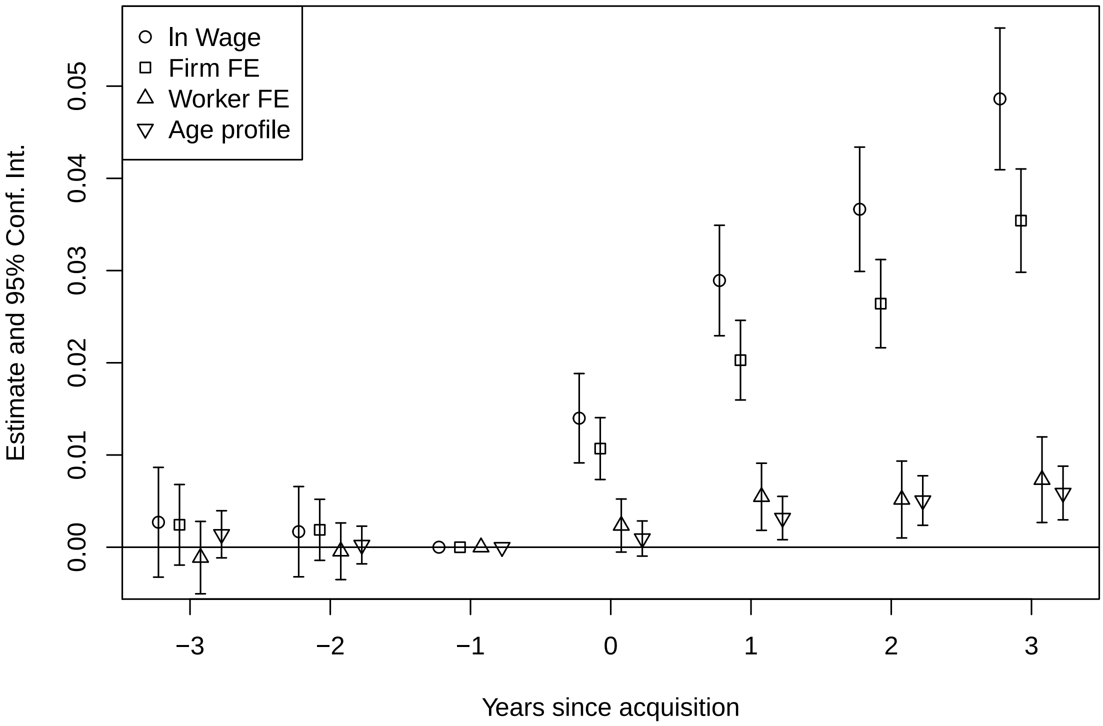
A Supporting figures

Figure A1: Firm size and NACE industry of target firms in pre-acquisition year (matched sample).



Notes: The figure shows the distribution of matched target firms of foreign acquisitions in the year before acquisition across different firm size categories and selected NACE industries. Target firms are domestic firms that were never foreign owned and never had any foreign affiliates before the acquisition. Firms are matched using propensity score matching. Propensity scores are estimated within industry-year groups and using firm-level characteristics at $s = -1$; see Section 2.3 for details.

Figure A2: Relative share of firm and worker contributions to the post-acquisition wage premium.



Notes: The figure shows the coefficients and 95% confidence intervals of the results in Table 1. Coefficients are estimated using difference-in-differences regression (1) on propensity score matching sample. Confidence intervals are based on clustered standard errors (Firm ID). Dependent variables are firm-level averages of the decomposition on (2). The regressions include a fixed effect for each firm and each year of matched pairs of firms. Propensity scores are estimated within industry-year groups and using firm-level characteristics at $s = -1$; see Section 2.3 for details.

B Data Appendix

Our data is compiled from various worker- and firm-level administrative datasets of Statistics Netherlands.

B.1 Worker-level data

Our main source for worker-level data is the Polisadministratie. The Polisadministratie is compiled from mandatory information sent by firms to the Dutch Employee Insurance Agency (UWV) and tax authorities. This data is very detailed and accurate because its main use is to identify payroll tax, and pension and unemployment insurance claims. It covers all legal employer-employee relationships in the Netherlands on a monthly basis. For the years 2006 to 2018 and for each employer-employee relationship we extract information on workers' monthly base, overtime and bonus income; contract and overtime hours; and contract type (permanent or temporary). We additionally enrich the data with information on birth years from the population register and workers' socio-economic status.

We aggregate the monthly data to the yearly level. We calculate hourly wages as total income over total hours worked and use the consumer price index to adjust wages to real values. Around 20% of workers are linked to more than one employer within the same year and around 42% of these hold two or more jobs at the same time. Because the AKM decomposition (2) relies on unique linkages between workers and firms, we assign a main employer for each worker according to the highest base income. For the few cases where base incomes overlap (less than 1%) we use most contract hours, contract type and tenure.

We focus on workers aged 20 to 60 and only keep such observations for which a workers' main source of income stems from employment according to the socio-economic status. We further delete the full earnings history of workers with hourly wages outside 5 to 1,000 Euro, year-on-year changes in log hourly wages outside -1 and 1 and those workers with a single employment year.

In sum, we assemble a matched employer-employee dataset for the Netherlands that covers 9.35 million workers and 0.77 million firms over the years 2006 to 2018. The AKM decomposition (2) is estimated on a set of firm-years that are connected through worker movements, which covers virtually all of the workers and 94% of the firm years in the data.

B.2 Firm-level data and firm ID linkages

Our firm-level data comes from the Structural Business Statistics and Foreign Affiliates Statistics. We focus on firms that are not in the financial sector and for each firm we collect yearly information on the firm's NACE industry classification, age, real value of exports and ownership. In particular, we observe whether a firm has any foreign affiliates and the ultimate controlling institutional unit of the firm. Ownership is determined by the concept of control, where control refers to a majority stake of voting rights. The ultimate controlling institutional unit reports the country of residence of the ultimate owner at the top of a foreign affiliate's chain of control. We define a firm as foreign owned if the ultimate controlling institutional unit is non-Dutch. Similarly, a Dutch firm in our dataset has foreign affiliates if it exerts decisive control over a foreign firm and its ultimate controlling institutional unit is Dutch. We include Dutch multinationals for the estimation of the AKM decomposition but exclude them in the main analysis to avoid comparing foreign with Dutch multinationals.

Before we describe how we select foreign acquisitions, it is necessary to explain how we deal with firm IDs in our data. All firms in our dataset are assigned a unique firm ID. These firm IDs are mostly consistent over time. However, in some cases foreign acquisitions can trigger a change in firm ID. This is, for example, the case when a new owner files for a new chamber of commerce registration or receives a new identification number by the UWV. To overcome this issue, we follow Benedetto et al. (2007) in identifying firm ID linkages through worker flows based on the monthly worker data. Specifically, we define the month of a firm ID entry as a large inflow of workers. We require that the firm ID entered the data within the last 6 month and that for each of these months the firm ID's employment is below 5 workers and 10% of the employment that we observe in the entry month. Reversely, we identify the moment of a firm ID exit whenever employment in the next month drops below 10% of the employment in the current month, the firm ID's total employment stays below this 10% threshold and the firm ID exits within 6 months. We consider two firm IDs in the dataset to be linked if at the moment of entry of a new firm ID that firm ID is made up of at least 80% of the workers of a firm ID that exited in the previous month. For our analysis we use the aggregated yearly version of the data and treat linked firm IDs as identifying the same firm.

We identify a foreign acquisition of a domestic firm by a change in ultimate controlling institutional unit from Dutch in the previous year to foreign in the current year. We remove all firms that ever reported foreign affiliates under Dutch ownership or were ever foreign owned before the acquisition. We further select such foreign acquisitions where we continuously observe the firm for at least three years before and three years after the acquisition year. We also require these firms to remain foreign owned until 2018 and to employ at least 5 workers in all of the years. In total, we identify 1,357 foreign acquisitions, of which 279 are firm ID linkages.

B.3 Descriptive statistics before matching

In line with earlier research, target firms of acquisitions and domestic firms differ substantially in our data. Table B1 reports descriptive statistics for the 1,357 targets of foreign acquisitions over pre-acquisition years and all 670,301 domestic firms. On average target firms employ more workers, export more, pay higher wages and feature higher levels of firm and worker fixed effects. They also experience sharper employment, firm and worker fixed effect growth rates than domestic firms. These differences in observed characteristics suggests that foreign acquisitions are not random. This may cause a selection issue for our difference-in-differences estimation approach because the coefficients could depict underlying differences between acquired and domestic firms. We apply propensity score matching to account for such ex-ante differences.

B.4 Covariate balance before and after propensity score matching matching

Table B1: Descriptive statistics of domestic and target firms of foreign acquisitions in unmatched sample.

	Domestic firms	SD	Target firms	SD
Firms	670,301		1,357	
Firm years	3,478,916		7,073	
Ln employment	1.32623	1.24598	2.99169	1.05791
Ln employment growth	0.00733	0.42452	0.10814	0.31277
Export value	187.61493	3035.27333	3708.64606	23043.41697
Wage components				
Mean ln wage	2.89849	0.35054	3.19477	0.30804
Mean ln wage growth	0.01449	0.1478	0.01236	0.1064
Firm fixed effect	0.02563	0.22312	0.0604	0.1323
Firm fixed effect growth	-0.00050	0.11807	0.01196	0.07895
Mean worker fixed effect	-0.192	0.26199	0.0502	0.2452
Mean worker fixed effect growth	-0.00044	0.09879	-0.0137	0.07653
Variance worker fixed effect	0.0522	0.07199	0.11988	0.09285

Notes: Mean and standard deviation of key covariates for domestic and target firms in the unmatched sample. Domestic firms are neither foreign-owned nor Dutch multinationals. Target firms are selected foreign acquired firms over observed pre-acquisition years; see Section 3. Wage components are firm-level averages of the decomposition on (2). Growth refers to the yearly log difference.

Table B2: Covariate balance before and after propensity score matching.

	Unmatched	Matched
Target firms	1,357	1,269
Control firms	71,681	1,269
Mean ln wage	0.80020	0.02176
	(0.03744)	(0.02754)
Mean ln wage 1-year growth rate	0.03806	-0.01967
	(0.03818)	(0.04122)
Mean ln wage 2-year growth rate	0.03524	-0.02408
	(0.03629)	(0.03818)
Ln employment	0.37908	0.01093
	(0.04032)	(0.03731)
Ln employment 1-year growth rate	0.15759	-0.01606
	(0.03754)	(0.03907)
Ln employment 2-year growth rate	0.15820	0.01622
	(0.03752)	(0.03636)
Firm fixed effect	0.25367	-0.00763
	(0.03894)	(0.03701)
Firm fixed effect 1-year growth rate	0.13747	-0.02933
	(0.03975)	(0.03717)
Firm fixed effect 2-year growth rate	0.18163	-0.02899
	(0.03694)	(0.03758)
Mean worker fixed effect	0.82387	0.02174
	(0.03685)	(0.02613)
Mean worker fixed effect 1-year growth rate	-0.12757	0.00807
	(0.03753)	(0.04061)
Mean worker fixed effect 2-year growth rate	-0.19009	0.01409
	(0.03643)	(0.03547)
Variance worker fixed effects	0.76316	0.02272
	(0.03763)	(0.03425)
Ln firm age	-0.20395	-0.03073
	(0.03798)	(0.03634)
Ln exports	0.69232	-0.01474
	(0.04048)	(0.02964)

Notes: The table reports the average normalized difference in propensity score matching covariates between target firms of foreign acquisitions in the year before acquisition and control firms, in the unmatched and matched sample. The differences are normalized by the variation across target firms (before matching) as suggested by Imbens and Wooldridge (2009). Standard errors in parentheses. Target firms are domestic firms that were never foreign-owned and never had any foreign affiliates before the acquisition; remain foreign-owned after acquisition; are continuously observed for seven years; and employ at least five workers throughout those years. Control firms are domestic firms (never foreign-owned, never owning any foreign affiliates) that are selected by the same criteria as target firms and operate in the same 2-digit NACE industries as target firms.

C Limited Mobility Bias

Our main results are based on the unbiased identification of fixed effects in a firm-worker network. One concern with the identification of the fixed effects is limited mobility bias. Limited mobility bias is an incidental parameter problem that arises when few workers move between firms in the AKM wage decomposition which causes the firm fixed effects to be identified by wages of few workers and to be potentially biased (Andrews et al., 2008). The issue is amplified when firms are connected within clusters with little worker mobility between the clusters (Jochmans and Weidner, 2019).

In our AKM decomposition (2) with yearly firm fixed effects, limited mobility bias occurs when few workers connect individual firm years with the rest of the network. As the worker fixed effects are estimated conditional on firm fixed effects, an overestimation of firm fixed effects results in an underestimation of worker fixed effects. This is an issue for our firm-level decomposition because the estimates rely on a clear separation of firm and worker fixed effects to measure the differential impact of firms and workers on the acquisition premium. Whether limited mobility bias actually causes a bias in the difference-in-differences estimates depends on how well the firm and worker fixed effects in the matched sample are connected.

To describe the frequency of connection in our sample, we calculate the empirical measure for network connectivity proposed by Jochmans and Weidner (2019). Their global connectivity measure uses a graph in which nodes of firms are connected by edges with weights according to the number of worker movements, to signal the scope for limited mobility bias. The measure is the first non-zero eigenvalue of the normalized laplacian of the network. It is bounded between zero and one and measures how easily the network can be split into separate networks. For our main network, we find a value of 0.0017. This is lower than Jochmans and Weidner (2019)'s example of a weakly connected network (the student-teacher network), indicating that limited mobility bias is a potential concern for our difference-in-differences estimates.

In order to probe their sensitivity to potential limited mobility bias, we examine our results in a subset of the firm-worker network with higher connectivity. To construct the subnetwork, we require each firm year to be connected with the rest of the network through at least two other firm years and to consist of at least five workers. Because we only include workers that are present in the data for more than one year, this network features firm fixed effects that are identified by wages of at least 5 workers and that are connected to other firm fixed effects through at least two other firm years. The global connectivity measure increases fourfold to 0.0070 and now lies above the weak connection threshold in Jochmans and Weidner (2019)'s example, suggesting that limited mobility bias is less of a concern in the subnetwork. This increase in connectivity comes at the cost of a great decline in the number of included firms, but with little change in the number of included workers: The subnetwork contains about 40% of the firm years and 97% of the workers that are in the main network (see Table C1).

Although the number of firm-year observations declines substantially, we still find matches for 1,268 firms in the subnetwork. The overlap of acquired firms between our main matched sample and the matched sample of the subnetwork is more than 98%, while the overlap in counterfactual firms is 27%. To assess the impact of limited mobility bias on our difference-in-differences estimates, we match worker and firm fixed effects of the main network to the firms in the matched sample of the subnetwork. We isolate the impact of changes in the firm and worker fixed effect estimates by running our difference-in-differences regression on this matched sample with the fixed effects from both networks as the dependent variables. Table C2 compares the difference-in-differences estimates. The estimates for changes in firm fixed effects around foreign acquisitions are marginally lower in the subnetwork and the estimates for changes in worker fixed effects are marginally higher. However, as the difference amounts to about 0.0001 log-points, limited mobility bias appears of little concern, as dropping a set of weakly connected, mostly small firms from the sample has very little impact on the outcomes.

Table C1: Overview of networks.

	Main network	Subnetwork
Firms	696,912	248,413
Firm years	3,675,170	1,479,060
Workers	9,268,401	9,077,808
Observations	78,430,113	72,976,743
Global connectivity	0.001712	0.007005

Notes: Main network is the firm-worker network used for the estimation of the decomposition on (2) in the main text. Subnetwork is a subset of the main network with higher connectivity. It includes firm years with a minimum of 5 worker connections to other firm years; and with connections to a minimum of 2 other firm years. Global connectivity is the limited mobility bias indicator of Jochmans and Weidner (2019).

Table C2: Comparison of difference-in-differences estimates for the main firm-worker network and a well-connected subnetwork (limited mobility bias).

	Main network		Subnetwork	
Years since	Firm FE	Worker FE	Firm FE	Worker FE
acquisition	(1)	(2)	(3)	(4)
$s = -3$	-0.0007	0.0016	-0.0004	0.0013
	(0.0023)	(0.0021)	(0.0023)	(0.0020)
$s = -2$	-0.0007	0.0005	-0.0007	0.0005
	(0.0017)	(0.0015)	(0.0017)	(0.0015)
$s = 0$	0.0100***	0.0031*	0.0099***	0.0032*
	(0.0018)	(0.0015)	(0.0018)	(0.0015)
$s = 1$	0.0171***	0.0058**	0.0169***	0.0060**
	(0.0022)	(0.0020)	(0.0022)	(0.0020)
$s = 2$	0.0220***	0.0044*	0.0218***	0.0046*
	(0.0024)	(0.0022)	(0.0024)	(0.0022)
$s = 3$	0.0309***	0.0030	0.0309***	0.0031
	(0.0027)	(0.0025)	(0.0027)	(0.0025)
Fixed-effects				
Firm ID (2,536)	✓	✓	✓	✓
Pair-year (8,876)	✓	✓	✓	✓
Observations	17,752	17,752	17,752	17,752
R^2	0.9079	0.9734	0.9068	0.9733
Pre-trends				
P-value	0.9117	0.7078	0.9195	0.8115

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Clustered standard errors (Firm ID) in parentheses. s identifies years since acquisition. Main network is the firm-worker network used for the estimation of the decomposition on (2) in the main text. Subnetwork is a subset of the main network with higher connectivity. It includes firm years with a minimum of 5 worker connections to other firm years; and with connections to a minimum of 2 other firm years. Dependent variables are firm fixed effects and firm-level average worker fixed effects of the decomposition on (2). Estimated using difference-in-differences regression (1) on propensity score matching sample based firms on the subnetwork. The regressions include a fixed effect for each firm and each year of matched pairs of firms. Propensity scores are estimated within the subnetwork and within industry-year groups and using firm-level characteristics at $s = -1$; see Section 2.3 for details. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

D Outputs for Robustness Checks

Table D1: Decomposition of the acquisition wage premium on unmatched sample.

Years since acquisition	ln Wage (1)	Firm FE (2)	Worker FE (3)	Age profile (4)
$s = -3$	-0.0010	-0.0140***	0.0115***	0.0015
	(0.0031)	(0.0023)	(0.0022)	(0.0013)
$s = -2$	-0.0029	-0.0095***	0.0057***	0.0009
	(0.0023)	(0.0017)	(0.0015)	(0.0010)
$s = 0$	0.0062**	0.0129***	-0.0057***	-0.0011
	(0.0023)	(0.0018)	(0.0015)	(0.0009)
$s = 1$	0.0151***	0.0260***	-0.0108***	-0.0002
	(0.0030)	(0.0022)	(0.0020)	(0.0011)
$s = 2$	0.0200***	0.0373***	-0.0169***	-0.0004
	(0.0034)	(0.0025)	(0.0023)	(0.0013)
$s = 3$	0.0284***	0.0507***	-0.0220***	-0.0002
	(0.0039)	(0.0029)	(0.0027)	(0.0014)
Fixed-effects				
Firm ID (73,038)	✓	✓	✓	✓
Industry-year (871)	✓	✓	✓	✓
Observations	3,481,856	3,481,856	3,481,856	3,481,856
R^2	0.9050	0.7651	0.9118	0.7522
Pre-trends				
P-value	0.3897	0***	0***	0.4725

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Clustered standard errors (Firm ID) in parentheses. s identifies years since acquisition. Dependent variables are firm-level averages of the decomposition on (2). Estimated using difference-in-differences regression (1) on unmatched sample. The regressions include a fixed effect for each firm and each 2-digit-industry-year. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

Table D2: Decomposition of the acquisition wage premium (propensity score matching within 2-digit-nace-strata).

Years since acquisition	ln Wage (1)	Firm FE (2)	Worker FE (3)	Age profile (4)
$s = -3$	0.0013	0.0012	-0.0007	0.0007
	(0.0034)	(0.0025)	(0.0022)	(0.0015)
$s = -2$	0.0029	0.0025	-0.0004	0.0007
	(0.0028)	(0.0019)	(0.0017)	(0.0012)
$s = 0$	0.0120***	0.0085***	0.0027·	0.0007
	(0.0027)	(0.0019)	(0.0016)	(0.0011)
$s = 1$	0.0261***	0.0175***	0.0063**	0.0023·
	(0.0034)	(0.0024)	(0.0021)	(0.0014)
$s = 2$	0.0358***	0.0248***	0.0060*	0.0050**
	(0.0039)	(0.0027)	(0.0024)	(0.0015)
$s = 3$	0.0481***	0.0337***	0.0090***	0.0055**
	(0.0044)	(0.0032)	(0.0027)	(0.0017)
Fixed-effects				
Firm ID (2,018)	✓	✓	✓	✓
Pair-year (7,063)	✓	✓	✓	✓
Observations	14,126	14,126	14,126	14,126
R^2	0.9648	0.9047	0.9741	0.9259
Pre-trends				
P-value	0.5598	0.3925	0.9564	0.8188

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Clustered standard errors (Firm ID) in parentheses. s identifies years since acquisition. Dependent variables are firm-level averages of the decomposition on (2). Estimated using difference-in-differences regression (1) on propensity score matching sample. The regressions include a fixed effect for each firm and each year of matched pairs of firms. Propensity scores are estimated within industry-year groups (2-digit NACE) and using firm-level characteristics at $s = -1$; see Section 2.3 for details. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

Table D3: Stayers' residual wage developments.

Years since acquisition	Residual wage (Stayers) (1)	Firm FE (2)	Residual (Stayers) (3)
$s = -3$	0.0032	0.0013	0.0018
	(0.0028)	(0.0022)	(0.0017)
$s = -2$	0.0016	0.0014	0.0003
	(0.0022)	(0.0017)	(0.0015)
$s = 0$	0.0106***	0.0105***	0.0001
	(0.0021)	(0.0017)	(0.0015)
$s = 1$	0.0219***	0.0203***	0.0016
	(0.0027)	(0.0023)	(0.0017)
$s = 2$	0.0271***	0.0257***	0.0014
	(0.0030)	(0.0025)	(0.0019)
$s = 3$	0.0375***	0.0351***	0.0024
	(0.0035)	(0.0029)	(0.0022)
Fixed-effects			
Firm ID (2,430)	✓	✓	✓
Pair-year (8,505)	✓	✓	✓
Observations	17,010	17,010	17,010
R^2	0.9929	0.9798	0.6332
Pre-trends			
P-value	0.4431	0.5158	0.8004

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Clustered standard errors (Firm ID) in parentheses. s identifies years since acquisition. Stayers are workers that stay with the firm from $s = -3$ to $s = 3$. The dependent variable in Column 1 is stayers' average residual wage (adjusted for observable worker characteristics). The dependent variable in Column 2 is the firm fixed effect from the decomposition on (2). The dependent variable in Column 3 is the firm-level average of stayers' residual from the decomposition on (2). Estimated using difference-in-differences regression (1) on propensity score matching sample. The regressions include a fixed effect for each firm and each year of matched pairs of firms. Propensity scores are estimated within industry-year groups and using firm-level characteristics at $s = -1$; see Section 2.3 for details. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

Table D4: Comparison of different matching covariates (Propensity Score Matching).

	A		B		C	
Years since	Firm FE	Worker FE	Firm FE	Worker FE	Firm FE	Worker FE
acquisition	(1)	(2)	(3)	(4)	(5)	(6)
$s = -3$	-0.0002	0.0021	-0.0026	0.0002	-0.0018	0.0100***
	(0.0023)	(0.0020)	(0.0031)	(0.0027)	(0.0029)	(0.0025)
$s = -2$	-0.0008	0.0015	0.0002	0.0007	0.0000	0.0079***
	(0.0018)	(0.0015)	(0.0023)	(0.0019)	(0.0023)	(0.0017)
$s = 0$	0.0161***	-0.0004	0.0090***	0.0016	0.0142***	0.0026
	(0.0018)	(0.0014)	(0.0023)	(0.0018)	(0.0022)	(0.0017)
$s = 1$	0.0293***	-0.0005	0.0174***	0.0027	0.0269***	0.0043·
	(0.0022)	(0.0018)	(0.0026)	(0.0025)	(0.0029)	(0.0023)
$s = 2$	0.0368***	-0.0025	0.0245***	0.0047·	0.0349***	0.0043·
	(0.0025)	(0.0020)	(0.0031)	(0.0028)	(0.0033)	(0.0025)
$s = 3$	0.0471***	-0.0017	0.0254***	0.0052·	0.0406***	0.0025
	(0.0028)	(0.0023)	(0.0035)	(0.0031)	(0.0036)	(0.0029)
Fixed-effects						
Firm ID	✓	✓	✓	✓	✓	✓
Pair-year	✓	✓	✓	✓	✓	✓
# Firm ID	2,580	2,580	1,254	1,254	1,240	1,240
# Pair-year	9,030	9,030	4,389	4,389	4,340	4,340
Observations	18,060	18,060	8,778	8,778	8,680	8,680
R^2	0.9032	0.9744	0.9026	0.9731	0.9024	0.9767
Pre-trends						
P-value	0.9049	0.5423	0.5554	0.9119	0.7222	0.0000

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Clustered standard errors (Firm ID) in parentheses. s identifies years since acquisition. Dependent variables are firm-level averages of the decomposition on (2). Estimated using difference-in-differences regression (1) on different propensity score matching samples. The regressions include a fixed effect for each firm and each year of matched pairs of firms. Propensity scores are estimated within industry-year groups and using firm-level characteristics at $s = -1$. Propensity scores estimated on (A) mean ln wage, ln employment and their two-year growth rates, ln firm age, ln real value of exports; (B) mean ln wage, ln employment, firm fixed effects, worker fixed effects and their one and two-year growth rates, the within-firm variance of worker fixed effects, ln firm age, ln real value of exports, ln sales, ln value added, share of female workers; (C) mean ln wage, ln employment, ln employment squared, ln sales, sales/exports, sales/exports squared, mean age. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

Table D5: Comparison of different matching covariates (Coarsened Exact Matching).

	D		E		F	
Years since	Firm FE	Worker FE	Firm FE	Worker FE	Firm FE	Worker FE
acquisition	(1)	(2)	(3)	(4)	(5)	(6)
$s = -3$	-0.0020	0.0054	0.0057*	0.0087**	0.0018	-0.0029·
	(0.0051)	(0.0044)	(0.0028)	(0.0033)	(0.0017)	(0.0016)
$s = -2$	-0.0030	-0.0001	-0.0010	0.0066**	0.0023	-0.0018
	(0.0036)	(0.0033)	(0.0022)	(0.0024)	(0.0014)	(0.0012)
$s = 0$	0.0068*	0.0011	0.0159***	0.0022	0.0106***	-0.0113***
	(0.0034)	(0.0028)	(0.0019)	(0.0022)	(0.0025)	(0.0021)
$s = 1$	0.0198***	0.0037	0.0233***	0.0107***	0.0259***	-0.0172***
	(0.0042)	(0.0037)	(0.0024)	(0.0027)	(0.0031)	(0.0028)
$s = 2$	0.0286***	0.0034	0.0349***	0.0070*	0.0422***	-0.0265***
	(0.0044)	(0.0040)	(0.0028)	(0.0030)	(0.0036)	(0.0034)
$s = 3$	0.0398***	-0.0010	0.0439***	0.0084*	0.0607***	-0.0388***
	(0.0049)	(0.0050)	(0.0031)	(0.0034)	(0.0042)	(0.0039)
Fixed-effects						
Firm ID	✓	✓	✓	✓	✓	✓
Pair-year	✓	✓	✓	✓	✓	✓
# Firm ID	496	496	2,172	2,172	1,404	1,404
# Pair-year	1,736	1,736	7,602	7,602	4,914	4,914
Observations	3,472	3,472	15,204	15,204	9,828	9,828
R^2	0.9040	0.9772	0.8926	0.9525	0.9232	0.9754
Pre-trends						
P-value	0.6863	0.2250	0.0149	0.0114	0.2622	0.1745

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Clustered standard errors (Firm ID) in parentheses. s identifies years since acquisition. Dependent variables are firm-level averages of the decomposition on (2). Estimated using difference-in-differences regression (1) on different coarsened exact matching samples. The regressions include a fixed effect for each firm and each year of matched pairs of firms. Coarsened exact matching on 2-digit NACE industry, year; and percentile distribution of (D) firm fixed effects, worker fixed effects, ln employment, ln firmage and ln real value of exports; (E) firm fixed effects, worker fixed effects and within-firm variance of worker fixed effects; (F) one- and two-year growth rates of firm fixed effects and worker fixed effects. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

Table D6: P-values of different approaches to standard error calculation.

A: Firm FE

Years since acquisition	Coef. (1)	Firm ID (2)	Pre-Post (3)	Two way (4)	RI (5)
$s = -3$	0.0024	0.2753	0.2753	0.2825	0.4382
$s = -2$	0.0019	0.2643	0.2643	0.1864	0.4277
$s = 0$	0.0107	0.0000	0.0000	0.0000	0.0000
$s = 1$	0.0203	0.0000	0.0000	0.0000	0.0000
$s = 2$	0.0264	0.0000	0.0000	0.0000	0.0000
$s = 3$	0.0354	0.0000	0.0000	0.0000	0.0000

B: Worker FE

Years since acquisition	Coef. (1)	Firm ID (2)	Pre-Post (3)	Two way (4)	RI (5)
$s = -3$	-0.0011	0.5741	0.5741	0.6328	0.6885
$s = -2$	-0.0004	0.7786	0.7786	0.7965	0.8420
$s = 0$	0.0024	0.1090	0.1379	0.2045	0.2604
$s = 1$	0.0055	0.0033	0.0006	0.0367	0.0377
$s = 2$	0.0052	0.0150	0.0027	0.0147	0.0870
$s = 3$	0.0073	0.0020	0.0002	0.0551	0.0305

Notes: Comparison of different p-values for the coefficients in Columns 2 and 3 of Table 1. One-way clustering at firm level (Column 2). Separate pre- & post-acquisition clustering (Column 3). Two-way clustering at firm and year level (Column 4). Randomization Inference (Column 5). Randomization Inference with 99,999 repetitions of treatment reassignment between matched firms in 600 randomly drawn pairs. Randomization Inference p-values are calculated as the ratio of t-values more extreme than t-values from clustering at firm level.

Table D7: Bootstrapped standard errors (Firm FE).

			Bootstrapped clustered using within-firm variation σ		
Years since acquisition	Coef. (1)	Clustered (2)	$1 \times \sigma$ (3)	$2 \times \sigma$ (4)	$3 \times \sigma$ (5)
$s = -3$	0.0024	0.0022	0.0058	0.0101	0.0149
$s = -2$	0.0019	0.0017	0.0054	0.0097	0.0137
$s = 0$	0.0107	0.0017***	0.0054*	0.0101	0.0144
$s = 1$	0.0203	0.0022***	0.0058***	0.0102*	0.0149
$s = 2$	0.0264	0.0024***	0.0060***	0.0103*	0.0148$^{\cdot}$
$s = 3$	0.0354	0.0029***	0.0063***	0.0105***	0.0150*

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Comparison of different (bootstrapped) clustered standard errors for the coefficients in Column 2 of Table 1. Column 2 shows the standard errors of Table 1. Columns 3 to 5 show bootstrapped clustered standard errors calculated across 9,999 difference-in-differences estimations. For each estimation new firm fixed effects are drawn from a normal distribution with mean equal to Coef. and standard deviation equal to the within-firm standard deviation of firm fixed effects σ (Column 3); two times the within-firm standard deviation $2 \times \sigma$ (Column 4); and three times the within-firm standard deviation $3 \times \sigma$ (Column 5).

Explanation of figures

Empty cell	Figure not applicable
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–	(between two numbers) inclusive
0 (0.0)	Less than half of unit concerned
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2020/2021	Average for 2020 up to and including 2021
2020/’21	Crop year, financial year, school year, etc., beginning in 2020 and ending in 2021
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