Do workers or firms drive the foreign acquisition wage gap?

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Abstract

Foreign-acquired firms pay higher wages. The wage gap may arise with worker composition (e.g., sorting of high-quality workers) or firm-level premia (e.g., productivity improvements). We propose a dynamic decomposition on the Netherlands' universal employer-employee data to understand the drivers of the post-acquisition wage gap. The wage gap rises from 1% to 5% after the acquisition, and firm level premia account for roughly three-quarters of the gap. The contribution of the workforce composition is initially absent, but grows to one-fifth of the wage gap, driven solely by new hires. Firm-level premia associate with higher management pay, worker training, and firms' internationalization strategies. We show how the implied relative importance of worker sorting and firm-level development varies with assumptions on the counterfactual of the acquisition.

Keywords: foreign acquisition, wage decomposition, matched employer-employee data, labor mobility, Netherlands

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1 Introduction

Most studies of multinationals' wages find that foreign firms pay higher wages because they hire better workers (e.g., Setzler and Tintelnot, 2021; Balsvik, 2011; Heyman et al., 2007). Employees of foreign-owned firms have higher levels of education, experience, and other measures of quality (Heyman et al., 2007; Andrews et al., 2009; Hijzen et al., 2013). The sorting of workers into multinational firms explains large shares of the overall pay gap of foreign over domestic firms, which ranges from 2% to 50% across many countries (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).

However, a foreign acquisition changes the firm, for instance in its productivity, its practices and management, and its training of workers (Girma and Görg, 2007; Bircan, 2019; Koch and Smolka, 2019). Such changes plausibly lead foreign-owned firms to pay higher wages. An expansion of the firm's activity can also raise wages through local labor demand (Kovak et al., 2021), increase aggregate productivity, and generate local spillovers (Haskel et al., 2007; Keller and Yeaple, 2009; Stoyanov and Zubanov, 2012; Poole, 2013). The improvements in firm operations that increase local wages, including increased productivity, technology transfers, or spillovers, form a common justification for substantial policies to attract multinationals. They are also central in the debate on how multinationals affect local labor markets. Still, there is little evidence from labor market studies that foreign ownership leads to change at the firm level beyond the increased sorting of high-quality workers.

It is important to understand whether wages in foreign-acquired firms are higher because of the selection of workers, or because the firm itself contributes to higher worker pay. If the wage gaps of foreign-owned firms only reflects the high quality of workers, multinationals may merely herd productive workers, casting a pessimistic light on the contribution of foreign firms to their host economies. Instead, the benefits of foreign acquisitions that most policy makers hope for, including technology transfers and productivity growth, materialize in firm-level contributions to wage premia after an acquisition.

This study identifies the relative contributions of workers and firms in the wage premia after a foreign acquisition. We use the universal employer-employee data of the Netherlands for the years 2006 to 2018 to identify the wage developments of workers in foreign-acquired firms. We estimate the causal impact of a foreign acquisition on the wage gap and its constituent components for over 1,200 firms. We use a dynamic difference-in-differences regression that differs out fixed effects for firms, and yearly fixed effects for the acquired firm and its matched counterfactual firm. To match firms, we use a propensity score based on pre-acquisition size, wage variation, age and export status as covariates, which yields high pre-acquisition similarity between the acquired firms and the non-acquired matched firms. We estimate the impacts of the acquisition on the wage and its different components: the time-varying fixed effects for firms, its workers' fixed effects, and the remaining worker observables (Abowd et al., 1999; Engbom et al., 2023).

Our results show that three quarters of the wage gap of foreign-acquired firms originate from firm-level changes, and only a minor share originates from worker sorting. The total wage gap after a foreign acquisition rises up to 5% in three years, and firm-level premia account for 1.1% to 3.6% of wage over that period. The workforce composition, by contrast, cannot explain wage differences between acquired and domestic firms at the time of acquisition, and explains around 0.7% in wage difference by the third year after acquisition. These findings contrast the majority of the literature, which traces the wage gap of foreign-owned firms to the workforce composition, rather than to changes at the firm level.

Given these contrasting findings, we explore several explanations for the importance of firm-level wage premia after an acquisition put forward in the literature. The strongest firm-level pay growth after an acquisition concentrates in firms with fewer than one hundred employees, and in knowledge intensive services as well as (low-tech) manufacturing. We find that managers' wages rise about twice as fast after a foreign acquisition than the wages of other workers in the same firm. Firm-level explanations account for 57% of the managers' pay increase in acquired firm, and acquired firms attract better paid new managers. We also find that in later jobs, workers who left acquired firms earn more than workers who left the matched, non-acquired firms, conditional on quality and sorting. Hence, employment in an acquired firm may come with human capital improvements or signalling value, for instance. Additionally, for firms with available sales data, we document a modest increase in sales and exports, but not in value added, suggesting that a shift in firms' internationalization strategies affects firm-level wage premia. To understand why worker composition effects of the acquisition are slow to materialize, we explore how the workforce composition of acquired firms evolves. Acquired firms hire more new workers, and their new workers have significantly higher earnings capacities, leading to a gradual increase in pay over the years after an acquisition. New hiring explains the composition effect entirely: The quality and rate of leaving workers are the same between acquired and matched firms.

Our results on acquisitions contribute to the literature that explains why foreign firms pay higher wages. Our estimates of the wage gaps following an acquisition, growing from roughly 1 to 5% in the years after acquisition, are similar to the results for other developed economies (Hijzen et al., 2013; Heyman et al., 2007; Andrews et al., 2009, e.g. for Portugal, Germany, the UK, and Sweden). The central role we document for firm-level premia, but not for worker composition, is a sharp contrast to most related studies. They largely identify worker composition as the major explanation of the wage gap after a foreign acquisition, as the firm-level contributions are minor or zero (Portugal, Germany, the UK) or even significantly negative (Sweden). In a larger linked literature on the general (cross-sectional) premium associated with foreign ownership, worker composition also explains most of the wage gap. For the United States, Setzler and Tintelnot (2021) show that multinationals' worker compositions explain two thirds of the cross-sectional multinational wage gap, conditional on a fixed effect for grouped firms (Bonhomme et al., 2019). Balsvik (2011) relatedly shows that the wage premium in worker fixed effects at multinational firms is almost as large as the overall wage gap.

Our empirical approach is novel relative to most of this literature. We use a more complete decomposition of the Abowd et al. (1999) wage equation, by allowing for firm-year fixed effects and estimating it on the full employer-employee network. In our approach, the estimated individual wage components necessarily add up to the aggregate wage effect. This allows a comparison of the relative importance of selection, firm-level changes, and other factors. Almost all other literature employs sub-sampling strategies, such as matching workers (e.g., Egger et al., 2020) or identification based on staying workers within the firm (e.g., Heyman et al., 2007) to difference out worker composition or firm-level effects. The isolated components from such sub-sampling strategies typically do not add up to the estimate of the aggregated wage premium. If the estimated individual components of wage change do not add up to the total wage effect, it is difficult to evaluate the relative contribution of every component in the overall wage change. The contributions may be estimated from different samples, and it may be unclear how to weigh worker-level results against firm-level results. In our approach, by contrast, the wage components always sum up to the total wage gap, permitting a direct comparison of their importance.

Our analysis focuses on acquisitions for two reasons. First, analyzing acquisitions offers insight into the dynamic effect of multinationals on the labor market. Recent advances in network estimators allow the the identification of time-varying firm fixed effects instead of static firm fixed effects. Tracing the immediate development of wage components in the years after the event reveals short-term impacts of foreign ownership that are relevant to workers' job choices and the strategic decisions of policy makers.

Second, the event of an acquisition offers a plausible counterfactual, as the difference-in-differences analysis between a pair of matched firms draws a comparison between two similar firms that were initially not acquired. Such a difference-in-differences strategy cannot be applied in a static comparison of foreign- and domestically-owned firms. We find stronger firm-level effects and weaker worker selection effects than studies that identify the static wage (component) differences between multinationals and domestic firms (e.g., Setzler and Tintelnot, 2021; Balsvik, 2011, for the U.S. and Norway respectively, and broader results for developed countries in amongst others Hijzen et al., 2013). We find similarly large roles for worker selection when applying the methodologies of this literature in the Netherlands, both for ownership and for acquisitions. Instead, we show that the methodological advance of more closely identifying a non-acquired counterfactual firm explains why we find larger firm-level premia of acquisitions and smaller worker selection effects.

Our results also relate to studies that question how a foreign acquisition changes the firm's organization and strategies. First, our result that managers benefit disproportionately from firm-level changes corresponds with evidence that firms pay higher wages to managers (Egger et al., 2020) or generally to high-skilled workers (Heyman et al., 2011; Martins, 2011) after an acquisition. However, our results additionally show that higher management pay is not only driven by the selection of workers into the management of acquired firms, but by a firm-wide pay change to management. Second, comparing movers in and out of acquired firms in Germany, Andrews et al. (2009) document that exiters from acquired firms may experience up to 5 per cent higher wages at their next domestic employer. In our data, the estimated premium of previous employment in an acquired firm is 3 per cent, of which 1 percentage point is explained by the sorting of exiters towards high-paying employers, and 0.7 percentage points of the premium remains after accounting for worker selection and sorting into the new job. Third, our results indicate that firm-level premia arise more strongly in industries such as knowledge intensive services than in others. This analysis across industries is novel as few datasets to date provide enough detail for its identification. It refines the insight that wage premia in the wake of a foreign acquisition mostly arise in innovation and skill intensive industries (e.g. Egger et al., 2020), indicating that firm-level changes and worker selection effects play out differently across industries.

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 matched firms that remain domestic.

Domestic firms are arguably not plausible counterfactuals for foreign acquired firms (had they not been acquired), as the groups differ along several dimensions. Therefore, we use pre-acquisition characteristics to match acquired firms to firms that remain domestic. Matching on the propensity score for foreign acquisition is a conventional solution to eliminate potential biases from firm target selection in difference-in-differences 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 the post-acquisition wage gap, and the worker composition and firm developments that contribute to the gap.

We use an auxiliary step to identify the contributions of individual workers and firms to wages. In the universal employer-employee dataset, we decompose wages into wage variation attributable to the firm and to the worker (in observed and unobserved characteristics). The next subsections lay out the steps of our empirical strategy in detail.

2.1 Difference-in-differences framework and matching

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 the pair of firms m, which consists of an acquired firms and a matched firm. The matching procedure is described in the next subsection 2.2. With this pair-year 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 controls for time-varying omitted variables that both firms in the pair experience, such as local policy changes, demand fluctuations, or labor market developments. Second, our specification contains a firm-level time-invariant fixed effect, Ψ_j. It controls for any unobserved firm-level confounders and prevents level differences between the firms from explaining the estimated wage effects. While matching may control for most differences in the firm-level fixed effects within the matched pair, unobserved time-invariant differences, such as material assets or management practices, are controlled for with the firm-level fixed effect.

2.2 Matching 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). In our data, we confirm substantial differences in levels and growth of employment, wages, and fixed effects between domestic and target firms (see Table B1 in Appendix B.3). As a consequence, wage changes 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. We indeed find significant deviations in pre-acquisition developments when applying the difference-in-differences framework on the unmatched sample (see Table A4 in Appendix A). 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 pre-trends with our difference-in-differences coefficients. In Section 3.1, we discuss the balance in covariates after matching (covariate balance is documented in Table B2 in Appendix B). In Section 4.2.3, we also discuss the robustness of our results to using different sets of covariates for matching and to employing coarsened exact matching instead of propensity score matching.

2.3 Wage decomposition

To understand what causes wages to change after an acquisition, we decompose workers' observed wages into a worker-specific unobserved component, a firm-level premium, and observable characteristics of the worker. We use a variant of the decomposition of Abowd et al. (1999, AKM henceforth) that allows firm contributions to vary by calendar year (Engbom et al., 2023). 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 the time- and employer-invariant worker-specific component of wage. It is often interpreted as a measure of worker productivity, and captures workers' observed and unobserved capacity 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., 2018). The yearly 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. Figure C1 in Appendix C confirms this intuition by showing step-wise wage losses and gains for workers moving between firms of different fixed effects (Card et al., 2013). In Section 4.2.1, we discuss the identification of the firm fixed effects in detail.

The estimation of equation (2) forms the full wage decomposition as the components add up to the full observed wage for every worker by construction. 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., 2023).

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.

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. As usual in the literature, we focus our estimation on the subset of firm fixed effects that are connected through worker movements (Abowd et al., 1999, 2002). This 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 five 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 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 2.2. 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 Appendix D).

In total, we find matches for 1,269 target firms. Table B2 in Appendix B.4 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

[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.

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 wage gap, 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 Appendix A 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 Appendix A). 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 Main Results

In our main set of results, we estimate what share of the post-acquisition wage gap 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 offer several robustness checks on the result. In Section 5, we explore possible causes of changes in firm premia and worker composition.

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

Figure 1 presents the results of the difference-in-differences regressions that compare wage developments in acquired firms to the counterfactual matched firms.[2,3] The development of the mean log wage is depicted by circles. The remaining estimates show the separate impact of the firm- and worker-level wage components that jointly explain the overall log wage development (as decomposed from the AKM model, see Section 2.3). We show 95%-confidence intervals with standard errors clustered at the firm level to account for within-firm serial correlation of errors. In Section 4.2.4, we explore alternative approaches for calculating these standard errors.

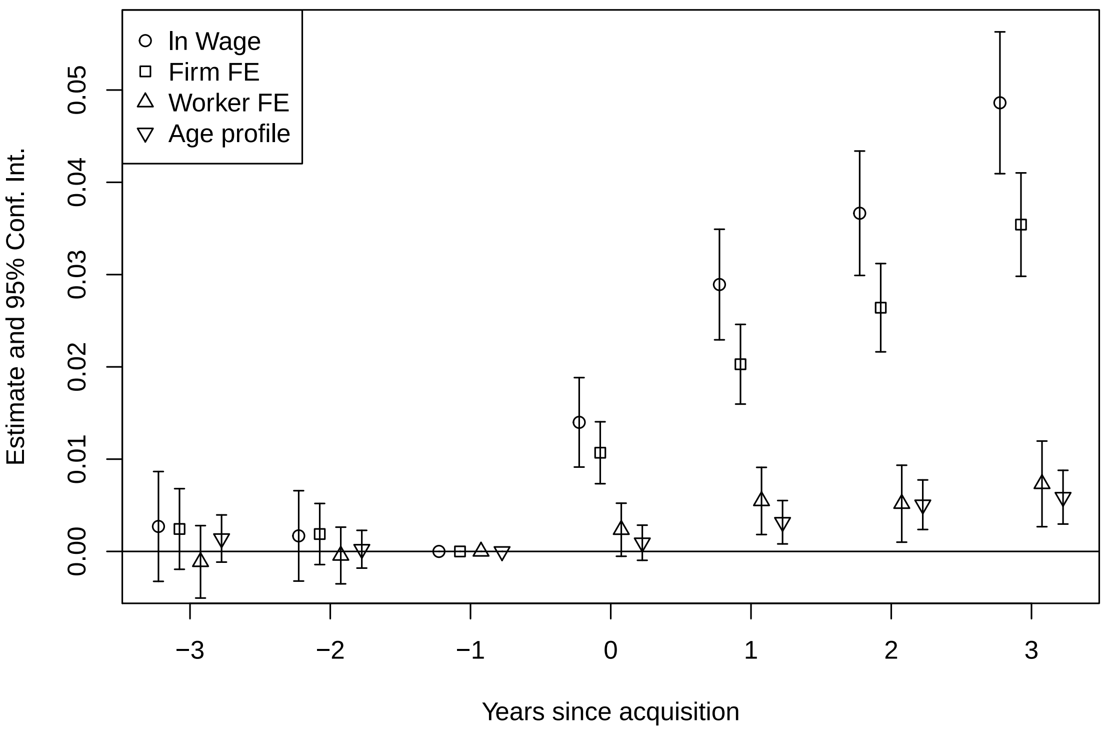
Figure 1 shows a statistically significant wage gap of around 1.41% (or 0.014 log points, $e^{0.014} \approx 1.0141$) between the acquired and its counterfactual matched firm in the year of acquisition.[4] The wage gap grows over

[2]The estimates are in Table A1 in Appendix A.

[3]In Table A4 in Appendix A, 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.

[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

Figure 1: Decomposition of the post-acquisition wage gap.



Notes: The figure shows the coefficients and 95% confidence intervals of the main decomposition result. The estimates are in Table A1 in Appendix A. Confidence intervals are based on clustered standard errors (Firm ID). Coefficients are estimated using difference-in-differences regression (1) on propensity score matching sample. Dependent variables are firm-level averages of the AKM decomposition on equation (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 2.2 for details. Wald tests on the joint-significance of pre-acquisition coefficients ('Years since acquisition' < 0) show no sign of diverging pre-trends, see Table A1.

time, to 2.93%, 3.72% and 4.98% in the first, second and third year after the acquisition has taken place.

The development of the firm fixed effect after acquisition is depicted by squares. 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 gap. The development of the firm fixed effect is significantly different from zero for all post-acquisition years. Upward facing triangles plot the development of the average worker fixed effect in the acquired firm. The magnitude is considerably lower than that of the firm fixed effects, with statistically significant increases of around 0.5 to 0.7% in the years after acquisition. Finally, downward facing triangles plot the development of wage 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 gap. The share of the wage gap explained by firm fixed effects is largest in the acquisition year where it explains 76% ($\approx 0.0107/0.0140$, see Table A1). 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 gap. Two and three years after the acquisition, they explain 14% and 15%. Changes in the age profile explain less of the wage gap, 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 gap and remains its main driver throughout the following years.

The causal interpretation of the results in Figure 1 assumes that the matched firm appropriately proxies for the acquired firm's development if it had not been acquired. We apply several tests. First, the difference-in-differences comparison relies on a parallel trends assumption. The pre-acquisition coefficients ($s = -3$ and $s = -2$) are all statistically insignificant and show no signs of divergence between matched firms before the acquisition. As a formal test, we report the p-value of the joint Wald test on the pre-acquisition coefficients in Table A1 in Appendix A. All p-values are higher than any conventional level, implying that the matched firms' trends were parallel before the acquisition. Second, in Section 4.2.3 we explore the sensitivity of our result to the matching procedure and set of matched firms. No qualitative differences arise when varying the set of matched firms. Third, we drop the control group entirely and estimate a before-and-after comparison using only acquired firms (see Column 2 of Table D1 in Appendix D). Not accounting for parallel developments in the control group leads to marginally higher estimates of the impact of acquisition. Fourth, we adjust the firm fixed effect estimates of equation (2) with industry- and location-year fixed effects in the first step (Column 4). Accounting for industry- and location-specific annual shocks leads to no qualitative differences in the conclusions. Finally, we check whether our difference-in-differences estimates hold up to the Callaway and Sant'Anna (2021) correction and find robust results (see Column 5 of Table D1).

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acquisition.

4.2 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 changes in the firm premium 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.

4.2.1 Firm-worker network

The yearly firm fixed effects that serve as a dependent variable in the difference-in-differences regression are estimated from a network dataset of firms and workers. The level estimates for the fixed effects are unbiased under standard OLS assumptions (Abowd et al., 1999; Bonhomme et al., 2023; Andrews et al., 2008).[5] Still, the fixed effects estimates may be noisy if few workers move across firm, in which case some parts of the network are not well connected. The potential measurement error in the fixed effects is addressed in the difference-in-differences regression, but it might affect our standard errors. Section 4.2.4 explores alternatives approaches to calculating the standard errors. In Appendix C.1, we additionally explore the ramifications of worker mobility for our estimates. In particular, we find that acquired and matched firms are typically highly connected within the network, and hardly appear in parts of the network where there is scope for a weaker identification of the fixed effects. We also constrain our analysis to a subset of firms that are well connected to construct a dataset in which the lack of moving workers is not plausibly an issue. We find very similar results in that subset, where the connectivity measure is above the weak connection threshold (Jochmans and Weidner, 2019).

A related assumption of the AKM model is that the firm and worker fixed effects are additively separable in log wages (Bonhomme et al., 2019). If firm and worker fixed effects instead interact in determining the wage, then omitting the interaction from the model may lead the estimates of firms' fixed effects to be overstated. In our context, this could be problematic, if firm-worker interaction is structurally different between acquired firms and other firms. To test for differences in such interactions across groups of firms, we introduce firm-worker interacted premia in the AKM model (the first step of our analysis) that are specific to groups of firms (De la Roca and Puga, 2017). We then re-estimate the difference-in-differences regression using the fixed effects estimated conditional on interaction effects (for details see Appendix C.2). The estimate for complementarity rises after an acquisition: A worker with a higher fixed effect than a peer commands higher excess wage than a similar worker to a similar peer in a control firm. A standard deviation higher worker fixed effect is associated with up to 2% higher pay after the acquisition, leading to a divergence in pay within acquired firms. However, the contribution of that complementarity to the change in the average wage in an acquired firm is very close to zero and insignificant. Hence, complementarity effects are by far too small to explain the acquisition wage gap.

[5] The "limited mobility bias" (Jochmans and Weidner, 2019; Bonhomme et al., 2023), stemming from sparse connectedness in the network, represents a bias in the second moment of the fixed effect distribution but not in the level estimates, which we use.

4.2.2 Identification from stayers

In order to isolate post-acquisition firm-level wage developments from changes in the workforce, most earlier studies exploit a sub-sample 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 sub-sample 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 Appendix D, 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 sub-sample of stayers.

4.2.3 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 Appendix D 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 change in firm fixed effect and firm-average worker fixed effect. 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 pre-trend 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 firm-average worker fixed effects. This is not entirely surprising as the firm sets differ substantially by matching strategy, and the evidence in Section 5 will suggest that the estimated changes in firm-average worker fixed effects are determined by a limited number of firms.[6] Together, this suggests that the main result that growth in the firm fixed effect explains most of the acquisition wage gap is stable both across matching methods and sets of covariates used to match firms.

4.2.4 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., 2023). 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 infeasible 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 of 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] When matching on the within-firm variance of the worker fixed effects, we find significant pre-trends.

4.3 Comparison to cross-sectional estimates of the multinational wage gap

Relative to earlier results on multinational wages, we find considerably larger roles for firm-level changes and a considerably smaller worker selection effects after an acquisition (e.g., Balsvik, 2011; Schröder, 2020; Setzler and Tintelnot, 2021; Tanaka, 2022). These earlier studies differ on various dimensions: they study different contexts, they study static (cross-sectional) ownership premia instead of acquisition effects, and accordingly they make different methodological choices. In this subsection, we discuss auxiliary results that suggest the difference originates from methodological choices rather than from context or the focus on acquisitions.

First, the Netherlands may be a specific context. To understand whether the context matters, we apply a commonly used cross-sectional methodology in our dataset. We use dummies for foreign ownership to estimate the impact of foreign ownership on worker wages, firm-level fixed effects and average worker fixed effects, conditioning of industry-year fixed effects. The results are in Table A2 in Appendix A. They show a foreign-owned wage gap estimate of around 32% ($\approx exp(0.277) - 1$), and importantly, firm fixed effects account for around a third of that premium. Hence, when applying the cross-sectional approach in the Netherlands, we find very similar results as studies for other developed countries, making it less likely that the Dutch context accounts for the differences (e.g., Setzler and Tintelnot, 2021; Balsvik, 2011, for the US and Norway).

Second, our results focus on the wage gaps after an ownership change due to an acquisition, while most related literature focuses on cross-sectional wage gaps associated with foreign ownership (e.g., Setzler and Tintelnot, 2021; Balsvik, 2011). To understand whether the difference in focus explains the difference in results, we estimate the effects of ownership on wages, firm fixed effects, and worker fixed effects in the sample consisting of the acquired firms and years from our baseline estimate, as well as all domestic firms. Thus, we identify the static, cross-sectional foreign ownership wage gap in the firms acquired during our sample period (a subset of all foreign-owned firms) relative to domestic firms. Table A3 in Appendix A shows the results. When applying the methodology to identify (cross-sectional) ownership premia on the acquired firms exclusively, we similarly find a wage gap of over 20% - significantly larger than our baseline estimate. Likewise contrasting our baseline estimate, the majority of the estimated cross-sectional wage gap derives from worker selection, and one third from firm fixed effects. As the cross-sectional methodology applied to a sample of acquired firms yields similarly large estimates of worker selection effects as earlier studies, the focus on acquired firms is not plausibly the origin of the smaller wage gap and the larger role of firm fixed effects.

Instead, two methodological choices can be the source of the changed results. The first is the difference-in-differences estimator. The cross-sectional estimates cannot be easily compared to the difference-in-differences estimator, as there is no pre and post comparison in the cross-sectional approach. To understand the importance of the difference-in-differences estimator relative to the cross-sectional approach, we estimate the difference-in-differences regression on the broad sample instead of the matched sample, now accounting for industry-year fixed effects instead of match-year fixed effects. Table A4 in Appendix A shows the results. Changes in firm fixed effects explain the full wage gap after an acquisition; in fact, the estimates of worker selection effects are negative. Hence, considering changes before and after the acquisition, as in the difference-in-differences estimate, may be the source of the result that firm fixed effects explain large shares of the post-acquisition wage gap. Table A4 in Appendix A also shows considerable pre-trends when estimating a difference-in-differences regression in the full sample, which suggests that acquisitions come with significant selection effects (Almeida, 2007) that require addressing.

The second methodological difference of our paper from related studies is the matching of acquired firms to control firms. The cross-sectional approach does not permit such matching, as that approach cannot use an acquisition event to match firms. To draw a comparison between our results and the cross-sectional analysis without matching, we estimate the ownership wage gaps in the sample of acquired firms and their matched firms, but only after the acquisition. Unlike the difference-in-difference estimates, there are no firm fixed effects in this specification. The results are in Table A5 in Appendix A. In the matched sample with only post-acquisition observations, over half of the wage gap is explained by the firm fixed effects, and about a third by worker selection effects.

The importance of the difference-in-differences with matching methodology suggests that the assumption of the counterfactual for foreign ownership is central to the conclusions on the relative importance of firm effects and worker selection. The elevated role of firm-level fixed effects in our main results suggests that our estimates account for selection of foreign ownership status on the average worker fixed effects of the firm, confirming that matching, like the difference-in-difference estimator, can account for the larger role of firm-level wage premia.

We illustrate the importance of the counterfactual selection in the cross-sectional estimates by additional results from the acquisition sample. Although focusing on the acquisition sample is clearly not comprehensive, the sample may still be informative as the cross-sectional estimates of wage impacts are comparable between firms that were foreign-acquired during our sample period (Table A3 in Appendix A) and foreign-owned firms overall (Table A2 in Appendix A). In this sample, we can examine the wage premia in firm that are candidates for acquisition but have not been acquired yet. Table A6 in Appendix A shows the estimates of wage gaps i) of domestic firms that will later be acquired (i.e. over the firms' pre-acquisition observations) relative to firms that are always domestic and ii) of firms that are always under foreign ownership ("always foreign") relative to firms that are always domestic. Firms that will later be acquired already pay 17% ($\approx exp(0.16) - 1$) higher wages than other domestic firms, while always foreign firms pay 33% ($\approx exp(0.29) - 1$) higher wages. The wage gap between domestic firms that will later be acquired and always domestic firms is almost exclusively explained by the (pre-acquisition) difference in their workers' fixed effects. The contribution of workers' fixed effects to the wage gap over domestic firms is very similar between to-be-foreign-acquired firms and always foreign firms. Hence, this suggests that the higher worker level fixed effects are already present in firms that will later be the targets of a foreign acquisition. Accordingly, our baseline estimates possibly show lower overall wage gaps and a large role for firm fixed effects, because our methodology uses firms with similar pre-acquisition worker compositions as a counterfactual. It is not possible to confirm that in the cross-sectional data, because there is no feasible matching procedure in that setting.

5 What drives firm-level premia after an acquisition?

Our main decomposition documents that increasing firm-level premia account for a large share of the wage gap after a foreign acquisition. In this section, we explore the rise in firm-level premia. First, we document firm size and industry differences. Second, we separate the contribution of managers' and non-managers' wages. Third, we examine the value of training or experience in acquired firms. Fourth, we trace changes to the operations and internationalization strategies of acquired firms for a subset of the acquisitions in our data.

5.1 Firm size and industry heterogeneity

Larger firms may respond differently to an acquisition than smaller firms. Search frictions and imperfect labor markets can cause larger, more productive firms to 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). Similarly, large firms may have a different scope for productivity improvements through transfers of technology, knowledge and management practices.

Table 1 shows the estimates of the difference-in-differences regression of changes in firm and firm-average worker fixed effects after acquisition by the size class of the firm. The size class is expressed in the number of employees 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 growth in 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 changes also vary substantially with firm size. For firms of size 20-49 and of size 50-99, acquisition leads to significant improvements in the average worker fixed effect. 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 premia and worker composition differently. To examine whether the impact varies with the use of technology, we run our analysis on a sample split according to Eurostat's definitions of knowledge-intensive and high-tech sectors.[7] Table 2 shows the regressions in the respective samples. For services, growth in firm fixed effects (Columns 1 and 3) are more important in explaining the acquisition wage gap than growth in firm-average worker fixed effects. For knowledge-intensive service sectors, the estimated wage gap explained by the change in firm fixed effect is more than twice as large as for non-knowledge intensive sectors. A Wald-test on the pooled

[7]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 1: 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 2.2 for details.
Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

sample shows that the difference between the sectors is statistically significant (χ^2(4, 5024)=7.32, p $<$ 0.001). In knowledge-intensive service sectors, there is some evidence for a change 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 the change in average worker fixed effects between the sectors is statistically insignificant (χ^2(4, 5024)=1.59, p = 0.17). Among manufacturing firms, firm fixed effect growth explains the acquisition wage gap 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 (χ^2(3, 844) = 2, p = 0.11). In contrast to services, in high-tech manufacturing the growth in average worker fixed effects is more important than growth in firm fixed effects just after the acquisition: In the first two years, the estimates for changes in average worker fixed effects are significant and larger than the estimates for changes in the 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 firm-average worker fixed effects in firms between worker size 20 and 99, and in knowledge-intensive services and high-tech manufacturing.

5.2 Managers

The rise in firm-level premia in Figure 1 may apply specifically to managers (Heyman et al., 2011; Egger et al., 2020). Profits from internationalisation are often shared with the management, for example through incentive-based contracts (Egger et al., 2020). Similarly, an acquisition can change the internal organization and management practices of the firm, raising the average wage of managers more than that of non-managers (Bastos et al., 2018).

We examine whether the wage premia for managers rise faster than those for non-managers by examining the residual wage variation from our decomposition (2). As the firm fixed effects in equation (2) are time-variant, the mean residual for workers at the firm level is zero, and deviations for specific groups is captured by their respective residuals.[8] In addition, in our framework, higher wages for managers may arise through composition if the average worker fixed effect for managers in a firm changes.

We make use of two sources to identify managers. We identify members of firms' boards of directors, owners and upper management through the firm's Chamber of Commerce listing starting in 2010. We complement this data with information on ISCO-08 occupations, from a 4% random sample of workers in each year over the entire sample period from 2006 to 2018. We identify workers as managers if within the firm-worker match, the worker is ever identified as a manager according to either of the two sources.[9] Managers' and non-managers' wages cannot be separated for all firm-years. Therefore, we apply the single difference-in-differences estimator (see equation

[8]In Appendix C we show that the interactions between the firm and worker fixed effects do not generally explain the post-acquisition wage premium.

[9]We also check sub-sample estimates using only the Chamber of Commerce data to identify managers; sub-samples up from 2010; and a sample using only the observations where we clearly identify a worker as a manager from either of the two sources. We find no qualitative difference in the conclusion.

Table 2: 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 2.2 for details. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

(5)).

Table 3 shows a separate decomposition for managers and non-managers. The estimates imply that after acquisition, managers' average wages (Column 2 of Panel 3a) rise by 6%, while non-managers' wages rise by 3% (Panel 3b), compared to the matched firms. As the set of firms is the same across the two tables, the estimates for the change in the firm premium (Columns 2) are identical for managers and non-managers. Any firm-level deviation from the firm-level premia in the wages of managers and non-managers is reflected in changes in the residual. The estimates in Columns 5 suggest that managers receive around 1.6% higher wage than the general firm-premium and non-managers are below the firm premium by around 0.2%. After an acquisition, the worker fixed effects of managers also show a significant increase of 1.7% (Column 3), indicating that the average worker fixed effects in the management workforce increase after an acquisition, relative to the matched firm. Among non-managers, the observable characteristics change to increase wages (Column 4).

These estimates imply that both managers and non-managers benefit from acquisitions. However, the excess pay for managers over non-managers rises faster in acquired firms, and 57% ($(0.0162+0.0016)/(0.0625-0.0312)$) is driven by the firm-level premia that acquired firms pay to their managers. The remaining 43% result from differences arising from the composition of managers and non-managers, as acquired firms attract managers that earn more.

5.3 Worker-specific post-acquisition premia

An alternative explanation for the firm-level improvements in wage after an acquisition (as in Figure 1) is that the workers at the time of acquisition collectively increase their earnings potential. For instance, Bastos et al. (2018) present evidence that foreign-owned firms actively raise their workers' skills through on-the-job training. For workers that stay with the firm after an acquisition, a collective increase in worker fixed effects is observationally equivalent to a rise in firm fixed effects. As our specification (2) necessarily contains time-invariant worker fixed effects, across-the-board worker fixed effect changes, for instance through experience or training, may reflect in the time-varying firm fixed effect. This distinction is semantic for workers who stay with the acquired firm, as wages rise through firm or worker-level improvements of the fixed effect without selection effects.

For workers who move after an acquisition, we can better identify whether the wage premium after an acquisition was tied to the firm or to the worker. We compare workers who leave an acquired firm to workers who leave a control firm. First, we extract the wage components at the new employer (estimated on the full firm-worker network) of workers who left a firm in the matched sample. Then, we employ difference-in-differences regression (5) to decompose changes in the moving workers' average wages at their new employers into i) differences in the fixed effect of the new employer, ii) movers' average worker fixed effects, iii) worker observables, and iv) the residual. Given the constraint that worker fixed effects are constant over time in our initial regression, any structural worker-level improvement in earnings capacity after an acquisition reflects in a higher residual at the workers' new job.

Table 4 shows how the components of the wage differ between workers who left an acquired firm, relative to workers who left a matched control firm. The coefficient in Column 1 implies that workers who left an acquired

Table 3: Wage decomposition of managers' and non-managers' wages.

(a) Wage decomposition of managers' wages.

	Ln Wage	Firm FE	Worker FE	Age profile	AKM residual
	(1)	(2)	(3)	(4)	(5)
Post-Acquisition	0.0625***	0.0263***	0.0168*	0.0033	0.0162***
	(0.0097)	(0.0027)	(0.0077)	(0.0027)	(0.0035)
Fixed-effects					
Firm ID (1,946)	✓	✓	✓	✓	✓
Pair-post (1,722)	✓	✓	✓	✓	✓
Observations	11,307	11,307	11,307	11,307	11,307
R^2	0.8802	0.8549	0.9017	0.8015	0.3964

(b) Wage decomposition of non-managers' wages.

	Ln Wage	Firm FE	Worker FE	Age profile	AKM residual
	(1)	(2)	(3)	(4)	(5)
Post-Acquisition	0.0312***	0.0263***	0.0005	0.0060***	-0.0016*
	(0.0036)	(0.0027)	(0.0023)	(0.0015)	(0.0006)
Fixed-effects					
Firm ID (1,946)	✓	✓	✓	✓	✓
Pair-post (1,722)	✓	✓	✓	✓	✓
Observations	11,307	11,307	11,307	11,307	11,307
R^2	0.9475	0.8549	0.9578	0.8862	0.3527

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. The sample includes average wages of managers (panel 3a) and non-managers (3b). Dependent variables are the firm-occupation-level average wage components as estimated by the decomposition on equation (2). The regressions include fixed effects for each firm and matched-pair fixed effects that differentiate between pre-acquisition and post-acquisition years. Estimated using difference-in-differences regression (5) on propensity score matching sample. Propensity scores are estimated within industry-year groups and using firm-level characteristics in the pre-acquisition year.

firm earn around 3% higher wage at their new employer, compared to workers who left a matched, non-acquired firm. Most importantly, the result in Column 5 indicates that the residual explains around 20% (0.0067/0.033) of that wage benefit: Having left an acquired firm instead of a non-acquired firm accounts for an increased wage of around 0.7%, conditional on the worker's own fixed effect and observables, and on the fixed effect of the new firm. The selection of exiters does not explain the higher wage for workers leaving acquired firms: column 2 shows no differences in the worker-level fixed effects of workers who left an acquired firm relative to a control firm. Instead, the largest share of the higher wage for exiters from acquired firms follows from the result that exiters from an acquired firm end up at firms with significantly higher firm-level fixed effects (column 2). Together, this suggests that a small share of the increase in firm-level fixed effects after an acquisition may effectively be tied to the worker.

5.4 Operations and internationalization

For a sub-sample of the dataset, we observe changes to the internationalization strategies of firms. As the outcomes are not observed for the full sample, but multiple observations exist for just under half of the firms, we interpret these results with more caution and summarize the findings, leaving the details of the analysis in Appendix A.1.

The results show that in a sample where we observe firms' aggregate sales, acquired firms grow significantly faster in terms of sales and employment but not in value added and the value of production. The value of exports rises by around 14%, with no change in the number of export destinations, and no change in imports. Exports and imports are observed for the universal sample. Extending the analysis to the large sample shows that acquired firms increase the value of both exports and imports without updating the number of origin and destination countries. These results indicate that foreign-acquired firms may change internationalization strategies along the intensive margin.

Table 4: Decomposition of moving workers' wage at new firm.

	ln Wage	Firm FE	Worker FE	Age profile	AKM residual
	(1)	(2)	(3)	(4)	(5)
Post-Acquisition	0.0328***	0.0121***	0.0065	0.0075*	0.0067*
	(0.0072)	(0.0026)	(0.0054)	(0.0034)	(0.0030)
Fixed-effects					
Firm ID (2,170)	✓	✓	✓	✓	✓
Pair-post (2,170)	✓	✓	✓	✓	✓
Observations	12,433	12,433	12,433	12,433	12,433
R^2	0.6155	0.4484	0.5885	0.4568	0.3002

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. Dependent variables are the average wage components of a firm's recently separated workers at their new employer, as estimated by the decomposition on equation (2). The regressions include fixed effects for each firm and matched-pair fixed effects that differentiate between pre-acquisition and post-acquisition years. Estimated using difference-in-differences regression (5) on propensity score matching sample. Propensity scores are estimated within industry-year groups and using firm-level characteristics in the pre-acquisition year; see Section 2.2 for details.

6 Hires and separations in the worker composition effect

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

The evolution of a firm's 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.

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 worker fixed effect by lowering the average fixed effect of leaving workers, or, if the fixed effect of leaving workers is generally lower, by letting more workers go.

To analyze the margins by which the average worker fixed effect adjusts 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 estimate a single post-move change across matched firm pairs with observed hires and separations. The regression specification is

$$r_{jmt} = \delta FA_{jmt} + \omega_{mt} + \Psi_j + u_{jmt}, \tag{5}$$

where r_{jt} is the outcome associated with firm j at time t; FA_{jmt} identifies post-acquisition firms; ω_{mt} is a fixed effect for each matched pair (pre and post acquisition); Ψ_j a firm fixed effect; and u_{jmt} an error term.

Equation (5) compares the average change in an acquired firm before and after acquisition to that in the matched sister firm, using pairs of firms in which the outcomes are observed in both periods. 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 Figure 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 average worker fixed effect. The separations margin is very close to zero and statistically insignificant.[10]

It does not seem plausible that the absence of separation effects is driven by a lack of employee churning. The average firm in the sample separated from 16 out of 100 (sd = 14) workers in between the previous and current year, against 19 hired out of 100 (sd = 16). Column 4 of Table 5 shows that acquired firms grow the size of their workforce by about 5%, suggesting that the increase in fixed effects could be a consequence of net employment growth in acquired firms.

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.

[10] We also ran separate difference-in-differences regressions using the share and average fixed effect of separated workers as the dependent variable. We find no evidence that foreign acquisition impacts these margins separately.

Table 5: Hire and separation margins.

		Components			Worker FE	Share of hires
	Δ Worker FE	Hires	Separations	ln Workers	of hires	in workforce
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Acquisition	0.00404**	0.00395**	−0.00009	0.04533***	0.01990***	0.00688*
	(0.00145)	(0.00122)	(0.00086)	(0.00516)	(0.01176)	(0.00327)
Fixed-effects						
Firm ID (2,090)	✓	✓	✓	✓	✓	✓
Pair-post (2,090)	✓	✓	✓	✓	✓	✓
Observations	12,060	12,060	12,060	12,060	12,060	12,060
R^2	0.2522	0.3819	0.3797	0.9676	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. Average worker fixed effects come from the decomposition on (2). Columns 1 to 3 show the estimates of decomposition (4). The dependent variable in Column 4 the number of workers in the firm in a given year. The dependent variable in Column 5 is the average fixed effect of workers entering the firm in a given year. The dependent variable in Column 6 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 2.2 for details.

7 Conclusions

We estimate whether changes in the workforce composition or firm-level premia explain the wage gap after a firm is acquired by a foreign owner. We estimate a wage equation on the universal matched employer-employee data of the Netherlands for the years 2006-2018 and compare the dynamics of wage change in foreign-acquired firms and matched counterfactual firms in a difference-in-differences strategy. The wage in acquired firms rises faster, from 1.4% in the year of acquisition up to 5% by the third year after acquisition, in line with results on other advanced economies.

Roughly three quarters of the wage gap after an acquisition originates in changes of firm-level premia, as measured by time-varying firm-level fixed effects in the wage equation. The worker composition effect, defined as the fixed effects of the firm's workers, accounts for less than 20% of the wage gap. Our result that firm-level premia explain most of the post-acquisition wage gap diverges from the consensus in the related literature. It raises new questions on how acquired firms change, and contrasts frequent scepticism that cross-border acquisitions only reshuffle the local workforce.

We explore several explanations for the rise in firm-level premia after a firm is acquired. We find that the wages of workers in management positions rise most sharply after an acquisition. The rise originates chiefly from firm-wide premia in managers' wages, and to a minor extent from composition effects in the management. Hence, managers appropriate larger wage benefits following an acquisition, increasing wage dispersion within the firm. We also find that workers who exit acquired firms receive higher pay in subsequent employment than workers exiting non-acquired firms. Considerable shares of the wage benefit of earlier employment in an acquired firm persist even after controlling for worker quality and for the characteristics of the next employer. That suggests that acquisition increases workers' later earnings potentials, in addition to an arising selection advantage (as workers typically move to higher paying firms). We also show that the relative contribution of firm-level premia in the post-acquisition wage gap is significantly larger in some industries, and we document in a smaller sample that acquired firms, while not growing in value added, increase the value of their imports and exports.

The role for worker composition in post-acquisition wages is small, relative to estimates in the related literature. As in related literature, we take worker fixed effects to be time-invariant, so composition effects in wages only occur through hires and separations. We find significant increases in the firms' average worker fixed effect through limited hiring, and no changes in the separations of acquired firms. As acquired firms only gradually hire higher-paid workers, composition effects materialize slowly. This incremental change is consistent with studies that show delayed improvements in the technical and labor productivity after a firm's acquisition (Chen, 2011; Fons-Rosen et al., 2021).

One reason why we find a larger role for firm-level premia relative to most worker-level studies of the multinational wage gap, is that we employ a different methodology. We first estimate a two-way fixed effects model to decompose wage into firm- and worker-level components (Abowd et al., 1999; Engbom et al., 2023). In a second step, we identify the impact of acquisition by comparing 1,269 acquired firms to their matched firms, using a combined difference-in-differences matching strategy. Our approach implies a comprehensive decomposition, so that all the individual wage components are measured on the same scale, and the components necessarily add up to the estimate of the overall post-acquisition wage gap. Matching similar firms before an acquisition also establishes an intuitive counterfactual for the acquisition, which is harder when making cross-sectional comparisons between domestic and foreign-owned firms. Indeed, when using a cross-sectional estimator in our data instead of the difference-in-differences counterfactual, the results are considerably closer to the related literature.

A second reason is the broad coverage of our sample relative to related studies. Our sample includes smaller firms, and firms in the service sectors, where the contributions of firm-level premia to the foreign acquisition wage gap are particularly high. While we find higher firm-level contributions across firm sizes and industries, sub-sample analyses suggest substantial heterogeneity in the importance of firms and workers for the wage gap. 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 gap than firm-level developments. For service sectors, we find that the increase in firm-level premia is more than twice as large in knowledge-intensive than in non-knowledge intensive sectors.

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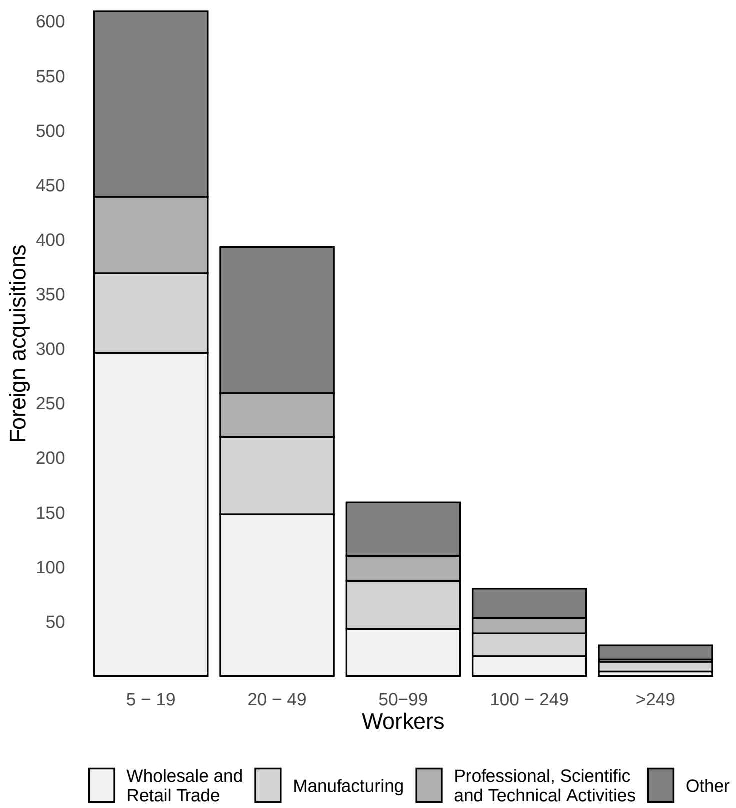
Appendix

A Supporting tables and 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 2.2 for details.

Table A1: Decomposition of the acquisition wage gap.

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 2.2 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 A2: Decomposition of the cross-sectional foreign firm wage gap.

	Ln Wage	Firm FE	Worker FE	Age profile
	(1)	(2)	(3)	(4)
Foreign MNE	0.2766***	0.0931***	0.1530***	0.0306***
	(0.0034)	(0.0015)	(0.0023)	(0.0008)
Fixed-effects				
Industry-year (1,108)	✓	✓	✓	✓
Observations	848,893	848,893	848,893	848,893
R^2	0.4420	0.2610	0.3942	0.2013

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. Ln is the natural logarithm. Dependent variables are firm-level averages of the wage decomposition on (2). The regression includes 2-digit-industry-year fixed effects. Foreign multinationals include all foreign owned firms. The comparison includes all firms with at least five employees that are never observed as Dutch multinationals. It excludes the observations of a foreign firm when it is observed as domestic, such as before an acquisition.

Table A3: Cross-sectional wage decomposition, matched post-acquisition vs. always domestic firms.

	Mean ln wage	Firm fe	Mean worker fe	Mean wage-age pr
	(1)	(2)	(3)	(4)
Foreign MNE & Post-Acquisition	0.1990***	0.0630***	0.1216***	0.0144***
	(0.0078)	(0.0034)	(0.0051)	(0.0020)
Fixed-effects				
Industry-year (840)	✓	✓	✓	✓
Observations	577,304	577,304	577,304	577,304
R^2	0.4230	0.2741	0.3592	0.1949

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. Ln is the natural logarithm. Dependent variables are firm-level averages of the wage decomposition on (2). The regression includes 2-digit-industry-year fixed effects. 'Foreign MNE & Post-Acquisition' includes matched acquired firms over their post-acquisition observations. Pre-acquisition observations are removed from the regression sample. The comparisons group includes all firms with at least five employees that are never observed as Dutch or foreign multinationals.

Table A4: Decomposition of the acquisition wage gap 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 A5: Cross-sectional wage decomposition, matched post-acquisition vs. matched control firms.

	Mean ln wage	Firm fe	Mean worker fe	Mean wage-age pr
	(1)	(2)	(3)	(4)
Foreign MNE & Post-Acquisition	0.0432***	0.0231***	0.0141*	0.0060*
	(0.0097)	(0.0044)	(0.0065)	(0.0026)
Fixed-effects				
Industry-year (517)	✓	✓	✓	✓
Observations	10,152	10,152	10,152	10,152
R^2	0.2941	0.1555	0.3270	0.2016

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. Ln is the natural logarithm. Dependent variables are firm-level averages of the wage decomposition on (2). The regression includes 2-digit-industry-year fixed effects. 'Foreign MNE & Post-Acquisition' includes matched acquired firms over their post-acquisition observations. Pre-acquisition observations are removed from the regression sample. The comparisons group includes all matched firms in the propensity score matching sample. Propensity scores are estimated within industry-year groups and using firm-level characteristics at $s = -1$; see Section 2.2 for details.

Table A6: Cross-sectional wage decomposition, pre-acquisition domestic and always foreign vs. always domestic firms.

	Mean ln wage	Firm fe	Mean worker fe	Mean wage-age pr
	(1)	(2)	(3)	(4)
To-be-acquired Domestic	0.1642***	0.0137***	0.1409***	0.0096***
	(0.0079)	(0.0034)	(0.0059)	(0.0023)
Always Foreign MNE	0.2946***	0.0987***	0.1615***	0.0345***
	(0.0043)	(0.0018)	(0.0029)	(0.0010)
Fixed-effects				
Industry-year (1,108)	✓	✓	✓	✓
Observations	832,293	832,293	832,293	832,293
R^2	0.4359	0.2605	0.3887	0.1986

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. Ln is the natural logarithm. Dependent variables are firm-level averages of the wage decomposition on (2). The regression includes 2-digit-industry-year fixed effects. 'To-be-acquired Domestic' includes domestic firms that will be acquired later, excluding all observations of the firm once it is acquired. 'Always Foreign MNE' includes all firms that are always observed under foreign ownership, excluding all firms that ever go through a status change. The comparisons group includes all firms with at least five employees that are never observed as Dutch or foreign multinationals.

A.1 Sales, value added and internationalization

We investigate whether acquired firms start to pay more because the firm's size and internationalization strategy changes. Sales, value added and the value of production are available only for a small subset of the firm-years in our data, so we estimate their impact using the single difference-in-differences regression again (as in equation (5)).[11]

Acquired firms grow significantly faster in sales and employment after acquisition (see Table A7). However, acquired firms do not show significant improvements in value added and the value of production. We observe exports for the universe firms, but aggregate sales for only a subset of firms. For that subset, we find that exports rise by about 14%, with no significant change in the number of export destinations, and little evidence of a change in imports (Table A9).[12] These results signal that changes in the acquired firm's internationalization strategy contribute to the observed change in firm premia in Figure 1.

[11] Table A8 shows that we also observe growth in firm fixed effects for the sub-sample of observations with observed sales, value added and value of production.

[12] Tables A10 and A11 show event-study estimates for the impacts on exports and imports for the full sample of matched firms (instead of the sample for which aggregate sales are observed). In this larger sample, the coefficients of acquisitions on imports are statistically significant. The event-study estimates show no sign of pre-trends for the difference-in-differences regressions, except for diverging trends in the firm's number of export destinations.

Table A7: Firm operations.

	Ln workers	Ln sales	Ln value added	Ln prod. value
	(1)	(2)	(3)	(4)
Post-Acquisition	0.0679***	0.0700*	0.0122	0.0317
	(0.0153)	(0.0275)	(0.0287)	(0.0290)
Fixed-effects				
Firm ID (1,010)	✓	✓	✓	✓
Pair-post (1,010)	✓	✓	✓	✓
Observations	5,285	5,285	5,285	5,285
R^2	0.9759	0.9221	0.8931	0.9093

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. The regressions include fixed effects for each firm and matched-pair fixed effects that differentiate between pre-acquisition and post-acquisition years. Estimated using difference-in-differences regression (5) on propensity score matching sample. Propensity scores are estimated within industry-year groups and using firm-level characteristics in the pre-acquisition year.

Table A8: Wage decomposition in firm operations sample.

	Ln Wage	Firm FE	Worker FE	Age profile
	(1)	(2)	(3)	(4)
Post-Acquisition	0.0255***	0.0135***	0.0072**	0.0046**
	(0.0040)	(0.0030)	(0.0027)	(0.0015)
Fixed-effects				
Firm ID (1,010)	✓	✓	✓	✓
Pair-post (1,010)	✓	✓	✓	✓
Observations	5,285	5,285	5,285	5,285
R^2	0.9582	0.8823	0.9662	0.9209

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. The estimation sample includes the same firms as the sample underlying Table A7. Dependent variables are the firm-level average wage components as estimated by the decomposition on equation (2) in the paper. The regressions include fixed effects for each firm and matched-pair fixed effects that differentiate between pre-acquisition and post-acquisition years. Estimated using difference-in-differences regression (5) on propensity score matching sample. Propensity scores are estimated within industry-year groups and using firm-level characteristics in the pre-acquisition year.

Table A9: Change in exports and imports in acquired firms. Sample with observed sales.

(a) Exports.

	Exports	Exp. Destinations	Exporter
	(1)	(2)	(3)
	Poisson	Poisson	OLS
Post-Acquisition	0.1434*	0.0002	-0.0143
	(0.0712)	(0.0342)	(0.0150)
Fixed-effects			
Firm ID (1,010)	✓	✓	✓
Pair-post (1,010)	✓	✓	✓
Observations	5,285	5,285	5,285
R^2			0.7631

(b) Imports.

	Imports	Imp. Destinations	Importer
	(1)	(2)	(3)
	Poisson	Poisson	OLS
Post-Acquisition	0.1306	0.0511·	0.0168
	(0.1043)	(0.0275)	(0.0132)
Fixed-effects			
Firm ID (1,010)	✓	✓	✓
Pair-post (1,010)	✓	✓	✓
Observations	5,285	5,285	5,285
R^2			0.6842

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. Columns (1) and (2) estimated using Poisson regression. The regression includes the subsample of observations for which sales, value added and the value of production is observed. The regressions include fixed effects for each firm and matched-pair fixed effects that differentiate between pre-acquisition and post-acquisition years. Estimated using difference-in-differences regression (5) on propensity score matching sample. Propensity scores are estimated within industry-year groups and using firm-level characteristics in the pre-acquisition year.

Table A10: Change in exports in acquired firms. Event study estimates on full sample.

	Exports	Exp. Destinations	Exporter
	(1)	(2)	(3)
	Poisson	Poisson	OLS
s=-3	0.0472	-0.0057	-0.0047
	(0.0771)	(0.0307)	(0.0106)
s=-2	-0.0278	-0.0525*	-0.0087
	(0.0427)	(0.0216)	(0.0102)
s=0	0.2037***	0.0172	0.0079
	(0.0516)	(0.0192)	(0.0094)
s=1	0.2164**	0.0061	0.0142
	(0.0680)	(0.0218)	(0.0106)
s=2	0.1763*	0.0038	0.0110
	(0.0736)	(0.0276)	(0.0114)
s=3	0.2187**	0.0035	0.0008
	(0.0831)	(0.0319)	(0.0114)
Pre-trends			
P-value	0.4242	0.0309	0.6952
Fixed-effects			
Firm ID (2,538)	✓	✓	✓
Pair-year (8,883)	✓	✓	✓
Observations	17,766	17,766	17,766
R^2			0.8664

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. Columns (1) and (2) estimated using Poisson regression. Estimated using difference-in-differences regression (2) 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 2.2 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 A11: Change in imports in acquired firms. Event study estimates on full sample.

	Imports	Imp. Destinations	Importer
	(1)	(2)	(3)
	Poisson	Poisson	OLS
s=-3	-0.0199	-0.0138	-0.0047
	(0.0670)	(0.0234)	(0.0102)
s=-2	-0.1408	-0.0010	0.0095
	(0.0896)	(0.0192)	(0.0105)
s=0	0.1648**	0.0216	0.0142
	(0.0608)	(0.0169)	(0.0094)
s=1	0.2154**	-0.0044	0.0110
	(0.0665)	(0.0205)	(0.0101)
s=2	0.1599**	0.0134	0.0126
	(0.0598)	(0.0242)	(0.0103)
s=3	0.3817*	0.0381	0.0039
	(0.1894)	(0.0267)	(0.0108)
Pre-trends			
P-value	0.1564	0.8166	0.3753
Fixed-effects			
Firm ID (2,538)	✓	✓	✓
Pair-year (8,883)	✓	✓	✓
Observations	17,766	17,766	17,766
R^2			0.8399

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. Columns (1) and (2) estimated using Poisson regression. Estimated using difference-in-differences regression (2) in the paper 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 2.2 for details. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

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 five 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 six 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 five 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 firm-average worker fixed effects. They also experience sharper employment, firm and firm-average 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

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, firm-worker interactions and AKM assumptions

C.1 Weakly connected firms

If few workers move between firms, the estimate of the fixed effects of firms are unbiased but might be imprecise. In order to understand the implications for our difference-in-differences estimates, this section lays out the estimation strategy and three corresponding checks.

In the first step of our strategy, we estimate the decomposition

$$ln(w_{ijt}) = \alpha_i + X_{it}\beta + \psi_{jt} + \gamma_t + \epsilon_{ijt}, \tag{6}$$

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-year fixed effect; γ_t is a calendar year fixed effect; $X_{it}\beta$ is a wage-age profile; and ϵ_{ijt} is an error term. As noted in Abowd et al. (1999) as well as the literature that follows it (Bonhomme et al., 2023; Engbom et al., 2023; Kline et al., 2020), the estimate for the worker and firm fixed effects levels ($\hat{\psi}_{jt}$) are unbiased at the population level under the standard exogeneity assumption $E[\epsilon_{ijt}|\alpha_i + X_{it}\beta + \psi_{jt} + \gamma_t] = 0$.

We then retrieve the level estimates $\hat{\psi}_{jt}$ and $\hat{\alpha}_i$ and use them as the dependent variable in a difference-in-differences regression. A coefficient in the difference in difference regression is identified as

$$DiD^{\psi} = (\hat{\psi}_{T,s} - \hat{\psi}_{T,-1}) - (\hat{\psi}_{C,s} - \hat{\psi}_{C,-1}), \tag{7}$$

where we use T to identify the treated firm; C to identify its matched control firm; and s to index time relative to the acquisition moment at $s = 0$.

A concern could be that the weak connectivity of firms causes a (mean zero) measurement error, say b_{jt} in the level estimates of the fixed effects,[13] such that

$$\hat{\psi}_{jt} = \psi_{jt} + b_{jt}.$$

Our identification strategy (equation 7) is only affected by a limited mobility bias if the change in the bias differs structurally between acquired firms and matched firms. This could arise if the level estimates of the fixed effects in the sample used for the difference-in-differences regressions are biased due to few worker moves between

[13] Several propositions have been made to correct for a limited mobility bias in the *variance* estimator of the fixed effects estimates (e.g. Andrews et al., 2008; Bonhomme et al., 2019; Kline et al., 2020), as

$$Var(\hat{\psi}_{jt}) = Var(\psi_{jt}) + 2 \times \psi_{jt} \times E[b_{jt}] + Var(b_{jt})$$

is structurally biased, but the level estimate is unbiased.

these fixed effects and the rest of the firm-worker network (Jochmans and Weidner, 2019), such that

$$E[b_{j,t}| \text{ firm is treated }] \neq 0 \tag{8}$$

and

$$E[b_{j,t}| \text{ firm is control }] \neq 0. \tag{9}$$

Following Jochmans and Weidner (2019), such a bias could arise if firms of interest in a given year are connected to very few other firms, or if they are in "corners" of the network. In our difference-in-differences context of equation (7), additionally, the limited mobility bias does not affect our conclusions if the treated and control firms' fixed effect estimates are equally biased ($E[b_{j,t}|$ firm is treated $] = E[b_{j,t}|$ firm is control $]$). Similarly, the bias is eliminated by (7) if it is the same within the time series of the treated and control firms ($E[b_{jt}] = E[b_{j,t-1}] \; \forall \; t$). This occurs when individual firm-years are well connected, for example through stayers that move from one firm-year to the next, but weakly connected to the rest of the network. Hence, a bias could follow from weak connectivity of firms if treated firms are structurally within one weakly connected subset of the network and matched firms are structurally within another weakly connected set, and the connectivity in those subsets changes structurally with the acquisition.

We perform three tests to check the sensitivity of the matched sample to potential structural differences in weak connectivity. As a first test, we calculate the eigenvector centrality of each firm-year and relative to the full network. The eigenvector centrality of a firm-year measures how centrally located a specific firm-year is within the full firm-worker network, taking into account the centrality of the directly connected firm-years. It is scaled to sum up to one across all firm-years in the network. Table C1 shows the quantile distribution of the eigenvector centrality split up by the treated firms, their matched sister firms and the remaining firms that are not in the matched sample. The table shows that treated and matched firms are located substantially more centrally in the network than other firms (some of the most central firms are not in the matched sample as the density of foreign acquisitions is low there). This implies that the firm-years in our matched sample are not located in "corners" of the firm-worker network, indicating that the connectivity differences are unlikely to bias their fixed effect level estimates.

Table C1: Quantiles of the eigenvector centrality of firm-year fixed effects per firm type.

	0%	25%	50%	75%	100%
treated	2.4E-19	6.6E-10	4.6E-09	1.6E-08	5.0E-05
matched	0.0E+00	2.9E-10	3.2E-09	1.5E-08	2.4E-04
other	0.0E+00	1.4E-13	4.9E-11	1.6E-09	3.0E-01

As a second test, we check how well the firm-years in the matched sample are connected to each other. The firm-years and workers in our matched sample are only a small sub-network of our full firm-worker network. To check whether the firm-years are immediately connected through worker mobility, we find the largest connected set of firm-years within that sub-network. We find that this connected set contains 90% of the firm-years in the matched sample. This implies that acquired firms are unlikely to be structurally positioned in a different "corner" of the network than control firms, and so any biases that might arise should affect the matched firms similarly, whereby it is eliminated by the difference-in-differences comparison.

Third, we examine our results in a subset of the firm-worker network with higher connectivity. In order to isolate the effect of limited mobility, we apply the propensity score matching procedure to the firms in the sub-network and estimate our difference-in-differences regressions using the new set of matched firms. This prevents any poorly connected firm-year from entering the matched firm set. To construct the sub-network, 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 yearly firm fixed effects that are identified by wages of at least five 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), suggesting that connectivity is unlikely to bias fixed effect estimates in the sub-network. 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 sub-network contains about 40% of the firm-years and 97% of the workers that are in the main network (see Table C2).

Although the number of firm-year observations declines substantially, we still find matches for 1,268 firms in the sub-network. The overlap of acquired firms between our main matched sample and the matched sample of the sub-network is more than 98%, while the overlap in counterfactual firms is 27%.

To finally assess the impact of changing the connectivity 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 sub-network. 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 C3 compares the difference-in-differences estimates. As the coefficient differ typically by at most 0.0002 points, the connectivity has little impact of the estimates. This is not surprising, as the weakly connected firms that are dropped in the comparison are typically small and hardly used in the identification of the impact of the foreign acquisition of wages.

Table C2: 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 five worker connections to other firm years; and with connections to a minimum of two other firm-years. Global connectivity is the limited mobility bias indicator of Jochmans and Weidner (2019).

Table C3: Comparison of difference-in-differences estimates for the main firm-worker network and a well-connected subnetwork.

	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 five worker connections to other firm-years; and with connections to a minimum of two 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 2.2 for details. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

C.2 Firm-worker interactions

Our decomposition approach assumes that the firm-year fixed effects are additively separable from the worker fixed effects in explaining log wages (Engbom et al., 2023). In Section 5 we find that foreign acquisition impacts the decomposition's residual differently for moving workers and managers. If firm-worker interactions, or *skill complementarity* (Bonhomme et al., 2019), is important in explaining wage dynamics in general, our decomposition approach could suffer from an omitted variable. In this section, we describe a test for the impact of skill complementarity on the post-acquisition wage gap between foreign and domestic firms.

One approach to estimate skill complementarity is the method of Bonhomme et al. (2019). Comparing models with and without skill complementarity, Bonhomme et al. (2019) find that complementarity plays a minor role in explaining aggregate wage dynamics. Applying the same method to the static wage gap between foreign-owned and domestic firms in the United States, Setzler and Tintelnot (2021) find some evidence of skill complementarity in foreign firms. Unfortunately, the Bonhomme method does not extent to our setting of the wage dynamics when a domestic firm is acquired by a foreign owner. The reason is that identifying the Bonhomme complementarity parameter requires very high mobility of different workers between firms. In the setting of Bonhomme et al. (2019) and Setzler and Tintelnot (2021) the firm fixed effects are time-invariant; and firms and workers are grouped using a k-means clustering algorithm. This artificially increases mobility compared to our setting with firm-year fixed effects and allows for the direct identification of skill complementary. However, it prevents an analysis of the dynamics around an acquisition. Instead, we gauge the dynamic impact of skill complementary on the post-acquisition wage gap based on the iterative method of De la Roca and Puga (2017). Their method allows us to introduce time-variation in an interaction between the worker fixed effects and the set of acquired and matched control firms.

We proceed in two steps. First, we introduce the De la Roca and Puga (2017) skill complementarity parameter in our AKM decomposition. Second, we estimate the impact of skill complementarity (defined by the complementarity parameter and the sorting of workers) on the firm-level wage gap that arises due to the acquisition.

To estimate the skill complementarity parameter, we augment our main wage specification (6) with interactions between the worker fixed effects and identifiers for the treated (acquired) and matched control firms in our sample. Our wage decomposition with skill complementarity is

$$\begin{aligned} ln(w_{ijt}) &= \\ + \underbrace{\delta_0 \times D_j^C \times \alpha_i + \delta_1 \times D_j^T \times \alpha_i + \delta_2 \times D_{jt}^P \times \alpha_i + \delta_3 \times D_j^T \times D_{jt}^P \times \alpha_i}_{\text{skill complementarity}} &+ \alpha_i + \psi_{jt} \\ &+ X_{it}\beta + \gamma_t + \epsilon_{ijt}, \end{aligned} \tag{10}$$

where i, j and t index worker, firm and calendar year; $ln(w_{ijt})$ is log real hourly wage; D^T and D^C are dummies that identify worker-firm matches in the treated and matched control firms; D^P identifies worker-firm matches in treated and control firms that fall in the post-acquisition period; α_i is a worker fixed effect; ψ_{jt} is a firm-year fixed effect; X_{it} is a wage-age profile; γ_t is a year fixed effect and ϵ_{ijt} is an error term.

The coefficients δ_0 and δ_1 in equation (10) introduce a skill complementary parameter for treated and control firms. They allow wage to differ by the workers' fixed effect α_i and the type of firm where the worker is employed. Firms not in the matched sample serve as the reference category, whereby the coefficients measure the wage return that a worker with a one-log-point higher worker fixed effect experiences in a treated and control firm relative to all other firms in the data. We introduce time variation trough the coefficient δ_3, which captures the change in the parameter in the years after acquisition that is common to treated and control firms. Finally, δ_3 measures the difference that arises in treated firms after acquisition. The coefficient is a direct difference-in-differences estimate of the change in the skill complementarity parameter in acquired firms following acquisition, relative to the matched control firms. As these parameters cannot be estimated directly from the data due to the interaction with α_i, we employ the iterative algorithm of De la Roca and Puga (2017): We start with an initial guess for the estimates of the α_i's; estimate equation (10) using these estimates and derive new estimates for the α_i's. We repeat this procedure until all coefficients (including the fixed effect estimates) converge up to an error of 10^{-3} between two successive iterations.

Table C4 shows the estimates for the skill complementarity parameters. The coefficients apply to a one-log-point increase in the worker fixed effect. The estimates show that the skill complementarity parameter is 1.8% and 4.2% lower in acquired and control firms, and increases by about 8% from the pre- to the post-acquisition period. Most importantly, the significant coefficient $\hat{\delta}_3 = 0.053$ implies that acquisition increases the skill complementarity parameter by about 5%. Across the entire dataset the within-firm standard deviation of worker fixed effects is 0.16, implying a small impact of skill complementarity on aggregate wage dynamics, in line with the finding of Bonhomme et al. (2019). As the within-firm standard deviation in acquired firms is around 0.31, the estimate suggests that two workers within an acquired firm that are one standard deviation apart in worker fixed effect expect a divergence of their wage of 1.6% more than a similar pair in a control firm.

Even if the rise in the complementarity parameter for acquired firms can increase the wage variation within the firm, the impact on the firm's average wage is not clear. To test for the impact of skill complementarity on the wage gap between acquired and domestic firms, we estimate our difference-in-differences decomposition of the firm-level average components of equation (10). At the firm level, the wage gap is explained by the change in firm fixed effects, firm-average worker fixed effects, worker observables; and skill complementarity. Skill complementarity impacts the wage gap through the complementarity parameters (δ_0, δ_1, δ_2 and δ_3), and the composition of the workforce (the specific α_i's observed in the firm). Note that with an unchanged composition of the workforce, the impacts of δ_0 and δ_1 do not surface in a difference-in-differences estimate.

The firm-level decomposition with skill complementarity included is in Table C5. The estimates for the wage gap, change in firm fixed effects, firm-average worker fixed effects and worker observables are very similar to the ones of a decomposition using the main wage component estimates of the paper (see Table C6). Although the within-firm deviation from the mean wage may increase due to complementarity in acquired firms, we find no evidence that this increases the acquisition wage gap. The impact of an acquisition on the wage gap that derives from the complementarity term in Column 5 of Table C5 is very close to zero and statistically insignificant. Taking the complementarity term into account in the decomposition does not lead to material changes in the estimates of the relative importance of the firm premium and the worker composition in explaining the acquisition wage gap.

Table C4: Worker complementarity.

	ln Wage
	(1)
$\hat{\delta}_0$	-0.0182
	(0.0148)
$\hat{\delta}_1$	-0.0422***
	(0.0056)
$\hat{\delta}_2$	0.0872***
	(0.0051)
$\hat{\delta}_3$	0.0530***
	(0.0080)
age-profile	✓
Fixed-effects	
Worker (9,268,401)	✓
Firm-year (3,675,170)	✓
Year (13)	✓
Observations	78,430,113
R^2	0.9160

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. The table shows an estimation of equation (10). Interactions with the worker fixed effects are estimated iteratively until coefficient convergence up to an error of 10^{-3} (De la Roca and Puga, 2017). Dependent variable is the log real hourly wage. Age-profile is a third-order polynomial that is flat at the age of 40.

Table C5: Difference-in-differences decomposition of the acquisition wage gap, including worker complementarity.

	Ln Wage	Firm FE	Worker FE	Age profile	Complementarity
	(1)	(2)	(3)	(4)	(5)
Post-Acquisition	0.0306***	0.0224***	0.0055**	0.0032**	-0.0005
	(0.0029)	(0.0020)	(0.0018)	(0.0011)	(0.0005)
Fixed-effects					
Firm ID (2,538)	✓	✓	✓	✓	✓
Pair-post (2,538)	✓	✓	✓	✓	✓
Observations	17,766	17,766	17,766	17,766	17,766
R^2	0.9448	0.8431	0.9579	0.8812	0.7843

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. Dependent variables are the firm-average wage components as estimated by the decomposition on (10). The regressions include fixed effects for each firm and matched-pair that differentiate between pre-acquisition and post-acquisition years. Estimated on propensity score matching sample. Propensity scores are estimated within industry-year groups and using firm-level characteristics in the pre-acquisition year; see Section 2.2 for details.

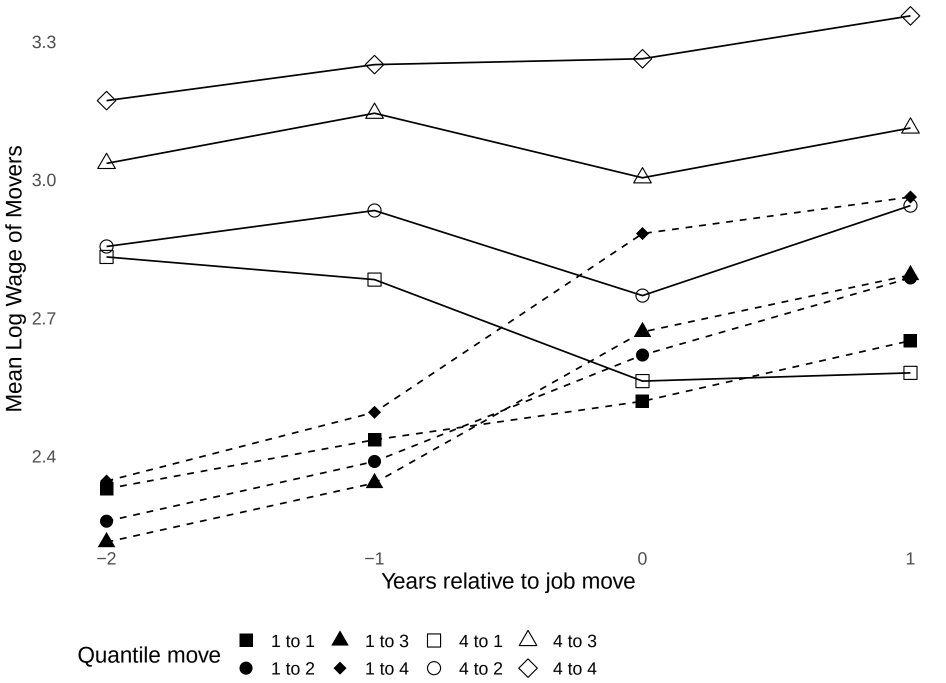
Table C6: Difference-in-differences decomposition of the acquisition wage gap, excluding worker complementarity.

	Ln Wage	Firm FE	Worker FE	Age profile
	(1)	(2)	(3)	(4)
Post-Acquisition	0.0306***	0.0218***	0.0056**	0.0032**
	(0.0029)	(0.0021)	(0.0019)	(0.0011)
Fixed-effects				
Firm ID (2,538)	✓	✓	✓	✓
Pair-post (2,538)	✓	✓	✓	✓
Observations	17,766	17,766	17,766	17,766
R^2	0.9448	0.8460	0.9571	0.8812

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. Dependent variables are firm-level averages of the decomposition on (2). The regressions include fixed effects for each firm and matched-pair that differentiate between pre-acquisition and post-acquisition years. Estimated on propensity score matching sample. Propensity scores are estimated within industry-year groups and using firm-level characteristics in the pre-acquisition year; see Section 2.2 for details.

C.3 Mover plot

Figure C1: Log hourly wage developments of job mover between quartiles of firm fixed effect distribution.



Notes: The figure shows the average log hourly wage developments of workers that move from a firm in the fourth (first) quantile of the firm-year fixed effect distribution to a different firm (Card et al., 2013, 2018). The plot uses workers that are employed at the previous and next employer around the job move for a minimum of two years. Quantile assignment is according to the firm-year fixed effect right before and after the job move ('Years relative to job move' at -1 and 0).

D Outputs for Robustness Checks

Table D1: Change in firm-level premium under alternative specifications.

	Main Firm fe estimate		Location/Industry-year adjusted		Callaway/Sant'Anna
	Control group	No control group	Control group	No control group	Control group
	(1)	(2)	(3)	(4)	(5)
s=-3	0.0024	-0.0123***	0.0012	-0.0195***	0.0024
	(0.0022)	(0.0022)	(0.0023)	(0.0023)	(0.0022)
s=-2	0.0019	-0.0059***	0.0013	-0.0093***	0.0019
	(0.0017)	(0.0017)	(0.0017)	(0.0018)	(0.0017)
s=0	0.0107***	0.0112***	0.0087***	0.0104***	0.0107***
	(0.0017)	(0.0018)	(0.0018)	(0.0019)	(0.0017)
s=1	0.0203***	0.0230***	0.0178***	0.0241***	0.0203***
	(0.0022)	(0.0022)	(0.0023)	(0.0023)	(0.0022)
s=2	0.0264***	0.0300***	0.0236***	0.0338***	0.0264***
	(0.0024)	(0.0026)	(0.0025)	(0.0027)	(0.0024)
s=3	0.0354***	0.0415***	0.0327***	0.0476***	0.0354***
	(0.0029)	(0.0029)	(0.0029)	(0.0031)	(0.0029)
Fixed-effects					
Firm ID	✓	✓	✓	✓	✓
Pair-year (8,883)	✓		✓		✓
# Firm ID	2,538	1,269	2,538	1,269	2,538
Observations	17,766	8,883	17,766	8,883	17,766
R^2	0.9097	0.7871	0.9532	0.8931	0.9099

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 variable in Columns 1, 2, 5 is the firm fixed effects of the decomposition on (2). Dependent variable in Columns 3, 4 is the firm fixed effect of the decomposition on (2), augmented with industry-year and location-year fixed effects. All regressions include a fixed effect for each firm, and Columns 1, 3, 5 an additional fixed effect for each year of matched pairs of firms. Columns 1, 3 estimated using difference-in-differences regression (1) on the propensity score matching sample. Columns 2, 5 include only acquired firms of the propensity score matching sample. Column 5 is the estimator of Callaway and Sant'Anna (2021) which includes interactions between s and year indicators in a first step. In the second step, the estimates are aggregated to the s level by taking averages. Propensity scores are estimated within industry-year groups and using firm-level characteristics at $s = -1$; see Section 2.2 for details.

Table D2: Decomposition of the acquisition wage gap (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 2.2 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 2.2 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 A1. One-way clustering at firm level (Column 2). Separate pre- and 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·
$s = 3$	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 A1. Column 2 shows the standard errors of Table A1. 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).