MULTINATIONAL FIRMS AND LOCAL WORKERS

Multinational Firms and Local Workers

Multinationale bedrijven en lokale werknemers

Thesis

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To Pam, for always believing in me.

Preface

Many have helped and supported me throughout the years of work that led to this thesis. I would like to thank my supervisors, colleagues, friends, and family for all their support.

First of all, I want to thank my supervisory team, Prof. Frank van Oort, Bas Karreman, and Michiel Gerritse. I am immensely grateful to have had the opportunity to learn from them about economics, life, and strategy (in economics and life). Already during my interview, when they alternated between serious scholarly discussions and fun talk, I was sure that they would make a great supervisory team. This mix continued throughout the past five years. On the serious side, Bas and Michiel, in particular, were always available and ready to offer advice whenever I needed it. I will genuinely miss storming into their offices and turning a "Do you have 5 minutes?" question into an hour-long discussion. Frank similarly made time for me in his busy schedule, and I am especially grateful for the support he provided when I felt lost. His tendency to say 'yes' more than 'no,' along with his reassuring words when granting approval, often gave me the confidence I needed. On the fun side, I will not forget our communal dinners and beers in various constellations and the jokes and sometimes bizarre messages in our chat group.

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Behind the scenes of this thesis, I traveled to different conferences, participated in teaching, and benefited from all sorts of administrative support. Therefore, I would like to extend my gratitude to the support staff at Erasmus University. The secretariats at Applied Economics over the years, Linda, Ylaise, Cindy, Karin, Manuela, Petra, Ellie, and Mads, have often gone out of their way to provide support and process my various requests. Additionally, I thank Carine from Tinbergen Institute for extending my student account again and again, and Kim and Lindsey for their on-point support with printing this thesis.

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Marcus Rösch

Rotterdam, July 2024

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Chapter 1

Introduction

High wages, advanced technology, and productivity in multinational firms (MNEs) encourage governments across the world to attract Foreign Direct Investment (FDI). The increase in global FDI flows from around \$200 billion in 1990 to \$1,300 billion by 2022 (UNCTAD, 2022) partly reflects policymakers' increased efforts to attract MNEs to their local economies. A primary motivation for these efforts is the potential for MNEs to enhance local employment conditions. The Netherlands, for example, actively promotes itself as an attractive destination for multinational investment through its Foreign Investment Agency. The country provides several incentives to potential investors, such as low institutional barriers and favorable corporate tax rates and rulings. These proactive industrial policies have made the Netherlands a magnet for multinational investment, with MNEs employing about 40% of the local workforce (Berkenbos et al., 2022).

As workers in the Netherlands and worldwide increasingly rely on MNEs for employment, concerns about the quality of their jobs continue to persist. While high wages in MNEs are often seen as indicators of high-quality employment, MNEs may not pay the same worker a higher wage than a domestic firm would (Heyman et al., 2007; Hijzen et al., 2013). Moreover, reports of layoffs and wage reductions due to restructuring, automation, and outsourcing, raise doubts about the true benefits MNEs bring to local labor markets. Despite these concerns, studies in Economics that examine the direct impact of MNEs on local labor markets remain scarce, with most research focusing on the productivity advantages of MNEs, rather than their effects on workers.

My purpose in this thesis is to contribute to our understanding of how MNEs shape local labor markets. Across three self-contained chapters, I employ the universal matched employer-employee data of the Netherlands to attempt answering the questions:

1. What explains the wage gap between MNEs and domestic firms?
2. How do MNEs impact workers' careers? Does MNE employment create a career value? Do MNEs internalize this career value?
3. How does automation in MNEs impact separation and wage dynamics?

Multinationals are often defined as firms that control and manage production establishments in at least two countries (Antràs and Yeaple, 2014; Caves, 2007). Opening foreign locations through FDI requires high upfront investments, as firms need to set up new productive facilities in foreign markets, where they are unfamiliar with local rules, regulations and consumer preferences. Hence, only the most productive firms can offset the high costs associated with turning multinational (Helpman et al., 2010, 2004; Melitz, 2003). A vast firm-level literature in Economics documents that MNEs use superior technologies, invest extensively in research and development activities (e.g., Antràs and Yeaple, 2014; Girma and Görg, 2007b; Guadalupe et al., 2012; Koch and Smolka, 2019), and employ effective production and management practices (e.g., Bastos et al., 2018; Girma and Görg, 2007a). Policymakers draw MNEs to their local economies because such efficiencies plausibly raise local output, and may create spillover effects to local firms (e.g., Haskel et al., 2007; Javorcik, 2004; Keller and Yeaple, 2009).

There are at least three good reasons to study how multinationals impact local workers. First, it allows to dissect the origins of the wage gap between MNEs and domestic firms, which ranges from 2% to 50% across the developed world (e.g., Andrews et al., 2009; Earle et al., 2018; Heyman et al., 2007, 2011; Hijzen et al., 2013; Huttunen, 2007). While policymakers and economists often view this gap as evidence that MNEs enhance local productivity, work conditions, and wages, other factors can also explain the wage gap. For instance, MNEs frequently select high-wage domestic firms for acquisition and employ high-wage workers who would likely command high salaries regardless of their employer (e.g., Almeida, 2007; Balsvik, 2011; Setzler and Tintelnot, 2021). In consequence, the wage gap may result from selection effects rather than better work conditions or productivity in MNEs. True benefits to local economies only arise if MNEs lead the same worker to earn a higher wage. Moreover, worker mobility from MNEs to domestic firms is often seen as a key mechanism for spillovers of productivity and wage advantages to local firms (e.g., Balsvik, 2011; Poole, 2013). Such spillovers depend on workers acquiring skills and knowledge during their MNE employment that are valued by other employers. Hence, it is important to understand whether the presence of MNEs, or selection effects explain the high wages of MNE workers. Chapters 2 and 3 of this thesis contribute to our understanding of the factors driving the wage gap. Chapter 2 combines a dynamic decomposition of wage with the counterfactual arising from foreign acquisitions and documents the important role that firms, rather than workers, play in explaining the wage gap. Chapter 3 studies the dynamic accumulation of wage within MNEs, highlighting that employment at an MNE turns a worker into a high-wage earner, within the MNE and at outside firms. This chapter also challenges conventional perceptions of MNE employment benefits by emphasizing the impact of the MNE's career wage premia on hires, promotions, and the wage setting within MNEs.

Second, MNEs may leverage their workforce in specific ways to increase productivity. Theories of firm selection into international trade suggest that firms with high initial productivity are more likely to overcome the challenges of becoming multinational (Helpman et al., 2010, 2004). Firm-level studies attribute the productivity advantage of MNEs to factors such as innovation, technology, and management practices (e.g., Bastos et al., 2018; Guadalupe et al., 2012). However, research into the organization of production shows that firms also derive productivity benefits by adapting their organizational structures, for instance, as they implement standardized production practices or adopt new technologies (e.g., Caliendo and Rossi-Hansberg, 2012; Mariscal, 2020). Chapter 3 highlights an additional mechanism through which MNEs may leverage their workforce and derive productivity advantages. The model and stylized facts suggest that MNEs internalize their wage externalities by hiring young workers at low wage and selectively promoting them to higher wage positions.

Third, the potential labor market consequences of technological progress are a widespread source of public anxiety. The recent rise of technologies such as industrial robots, artificial intelligence and machine learning raise fears that automation could lead to significant job losses and wage reductions. MNEs, as international drivers of technological adoption, are often at the heart of this debate. Although economic research often suggests that the adverse effects of automation are localized and affect specific worker groups (e.g., Acemoglu et al., 2023; Bessen et al., 2023; Dauth et al., 2021), the distinctive impact of MNEs on individual workers remains underexplored. Given their extensive resources and global production networks, automation in MNEs possibly has a large impact on workers. Chapter 4 contributes to our understanding of the role of MNEs by showing that, compared to domestic firms, automation in MNEs leads to higher rates of worker separations but also greater wage growth for those workers who remain with the MNE post-automation.

1.1 Empirical context

The empirical analyses in this thesis are set in the Netherlands, which offers distinct advantages for studying the impact of multinationals on workers. Ranking among the top countries for FDI, the Netherlands' openness to international trade, strategic location, developed infrastructure, and educated workforce attract numerous MNEs (UNCTAD, 2022). Consequently, the Dutch labor market is heavily impacted by MNEs, as they employ about 40% of the local workforce and hire approximately one third of graduates entering the labor market each year (Berkenbos et al., 2022; Roesch et al., 2024). This high presence allows me to examine the impact of MNEs on workers across various sectors and firm sizes, extending beyond the focus on large manufacturing firms often found in earlier research.

Statistics Netherlands provides highly detailed firm- and worker-level data. At the firm level, the data includes metrics such as company group linkages, industry classification, exports, and investments in automation services. Crucially, it allows me to observe whether a firm exerts decisive control over a foreign firm and the nationality of its ultimate owner, which is determined by a majority stake of voting rights. Throughout this thesis, I classify a firm as a foreign MNE if its ultimate owner is non-Dutch, and as a Dutch MNE if it owns foreign affiliates under Dutch ownership. This leads me to identify more than 20k foreign and domestic MNEs in the data.

Through unique identifiers, I link the firm-level data to the universal employer-employee data of the Netherlands, allowing me to closely track all workers' labor market outcomes since 2006. This dataset is highly detailed and accurate because its primary use is to administer payroll taxes and claims for pension and unemployment insurances. All Dutch employers are mandated to submit monthly reports detailing their workers' hours and full earnings, including base, overtime, and bonus pay. Unlike many other matched employer-employee datasets, the wages of top earners are not censored, which is important as MNEs employ many high-wage earners. Consistent worker identifiers allow me to follow the same worker over time and incorporate other worker-level outcomes. Beside demographics, I also observe information such as workers' high-level management status and educational attainment, including the timing of their entry into the labor market post-education.

Despite its strengths, the Dutch data has limitations. First, workers' occupations are recorded for only a 4% random sample of the workforce each year, which constrains the data's ability to measure the skill content of occupations among MNE workers. This is particularly significant in light of literature that emphasizes the impact of automation at the occupation level (e.g., Acemoglu and Restrepo, 2020; Autor et al., 2013). Given this constraint, I proxy workers' specific skills through their observed field of education. Second, foreign acquisitions sometimes lead to inconsistent firm identifiers, for example if a new owner files for a new Chamber of Commerce registration. I address this issue by linking firm identifiers through the employment patterns of their workers (Benedetto et al., 2007). Third, while the data comprehensively records workers' wages, reported hours, and employment relations, it does not capture other important factors of jobs. For instance, workplace culture and amenities, training opportunities, unreported overtime and work stress are likely important determinants of the quality of jobs provided by MNEs. Unfortunately, like most employer-employee datasets available to researchers, the Dutch data does not allow me to measure these outcomes directly.

Due to its high detail and ability to track individuals, the data used for this thesis is proprietary. Researchers can request access to the individual datasets from Statistics Netherlands. With access to the data, the results in this thesis can be reproduced through replication packages.[1]

1.2 Thesis outline

This thesis consists of three self-contained studies that employ the Dutch employer-employee data to study the impact of multinationals on local workers. Chapters 2 and 3 asses sources of higher pay in MNEs. Chapter 2 decomposes the wage gap of foreign-acquired firms into firm and workforce contributions. Chapter 3 tracks career outcomes of workers in MNEs and provides empirical evidence on the accumulation and transferability of MNE wage premia over time and across different employment contexts. This chapter also studies the impact of these premia on hires and promotions. Finally, Chapter 4 addresses technological transformations within MNEs, particularly in the context of automation, and its impact on separations and wages.

In Chapter 2, I examine whether firms or workers drive the wage gap that arises when a foreign multinational acquires a domestic firm. I propose a new identification strategy that integrates a dynamic, full-sample wage decomposition with the natural counterfactual arising from acquisitions. In the decomposition, the wage gap can result from foreign owners introducing changes to acquired firms that increase workers' wages over time, or from acquired firms attracting high-wage workers.

[1]For the replication packages see: https://mrcsrsch.github.io

Within the Dutch employer-employee data for the years 2006 to 2018, I confirm earlier results by showing that foreign acquisitions lead to a growing wage gap. Contrasting earlier literature, the decomposition shows that firm-level changes contribute about three-quarters to the wage gap. Workforce changes are less important and materialize only in later post-acquisition years, driven by the increased hiring of high-wage workers. I show that high firm contributions are a direct consequence of adjusting the counterfactual, as a static decomposition mirrors the high workforce contributions found in earlier studies. In dissecting the acquisition effect further, I document that managers' wages rise about twice as fast as those of other workers, and that firm and workforce contributions vary with industry and firm size.

Many workers view MNEs as better employers because they believe that experience at an MNE enhances their career prospects. In Chapter 3, I provide the first empirical evidence assessing this belief. By tracking all graduates entering the Dutch labor market from 2006 to 2021, I employ a wage regression model that allows for the dynamic accumulation of wage across firms, while accounting for static firm and worker contributions. My main findings offer three novel insights to the MNE wage literature. First, the wage premia of MNE careers do not materialize instantaneously but build up slowly over a worker's tenure within the firm, with faster increases for workers with initially higher abilities. Second, these wage premia are highly portable to subsequent employers and rise in earlier MNE tenure. Third, the sorting of high-wage workers to MNEs does not explain the MNE wage gap, as MNEs and domestic firms hire workers of comparable initial earnings capacities.

The existence of portable wage premia suggests that MNEs may adjust their hires and promotions to take advantage of their career value. Chapter 3 documents several novel stylized facts that are consistent with a theoretical model where MNEs internalize the value of careers. Over the course of a typical career, MNE workers earn relatively low entry wages, and higher wages at later career stages. Moreover, MNEs employ high shares of labor market entrants and a higher ratio of inexperienced to experienced workers than domestic firms. In turn, the average quality of workers that remain with the MNE over time increases with seniority. The intuition is that junior workers in MNEs accept low entry wages in exchange for experience that pays off later. As juniors are cheap relative to their marginal productivity, the MNE employs more junior workers per senior worker. Since seniors need to be paid their marginal productivity, they are relatively more expensive compared to juniors in the MNE, requiring the firm to promote only highly productive workers to senior positions. Consistent with the idea that MNEs derive part of their productivity advantage by adjusting to their workers' career premia, I find that these wage premia account for almost the entire productivity advantage of MNEs that translates into pay.

In Chapter 4, I turn to the impact of technological progress on workforce dynamics within MNEs. Exploiting Dutch firms' lumpy investments in automation costs, I explore how a firm's MNE identity mediates the impact of automation on worker separations and wages. Workers who remain in an MNE post-automation generally see an increase in wages, whereas those in domestic firms often face wage declines. This coincides with significantly higher worker separations in MNEs. I show that workforce flexibility, trade and automation intensity do not explain the distinct effects in MNEs. Additional analyses suggest that MNE automation affects different segments of the workforce than domestic firm automation. In domestic firms, automation tends to disadvantage low-educated workers in terms of wages and separations. In contrast, MNE automation benefits the wages of managers and technically skilled workers. However, all types of workers experience elevated separation risks in MNEs.

1.3 Individual contributions

Here, I list the order and major contributions of each co-author to the main chapters of this thesis:

Chapter	Order of authors	Major contribution
2	M.A. Roesch	Conceptualization, Data curation, Methodology, Analysis, Writing
	M.J.A. Gerritse	Conceptualization, Methodology, Writing
	B. Karreman	Conceptualization, Writing
	F.G. van Oort	Writing
	B. Loog	Data curation
3	M.A. Roesch	Conceptualization, Data curation, Methodology, Analysis, Writing
	M.J.A. Gerritse	Conceptualization, Analysis, Writing
	B. Karreman	Writing
4	M.A. Roesch	Single-authored

Notes: Michiel Gerritse, Bas Karreman and Frank van Oort provided feedback and supervision for all chapters. Bart Loog facilitated access to the data for all chapters.

Chapter 2

Do Workers or Firms Drive the Foreign Acquisition Wage Gap?

2

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.

This chapter is based on the Tinbergen Institute Working Paper (No. 22-014/V) titled "Do firms or workers drive the foreign acquisition wage premium?" and is joint work with Michiel Gerritse, Bas Karreman, Frank van Oort, and Bart Loog.

2.1 Introduction

Most studies of multinationals' wages find that foreign firms pay higher wages because they hire better workers (e.g., Balsvik, 2011; Heyman et al., 2007; Setzler and Tintelnot, 2021). Employees of foreign-owned firms have higher levels of education, experience, and other measures of quality (Andrews et al., 2009; Heyman et al., 2007; 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 (Andrews et al., 2009; Earle et al., 2018; Girma and Görg, 2007a; Heyman et al., 2007, 2011; Hijzen et al., 2013; Huttunen, 2007).

However, a foreign acquisition changes the firm, for instance in its productivity, its practices and management, and its training of workers (Bircan, 2019; Girma and Görg, 2007a; 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; Poole, 2013; Stoyanov and Zubanov, 2012). 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 policymakers 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 differences 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 (Andrews et al., 2009; Heyman et al., 2007; Hijzen et al., 2013, 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 for 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 policymakers.

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., Balsvik, 2011; Setzler and Tintelnot, 2021, for Norway and the U.S. 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 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.

2.2 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., Bastos et al., 2018; Egger et al., 2020; Girma and Görg, 2007a; Heyman et al., 2007; Hijzen et al., 2013; Huttunen, 2007; Koch and Smolka, 2019; Orefice et al., 2019). 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.

2.2.1 Difference-in-differences framework

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

$$y_{jmt} = \sum_{s=-3}^{3} \delta_s FA_{jms} + \omega_{mt} + \Psi_j + u_{jmt}, \tag{2.1}$$

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 Subsection 2.2.2. 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.

2.2.2 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 2.17 in Appendix 2.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 2.9 in Appendix 2.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 2.3.1.

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 2.3.1, we discuss the balance in covariates after matching (covariate balance is documented in Table 2.18 in Appendix 2.B). In Section 2.4.2.3, 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.

2.2.3 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:

$$ln(w_{ijt}) = \alpha_i + X_{it}\beta + \psi_{jt} + \gamma_t + \epsilon_{ijt}, \tag{2.2}$$

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 (2.2), 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 2.3 in Appendix 2.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 2.4.2.1, we discuss the identification of the firm fixed effects in detail.

The estimation of equation (2.2) 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.

2.3 Data and sample selection

We employ two types of administrative data from 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., 2002, 1999). This subset covers more than 99% of workers and more than 90% of firms with employees. In Appendix 2.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.

2.3.1 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 2.2.2. 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 2.26 in Appendix 2.D).

In total, we find matches for 1,269 target firms. Table 2.18 in Appendix 2.B.4 presents mean normalized differences of matching covariates between target and

[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.

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

Relative to earlier research on the acquisition 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 2.2 in Appendix 2.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 2.2 in Appendix 2.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.

2.4 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 2.5, we explore possible causes of changes in firm premia and worker composition.

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

Figure 2.1 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 2.2.3). We show 95%-confidence intervals with standard errors clustered at the firm level to account for within-firm serial correlation of errors. In Section 2.4.2.4, we explore alternative approaches for calculating these standard errors.

Figure 2.1 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 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

[2]The estimates are in Table 2.6 in Appendix 2.A.

[3]In Table 2.9 in Appendix 2.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 2.3, we identify acquisitions on a year-on-year basis, whereby the unobserved exact date of acquisition lies within the acquisition year. In consequence, our estimates at $s = 0$ only partially capture the effect of acquisition.

Figure 2.1: 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 2.6 in Appendix 2.A. Confidence intervals are based on clustered standard errors (Firm ID). Coefficients are estimated using difference-in-differences regression (2.1) on propensity score matching sample. Dependent variables are firm-level averages of the AKM decomposition on equation (2.2). 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 2.2.2 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 2.6.

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 (2.2), 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 2.6). 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 2.1 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 2.6 in Appendix 2.A. All p-values are higher than any conventional level, implying that the matched firms' trends were parallel before the acquisition. Second, in Section 2.4.2.3 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 2.25 in Appendix 2.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 (2.2) 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 2.25).

2.4.2 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.

2.4.2.1 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; Andrews et al., 2008; Bonhomme et al., 2023).[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

[5]The "limited mobility bias" (Bonhomme et al., 2023; Jochmans and Weidner, 2019), 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.

our standard errors. Section 2.4.2.4 explores alternatives approaches to calculating the standard errors. In Appendix 2.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 2.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, as overall worker fixed effect changes are small, 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.

2.4.2.2 Identification from stayers

In order to isolate post-acquisition firm-level wage developments from changes in the workforce, most earlier studies exploit a subsample of workers that stay within the firm. Assuming that the earnings capacity of stayers does not change with the acquisition, stayers' wages reflect firm-level premia. Our decomposition approach (2.2), by contrast, uses the wages of workers that stay in the firm as well as the wages of workers that move between firms to identify the firm-level premium. If stayers and movers differ systematically, the two estimates may diverge: If the firm-level premium after acquisition is higher for moving workers, the firm-level premium identified from stayers is lower than the firm-level premium experienced by the average worker.

We estimate the firm-level premium in our sample for a subsample of workers that stay within the firm, in order to examine whether sample selection on stayers or movers explains our findings. In an analysis of a sample of stayers in Table 2.27 in Appendix 2.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 subsample of stayers.

2.4.2.3 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 2.28 in Appendix 2.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 2.28 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 2.29. 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.[6] This is not entirely surprising as the firm sets differ substantially by matching strategy, and the evidence in Section 2.5 will suggest that the estimated changes in firm-average worker fixed effects are determined by a limited number of firms. 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.

[6]When matching on the within-firm variance of the worker fixed effects, we find significant pre-trends.

2.4.2.4 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 2.30, 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 2.31, 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.

2.4.3 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 2.7 in Appendix 2.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., Balsvik, 2011; Setzler and Tintelnot, 2021, for Norway and the U.S.).

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., Balsvik, 2011; Setzler and Tintelnot, 2021). 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 2.8 in Appendix 2.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 2.9 in Appendix 2.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 2.9 in Appendix 2.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 this chapter 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 2.10 in Appendix 2.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 2.8 in Appendix 2.A) and foreign-owned firms overall (Table 2.7 in Appendix 2.A). In this sample, we can examine the wage premia in firms that are candidates for acquisition but have not been acquired yet. Table 2.11 in Appendix 2.A shows the estimates of wage gaps of i) 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.

2.5 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.

2.5.1 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 2.1 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 (χ^2(4, 6322)=3.5, $p < 0.01$) as well as for firms with under 20 employees (χ^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 (χ^2(4, 6322)=2.13, $p < 0.1$) but not for large firms (χ^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 2.2 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 sample shows that the

[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 2.1: Change in firm and worker fixed effects by employment size.

	5 - 19		20 - 49		50 - 99		> 100	
Years since acquisition	Firm FE (1)	Worker FE (2)	Firm FE (3)	Worker FE (4)	Firm FE (5)	Worker FE (6)	Firm FE (7)	Worker FE (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 (2.2). Estimated using difference-in-differences regression (2.1) 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 2.2.2 for details. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

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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.

Table 2.2: 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·	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·	-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·	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·	0.0129·	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·	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 (2.2). Estimated using difference-in-differences regression (2.1) 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 2.2.2 for details. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

2.5.2 Managers

The rise in firm-level premia in Figure 2.1 may apply specifically to managers (Egger et al., 2020; Heyman et al., 2011). Profits from internationalization 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 (2.2). As the firm fixed effects in equation (2.2) 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 estimate a static difference-in-differences regression across firm pairs with observed managers and non-managers. The regression specification is

[8] In Appendix 2.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 subsample estimates using only the Chamber of Commerce data to identify managers; subsamples 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.

$$r_{jmt} = \delta FA_{jmt} + \omega_{mt} + \Psi_j + u_{jmt}, \tag{2.3}$$

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 (2.3) 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.

Table 2.3 shows a separate decomposition for managers and non-managers, based on equation (2.3). The estimates imply that after acquisition, managers' average wages (Column 2 of Panel (a)) rise by 6%, while non-managers' wages rise by 3% (Panel (b)), 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.

2.5.3 Worker-specific post-acquisition premia

An alternative explanation for the firm-level improvements in wage after an acquisition (as in Figure 2.1) 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 (2.2) 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 (2.3) 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 2.4 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 firm earn around 3% higher wage at their new employer, compared to workers who left a matched, non-acquired

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Table 2.3: Wage decomposition of managers' and non-managers' wages.

(a) Wage decomposition of managers' wages.

	Ln Wage (1)	Firm FE (2)	Worker FE (3)	Age profile (4)	AKM residual (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 (1)	Firm FE (2)	Worker FE (3)	Age profile (4)	AKM residual (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 2.3a) and non-managers (2.3b). Dependent variables are the firm-occupation-level average wage components as estimated by the decomposition on equation (2.2). 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 (2.3) on propensity score matching sample. Propensity scores are estimated within industry-year groups and using firm-level characteristics in the pre-acquisition year.

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.[10] 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.

2.5.4 Operations and internationalization

For a subsample 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 2.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.

[10] We discuss the role of firm-worker interactions in the AKM decomposition in Appendix 2.C (Bonhomme et al., 2019; De la Roca and Puga, 2017).

Table 2.4: Decomposition of moving workers' wage at new firm.

	ln Wage (1)	Firm FE (2)	Worker FE (3)	Age profile (4)	AKM residual (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 (2.2). 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 (2.3) 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 2.2.2 for details.

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2.6 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

$$N_t \times \alpha_t = N_{t-1} \times \alpha_{t-1} + H_t \times \alpha_t^h - S_t \times \alpha_t^s, \tag{2.4}$$

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

$$\underbrace{\alpha_t - \alpha_{t-1}}_{\Delta \text{ worker FE}} = \underbrace{s_t^h(\alpha_t^h - \alpha_{t-1})}_{\text{hires}} - \underbrace{s_t^s(\alpha_t^s - \alpha_{t-1})}_{\text{separations}}. \tag{2.5}$$

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 (2.5), 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 (see equation (2.3)).

The result in Table 2.5, Column 1, shows an increase in the average worker fixed effect after a firm is acquired, very close to the estimates in Figure 2.1 (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 (2.5). The margin of hires explains a change of around 97% of the total change in average worker fixed effect. The separations margin is very close to zero and statistically insignificant.[11]

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 2.5 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 (2.5), 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 2.5 have the fixed effect of incoming workers and the share of newly hired workers in the firm

[11]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.

as dependent variables. After an acquisition, newly hired workers have around 2% higher fixed effects. The impact of an acquisition on the share of newly hired workers in the firm is only around 0.7 percentage points. In comparison to the average share of new hires in firms before the acquisition of 24%, a 0.7 percentage point increase in the share suggests a small effect on the average worker fixed effect, implying that the entry of workers with higher fixed effect explains most of the increase in average worker fixed effects.

Table 2.5: Hire and separation margins.

	Δ Worker FE	Components		ln Workers	Worker FE	Share of hires
		Hires	Separations		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 (2.2). Columns 1 to 3 show the estimates of decomposition (2.5). 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 (2.1) 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 2.2.2 for details.

2.7 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 to 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).

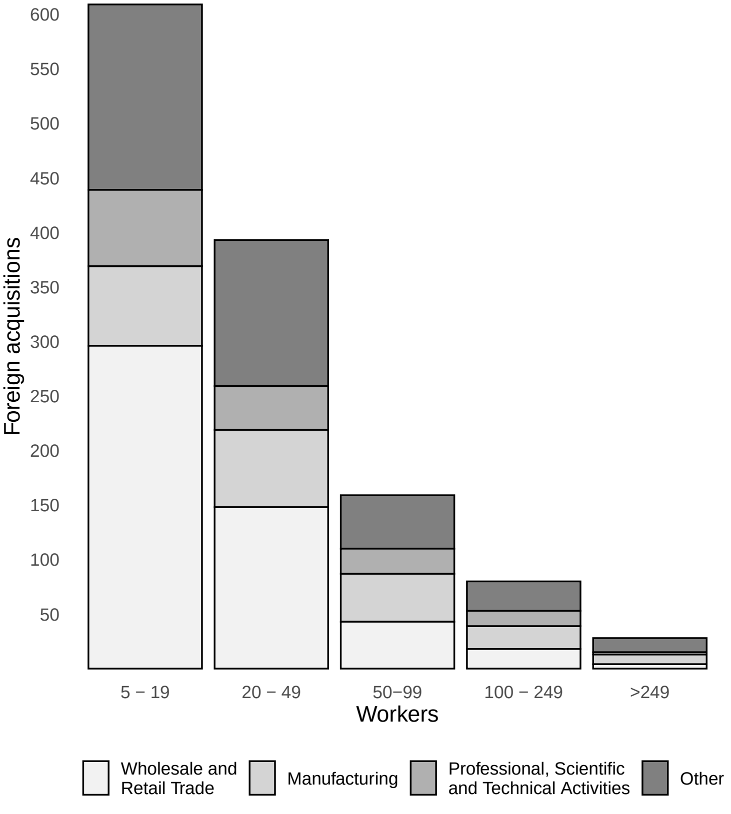
One reason why we find a larger role for firm-level premia relative to most worker-level studies of the multinational wage gap, is that we employ a different methodology. We first estimate a two-way fixed effects model to decompose wage into firm- and worker-level components (Abowd et al., 1999; Engbom 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, subsample 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.

Appendix to Chapter 2

2.A Supporting tables and figures

Figure 2.2: 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 2.2.2 for details.

Table 2.6: 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 (2.2). Estimated using difference-in-differences regression (2.1) 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 2.2.2 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 2.7: Decomposition of the cross-sectional foreign firm wage gap.

	Ln Wage (1)	Firm FE (2)	Worker FE (3)	Age profile (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 (2.2). 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 2.8: Cross-sectional wage decomposition, matched post-acquisition vs. always domestic firms.

	Mean ln wage (1)	Firm fe (2)	Mean worker fe (3)	Mean wage-age pr (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. Ln is the natural logarithm. Dependent variables are firm-level averages of the wage decomposition on (2.2). 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 2.9: 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 (2.2). Estimated using difference-in-differences regression (2.1) 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 2.10: Cross-sectional wage decomposition, matched post-acquisition vs. matched control firms.

	Mean ln wage (1)	Firm fe (2)	Mean worker fe (3)	Mean wage-age pr (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 (2.2). 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 2.2.2 for details.

Table 2.11: Cross-sectional wage decomposition, pre-acquisition domestic and always foreign vs. always domestic firms.

	Mean ln wage (1)	Firm fe (2)	Mean worker fe (3)	Mean wage-age pr (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 (2.2). 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.

2.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 (as in equation (2.3)).[12]

Acquired firms grow significantly faster in sales and employment after acquisition (see Table 2.12). 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 2.14).[13] These results signal that changes in the acquired firm's internationalization strategy contribute to the observed change in firm premia in Figure 2.1.

[12]Table 2.13 shows that we also observe growth in firm fixed effects for the subsample of observations with observed sales, value added and value of production.

[13]Tables 2.15 and 2.16 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 2.12: Firm operations.

	Ln workers (1)	Ln sales (2)	Ln value added (3)	Ln prod. value (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 (2.3) on propensity score matching sample. Propensity scores are estimated within industry-year groups and using firm-level characteristics in the pre-acquisition year.

Table 2.13: Wage decomposition in firm operations sample.

	Ln Wage (1)	Firm FE (2)	Worker FE (3)	Age profile (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 2.12. 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 (2.3) on propensity score matching sample. Propensity scores are estimated within industry-year groups and using firm-level characteristics in the pre-acquisition year.

Table 2.14: Change in exports and imports in acquired firms. Sample with observed sales.

(a) Exports.

	Exports (1) Poisson	Exp. Destinations (2) Poisson	Exporter (3) OLS
Post-Acquisition	0.1434* (0.0712)	0.0002 (0.0342)	-0.0143 (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 (1) Poisson	Imp. Destinations (2) Poisson	Importer (3) OLS
Post-Acquisition	0.1306 (0.1043)	0.0511· (0.0275)	0.0168 (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 (2.3) on propensity score matching sample. Propensity scores are estimated within industry-year groups and using firm-level characteristics in the pre-acquisition year.

2

Table 2.15: 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 (2.2) 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 2.2.2 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 2.16: Change in imports in acquired firms. Event study estimates on full sample.

	Imports (1) Poisson	Imp. Destinations (2) Poisson	Importer (3) 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 (2.2) 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 2.2.2 for details. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

2.B Data appendix

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

2.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 (2.2) 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 (2.2) 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.

2.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.

2.B.3 Descriptive statistics before matching

In line with earlier research, target firms of acquisitions and domestic firms differ substantially in our data. Table 2.17 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.

Table 2.17: 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 2.3. Wage components are firm-level averages of the decomposition on (2.2). Growth refers to the yearly log difference.

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

Table 2.18: 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.

2.C Limited mobility, firm-worker interactions and AKM assumptions

2.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

$$ln(w_{ijt}) = \alpha_i + X_{it}\beta + \psi_{jt} + \gamma_t + \epsilon_{ijt}, \tag{2.6}$$

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-differences regression is identified as

$$DiD^{\psi} = (\hat{\psi}_{T,s} - \hat{\psi}_{T,-1}) - (\hat{\psi}_{C,s} - \hat{\psi}_{C,-1}), \tag{2.7}$$

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,[14] such that

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

Our identification strategy (equation (2.7)) 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 these fixed effects and the rest of the firm-worker network (Jochmans and Weidner, 2019), such that

2

$$E[b_{j,t}|\text{ firm is treated }] \neq 0 \tag{2.8}$$

and

$$E[b_{j,t}|\text{ firm is control }] \neq 0. \tag{2.9}$$

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 (2.7), additionally, the limited mobility bias does not affect our conclusions if the treated and control firms' fixed effect estimates are equally biased ($E[b_{j,t}|\text{ firm is treated }] = E[b_{j,t}|\text{ firm is control }]$). Similarly, the bias is eliminated by (2.7) 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

[14]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.

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 2.19 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 2.19: 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 2.20).

Table 2.20: 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 (2.2) 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).

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 2.21 compares the difference-in-differences estimates. As the coefficients differ typically by at most 0.0002 points, the connectivity has little impact on the estimates. This is not surprising, as the weakly connected firms that are dropped in the comparison are typically small and hardly used for the identification of the impact of foreign acquisitions on wages.

Table 2.21: Comparison of difference-in-differences estimates for the main firm-worker network and a well-connected subnetwork.

	Main network		Subnetwork	
Years since acquisition	Firm FE (1)	Worker FE (2)	Firm FE (3)	Worker FE (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 (2.2) 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 (2.2). Estimated using difference-in-differences regression (2.1) 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 2.2.2 for details. Pre-trends shows the p-value of a Wald test on the joint significance of pre-acquisition effects ($s = -3$ and $s = -2$).

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 2.5 we find that foreign acquisition impacts the decomposition's residual differently for moving workers and managers. If firm-worker interaction, 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 (2.6) 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

$$
\begin{aligned}
ln(w_{ijt}) = \\
&+ \underbrace{\delta_0 \times D_j^C \times \alpha_i + \delta_1 \times D_j^T \times \alpha_i + \delta_2 \times D_{jt}^P \times \alpha_i + \delta_3 \times D_j^T \times D_{jt}^P \times \alpha_i}_{\text{skill complementarity}} \\
&+ \alpha_i + \psi_{jt} + X_{it}\beta + \gamma_t + \epsilon_{ijt}, \qquad (2.10)
\end{aligned}
$$

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 (2.10) 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_2, 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 (2.10) 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 2.22: 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 (2.10). 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 2.22 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, compared to the unmatched firms, 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 (2.10). 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 2.23. 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 chapter (see Table 2.24). 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 2.23 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 2.23: Difference-in-differences decomposition of the acquisition wage gap, including worker complementarity.

	Ln Wage (1)	Firm FE (2)	Worker FE (3)	Age profile (4)	Complementarity (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 (2.10). 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 2.2.2 for details.

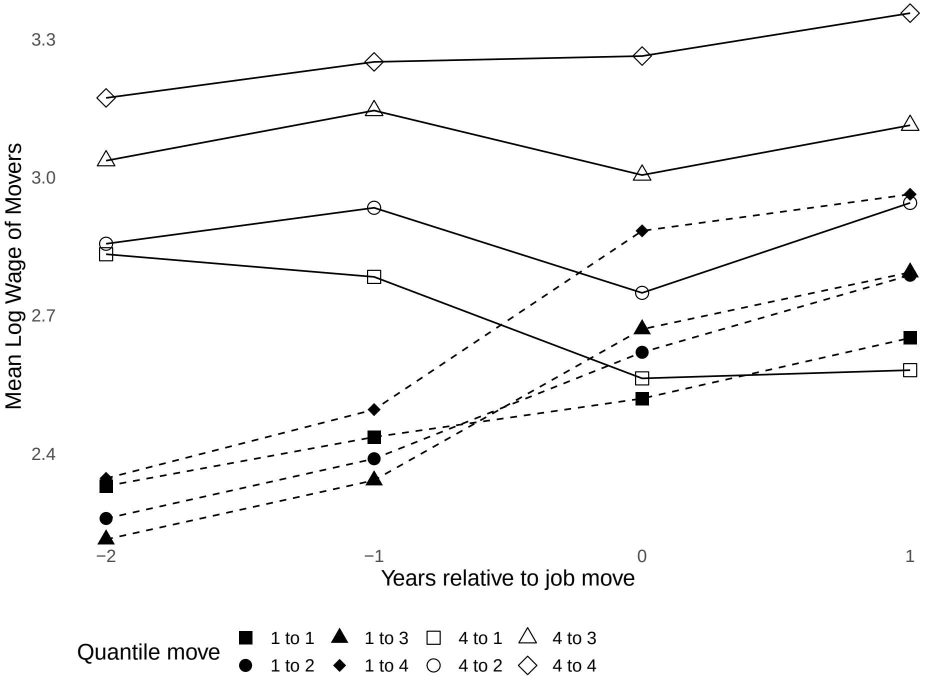
Table 2.24: Difference-in-differences decomposition of the acquisition wage gap, excluding worker complementarity.

	Ln Wage (1)	Firm FE (2)	Worker FE (3)	Age profile (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 (2.2). 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 2.2.2 for details.

2.C.3 Mover plot

Figure 2.3: 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., 2018, 2013). 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).

2.D Outputs of robustness checks

Table 2.25: 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 (2.2). Dependent variable in Columns 3, 4 is the firm fixed effect of the decomposition on (2.2), 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 (2.1) 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 2.2.2 for details.

Table 2.26: 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 (2.2). Estimated using difference-in-differences regression (2.1) 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 2.2.2 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 2.27: 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 (2.2). The dependent variable in Column 3 is the firm-level average of stayers' residual from the decomposition on (2.2). Estimated using difference-in-differences regression (2.1) 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 2.2.2 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 2.28: Comparison of different matching covariates (Propensity Score Matching).

Years since acquisition	A Firm FE (1)	A Worker FE (2)	B Firm FE (3)	B Worker FE (4)	C Firm FE (5)	C Worker FE (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 (2.2). Estimated using difference-in-differences regression (2.1) 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 2.29: Comparison of different matching covariates (Coarsened Exact Matching).

	D		E		F	
Years since acquisition	Firm FE (1)	Worker FE (2)	Firm FE (3)	Worker FE (4)	Firm FE (5)	Worker FE (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 (2.2). Estimated using difference-in-differences regression (2.1) 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 2.30: 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 2.6. 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 2.31: 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 2.6. Column 2 shows the standard errors of Table 2.6. 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).

Chapter 3

Careers in Multinational Enterprises

Abstract: Do workers in multinational enterprises (MNEs) build stronger CVs? We track the careers of all workers entering the Dutch labor market over the years 2006 to 2021 and find large and portable wage premia of MNE employment experience. Workers with experience at MNEs instead of domestic firms earn up to 14% higher wages within the MNE, and up to 11% higher wages after moving to another firm. Consistent with a model of MNEs that leverage the value of their employment experience, we find that MNEs hire more juniors, pay lower starting wages, and are more selective towards senior workers than domestic firms.

This chapter is based on the Tinbergen Institute Working Paper (No. 24-005/V) titled "Careers in Multinational Enterprises" and is joint work with Michiel Gerritse and Bas Karreman.

3.1 Introduction

For many workers, multinational enterprises (MNEs) are more attractive employers than domestic firms. An extensive literature documents that MNEs pay higher wages, are more productive, and attract better workers (Fons-Rosen et al., 2021; Girma and Görg, 2007a; Roesch et al., 2022; Setzler and Tintelnot, 2021). However, the impact of MNE employment on workers' careers is less clear. Experience at an MNE may improve workers' CVs, raising their future earnings potential, as MNEs provide more on-the-job training, use advanced technologies, and foster network effects of productive employees (Balsvik, 2011; Koch and Smolka, 2019; Poole, 2013). MNEs may also screen their workers more strictly than other firms. Hence, MNE employment could allow workers to build human capital or signal quality, leading to higher future wage.

This chapter shows that employment experience in an MNE increases workers' future wages, both within the MNE and at other firms. We embed such wage premia from MNE employment experience in a standard model of international firms with hiring and promotion strategies, and we show that the firms' predicted organizational choices are consistent with stylized facts in the data.

We employ the universal matched employer-employee data of the Netherlands over the years 2006 to 2021 to investigate the wage effects of MNE employment over the careers of workers. Our dataset combines detailed worker- and firm-level information with data on firm nationality and international trading activities. Focusing on cohorts of graduates, we track the wage impacts of MNE employment for up to 16 years after the worker enters the labor market. The Netherlands provides an appropriate setting to study the impact of MNE employment on workers' wages. In addition to the availability of highly-detailed data, the country is open to international trade and investment, and MNEs hire around one third of the workers who enter the labor market.

To identify the impact of MNE employment, we use a Mincer regression that explains wages based on previous employment experience (Abowd et al., 1999, AKM henceforth). Our regression controls for the fixed effects of a worker's current firm, to avoid conflating the impacts of earlier employment experience with the current employer's quality. It also controls for worker-level fixed effects to distinguish experience effects from unobserved skills and from the sorting of high-skilled workers into multinationals. In addition, we control for industry-year shocks, various dimensions of worker mobility, and direct observables of workers and firms. We also verify our results in settings of mass layoffs and company closures that generate exogenous variation in workers' CVs, and in multiple variations of the AKM regression.

Our results show significant wage premia of a worker's earlier employment in MNEs. As experience accumulates, a worker's wage increases by about 1 to 14% faster over 10 years when the worker is employed at an MNE, instead of a domestic firm. The wage premia following MNE employment also persist when the worker moves to another firm. At later employers, a worker with past MNE employment earns 2 to 11% higher wage than a worker with similar domestic employment spells, and the premium increases in the years of earlier experience in an MNE. Our point estimates reflect a total value of employment experience in MNEs of about 6% of labor income in the Netherlands in the cross-section of 2021, compared to when that experience had been accumulated in domestic firms.

In addition, we present several empirical results that suggest MNEs leverage the career prospects of employment at their firm. We set up a stylized model of international firms with hires and promotions, in which the firm recognizes its value for its workers' future wage. We document descriptive results that are consistent with the model's main predictions. First, MNEs pay relatively low wages to junior (inexperienced) workers. The intuition is that juniors accept lower wages as they are compensated with experience that pays off once they are seniors. In our estimates of career wage paths (conditional on worker fixed effects), workers with MNE experience earn higher wages over the course of their careers as they earn high wages in late career stages. At the career onset, MNEs pay lower wages than domestic firms, conditional on worker and firm characteristics. Second, MNEs employ higher ratios of junior to senior workers. As juniors have low wages relative to their marginal product, the MNE employs relatively few seniors. Consequently, the promotion probabilities to senior positions are low within MNEs. Consistent with such stricter selection, the average worker fixed effect in MNEs increases strongly with seniority, compared to domestic firms.

This chapter relates most closely to the studies by Pesola (2011) and Mion et al. (2022). Pesola (2011) shows that workers who leave a foreign-owned firm (instead of a domestic firm) earn higher wages at the next employer, conditional on worker fixed effects. The estimates of wage premia are about half the size of ours and suggest that early-career workers do not forego wage in exchange for experience. Whereas Pesola (2011) uses a sample of Finnish workers, we use the universe of cohorts of Dutch workers entering the labor market, allowing us to condition on the workers' full experience profile since labor market entry. Additionally, we focus on multinationals rather than foreign-owned firms, and we control directly for firm-level fixed effects, thus excluding worker sorting into productive firms as an explanation for the wage premia of previous employment experience.

Mion et al. (2022) show for a sample of managers in Portugal that employment in internationally active firms generates wage premia relative to employment in domestic firms. Their estimates of the magnitude of the premia are similar to ours. Mion et al. (2022) also find lower employment premia for blue-collar workers, which ties in with the lower premia of MNE employment that we find for workers with lower ability estimates (i.e. lower fixed effects in the Mincer regressions). In our analysis, we additionally distinguishes between MNEs and other internationally active firms, highlighting a dominant role of MNEs in the rewards to experience. Moreover, we find that worker experience, and not the average level of workers' fixed effects, is crucial in understanding why productive workers are employed in MNEs. We are able to dissect sorting (on fixed effects) from experience effects, because we directly estimate worker and firm fixed effects. Our approach exploits the universe of worker-employer relations instead of a sample, which is essential to identify the fixed effects in an network setting (Jochmans and Weidner, 2019). Lastly, the completeness of our data allows us to shed light on the consequences of the value of MNE experience for the wage setting and organization of MNEs. Our descriptive results indicate that MNEs adapt their hiring strategy to the value of their workers' experience, consistent with the predictions of our theoretical framework.

Our results additionally add to a large literature that examines why MNEs pay high wages in local labor markets. MNEs may have a higher level of productivity, driven by technology, management or connectivity (Andersson et al., 2022; Bircan, 2019; Girma and Görg, 2007a; Koch and Smolka, 2019), but the higher wage could also be driven by the MNE's selection of workers, as they select workers of higher ability (Hijzen et al., 2013; Roesch et al., 2022; Setzler and Tintelnot, 2021). Relative to this literature, we propose that careers play a central role in explaining the wage premia of multinationals. We find that MNEs have a substantial impact on local wages, as the workers who have worked for MNEs receive higher wages later on. Compared to this employment experience value, the immediate wage benefits of current employment at a multinational are minor. Our results also shed light on the sorting process: Where earlier literature suggests that workers select into MNE employment on time-invariant abilities (e.g., Heyman et al., 2007; Hijzen et al., 2013; Setzler and Tintelnot, 2021), we show that the past employment experience of workers, instead of their time-invariant ability, explains most of the premia that they earn at multinationals. Consistent with sorting on experience rather than on innate ability, we find that MNE premia are absent for junior workers, and only materialize later in the career. Additionally, we present evidence of selection within the MNE over time: Out of a cohort of workers entering an MNE, those with higher innate ability are more likely stay with the MNE over time. Similar results have been obtained for domestic firms of different productivity types (Adda and Dustmann, 2023; Serafinelli, 2019; Stoyanov and Zubanov, 2012). In contrast to these results, we focus on the distinction between multinationals and domestic firms as two discrete and directly observed firm types.

Related work also studies the spillovers that multinationals cause towards other firms. Most studies rely on firm-level data (e.g., Haskel et al., 2007; Javorcik, 2004;

Keller and Yeaple, 2009) but a few worker-level studies suggest that firms hiring workers with previous MNE experience become more productive (Balsvik, 2011; Poole, 2013). Balsvik (2011) shows that, within domestic firms, workers with previous MNE experience earn a wage premium over continuing domestic workers. Similarly, Poole (2013) shows that when ex-MNE workers join a domestic firm, the wages of continuing workers rise. Our approach does not identify spillovers directly, but establishes that experience in multinationals increases workers' future wage, and that the value of MNE employment experience is highly portable between firms.

The rest of the chapter is organised as follows. In Section 3.2, we present an off-the-shelf model of international firms, in which we introduce hires and promotions. The model motivates that firms with a higher experience value of their employment are likely to be multinationals. More importantly, it generates several predictions on how firms adapt their hiring and promotion strategies if they operate as multinationals. In Section 3.3, we introduce the dataset. Section 3.4 shows the main empirical results on the value of MNE experience and its implications for the interpretation of the AKM fixed effects. Section 3.5 explores our theoretical model's implications for the labor market strategies of multinationals. Finally, Section 3.6 presents several robustness checks and Section 3.7 concludes.

3.2 Theoretical motivation

We employ an off-the-shelf trade model to examine the consequences of valuable on-the-job experience for the organization of international firms. The setting is a monopolistic industry in which firms employ junior and senior workers. All workers produce the same product, but at potentially different productivity levels. Workers offer two periods of labor: one period as a junior, and one period as a senior. Junior workers can use their experience to earn higher wages in the senior stage of their career. If a worker moves between firms after the junior period of employment, the value of his or her experience is discounted relative to a worker who becomes a senior in the original firm.

Within this setting, we examine how the value of employment experience within a firm (i.e. the senior wage benefits associated with junior employment in that firm) affect the organization of the firm. In line with theories of international trade, firms may have different levels of overall productivity. In addition, its workers have a match-specific productivity, that is only observed after a period of employment. We focus on a static interpretation in which workers offer one junior and one senior unit of labor.

3.2.1 Output market

We consider a symmetric two-country economy in which consumers (indexed i) have CES preferences over products from different firms (indexed j), and there is an elasticity of substitution between products of $1 - 1/\sigma$. On the home output market, firms take the demand function as given; $d_j = p_{d,j}^{-\sigma} B$, where $p_{d,j}$ is the delivered price of firm j. The term $B = (\int\limits_i l(i)w(i))di)/(\int\limits_j p_{d,j}^{1-\sigma} dj)$ reflects the market size, consisting of the labor income $w(i)$ aggregated over all workers, divided by the (delivered) price index $\int\limits_j p_{d,j}^{1-\sigma} dj$. It is symmetric across the countries. Firms face iceberg transport costs τ to supply the other country. Firms charge a markup over marginal costs, charging $p_j = \sigma/(\sigma - 1)mc_j$ in the home market, and τp_j in the foreign market. The operating profits in the symmetric economy are proportional to $(\sigma/(\sigma - 1)mc_j)^{1-\sigma} B/\sigma$ in the home market and to $\tau^{1-\sigma}(\sigma/(\sigma - 1)mc_j)^{1-\sigma} B/\sigma$ in the foreign market, so that operating profits strictly decrease in the marginal cost level of the firm.

Firms face fixed costs of operating in different markets. The fixed costs are a production volume f required to operate: f_d for domestic operations, f_x for exports to foreign market, and f_m to operate a plant in the foreign market as a multinational. We assume that $f_x < f_m$. The framework thus far closely follows Helpman et al. (2004) and produces the same ordering of international strategies in the marginal cost level of the firms. Firms have a maximal marginal cost to operate domestically, mc_d, a maximal marginal cost to export, mc_x and a maximal marginal cost to operate as a multinational, mc_m, where $mc_m < mx_x < mc_d$. Hence, the firms with the lowest

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marginal costs become multinationals.

The marginal costs of the firm are determined by the level productivity of the firm, b_j, the productivity shifters of the specific workers employed by the firm, the value of experience of the workers in the firm, and the wage levels of those workers, which we detail below.

3.2.2 Labor supply

Workers have a junior stage and a senior stage in their career. They offer one unit of labor in either stage to one firm. The productivity of the worker has a component that is specific to the firm-worker match: $a(\theta_{i,j})$, where i indexes the worker and a increases in θ. The productivity shifter of worker i in firm j is only known after a period of employment and is equal in the junior and senior stage of the career. The density of the productivity distribution over the workforce $l(\theta_{i,j})$ is known and constant across all firms, and the expected value of the productivity shifter over the population is a.

In the junior stage, workers build up experience at their employer j, which increases their productivity in the senior period. If the worker remains with firm j, the worker's productivity in the senior stage is higher by a factor e_j. If the worker moves to another firm than j, the productivity is discounted by a factor $\delta \leq 1$, such that the productivity shifter from employment experience is $\delta e_j \geq 1$. This assumption reflects that the value of earlier employment experience may be imperfectly portable, for instance because some part of the worker's human capital is firm-specific, the worker is held back by intellectual property provisions, or the worker's tasks change between firms. The worker's match-specific productivity level $a(\theta_{i,j})$ is independent of the worker's match-specific productivity level in another firm $a(\theta_{i,j'})$.

Workers in the senior stage have no later employment opportunity and have full collective bargaining over their surplus, securing their marginal product of labor as wage.

3.2.3 Labor demand

The labor market decisions of firms determine the production process. Firms optimize three choices. First, firms choose how many juniors to promote to senior workers. Second, firms choose how many external workers to hire (who spent their junior stage in a different firm). Third, firms select how much output to produce.

The number of junior workers in the firm is L_j^J, the total number of internally promoted senior workers is L_j^S, and the number of externally hired senior workers is L_j^X. The terms s_j and x_j are the ratio of internal and external senior workers, such that $s_j L_j^J = L_j^S$ and $x_j L_j^J = L_j^X$.

The productivity by worker type is as follows. The firm has a general productivity shifter b_j. Junior workers have an average match-specific productivity shifter a as the match-specific productivity is unobserved at the junior stage. Hence, the average junior worker productivity is ab_j. After a period of employment, the firm observes workers' match specific productivities. It selects a minimum required productivity threshold for promotion $\theta(j)$, letting workers below the threshold go. Omitting the firm-specific subscript for brevity, the expected productivity shifter for the promoted workers is $a(\theta_j^*) = \int_{\theta_j^*}^{\infty} a(\theta)l(\theta)d\theta$, and the expected productivity of internally promoted workers is $b_j a(\theta_j^*)e_j$. As the firm promotes its workers by order of productivity, the match-specific shifter $a(\theta_j^*)$ declines in the rate of promotion s_j. The match-specific productivity distribution is not bound, so $a(\theta_j^*)$ tends to infinity as s_j tends to 0, and $a(\theta_j^*)$ tends to a as s_j tends to 1 (and $s_j \leq 1$ as the firm can only promote its own junior workers). As θ_j^* depends exclusively and inversely on s_j^*, this implies that $a(s_j)$ tends to infinity as s_j tends to 0 and $a(s_j)$ tends to a as s_j tends to 1. Finally, the firm can hire workers who left other firms on a market for senior workers. Outside workers require integration into the company, which we assume has convex costs such that the productivity of outside workers declines in the their ratio to internal junior workers, captured in a productivity shifter $z(x)$ with $dz/dx < 0$. The productivity of a worker in firm j who was previously employed at firm j' is $ae_{j'}\delta z(x)$. As workers on the external senior market are randomly matched to firms, the expected value of employment experience when hiring an external worker is the expected employment experience value over the population of workers who were not promoted at their initial firm $e = \int (1-s_j)L_j^J e_j dj / \int (1-s_j)L_j^J dj$.

Collecting the productivities of the three types of workers, the production function of the firm is

$$Q_j = b_j(aL_j^J + a(\theta_j^*)e_j L_j^S + ae_j'\delta z(x)L_j^X), \tag{3.1}$$

and the corresponding total cost function is

$$TC_j = L_j^J w_j^J + L_j^S w_j^S + L_j^X w_j^X. \tag{3.2}$$

3.2.4 Wage setting, promotion and hiring

In the senior stage of their careers, workers secure their marginal product as wage. The respective wages for workers who have been promoted inside their firm (S), who were hired from an outside firm (X), or who leave j and enter a next firm j' (O), are

$$w_j^S = p_j b_j a(s_j)e_j, \tag{3.3}$$

$$w_j^X = p_j b_j ae\delta, \tag{3.4}$$

$$w_j^O = p_j^o b_j^o ae_j\delta. \tag{3.5}$$

To employ a junior worker, firm j needs to offer a wage that is consistent with a lifetime income at least as large as the equilibrium lifetime income Y. This incentive implies that $Y = w_j^J + s_j w_j^S + (1-s_j)w_j^O$, or the required junior wage is

$$w_j^J = Y - s_j w_j^S - (1-s_j)w_j^O. \tag{3.6}$$

Note that from the senior wages w^S and w^O in (3.3), a higher employment experience e_j increases the expected senior wage of any worker who enters firm j.

Cost minimization for a given level of output implies the first-order conditions

$$w_j^S / w_j^J = a(s_j)/ae_j(1+\varepsilon_s), \tag{3.7}$$

and

$$w_j^X/w_j^J = e_j'\delta z(x)(1+\varepsilon_x), \tag{3.8}$$

where $\varepsilon_s = da(s_j)/ds_j * s_j/(a(s_j)$ and $\varepsilon_x = dz/dx * x/z$, which we assume to be sufficiently close to zero for internal solutions to x and s to exist (the intuition is that the distribution of match-specific productivity is smooth enough, so that promoting a marginal worker only has marginal impact on the average match-specific productivity of seniors). The first-order conditions equate the wages of either type of senior worker, relative to the junior wage, to the productivity of the senior worker, relative to the junior worker. The marginal products are constant in the employment ratios of the different types of workers, so the cost-minimizing ratios of senior to junior workers s and external seniors to juniors x are independent of the production quantity decision of the firm.

3.2.5 Marginal costs and internationalization strategy

Given the ratios of seniors to juniors, the firm maximizes profits by selecting the quantity of production, which is proportional to the quantity of junior hires. Facing the imperfectly competitive market, the quantity is determined by price, which in turn is a markup over marginal costs.

To see the marginal cost structure, we use the optimized promotion and external hiring rates in the cost and production functions to get

$$TC_j = L_j^J(Y-(1-s_j)w_j^O + x_j w_j^X), \tag{3.9}$$

$$Q_j = b_j a L_j^J(1+a(\theta_j^*)e_j s + e_j'\delta z(x)x). \tag{3.10}$$

As a consequence, the marginal cost function for the firm is

$$mc_j = \frac{dTC/dL_j}{dQ/dL_j} = \frac{Y - (1-s_j)w_j^O + xw_j^X}{b_j(1 + a(\theta_j^*)/ae_j s + ae_j'\delta z(x)x} \tag{3.11}$$

$$= \frac{1}{b_j}\frac{Y - (1-s_j)p_j^o ae_j\delta + xw_j^X}{1 + a(\theta_j^*)/ae_j s + ae_j^x\delta z(x)x}. \tag{3.12}$$

The marginal costs of the firm are inversely proportional to the productivity of the firm, b_j, as in standard theories of monopolistic firms. However, they additionally depend, among others, on the output prices p_j^o of the firms that hire firm j's exiting workers, and on the value of experience that incoming senior workers accumulated at other firms.

Comparing the level of marginal costs across firms, the marginal costs of firm j are low if i) the experience value of its employment e_j is high, ii) if the transferability of the experience δ to other firms is high, and iii) if the level productivity b_j of the firm is high. Note that the numerator decreases and the denominator increases both in e_j and δ. Moreover, by the envelope condition $dmc_j/ds_j = dmc_j/dx_j = 0$, and due to randomized matching of exiting workers to senior positions in other firms, p_j^o is constant across firms, as is Y.

The intuition is as follows. A firm with higher employment value e_j can offer junior workers a lower wage. The low wage balances with compensations later in the worker's career. If the worker is promoted, the firm pays the senior worker a higher wage in tandem with their productivity. However, a leaving worker will earn more at a next firm, but the original firm does not pay for that higher wage, thus allowing the firm to internalize its workers' value of employment experience.

Hence, in our stylized framework, following eq. (3.11), conditional on the firm's productivity level (b_j), a multinational strategy arises if the firm has a sufficiently high employment experience value for its juniors (e_j).

3.2.6 Testable results

If multinationals have higher employment experience values, our results bear three central implications on wages (following eq. (3.7)). First, workers who remain with a multinational experience higher wages, because they accumulate valuable experience. This is in addition to the possibility that MNEs i) generally pay their workers higher wages and ii) MNEs may be more selective in the worker types they attract. Second, workers who move away from a multinational earn higher wages at their next employer. Third, and related, the rise in pay over the course of the career rises faster in a multinational, with the possibility that multinationals pay junior workers a lower wage than domestic firms.

The presence of an experience value also translates into the organizational hierarchy of multinationals. First, a firm with higher employment value e_j promotes fewer workers, so that it has relatively many juniors. To see this, note that the right-hand side of the first-order condition for promotion in eq. (3.7) rises in the post-promotion productivity ($a(s_j)/a$), so it falls in the rate of promotion s_j. The right-hand side of eq. (3.7) is proportional to e_j while the wage ratio on the left-hand side rises more than proportionally in e_j, as witnessed from combining w^S and w^J from eq. (3.3). Second, MNEs will offer relatively low entry wages. Following eq. (3.6), the entry wages of multinationals may be below those of domestic firms. The intuition is that while a higher employment experience value raises the productivity and wages of senior workers proportionally, it also reduces the equilibrium junior wage. Junior workers accept a payment in employment experience, and so the relative marginal cost of seniors to juniors is higher, which requires the firm to select only highly productive senior workers.[1]

[1] By the same logic, the relative wage of externally hired seniors to juniors is high, and the relative productivity of external seniors is elevated by having a lower proportion of external hires through $z(x)$.

3.3 Data

We construct a yearly employer-employee matched dataset for the period 2006 to 2021 using different administrative sources of Statistics Netherlands. Our data follows workers in the Netherlands up from labor market entry, combined with the MNE (foreign and domestic), international or domestic status of their employers.

Leveraging the consistent identifiers across sources, we combine ownership, trade and employment databases that add up to universal coverage. At the employer-level, we extract information on ownership structures and broad NACE industry from the General Business Register and enrich the data with information on imports and exports from the official trade statistics. In a first step, we aggregate the employer-level information to the yearly company group level for those employers that are part of a company group. Hence, our empirical analysis treats company groups as a single firm and employer.[2] Subsequently, we identify three distinct types of firms. We classify a firm as an MNE if its ultimate owner, which controls strategic decisions, is non-Dutch, or if the domestic firm reports affiliates abroad. In total, around 19k of the 207k firms in our dataset are MNEs. We classify the remaining firms as either domestic, or international (importing and/or exporting but no multi-country establishments) if the yearly-average sum of imports and exports exceeds 10k Euro.

We can closely follow career choices, as our employee-level data is based on information that employers send to the Dutch national employment agency (Uitvoeringsinstituut Werknemersverzekeringen). Serving to identify pension and labor market insurance claims, the data contain the exact start and end dates of an employer-employee relation. At the worker-level, the data provides information on workers' demographics, regular- and overtime-pay, any additional allowances and the associated hours worked. It also allows us to identify full-time equivalents and internships. For the analysis, we remove internships from the dataset and aggregate the remaining observations to the yearly worker-firm level. We focus on workers in full-time positions (≥ 0.7 fte) and remove all workers who ever hold more than

[2]On average around 1.8% (sd = 2.2) of the firms in a given year are company groups.

one full-time position at the same time (about 5% of workers). For all remaining worker-firm matches in the data, we calculate hourly wages as total income over total hours worked to measure earnings.

To follow workers' careers up from labor market entry, we connect the graduations from Dutch middle- and higher-level educational institutions across the years 2004 to 2021 to the data.[3] We select workers aged 18 and above who start working within three years after graduation from a full-time study and who are not enrolled in another educational program after labor market entry. By using the start and end dates of their employment relations, we construct four measures of experience. Within firms, we allow the number of days spent at the current employer to accumulate over time. Across firms, we leverage worker mobility to separately identify the number of days a worker spent in MNEs, international and domestic firms prior to entering a new employer.

The final dataset follows the lifetime career developments of over one million workers for up to 16 years. Tables 3.4 and 3.5 in Appendix 3.A provide an overview of the dataset. Workers are around 24 years old when entering the labor market and hold two different jobs throughout the observation period. Although MNEs make up less than 10% of the firms, about 30% of workers start their career at an MNE. On average, workers in MNEs earn 9% higher wages than workers in domestic firms, while workers with past employment in an MNE earn 8% higher wages at their current employer than workers with past domestic firm employment.

3.4 The value of MNE employment experience

To identify the expected change in a worker's wage for a year of employment experience in an MNE, we use a Mincer regression with firm and worker fixed effects (Abowd et al., 1999). The regression explains a worker's log wage from time spent at the current employer, measuring experience within his or her own firm, and from

[3]Specifically, we include Dutch universities (wo), universities of applied sciences (hbo) and the theoretical track of the Dutch secondary vocational education institutions (mbo-bol).

earlier employment experience in other firms. Specifically, the regression equation is

$$\log(w_{ijt}) = \psi_j + \alpha_i + \gamma_t + \sum_{c \in C} \lambda_c h^c_{it} + \sum_{c \in C} \beta_c t^c_{ijt} + \mathbf{x}'_{ijt}\nu + \epsilon_{ijt}, \tag{3.13}$$

where $\log(w_{ijt})$ is the natural logarithm of the hourly wage of a worker indexed i, at a firm j, in year t. The variable t^c_{ijt} captures the years of experience within the worker's current firm. Similarly, h^c_{it} captures the years of experience at employers before the current firm. We differentiate the years of experience in a firm by the type of the firm, c: multinational, international (importing and/or exporting but no multi-country establishments), or domestic. We use the domestic firm type as the reference group.

The coefficients of interest are λ and β. The coefficient λ_{MNE} measures the log contribution to a worker's current wage of a year of experience at a previous employer that is an MNE, as compared to when that year of experience was with a domestic firm. The coefficient β_{MNE} measures the log wage contribution of a year of employment within the worker's current multinational firm, relative to when that firm would have been domestic. To allow the wage contributions to vary flexibly with years of employment experience, we estimate the impact using a set of indicator variables by year of (cumulative) experience.

We condition the estimates of the value of employment experience on a set of other explanations, following the literature on matched employer-employee data. Workers with valuable experience may sort into firms that are highly productive or pay high wages for other reasons. To exclude this interpretation, we introduce a firm-level fixed effect ψ_j in the specification. Similarly, we difference out worker-specific level difference in pay with a worker fixed effect, α_i. Exploiting within-worker variation, we identify the impact of experience from worker-specific changes in experience levels over time, accounting for the possibility that high-ability workers both earn more and accumulate multinational-specific experience more often. In addition, we difference out year-specific industry fixed effects γ_t to prevent industry shocks, such as technology or demand, that generate employment as well as wage differences from explaining the association between experience and wage.[4] Finally, vector $\mathbf{x}_{ijt}$ contains observed characteristics. We use the log of employment size of the firm to control directly for any size-related explanations of wage. To control for the mobility of workers, we also include dummies for the number of jobs a worker has held up to time t and their interaction with tenure inside the firm. The term ϵ_{ijt} is an error term.

To account for serial correlation of the errors within workers, we cluster the standard errors at the worker level. Later we will study estimates of the firm- and worker-level fixed effects. As usual in the literature studying the mobility of workers between firms, we estimate our fixed effects model on the largest set of firms that are connected by worker mobility (Abowd et al., 2002, 1999). In Section 3.6, we offer several robustness checks pertaining to the specification of our empirical model and the mobility of workers (Bonhomme et al., 2019).

Figure 3.1 summarizes the main estimates of eq. (3.13), based on the full results reported Table 3.11 in Appendix 3.F. Panel (a) of Figure 3.1 shows the coefficient estimates for λ_{MNE}, the expected wage premium for an additional year of MNE experience instead of domestic firm experience in an earlier firm. The results show an immediate and statistically significant premium of around 2% with the first year of experience in an MNE relative to a domestic firm. With more years of experience in a previous MNE employer, the expected premium rises monotonically, up to around 11% with seven years of experience. At that point, the coefficient curve is flatter, pointing to a smaller marginal contribution of an additional year of experience.

Panel (b) of Figure 3.1 summarizes the estimates of β_{MNE}, the wage premium of a year of multinational experience inside the same firm. The wage premia drawn from experience external to the firm (Panel (a)) and internal to the firm (Panel (b)) are every similar, although the internal premia are slightly higher.

For comparison, Figure 3.1 shows the wage premia for experience in an international firm relative to a domestic firm. The premia on experience in an international firm are significantly below those of the multinational firm, but positive

[4]In practice, we first demean log hourly wages by γ_t on a dataset that includes the wages of all workers in the Netherlands. Then we use the residuals to estimate the other parameters in the sample of labor market entrants.

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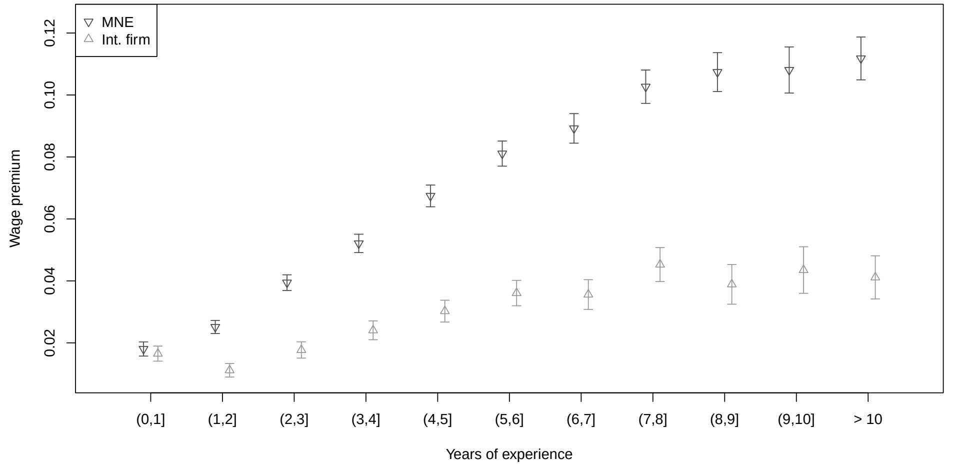
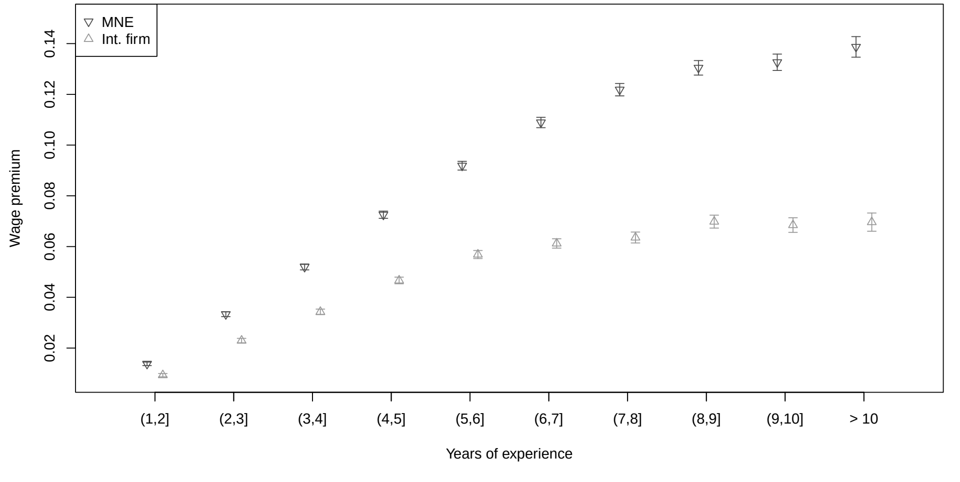
Figure 3.1: The wage premia of MNE experience.



(a) Across-firm



(b) Within-firm

Notes: The plots depict calculated wage premia and their 95%-confidence intervals (see eq. (3.13) and the discussion in Section 3.4). The full regression results are in Table 3.11 in Appendix 3.F. Wage premia are calculated as the coefficients for MNE/international firm experience minus the respective coefficients for domestic firm experience. Experience within and across firms are based on actual days worked and cut in yearly splines. MNEs comprise foreign firms (ultimate owner located abroad) and domestic firms with foreign subsidiaries. International firms are defined as (non-multinational) firms with an average yearly sum of imports and exports that exceeds 10k EUR. Standard errors are clustered at the worker level.

over domestic firm experience.

Section 3.6 shows a set of robustness checks with our main result. It covers among others, checks on definitions, size effects of firms, variation in the transferability of the wage premia, mobility decisions of workers, and the overall worker mobility across employers.

3.4.1 The value of experience and firm fixed effects

The firm fixed effects in the AKM wage model (3.13) reflect static differences across firms in pay premia, conditional on their workers' fixed effects. They are commonly used to measure static pay premia of MNEs (e.g. Andrews et al., 2009; Setzler and Tintelnot, 2021) and more broadly interpreted as measures of firm-level productivity as they correlate with the marginal product of labor in competitive labor markets. Firm internationalization models argue that high productivity selects firms into the MNE status (Helpman et al., 2004), as does our framework (for firms with high static productivity b_j). Plausibly, a firm's static productivity estimate correlates with the accumulated employment experience in the firm. Hence, in this section, we disentangle the wage contribution of the static firm fixed effect from the contribution of the employment experience of the firm's workforce.

To understand how workers' experience affects the estimate of firm fixed effects, we compare the distributions of firm fixed effect estimates with and without controlling for workers' experience. Figure 3.2 displays distributions of firm-level fixed effects of domestic firms (dashed line) versus those of MNEs (solid line). Panel (a) shows the distribution derived from a static AKM decomposition, which attributes wages to firm fixed effects, worker fixed effects, and industry-year fixed effects only. In the static model, MNEs have around 8% higher fixed effects than domestic firms.[5] Panel (b) shows AKM fixed effects estimates conditional on workers' for experience in different types (domestic, international, MNE) of previous employers. Once controlling for such experience, the average fixed effects of MNEs are 6% lower than that of domestic

[5]A formal comparison of these distributions can be found in Appendix 3.B (Combes et al., 2012; De la Roca and Puga, 2017).

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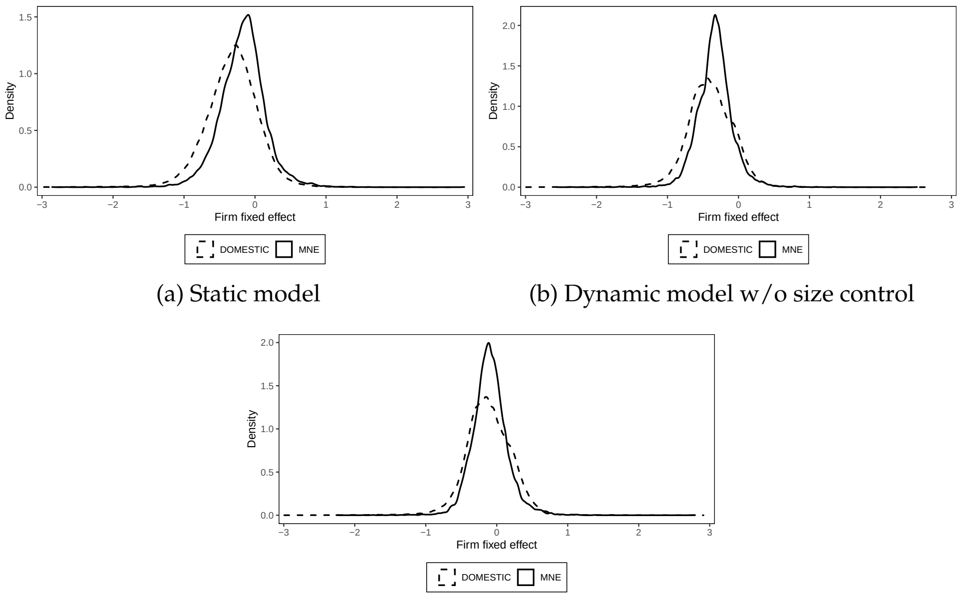
firms (see Table 3.8 in Appendix 3.B). Panel (c) shows fixed effects conditional on worker experience as well as a size control, resulting in fixed effects of MNEs that are about 1% lower than those of domestic firms. These results suggests that the full ex-ante wage premium of MNEs is explained by the employment experience of their workers.

Figure 3.2: Comparison of firm fixed effects, MNE vs. domestic firm.



(a) Static model

(b) Dynamic model w/o size control

(c) Dynamic model

Notes: The plots show the distributions of firm fixed effect estimates derived from different models. The estimates include the fixed effects of MNEs and domestic firms that do not change status. The firm fixed effects are derived from (a) a static model with only worker fixed effects, firm fixed effects and industry-year fixed effects; (b) a dynamic model with MNE, international and domestic firm experience included (see eq. (3.13)) but excluding the log firm size control; (c) our main dynamic model with MNE, international and domestic firm experience included (see eq. (3.13)). A formal comparison of the distributions is in Table 3.8 in Appendix 3.B.

3.4.2 The value of experience and estimates of worker sorting

In related literature, the sorting of workers with high fixed effects into MNEs explains most of the wage premia that MNEs pay (e.g., Balsvik, 2011; Setzler and Tintelnot, 2021). However, in an AKM regression, omitting workers' previous employment experiences may lead to an overestimation of the workers' static abilities in their time-invariant fixed effect. Consequently, if MNEs employ workers with valuable employment experiences, the value of experience might be wrongfully interpreted as sorting on time-invariant skill in a static AKM estimation. To examine this possibility, we compare distributions of the worker fixed effect estimates, with and without controlling for experience patterns.

Figure 3.3 displays the distributions of the worker-level fixed effects for workers in domestic firms (dashed line) and MNEs (solid line). As workers move between MNEs and domestic firms, we avoid observing workers multiple times by focusing on the fixed assignment of workers in 2014, roughly the middle of the data's time span.[6] When estimated from a static AKM model with worker-level, firm-level and industry-year fixed effects only, the mean worker fixed effect is approximately 8% higher in MNEs than in domestic firms.[7] This statistic would suggest that MNEs tend to hire workers with greater time-invariant earning abilities. Panels (b) and (c) exhibit the worker-level fixed effects from the dynamic AKM model with experience included (see eq. (3.13)), with a firm size control excluded (Panel (b)) or included (Panel (c)). Once accounting for workers' experience, the average difference in worker fixed effects between MNEs and domestic firms reduces to roughly 4 to 6%. This indicates that neglecting the value of prior employment experience (as in Panel (a)) overstates the role of innate worker ability in wage determination.

If workers with higher time-invariant ability also have higher returns, Panels (b) and (c) could overestimate ability at the top of the distribution and underestimate it at the bottom. We estimate a dynamic AKM model that permits heterogeneity in the value of employment experience according to a linear interaction with the worker fixed effect (De la Roca and Puga, 2017). We detail and discuss the implications of such heterogeneity in the wage returns to MNE experience in Section 3.4.3. Panel (d) of Figure 3.3 depicts distributions in the time-invariant worker fixed effects, accounting for variation in the returns to MNE employment experience. Allowing for such heterogeneity in the estimation, the distributions of time-invariant skill of workers largely coincide between MNEs and domestic firms (there is no significant difference

[6] The results are qualitatively similar when using a different year to fix worker assignment.
[7] See Appendix 3.B for a formal comparison (Combes et al., 2012; De la Roca and Puga, 2017).

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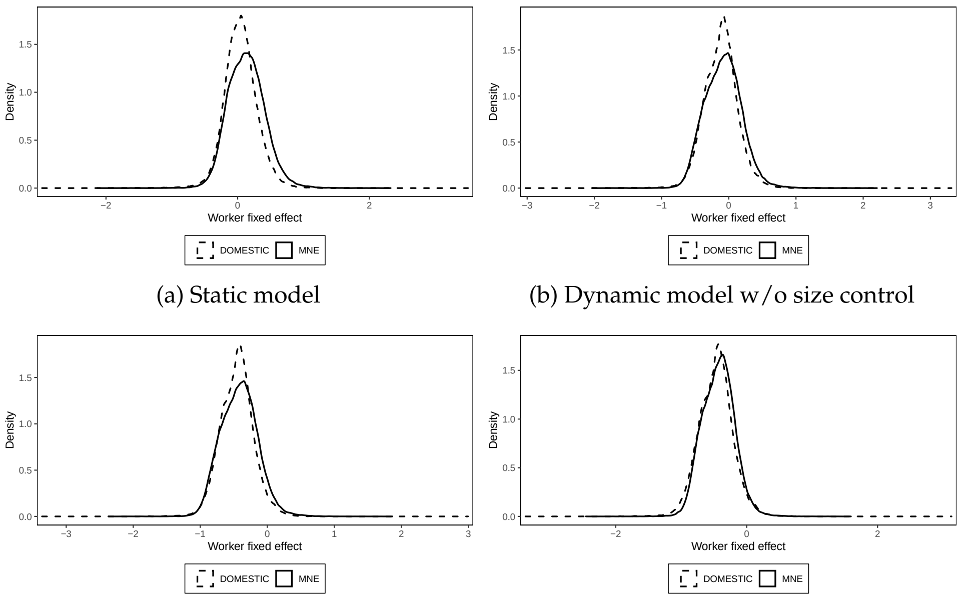
- see Appendix 3.B). Hence, difference in the value of MNE experience, and the higher returns to MNE experience for workers with high fixed effects, explain the original result that workers of high fixed effects sort into MNEs.

Figure 3.3: Comparison of worker fixed effects, MNE vs. domestic firm.



(a) Static model

(b) Dynamic model w/o size control

(c) Dynamic model

(d) Dynamic model with worker fe interactions

Notes: The plots show the distributions of worker fixed effect estimates derived from different models. The estimates are based on a snapshot of the data in 2014, and include the fixed effects of workers in MNEs and domestic firms that do not change status. The worker fixed effects are derived from (a) a static model with only worker fixed effects, firm fixed effects and industry-year fixed effects; (b) a dynamic model with MNE, international and domestic firm experience included (see eq. (3.13)) but excluding the log firm size control; (c) our main dynamic model with MNE, international and domestic firm experience included (see eq. (3.13)); (d) a dynamic model with MNE, international and domestic firm experience, and their interactions with the worker fixed effects included (see eq. (3.14)). A formal comparison of the distributions is in Table 3.7 in Appendix 3.B.

3.4.3 The value of MNE experience by workers' innate ability

It is plausible that workers with high fixed effects benefit more from MNE employment in the long run. For instance, workers with higher skill might learn faster, or learn more skills that complement their own skill set at an MNE. Alternatively, the MNE might be a more precise signalling device for higher skilled workers. The correlation between the workers' fixed effects and their returns to MNE employment is relevant for at least two reasons. First, it can lead to the false attribution of high returns from past employment experiences to high inherent ability in a classical omitted variable problem. Neglecting to account for experience might cause workers with valuable past employment experiences to be classified as high-ability workers. Second, the correlation between earnings ability and learning ability might have important implications for assortative matching on the labor market.

To allow worker fixed effects to associate systematically with wage returns from MNE employment, we augment our original wage model with the interaction between the two. Formally, the interaction is between the worker's level fixed effect α_i, and the external and internal employment experience terms h^c_{it} and t^c_{ijt}. The coefficients for the interactions are λ^α_c and β^α_c. We also account for a full set of interactions between within-firm experience, the number of jobs a worker has held, and the worker fixed effects. The resulting equation is

$$\log(w_{ijt}) = \psi_j + \alpha_i + \gamma_t + \sum_{c\in C}(\lambda_c + \lambda^\alpha_c \alpha_i)h^c_{it} + \sum_{c\in C}(\beta_c + \beta^\alpha_c \alpha_i)t^c_{ijt} + \mathbf{k}'_{ijt}\nu + \eta_{ijt}, \tag{3.14}$$

where the definitions of eq. (3.13) apply, vector $\mathbf{k}_{ijt}$ contains the control variables, adjusted for interactions with α_i, and η_{ijt} is an error term. Positive coefficient estimates for λ^α_c and β^α_c indicate that workers with higher level fixed effects (α_i) reap larger returns from employment in firms of type c compared to workers with lower level fixed effects.

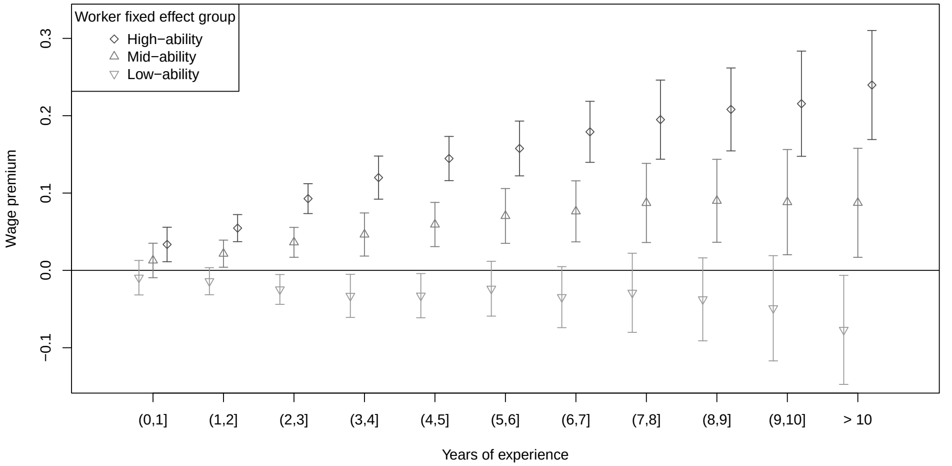
It is unfeasible to estimate the equation with worker fixed effect interactions (eq. (3.14)) directly. We follow De la Roca and Puga (2017) by employing an iterative method where the worker fixed effects from eq. (3.13) provide the initial estimate for α_i. These estimates are then updated in eq. (3.14) until convergence is reached up to an error margin of 10^{-4}.

To visualize the regression results, Figure 3.4 illustrates the wage premia of MNE (over domestic) experience for typical workers of high, medium and low ability, classified as the 75th, 50th, and 25th percentile of the fixed effects distribution.[8] Panel (a) displays wage returns to MNE experience for workers who moved to a different firm. High-ability workers gain the highest returns on previous MNE experience, reaching a wage premium of up to 26% over domestic firm experience. Medium-ability workers also receive a wage premium when changing employers, but this premium is smaller and increases less with experience. For low-ability workers, there is no significant advantage from MNE experience over domestic firm experience when changing employers. The patterns for intra-firm experience are similar. However, low-ability workers earn a significant negative wage premium during their first six years at an MNE.

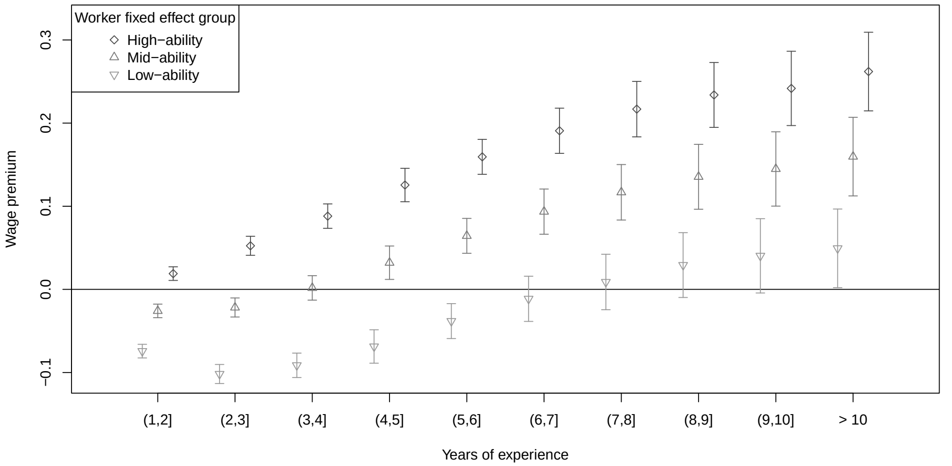
We report the complete set of coefficient estimates in Table 3.12 in Appendix 3.F. The interaction between worker fixed effect and multinational experience is positive and significant across all time frames, for both experience internal and external to the firm. The coefficient estimates increase invariably with the years of experience. Hence, workers with higher fixed effects realize higher wage returns from a year of experience in a multinational, compared to a year in a domestic firm, as illustrated in Figure 3.4.

[8]Figure 3.9 in Appendix 3.C illustrates the estimates for other international (non-multinational) firms. Notably, that group of firms shows no heterogeneity in returns.

Figure 3.4: Wage premia of MNE experience by worker ability.



(a) Across-firm



(b) Within-firm

Notes: The plots depict calculated wage premia per worker fixed effect group and their 95%-confidence intervals, as estimated by a wage regression with interactions between MNE/international firm/domestic firm experience and the worker fixed effects (see eq. (3.14)). The full regression results are in Table 3.12 in Appendix 3.F.2. Wage premia are calculated as the coefficients for MNE experience minus the respective coefficients for domestic firm experience. Experience within and across firms are based on actual days worked and cut in yearly splines. High-ability workers are in the 75th percentile and low-ability workers in the 25th percentile of the worker fixed effects distribution. Standard errors are block-bootstrapped at the worker level (re-estimating worker fixed effects until convergence in all 100 iterations).

3

3.4.4 Evidence from mass layoffs

A concern with the main result may be that workers' mobility towards MNEs may be endogenous. Our estimating equations (3.13) and (3.14) account for the sorting of workers on their own fixed effects, the firm fixed effects, and their past experience at different types of firms, thus limiting the scope for endogeneity arising from worker sorting. However, there may be unobserved time-varying factors that drive worker mobility. Workers might self-select into moving up the job ladder towards higher wage establishment over time, for example as workers' learn about new outside job opportunities (Woodcock, 2008); respond to idiosyncratic labor demand shocks (Helwege, 1992); or learn about their ability or match quality (Gibbons et al., 2005; Menzio and Shi, 2011). To assess the potential impact of such endogenous mobility on our estimates, we follow the literature and exploit a sample of workers that is involved in firm closures and mass layoffs. Such layoffs generate more exogenous variation in worker mobility, if displaced workers accept any job offer that is preferable to unemployment (see e.g., Dauth et al., 2021; Di Addario et al., 2023; Huttunen et al., 2018).

We focus our analysis on workers that were involved in a mass layoff of a firm with at least 10 employees. We identify a mass layoff when 80% or more of workers exit the firm in a given year, or the firm ceases to exist. We additionally require that less than 30% of the exiting workers enter the same new firm, to avoid classifying changes in the identifier of the same firm as mass layoffs (Benedetto et al., 2007). Then we focus on the observations of workers in their first job after the mass layoff, when worker-firm mobility is caused by the plausibly exogenous layoff event, rather than self-selection of workers into mobility. Table 3.9 in Appendix 3.D shows that around 2800 (4600) MNE (international firm) workers in the data are displaced by a mass layoff, while around 6000 workers are involved in a mass layoff of a domestic firm. As this is a small group of workers compared to the around 1 million workers in our sample, we estimate eq. (3.13) and eq. (3.14) with experience profiles that are linear over time. We follow Mion et al. (2022) in adding linear profiles for the respective estimated firm and worker fixed effects, in order to control for their effect on forming wage at the next employer after a mass layoff.

Figure 3.10 in Appendix 3.D shows the distribution of worker fixed effects in mass layoffs. Panel (a) compares the distribution of displaced workers (in all firms) against those of all other workers that are not involved in a mass layoff. The comparison shows that displayed workers exhibit around 6% lower fixed effects on average than non-displaced workers, suggesting that the two groups only differ slightly in their time-invariant ability to earn high wages. Panels (b) and (c) split the distribution of displaced workers up by the multinational status of the origin and destination firm. We find no diverging fixed effects between employees of MNEs and domestic firms in both the group of exiting workers and the group of workers that are hired after a mass layoff.

The results in the sample of workers involved in layoffs are summarized in Table 3.1.[9] Column 1 shows the within- and across-firm returns to experience across all worker classes, as in eq. (3.13). Column 2 allows for heterogeneity across the worker fixed effects. The results are similar to those in Figures 3.1 and 3.4, which use the full set of workers and allow for dynamic rather than linear wage profiles. The estimates in Column 1 suggest that for each year of previous MNE experience, a worker accumulates about 0.74% ($= exp(0.0469 - 0.0395)$; $t(12192) = 8.79$, $p < 0.001$) more wage than when that experience had been acquired in a domestic firm. Within the next employers, wage growth is higher by 0.74% per year spent in an MNE, relative to a domestic firm. The estimates in Column 2 of Table 3.1 show that the wage benefits of MNE experience are higher for workers with higher fixed effects, consistent with the result in Section 3.4.3.

[9]The full regression results are in Table 3.10 in Appendix 3.D.

Table 3.1: Evidence from mass layoffs (short table).

	log(hourly wage) (detrended)	
	(1)	(2)
Domestic firm experience	0.0395***	0.0523***
	(0.0007)	(0.0033)
International firm experience	0.0445***	0.0602***
	(0.0007)	(0.0032)
MNE experience	0.0469***	0.0825***
	(0.0007)	(0.0027)
Years in firm	0.0496***	0.0477***
	(0.0010)	(0.0053)
Years in firm × International firm	0.0021*	0.0082
	(0.0010)	(0.0051)
Years in firm × MNE	0.0074***	0.0285***
	(0.0010)	(0.0055)
<u>Worker fe interactions</u>		
Domestic firm experience × Worker fe		0.0389***
		(0.0057)
International firm experience × Worker fe		0.0438***
		(0.0056)
MNE experience × Worker fe		0.0858***
		(0.0046)
Years in firm × Worker fe		0.0151
		(0.0102)
Years in firm × International firm × Worker fe		0.0042
		(0.0097)
Years in firm × MNE × Worker fe		0.0393***
		(0.0102)
<u>Other variables</u>		
Controls	✓	✓
Worker fe	✓	✓
Firm fe	✓	✓
Observations	42,558	42,558
R^2	0.8067	0.8192

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. The full estimation results are in Table 3.10 in Appendix 3.D. The dependent variable is a workers' log hourly wage, detrended by industry-year fixed effects on the full firm-worker network. The estimations focus on the observations of workers at their first employer, after the worker was involved in a mass layoff; see Section 3.4.4. 'Years in firm' refers to experience accumulated while a worker is employed at the current employer of type MNE, international firm and domestic firm (reference category). MNE/international firm/domestic firm experience refers to experience accumulated before entering the current employer. Experience is calculated based on actual days worked. Column 1 adds the worker and firm fixed effects of an estimation of eq. (3.13) as linear regressors. Column 2 adds those of eq. (3.14). Standard errors in Column 1 are clustered at the worker level. Standard errors in Column 2 are block-bootstrapped at the worker level (re-estimating worker fixed effects until convergence in all 100 iterations).

3.5 Firm strategy and the labor market

In this section, we present stylised facts on the impact of MNE experience on labor market outcomes, guided by the predictions of the theoretical framework in Section 3.2. First, we leverage our empirical wage model from Section 3.4 to predict the typical wage trajectory of a worker starting his or her career in an MNE as opposed to a domestic firm. Second, we estimate a worker's probability to be employed at an MNE as the worker gains experience on the labor market. Third, we provide additional empirical findings that distinguish between the hiring of workers by MNEs based on earlier work experiences and the hiring based on innate abilities.

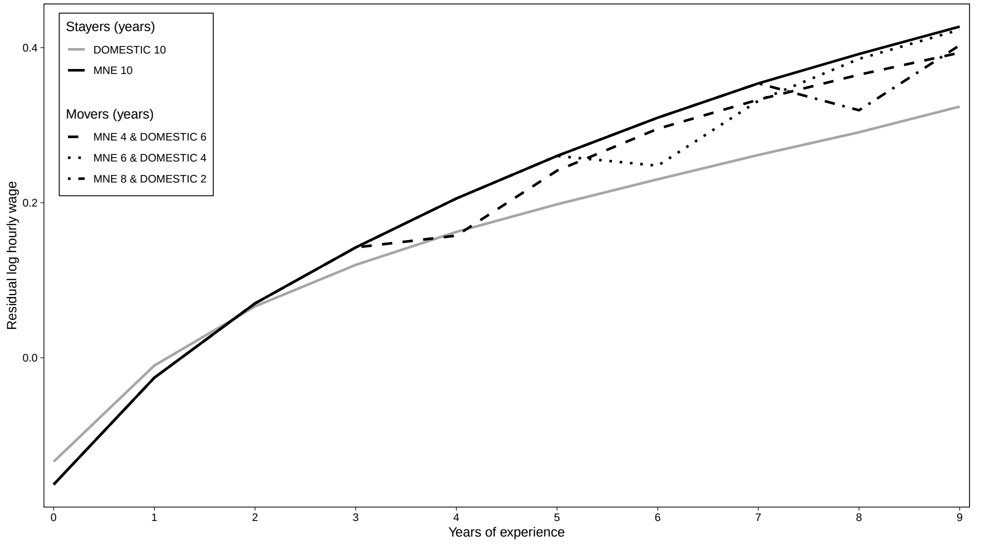
3.5.1 Career profiles

Workers with different employment experience earn different wages as their career progresses. We interpret our estimates by constructing the model's predicted wage over different career paths. We compare typical workers that switch firms at different career stages, using average MNE and average domestic firm characteristics.

Figure 3.5 illustrates the predictions, with the y-axis showing the predicted log hourly wage as experience accumulates over a ten-year period from the point of entry into the labor market. The wage paths are net of industry-year fixed effects, firm size, and worker fixed effects. The solid gray line indicates that a career in a domestic firm starts with a higher wage than a career that starts in an MNE (solid black line). However, as experience years accumulate, the wage level in the MNE rises faster, and in three years, the wage of a worker in an MNE surpasses the wage of a similar worker in a domestic firm.

The Figure shows that experience is portable between domestic firms and MNEs. In careers that start in an MNE but switch to a domestic firm (dotted lines), workers generally experience a drop in wage after the move. The drop may reduce the wage of a worker with MNE experience below that of a worker with experience in the domestic firm at the time of the move (for MNE 4, domestic 6 years), but the MNE experience leads to higher wages over the domestic experience in the subsequent years. An earlier move to a domestic firm leads to lower wages in the final years, as relatively more experience is domestic (and the first years of experience in a domestic firm lead to stronger increases in wage than later years of experience in the domestic firm).

Figure 3.5: Wage developments of different career paths.



Notes: The figure shows the predicted wage paths of a worker up from labor market entry and for different career paths. Predictions are based on eq. (3.13), while differencing out the effect of firm size and the worker fixed effect on log hourly wages. The average fixed effects of MNE and domestic firms are worker-population averages, see the estimates in Table 3.6 in Appendix 3.A.

The career profiles show that for workers with no experience, the domestic wages are above multinational wages. This is consistent with the theoretical framework in Section 3.2, in which multinationals effectively pay junior workers in deferred pay later in their career.

We test whether initial wages are significantly lower in MNEs more formally. We employ a least squares regression, weighted by the employment size of the firm, to explain firm fixed effect estimates from MNE status. As the estimated firm fixed effect is conditional on experience values of the workers and their fixed effects, it reflects the expected wage for a worker who enters the firm with no experience. As the firm fixed effects are time-invariant, we focus on MNEs and domestic firms that do not change status throughout the sample period.

The results are reported in Table 3.6 of Appendix 3.A. The fixed wage premium of MNE careers net of experience is around 3% below that of domestic careers. The difference is statistically significant under standard errors that are block-bootstrapped at the worker level. This holds both for the weighted fixed effects of our main dynamic model (see eq. (3.13)) and the model with worker ability interactions (see eq. (3.14)).

3.5.2 Selection within the multinational

In our theoretical framework in Section 3.2, the value of experience in MNEs forms an incentive to hire relatively more junior workers. As junior workers accept lower wages in exchange for higher later wages, they are cheap relative to their productivity, and the MNE hires more of them.

To examine whether junior workers are more likely to be employed by MNEs than senior workers, we explain the employment in an MNE ($MNE_{ijt} = 1$) from the worker's time since labor market entry. The regression equation is

$$MNE_{ijt} = \sigma_i + \psi_j + \gamma_t + \mu l_{it} + \mathbf{q}'_{ijt}\nu + u_{ijt}, \tag{3.15}$$

where again i, j, j index workers, firms and years; σ_i is a worker fixed effect; ψ_j is a firm fixed effect; γ_t is an industry-year fixed effect; vector $\mathbf{q}_{ijt}$ includes the controls log firm size and number of jobs; and u_{ijt} is an error term. Importantly, l_{it} is a set of dummies that reflect the number of years since labor market entry, with the year of entry as the reference category.

The coefficients for l_{it}, μ, capture the likelihood of observing a worker employed in an MNE at each year of their career conditional on the worker, firm and industry-year fixed effects. As such they measure how workers sort into MNE employment as they gain experience (high l_{it}), relative to their likelihood of MNE employment at the start of their career.

Panel (a) of Figure 3.6 plots the estimates for worker sorting into MNE employment.[10] The y-axis counts years since labor market entry and the x-axis shows the difference in the probability of a worker to be employed at an MNE relative to the entry year. MNEs employ relatively more inexperienced workers than experienced workers. The top coefficient implies that more than ten years after entry, workers are about 0.7 percentage points less likely to be employed at an MNE than right at labor market entry. As 31.2% of entrants start at an MNE, the coefficient suggests that the odds of MNE employment decrease by about 3.2%, reflecting a small sorting effect of workers out of MNE employment as they gain experience. The remaining coefficients show that sorting out of MNE employment is particularly relevant after six years of labor market experience when a trend break is visible. A Wald test confirms a statistically significant difference between the coefficients for seven and more years and less than seven years on the labor market ($\chi^2(1)$ = 5.2, p $<$ 0.05).

In our framework, MNEs are particularly selective for senior positions. Accordingly, we test whether workers with lower time-invariant abilities are less likely to be employed at later stages of their career than workers with higher time-invariant abilities. We augment the linear probability model in eq. (3.15) with interactions between ability (the worker fixed effect estimate of a wage equation) and years since labor market entry,

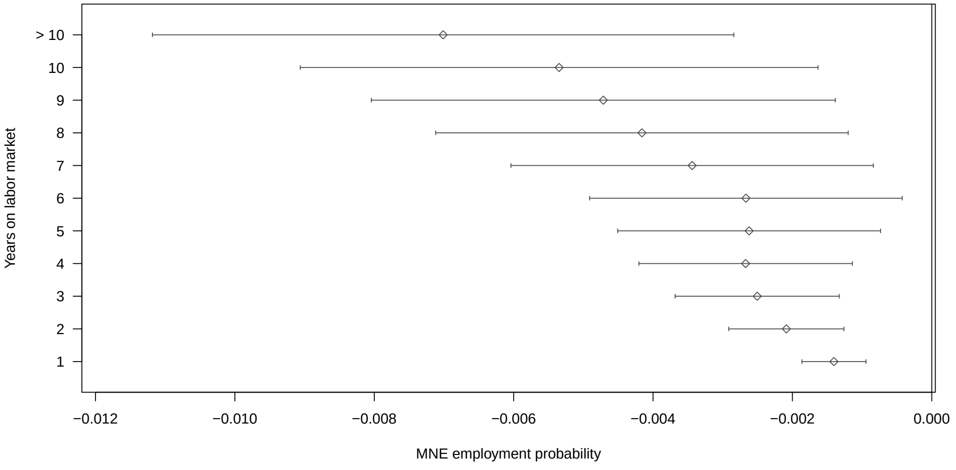
$$MNE_{ijt} = \sigma_i + \psi_j + \gamma_t + (\mu + \mu^{\alpha}\alpha_i)l_{it} + \mathbf{x_{ijt}}\nu + v_{ijt}. \tag{3.16}$$

where $\mu^{\alpha} \neq 0$ allows the impact of work experience l_{it} on MNE employment to vary with the worker's fixed effect estimate α_i (measured by the worker fixed effects in eq. (3.14)), and v_{ijt} is the error term.

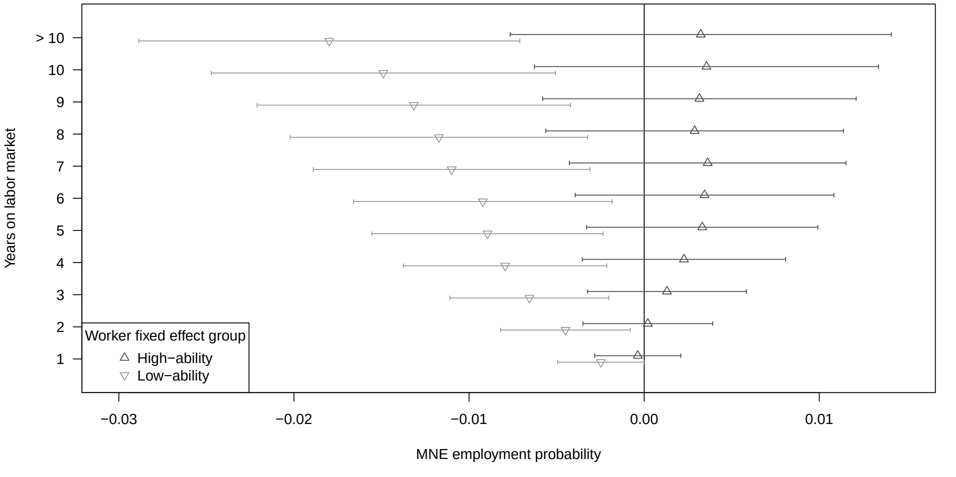
In Panel (b) of Figure 3.6, we show the impact of career progression on MNE employment for two distinct groups of worker ability. Downward facing triangles depict the sorting of low-ability workers (in the 25th percentile of the worker fixed effect distribution) and upward facing triangles those of high-ability workers (in

[10]The full regression results are in Table 3.13 in Appendix 3.F.

Figure 3.6: Sorting of experienced and inexperienced workers to MNEs.



(a) Overall



(b) By worker ability

Notes: The plots depict point estimates and their 95%-confidence intervals of labor market sorting relative to the year of labor market entry. Estimates in Panel (a) are based on a linear probability model with an MNE employment dummy as the dependent variable and are conditional on number of jobs, log firm size; and worker, firm and industry-year fixed effects (see eq. (3.15)). Panel (b) splits the estimates up by the worker fixed effects of a wage regression (see eq. (3.16) and the discussion in Section 3.5.2). High-ability workers are in the 75th percentile and low-ability workers in the 25th percentile of the worker fixed effects distribution. The full regression results are in Appendix 3.F.3. Standard errors in Panel (a) are clustered at the worker level. Standard errors in Panel (b) are block-bootstrapped at the worker level (re-estimating worker fixed effects until convergence in all 100 iterations).

the 75th percentile). The estimates for high-ability workers are mostly positive but not significantly different, suggesting they do not develop different likelihoods of working for an MNE over their career. By contrast, the estimates for low-ability workers are significantly negative and they decrease as the worker's labor market experience grows. Hence, low-ability workers are increasingly less likely to work for a multinational as their career progresses. As 28.5% of low-ability workers start their career in an MNE, the odds that a low-ability worker is in MNE employment ten or more years after labor market entry are about 7.8% lower than at labor market entry. Together, this indicates that the reduction in the number of senior workers in MNEs is explained by the lower employment by MNEs of low-ability workers at later career stages.

The full set of estimates for equation (3.16) is in Table 3.14 in Appendix 3.F.3. They show positive and statistically significant interaction effects between worker ability and labor market experience for all career stages. Moreover, the estimates increase with labor market experience.

3.5.3 Experience and hiring by multinationals

Our results suggest that the productivity advantages of MNEs derive from the employment histories of their workforce. Intuitively, MNEs may be more likely to hire a worker with valuable experience, over and above the innate ability of that worker. To examine this possibility, we analyze the moving workers in our data, and explain whether the move is to an MNE from the worker's earlier experience and the worker's fixed effect estimate. We control for industry-year fixed effects, to prevent an omitted variables bias from within-industry moves in industries with many multinationals.

Table 3.2 shows the regression results for observations of moving workers. The experience variables capture the years of experience in multinational, international and domestic firms prior to the move. For comparability of the estimates, Column 3 uses standardized measures of experience and ability (standardized to mean zero and variance one).

Column 3 of Table 3.2 shows that a one standard deviation increase in a worker's previous experience in an MNE increases the probability of moving to an MNE by 2.75%-points. This represents a considerable effect with a 12.6% increase in the odds of observing a (senior) job mover entering an MNE, relative to the mean of 35% of entries to MNEs. Previous experience in international and domestic firms have a negative effect. A one standard deviation increase in ability increases the probability by only 1.52%-points, or the odds by 6.8%. The standardized effect of a year of MNE employment is considerably larger than that of the worker fixed effect, which suggest that earlier MNE experience matters most in explaining mobility towards MNEs. The results are not explained by the industry and size composition of MNEs as the specification includes an industry-year fixed effect and controls for log firm size.

Table 3.2: Sorting of experienced workers to MNE entry positions.

	Entry to MNE		
	(1)	(2)	(3) standardized
MNE exp.	0.0232***	0.0178***	0.0275***
	(0.0003)	(0.0003)	(0.0004)
Int. firm exp.	-0.0112***	-0.0099***	-0.0143***
	(0.0003)	(0.0002)	(0.0004)
Dom. firm exp.	-0.0191***	-0.0063***	-0.0091***
	(0.0003)	(0.0003)	(0.0004)
Worker ability (standardized)	0.0334***	0.0152***	0.0152***
	(0.0006)	(0.0005)	(0.0005)
log(firm size)		0.0884***	0.0884***
		(0.0002)	(0.0002)
Fixed-effects			
Industry-year (324)	✓	✓	✓
Observations	1,271,658	1,271,658	1,271,658
R^2	0.2378	0.4306	0.4306

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Estimated on a sample of entry observations at new employers after the first employer. The dependent variable is a binary indicator that identifies a worker's entry into an MNE. MNE/international firm/domestic firm experience capture the years of experience a worker collected before entering the current employer. 'Worker ability' refers to the worker fixed effects of an estimation of eq. (3.14). MNE/international firm/domestic firm experience (Column 3) and worker ability (all columns) are standardized to mean zero and variance 1. Standard errors are block-bootstrapped at the worker level (re-estimating worker fixed effects until convergence in all 100 iterations).

3.6 Robustness checks

We estimate several variations of eq. (3.13) to explore the sensitivity of our main result to alternative explanations and our modeling choice. We report the results of the checks in Figures 3.7 and 3.8. Different model specifications provide varying estimates of the returns to MNE experience, yet a consistent trend emerges: MNE employment experience yields wage premia over domestic firm experience, both when workers continue at the MNE and when workers switch firms, including to non-MNEs.

1. **Base Salary.** We focus only on the hourly wage rate that follows from a workers' base income, excluding the impact of overtime and bonus payments. This approach arises from the understanding that MNEs may compensate overtime differently than domestic firms and provide substantial bonuses to certain employees (Vahter and Masso, 2019). Likewise, a worker's past MNE experience could be rewarded with bonus payments when joining a new employer. As our theoretical framework abstracts from such explanations, we verify that our results also hold for a worker's base hourly wage.

2. **Excluding acquisitions.** A concern could be that international acquirers 'cherry pick' targets with high wage growth for acquisition (Almeida, 2007). Then, wage effects might reflect target selection rather than the value of employment experience. We re-classify MNEs as "always an MNE" if it is observed as an MNE for at least three quarters of its years in the panel (even for the years in which the firm is not an MNE). MNEs that do not fulfill this requirement are consistently identified as either international or domestic firms. With this classification, acquisitions should have little to no impact on the wage equation.

3. **Large firms.** Since MNEs are generally larger than domestic firms, our estimates could capture wage premia due to firm size (Bloom et al., 2018), rather than the multinational status of firms, potentially bypassing our firm-size control variable. We restrict our focus to a subsample of large firms with at least 250 employees in a given year.

4. **Discounting.** Our estimates do not distinguish between the age of the experience on a worker's CV, while the value of experience may depreciate. Hence, the diminishing returns in Figure 3.1 could be an outcome of this modeling choice.

To examine this possibility, we introduce a worker-year-specific discount factor to eq. (3.13), which restricts experience gained T+1 years ago to contribute less to identifying the estimate, compared to experience obtained T years ago. We identify the best fit for the discount factor by estimating our model under different discount factors and selecting the one that results in the lowest root mean squared error. We find that a discount rate of around 1% per year fits the data slightly better than no discount rate. Adjusting our wage equation for this discount factor leads to very little change in our baseline results.

5. **MNE vs. industry experience.** Previous research has underscored the critical role of industry-specific human capital as a determinant of wage (e.g., Sullivan, 2010). If this industry-specific human capital proves to be integral in explaining worker mobility and wage structure, its omission could lead us to conflate wage accumulation within MNEs (and other firms) with wage growth due to industry-specific experience. In addition, MNEs are more present in some industries than in others (see e.g., Roesch et al., 2022). If MNEs are concentrated in industries with high values of experience generally, i.e. regardless of whether industry experience is gained in an MNE or a domestic firm, we could confuse a workers' past MNE experience with experience gained in those industries.

   To address these two possibilities, we verify that our results are robust to including two types of controls. First, we add a variable that captures a workers' past experience within the same 2-digit NACE industry as the current employer. Second, we add an additional variable that captures the employment-weighted share of MNEs in the 2-digit NACE industries that a worker has worked for in the past. Neither impacts our main result.

6. **Bargaining.** While our approach accounts for wage growth based on the internationalisation status of a workers' current and previous firms, internationalisation may correlate with other factors that determine a workers' bargaining position, such as the size of the previous employer and the wage that the worker earned there. We control for a workers' lagged (log) wage and

lagged (log) employer size, following the argumentation in Mion et al. (2022). Bonhomme et al. (2019) propose that a worker's previous wage and the identity of his or her previous employer capture complex wage negotiation structures, suggesting that lower past wage determines higher wage growth. Similarly, on-the-job search models like that of Postel-Vinay and Robin (2002), highlight the importance of both the present and potential employers in determining worker mobility and wages. In these models, the more productive a worker's firm is, the higher is the worker's wage growth, irrespective of whether the worker changes firms. In contrast, Di Addario et al. (2023) find that empirically the identity of a worker's previous employer is unimportant in determining wage at the next employer for a sample of Italian firms.

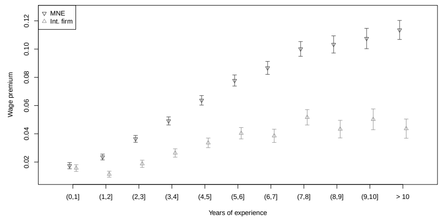
7. **Firm-year fixed effects.** We substitute the firm fixed effects with firm-year fixed effects to capture changes in a firm's wage policy over time, such as productivity shocks that are both firm-specific and time-varying, potentially driving the selection of firms into MNE status (Engbom et al., 2023; Roesch et al., 2022). With the inclusion of firm-year fixed effects, the coefficients on the within- and across-returns to MNE experience are identified by the variation in wage among workers with different experience levels within the same firm in the same year. In addition, they are identified by within-worker variation of employment experience and wage.

8. **Across-firm returns by firm type.** We examine the interaction between across-firm returns to experience and the firm type of a worker's current employer. Our empirical framework suggests that workers accrue benefits from past experience at an MNE as compared to experience at a domestic firm, irrespective of their subsequent post-MNE employment choices. However, it could be that only other MNEs value MNE experience, implying that workers transitioning from an MNE to a domestic firm might not receive any wage premium. To comprehensively assess whether workers generally benefit from MNE experience, we split the effect of the MNE experience premia accumulated at past employers up based on the firm type of the worker's current employer. The estimates show positive

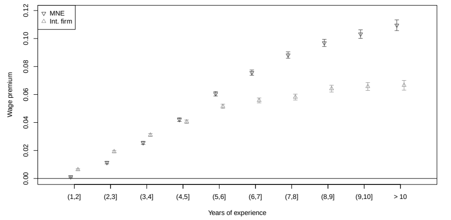
wage premia regardless of the MNE status of the worker's current employer.

9. **Spell fixed effects.** Selection into senior positions may be a function of the match quality between the firm and the worker. A concern could be that the match-specific quality also accounts for wage differences between domestic firms and MNEs, in which case within-firm experience might conflate with match quality. To examine this, we introduce firm-worker-spell fixed effects to examine wage variation within the employment duration of each contract. This leads to very minor shifts in the estimates of MNE premia in the within-firm returns to experience. As the spell fixed effects fully control for the worker's external employment history, the experience value across firms cannot be identified with spell fixed effects.

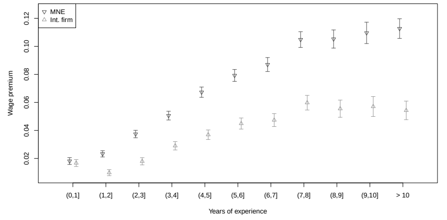
Figure 3.7: The wage premia of MNE experience under different regression specifications (1).



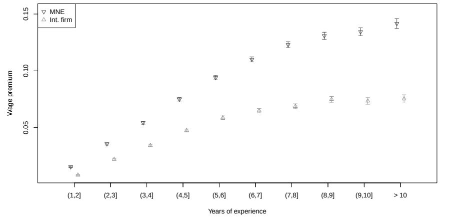
(a) Base salary: across-firm



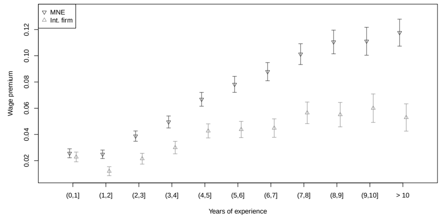
(b) Base salary: within-firm



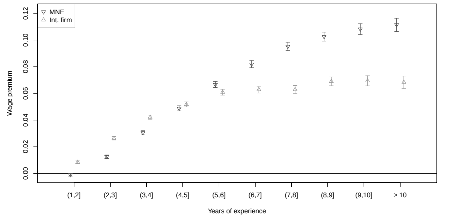
(c) Excluding acquisitions: across-firm



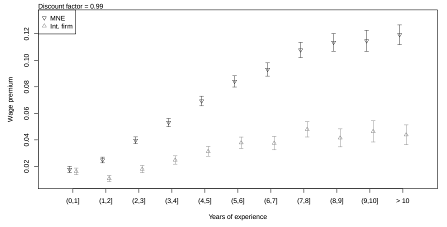
(d) Excluding acquisitions: within-firm



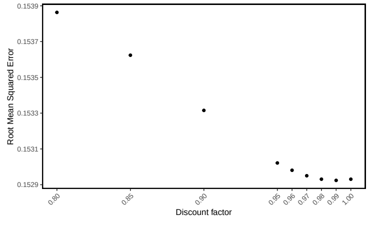
(e) Large firms ($\geq$ 250 workers): across-firm



(f) Large firms ($\geq$ 250 workers): within-firm



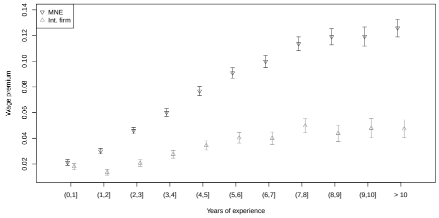
(g) Discounting experience: across-firm



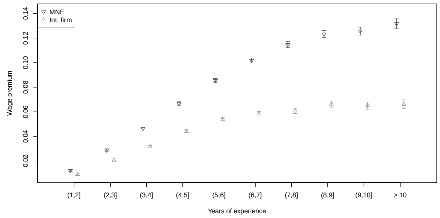
(h) Discounting experience: RMSE

Notes: The figure shows the estimated wage return to MNE experience under different variations of eq. (3.13); see Section 3.6 for details. The returns are split up by within-firm (continuing workers) and across-firm (moving workers) returns. Panel (h) shows the Root Mean Squared Error (RMSE) derived when applying different discount factors to the across-firm returns. Panel (g) shows the corresponding model with the lowest RMSE.

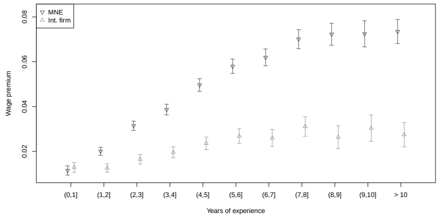
Figure 3.8: The wage premia of MNE experience under different regression specifications (2).



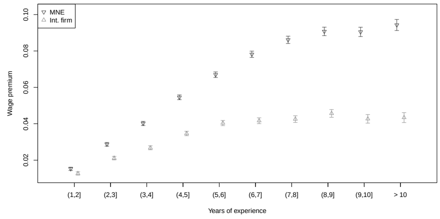
(a) Industry exp. control: across-firm



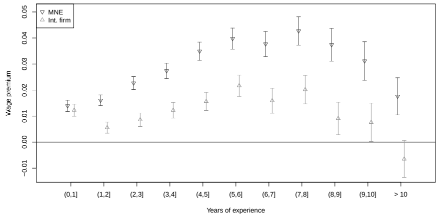
(b) Industry exp. control: within-firm



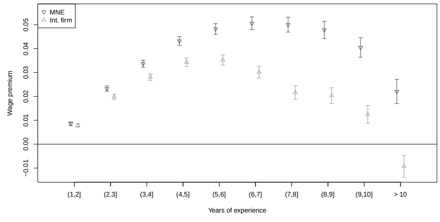
(c) Bargaining power controls: across-firm



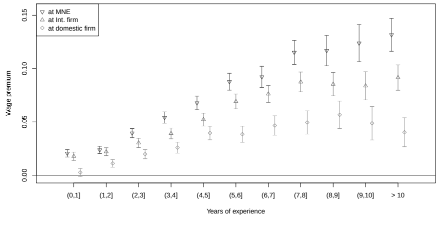
(d) Bargaining power controls: within-firm



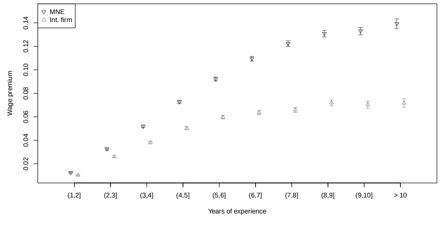
(e) Firm-year fixed effects: across-firm



(f) Firm-year fixed effects: within-firm



(g) By firm type: across-firm



(h) Spell fixed effects: within-firm

Notes: The figure shows the estimated wage return to MNE experience under different variations of eq. (3.13); see Section 3.6 for details. The returns are split up by within-firm (continuing workers) and across-firm (moving workers) returns.

3.6.1 Limited mobility bias

If few workers move between firms, the firm fixed effects in our data are identified from few worker movements, and a "limited mobility bias" might arise. While the level estimates of the fixed effects are generally unbiased under limited mobility, the "plug-in" estimator of their variance may yield biased estimates (see e.g., Andrews et al., 2008; Bonhomme et al., 2023; Jochmans and Weidner, 2019). This means that the OLS estimates of our wage profiles are unaffected by limited mobility concerns, as are the level estimates of the firm fixed effects that we use in computing the MNE wage premium in Section 3.4.1.[11] However, when comparing the distributions of firm fixed effects, we rely on the variance of the fixed effects which may be biased. In this subsection, we investigate whether limited mobility bias affects our conclusion that the value of employment experience drives the MNE wage premium.

To assess the influence of limited mobility, we employ a clustering approach to the firm fixed effects (Bonhomme et al., 2019). Specifically, we employ a K-means clustering algorithm to identify $K = 10, 20, 50$ clusters of firms, using percentile cutoffs of the within-firm wage distribution after demeaning log hourly wages by industry-year fixed effects. Same as in Section 3.4.1, we re-estimate eq. (3.13) with and without experience profiles included, but with cluster-level instead of firm-level fixed effects. Clustering addresses the limited mobility bias, since many movers identify a single cluster-level fixed effect (Bonhomme et al., 2019). Column 2 of Table 3.3 presents Jochmans and Weidner (2019)'s network connectivity measure, which indicates how susceptible the network is to limited mobility. The network with firm-level fixed effects features low connectivity, while cluster-level networks exhibit up to 133 times higher connectivity. This suggests that clustering effectively counteracts any biases stemming from limited mobility.

We extract the cluster-level fixed effect estimates to compute the MNE wage premium in both the static (without experience) and dynamic (with experience)

[11] Appendix 3.E includes Figure 3.11, which depicts our estimates for the wage returns to MNE experience when using $K = 10, 20, 50$ clusters for the firm fixed effects (Bonhomme et al., 2019). These estimates align with our main results in Figure 3.1, implying that limited mobility does not influence our findings on the wage premia of MNE experience.

model. To determine the MNE wage premium, we follow Setzler and Tintelnot (2021) in calculating the difference in the average cluster-level fixed effect experienced by MNE and domestic workers. A limitation of the clustering method is that it precludes the direct computation of standard errors and p-values for the MNE wage premium. To estimate the standard error, we apply a block-bootstrap approach over 100 iterations, which involves randomly sampling workers' entire employment histories with replacement and re-estimating all components, including the MNE wage premium.

Table 3.3 compiles the derived MNE wage premia under various clustering strategies. For reference, the table's bottom panel also provides the MNE wage premium calculated using firm-level fixed effects without clustering. The estimates fluctuate with the number of clusters and between the static and dynamic models. However, across all configurations, the static estimate is positive with smaller standard errors, whereas the MNE wage premium in the dynamic specification is near-zero with broad standard errors. This confirms our conclusion that considering the dynamic wage benefits of MNE experience diminishes the MNE wage premium, even after correcting for the limited mobility of workers.

Table 3.3: T-tests on MNE wage premium under different k-means clustering approaches.

Clusters	Network Connectivity	Specification	MNE premium	s.e. (bootstrapped)	p-value
10	0.6978	Static model	0.0662	0.0017	0.0160
10	0.6978	Dynamic model	0.0111	0.0021	0.1214
20	0.8083	Static model	0.0801	0.0081	0.0639
20	0.8083	Dynamic model	0.0232	0.0099	0.2561
50	0.8640	Static model	0.1007	0.0076	0.0478
50	0.8640	Dynamic model	0.0426	0.0103	0.1507
max	0.0065	Static model	0.0709	0.0010	0.0089
max	0.0065	Dynamic model	-0.0378	0.0017	0.0285

Notes: The table shows t-tests on the (weighted) average difference in fixed effects of MNEs and domestic firms. The fixed effects derive from an estimation of eq. (3.13) with $K = 10, 20, 50$ clusters of firm fixed effects. Clusters are found using a K-means clustering algorithm on the within-firm distribution of (detrended) hourly wages (Bonhomme et al., 2019), and by picking 20 random initial assignments. 'Clusters = max' are the fixed effects of a model without clustering. 'Network connectivity' refers to Jochmans and Weidner (2019)'s limited mobility indicator. 'MNE premium' shows the weighted average difference in (cluster) firm fixed effects between MNEs domestic firms, with weights according to observed worker-firm matches in the data. 's.e. bootstrapped' shows block-bootstrapped standard errors (worker-level). The bootstrap randomly samples with replacement the full employment histories of workers across 100 iterations.

3.7 Conclusion

We examine how employment in a multinational adds value to workers' CVs. While multinationals are known to be more productive and pay higher wages than domestic firms, little is known about the impact of MNE employment on workers' future wages. We show that the future wage returns to MNE experience represent a large share of the returns to working in an MNE.

Tracking workers' careers from labor market entry in the universal matched employer-employee data of the Netherlands for 2006 to 2021, we show that workers accumulate substantial wage premia at MNEs. Specifically, workers employed at MNEs, as opposed to domestic firms, accrue 1 to 14% higher wage growth if they remain in the MNE, with the premium rising in the time spent within the firm. These wage premia are portable both to other MNEs and to non-MNEs. When moving between firms, workers with MNE employment experience earn on average 2 to 11% more, increasing in the previous time spent in an MNE. Our estimation strategy excludes worker sorting, variation in the overall pay level across firms, and industry-year-level shocks from explaining the wage premia. Several checks show that the estimates are robust to wage explanations driven by firm size, industry-specific experience and various incentives to worker mobility. Additionally, we find that the premia persist in a context where variation in employment experience among workers is driven by mass layoffs. The estimates imply a substantial aggregate value of MNE employment: We estimate that the total accumulated value of experience in MNEs in 2021 amounts to around 6% of the total labor income in the Netherlands, relative to a counterfactual situation in which this experience had been accumulated in domestic firms.

Employment experience explains most of the wage premia that multinationals pay. Once we control for experience, the estimates of the level fixed effects for workers, often interpreted as innate ability or skill, are very similar between MNEs and domestic firms. Similarly, the level fixed effects of MNEs are no different from domestic firms, once we account for the experience of the workforce. Hence, our analysis implies that employment experience is a key driver in explaining the higher skill level and wage of the MNE workforce. This finding extends earlier literature, which, lacking workers' full employment histories, predominantly traces the wage premia of multinationals to worker sorting on innate ability rather than experience (e.g., Hijzen et al., 2013; Setzler and Tintelnot, 2021).

We present stylized facts suggesting that MNEs leverage the experience premia that their workers' accumulate. A standard international trade model, augmented with hiring and promotion decisions, suggests that a firm with higher experience value is likely to internationalize as it has low marginal costs. Such an MNE pays junior workers relatively low wages, as workers get compensated in higher wages later on. Accordingly, the MNE hires relatively many junior workers. Learning its junior workers' productivities, the MNE also promotes fewer but more productive junior workers to senior ranks, compared to a domestic firm. Consistent with these predictions, we estimate a 3% wage penalty at career onset for the average multinational career. We also find that MNEs employ relatively few seniors per junior worker. This is explained by the exit of workers with lower innate abilities (as proxied by worker fixed effects) from MNE employment over time, suggesting stronger selection in MNEs.

The stylized facts have more broad implications for theories of firm selection into international trade. These theories predict that firms with high levels of (ex-ante) productivity operate at lower marginal costs and are more likely to overcome the obstacles of internationalization (Felbermayr et al., 2011; Helpman et al., 2010, 2004). In our formalization, multinationals with high experience values for junior workers push down marginal costs by capitalizing on the later wage premia of their workforce, thus opening up opportunities for scale-intensive multinational investment. For the universe of Dutch firms, we find that level productivity estimates, as measured by firm-level fixed effects in Mincer wage equations, are not higher for MNEs than for other firms when we control for the experience of the firm's workforce. Hence, our results suggest that in addition to ex-ante productivity advantages, the careers of the MNE's workers explain the firm's internationalization strategy.

Appendix to Chapter 3

3.A Supporting tables and figures

Table 3.4: Overview of the matched employer-employee dataset.

Firm type	Firms	Workers	Observations
MNE	19,020	517,870	2,721,956
International	61,618	580,370	2,831,073
Domestic	133,357	517,872	2,501,994
	207,074	1,059,991	8,055,023

Notes: The table shows the number of firms, workers and observations split up by three firm types: MNE, international and domestic firms. MNEs comprise foreign firms (ultimate owner located abroad) and domestic firms with foreign subsidiaries. International firms are defined as (non-multinational) firms with an average yearly sum of imports and exports that exceeds 10k EUR. Domestic firms are all remaining firms.

Table 3.5: Summary statistics.

	All workers Mean	All workers SD	2006 cohort Mean	2006 cohort SD
Workers	1,059,991		37,496	
Age at labor market entry	23.84	3.11	23.31	3.12
Number of different employers	2.26	1.34	3.33	1.80
First job at				
MNE (probability)	0.31	0.46	0.28	0.45
International firm (probability)	0.34	0.47	0.34	0.47
Domestic firm (probability)	0.35	0.48	0.38	0.49
Years in firm				
MNE (years)	1.73	2.65	3.30	4.28
International firm (years)	1.92	2.73	3.44	4.17
Domestic firm (years)	1.56	2.45	2.65	3.62
MNE (hourly wage)	21.42	10.35	23.98	13.55
International firm (hourly wage)	21.07	8.26	22.12	12.24
Domestic firm (hourly wage)	19.64	8.42	20.02	9.27
Past experience from				
MNE (years)	0.94	2.00	2.24	3.51
International firm (years)	0.88	1.89	2.04	3.14
Domestic firm (years)	0.87	1.88	2.00	3.15
MNE (hourly wage)	24.80	12.37	28.11	13.86
International firm (hourly wage)	24.74	10.64	27.18	16.41
Domestic firm (hourly wage)	22.92	9.23	24.68	12.65

Notes: The table provides summary statistics for two groups of workers: all workers in the sample and workers who entered the labor market in 2006. 'Years in firm' shows the average maximum number of years that workers have been with their current employer. Past experience shows the average maximum number of years of prior employment experience of workers before joining their current employer. Hourly wages are averages of total reported earnings divided by total reported hours worked. MNEs comprise foreign firms (ultimate owner located abroad) and domestic firms with foreign subsidiaries. International firms are defined as (non-multinational) firms with an average yearly sum of imports and exports that exceeds 10k EUR. Domestic firms are all remaining firms.

3

Table 3.6: The static wage premium of careers in MNEs.

	Firm fixed effect (1)	Firm fixed effect (2)
Intercept	-0.1339***	-0.1193***
	(0.0038)	(0.0032)
MNE-career	-0.0295***	-0.0322***
	(0.0019)	(0.0019)
Observations	144,396	144,396
R^2	0.0038	0.0047

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Weighted Least Squares regressions with weights according to average employment size. The dependent variables are the respective firm fixed effects of two different specifications: (1) including MNE/international firm/domestic firm employment experience (see eq. (3.13)); (2) including full interactions between employment experience and the worker fixed effects (see eq. (3.14)). Bootstrapped standard errors in parentheses (re-estimating firm fixed effects until convergence in all 100 iterations).

3.B Comparing the fixed effects distributions

In this section we develop a formal decomposition of the difference in the (worker and firm) fixed effects distributions between MNEs and domestic firms to complement the discussion in Sections 3.4.1 and 3.4.2. Specifically, we decompose the difference in the firm and worker fixed effects distributions of MNEs and domestic firms into differences in mean and dilation, using the quantile approach of Combes et al. (2012). The approach minimizes the mean quantile difference between the observed fixed effects distribution in MNEs and an approximated distribution, where the approximation is formed by shifting and dilating the observed distribution in domestic firms. For the worker fixed effects and in order to avoid counting workers that move between MNEs and domestic firms more than once, we use a snapshot of the data in 2014 when applying the Combes et al. (2012) method. For the firm fixed effects, we focus on firms that never change status throughout our sample period.[12]

Table 3.7: Comparison of worker fixed effect distributions, MNE vs domestic firm.

	(1)	(2)	(3)	(4)
Shift	0.0842***	0.0418***	0.0591***	-0.00854
	(0.00152)	(0.00197)	(0.00573)	(0.00489)
Dilation	1.069***	1.053***	1.053***	0.900***
	(0.0119)	(0.0121)	(0.0121)	(0.00989)
Observations	241566	241566	241566	241566
R^2	0.776	0.404	0.396	0.599

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Estimates of shift and dilation in the Combes et al. (2012) method applied to the distribution of worker fixed effects. The estimates are based on a snapshot of the data in 2014, and include the fixed effects of workers in MNEs and domestic firms that do not change status. The columns refer to different worker fixed effect estimates described by (1) a static model with only worker fixed effects, firm fixed effects and industry-year fixed effects; (2) a dynamic model with MNE, international and domestic firm experience included (see eq. (3.13)) but excluding the log firm size control; (3) our main dynamic model with MNE, international and domestic firm experience included (see eq. (3.13)); (4) a dynamic model with MNE, international and domestic firm experience, and their interactions with the worker fixed effects included (see eq. (3.14)). Standard errors (in parentheses) are block-bootstrapped at the worker level (re-estimating the fixed effects until convergence in all 100 iterations).

Table 3.7 reports estimates of shift and dilation for the worker fixed effects in different specifications. In Column 1, we first consider the worker fixed effects of a static estimation that ignores the benefits of MNE experience, and includes only

[12]The results are qualitatively similar when using a different snapshot or including firms that change status.

industry-year fixed effects, worker fixed effects and firm fixed effects. The shift parameter is 0.08 and the dilation parameter is greater than one, implying that in this specification worker ability is more dispersed in MNEs than in domestic firms. The statistically significant shift parameter reflects the exclusion of the time-varying benefits of experience. Ignoring them causes the worker fixed effect estimates to not only pick up time-invariant characteristics of workers but also time-variant differences in the accumulation of experience. Columns 2 and 3 adjust the estimates for experience but restricts its returns to be homogeneous across worker ability (as in eq. (3.13)), with the difference being the inclusion of a firm size control. The shift parameter in Column 3 falls to 0.06, while the dilation parameter remains almost unchanged. As discussed in Section 3.4.3, the returns to MNE experience are heterogeneous across workers, whereby the fixed effects underlying Column 3 overestimate ability at the top of the distribution and underestimate ability at the bottom of the distribution. Column 4 addresses these issues and adjusts for heterogeneous time-varying differences in the wage accumulation with experience across different levels of the worker fixed effects (as in eq. (3.14)). The dispersion parameter is just below one, suggesting that worker ability varies less in MNEs. The mean difference estimate is negative, close to zero and statistically insignificant. In summary, the change in shift parameter across Columns 1 to 4 suggests that the worker-level disparities between MNEs and domestic firms arise from faster wage growth with MNE experience and heterogeneous returns to experience for workers of different ability, not from innate ability differences among workers.

Table 3.8 shows the estimates of shift and dilation for the firm fixed effects in the same set of modules. For the firm fixed effects of the static model in Column 1, the shift parameter is 0.08, implying that MNEs have higher average fixed effects than domestic firms. Column 2 adds experience to the estimation but ignores the effect of firm size on wages. The shift parameter turns negative to -0.06, implying that the fixed effects of MNEs are lower than those of domestic firms. Column 3 additionally adds a control for firm size and Column 4 allows the effect of experience on wage to vary by the worker fixed effects. In both cases, the shift parameter is negative. The estimates for the dilation parameters are consistently smaller than one, implying that the fixed effects of MNEs vary less than those of domestic firms. All estimates are statistically significant. Taken together, the change in shift parameter implies that the ex-ante wage premium of MNEs (Column 1) is fully explained by the accumulation of experience in the workforce of MNEs (Columns 2 to 4).

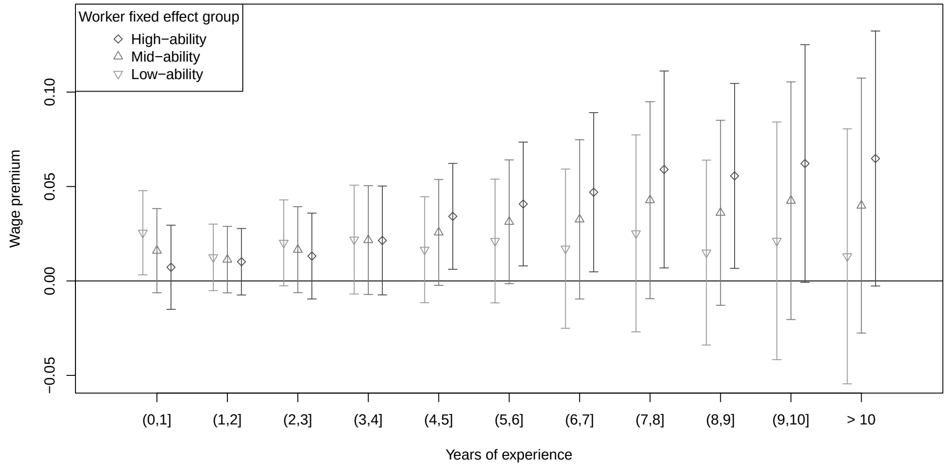
Table 3.8: Comparison of firm fixed effect distributions, MNE vs domestic firm.

	(1)	(2)	(3)	(4)
Shift	0.0829***	-0.0630***	-0.0160***	-0.0134***
	(0.00473)	(0.00540)	(0.00193)	(0.00234)
Dilation	0.792***	0.673***	0.684***	0.674***
	(0.0159)	(0.0141)	(0.0145)	(0.0151)
Observations	144396	144396	144396	144396
R^2	0.775	0.722	0.636	0.642

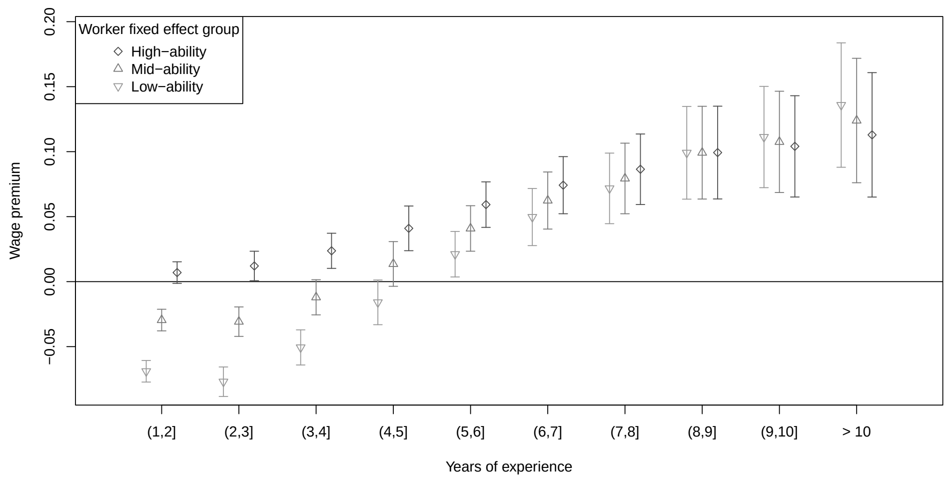
Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Estimates of shift and dilation in the Combes et al. (2012) method applied to the distribution of firm fixed effects. The estimates include the fixed effects of MNEs and domestic firms that do not change status. The firm fixed effects are derived from (1) a static model with only worker fixed effects, firm fixed effects and industry-year fixed effects; (2) a dynamic model with MNE, international and domestic firm experience included (see eq. (3.13)) but excluding the log firm size control; (3) our main dynamic model with MNE, international and domestic firm experience included (see eq. (3.13)); (4) a dynamic model with MNE, international and domestic firm experience, and their interactions with the worker fixed effects included (see eq. (3.14)). Standard errors (in parentheses) are block-bootstrapped at the worker level (re-estimating the fixed effects until convergence in all 100 iterations).

3.C Additional results on other international (non-multinational) firms

Figure 3.9: Wage premia of international firm experience by worker ability.



(a) Across-firm



(b) Within-firm

Notes: The plots depict calculated wage premia per worker ability group and their 95%-confidence intervals, as estimated by a wage regression with interactions between MNE/international firm/domestic firm experience and the worker fixed effects (see eq. (3.14)). The full regression results are in Table 3.12 in Appendix 3.F.2. Wage premia are calculated as the coefficients for international firm experience minus the respective coefficients for domestic firm experience. Experience within and across firms are based on actual days worked and cut in yearly splines. High-ability workers are in the 75th percentile and low-ability workers in the 25th percentile of the worker fixed effects distribution. Standard errors are block-bootstrapped at the worker level (re-estimating worker fixed effects until convergence in all 100 iterations).

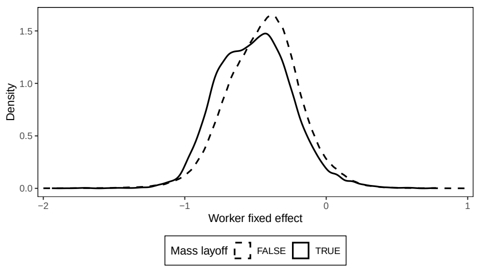
3.D Evidence from mass layoffs

Table 3.9: Mass layoffs.

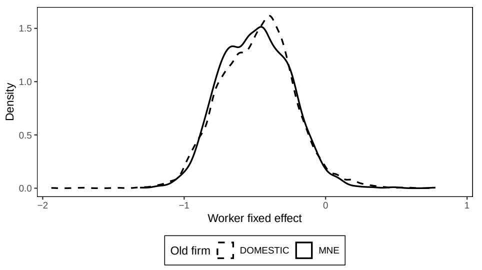
Firm type	Firms	Workers
MNE	386	2,813
International	979	4,602
Domestic	1,521	5,956

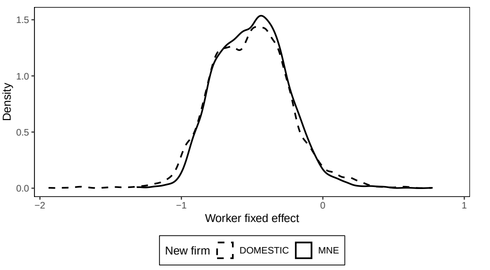
Notes: The table shows the number of firms and workers involved in mass layoffs, split up by three firm types: MNE, international and domestic firms. MNEs comprise foreign firms (ultimate owner located abroad) and domestic firms with foreign subsidiaries. International firms are defined as firms with an average yearly sum of imports and exports that exceeds 10k EUR. A mass layoff event identifies a firm with at least 10 employees that ceases to exist or lays off more than 80% of its workers in a given year, with less than 30% of the exiting workers entering the same new firm.

Figure 3.10: Worker fixed effects in mass layoffs.



(a) Workers in mass layoffs vs. other workers





(b) At old firm

(c) At new firm

Notes: The plots show different distributions of worker fixed effects derived from a wage regression (see eq. (3.14)). Panel (a) splits the distribution up by workers involved in a mass layoff event. A mass layoff event identifies a firm with at least 10 employees that ceases to exist or lays off more than 80% of its workers in a given year, with less than 30% of the exiting workers entering the same new firm. Panel (b) splits the distribution up by the MNE/domestic firm status of the origin firm where the mass layoff occurs. Panel (c) splits the distribution up by the destination firm where workers are observed following a mass layoff.

Table 3.10: Evidence from mass layoffs (full table).

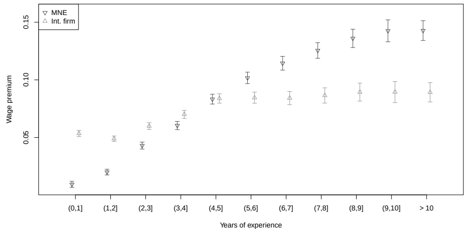
	log(hourly wage) (detrended)	
	(1)	(2)
Domestic firm experience	0.0395***	0.0523***
	(0.0007)	(0.0033)
International firm experience	0.0445***	0.0602***
	(0.0007)	(0.0032)
MNE experience	0.0469***	0.0825***
	(0.0007)	(0.0027)
Years in firm	0.0496***	0.0477***
	(0.0010)	(0.0053)
Years in firm $\times$ International firm	0.0021*	0.0082
	(0.0010)	(0.0051)
Years in firm $\times$ MNE	0.0074***	0.0285***
	(0.0010)	(0.0055)
log(firm size)	0.0363***	0.0297***
	(0.0004)	(0.0006)
Employer number = 3	0.0569***	0.0247·
	(0.0037)	(0.0131)
Employer number = 4	0.0538***	-0.0011
	(0.0047)	(0.0206)
Years in firm $\times$ Employer number = 3	-0.0089***	-0.0057
	(0.0012)	(0.0039)
Years in firm $\times$ Employer number = 4	-0.0098***	-0.0101·
	(0.0016)	(0.0059)
Worker fe	0.9239***	0.7476***
	(0.0042)	(0.0092)
Firm fe	0.9354***	0.9728***
	(0.0051)	(0.0112)
Domestic firm experience $\times$ Worker fe		0.0389***
		(0.0057)
International firm experience $\times$ Worker fe		0.0438***
		(0.0056)
MNE experience $\times$ Worker fe		0.0858***
		(0.0046)
Years in firm $\times$ Worker fe		0.0151
		(0.0102)

Worker fe × Employer number = 3		-0.0181
		(0.0237)
Worker fe × Employer number = 4		-0.0983**
		(0.0344)
Worker fe × years in firm × Employer number = 3		-0.0069
		(0.0080)
Worker fe × years in firm × Employer number = 4		-0.0135
		(0.0103)
Years in firm × Worker fe × International firm		0.0042
		(0.0097)
Years in firm × Worker fe × MNE		0.0393***
		(0.0102)
Observations	42,558	42,558
R^2	0.8067	0.8192

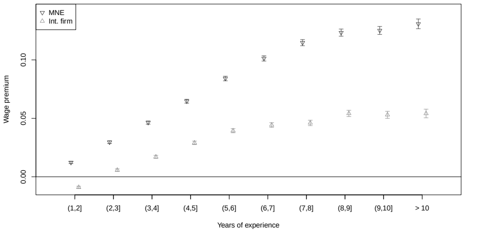
Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. The dependent variable is a workers' log hourly wage, detrended by industry-year fixed effects on the full firm-worker network. The estimations focus on the observations of workers at their first employer, after the worker was involved in a firm closure or mass layoff; see Section 3.4.4. 'Years in firm' refers to experience accumulated while a worker is employed at the current employer of type MNE, international firm and domestic firm (reference category). MNE/international firm/domestic firm experience refers to experience accumulated before entering the current employer. Experience is calculated based on actual days worked. Employer number refers to the cumulative number of distinct firms that a worker has been observed at, with two being the reference category. log(firm size) is the natural logarithm of the total number of (full-time) employees observed in a firm in a given year. Column 1 adds the fixed effects of an estimation of eq. (3.13) as linear regressors. Column 2 adds those of eq. (3.14). Standard errors in Column 1 are clustered at the worker level. Standard errors in Column 2 are block-bootstrapped at the worker level (re-estimating worker fixed effects until convergence in all 100 iterations).

3.E Limited mobility bias

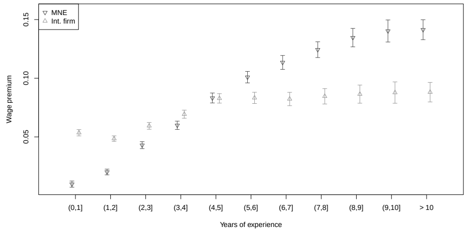
Figure 3.11: The wage premia of MNE experience with K = 10, 20, 50 clusters in the firm fixed effects.



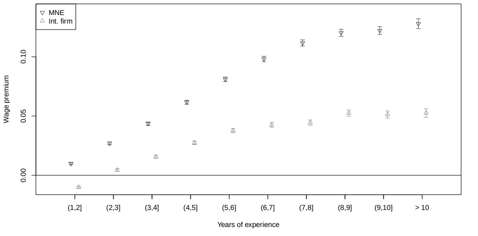
(a) 10 firm fe clusters: across-firm



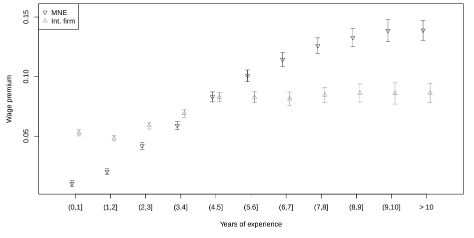
(b) 10 firm clusters: within-firm



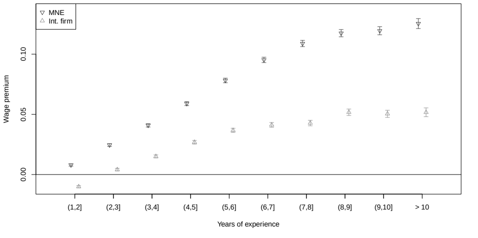
(c) 20 firm fe clusters: across-firm



(d) 20 firm fe clusters: within-firm



(e) 50 firm fe clusters: across-firm



(f) 50 firm fe clusters: within-firm

Notes: The plots depict calculated wage premia and their 95%-confidence intervals (see eq. (3.13) and the discussion in Sections 3.4 and 3.6.1) for $K = 10, 20, 50$ clusters in the firm fixed effects. Wage premia are calculated as the coefficients for MNE/international firm experience minus the respective coefficients for domestic firm experience. Experience within and across firms are based on actual days worked and cut in yearly splines. Firm fixed effect clusters are found using a K-means clustering algorithm on the within-firm distribution of (detrended) log hourly wages (Bonhomme et al., 2019), and by picking 20 random initial assignments. Standard errors are clustered at the worker level.

3

3.F Full regression tables for the main estimates

3.F.1 The value of MNE experience

Table 3.11: Wage profile estimates.

	log(hourly wage) (detrended)
	(1)
Years in firm = (1,2]	0.1240***
	(0.0003)
Years in firm = (2,3]	0.2003***
	(0.0004)
Years in firm = (3,4]	0.2538***
	(0.0005)
Years in firm = (4,5]	0.2964***
	(0.0006)
Years in firm = (5,6]	0.3319***
	(0.0007)
Years in firm = (6,7]	0.3642***
	(0.0008)
Years in firm = (7,8]	0.3956***
	(0.0009)
Years in firm = (8,9]	0.4249***
	(0.0010)
Years in firm = (9,10]	0.4580***
	(0.0012)
Years in firm = (10,Inf]	0.5189***
	(0.0014)
International firm × years in firm = (1,2]	0.0093***
	(0.0003)
International firm × years in firm = (2,3]	0.0229***
	(0.0004)
International firm × years in firm = (3,4]	0.0343***
	(0.0006)
International firm × years in firm = (4,5]	0.0466***
	(0.0007)

International firm × years in firm = (5,6]	0.0569***
	(0.0008)
International firm × years in firm = (6,7]	0.0612***
	(0.0009)
International firm × years in firm = (7,8]	0.0636***
	(0.0011)
International firm × years in firm = (8,9]	0.0698***
	(0.0013)
International firm × years in firm = (9,10]	0.0685***
	(0.0015)
International firm × years in firm = (10,Inf]	0.0696***
	(0.0018)
MNE × years in firm = (1,2]	0.0138***
	(0.0004)
MNE × years in firm = (2,3]	0.0333***
	(0.0005)
MNE × years in firm = (3,4]	0.0520***
	(0.0006)
MNE × years in firm = (4,5]	0.0726***
	(0.0007)
MNE × years in firm = (5,6]	0.0919***
	(0.0009)
MNE × years in firm = (6,7]	0.1089***
	(0.0010)
MNE × years in firm = (7,8]	0.1218***
	(0.0012)
MNE × years in firm = (8,9]	0.1304***
	(0.0015)
MNE × years in firm = (9,10]	0.1326***
	(0.0016)
MNE × years in firm = (10,Inf]	0.1387***
	(0.0021)
Years in firm = (1,2] × Employer number = 2	-0.0401***
	(0.0003)
Years in firm = (1,2] × Employer number = 3	-0.0506***
	(0.0004)
Years in firm = (1,2] × Employer number = 4	-0.0578***
	(0.0004)

3

Years in firm = (2,3] × Employer number = 2	-0.0624***
	(0.0004)
Years in firm = (2,3] × Employer number = 3	-0.0830***
	(0.0005)
Years in firm = (2,3] × Employer number = 4	-0.0991***
	(0.0006)
Years in firm = (3,4] × Employer number = 2	-0.0784***
	(0.0005)
Years in firm = (3,4] × Employer number = 3	-0.1061***
	(0.0007)
Years in firm = (3,4] × Employer number = 4	-0.1292***
	(0.0008)
Years in firm = (4,5] × Employer number = 2	-0.0888***
	(0.0006)
Years in firm = (4,5] × Employer number = 3	-0.1232***
	(0.0008)
Years in firm = (4,5] × Employer number = 4	-0.1502***
	(0.0010)
Years in firm = (5,6] × Employer number = 2	-0.0960***
	(0.0008)
Years in firm = (5,6] × Employer number = 3	-0.1349***
	(0.0010)
Years in firm = (5,6] × Employer number = 4	-0.1648***
	(0.0013)
Years in firm = (6,7] × Employer number = 2	-0.1004***
	(0.0009)
Years in firm = (6,7] × Employer number = 3	-0.1437***
	(0.0013)
Years in firm = (6,7] × Employer number = 4	-0.1763***
	(0.0017)
Years in firm = (7,8] × Employer number = 2	-0.1060***
	(0.0011)
Years in firm = (7,8] × Employer number = 3	-0.1499***
	(0.0016)
Years in firm = (7,8] × Employer number = 4	-0.1867***
	(0.0024)
Years in firm = (8,9] × Employer number = 2	-0.1040***
	(0.0014)

Years in firm = (8,9] × Employer number = 3	-0.1552***
	(0.0019)
Years in firm = (8,9] × Employer number = 4	-0.1934***
	(0.0031)
Years in firm = (9,10] × Employer number = 2	-0.1083***
	(0.0015)
Years in firm = (9,10] × Employer number = 3	-0.1664***
	(0.0023)
Years in firm = (9,10] × Employer number = 4	-0.2082***
	(0.0038)
Years in firm = (10,Inf] × Employer number = 2	-0.1244***
	(0.0019)
Years in firm = (10,Inf] × Employer number = 3	-0.1872***
	(0.0031)
Years in firm = (10,Inf] × Employer number = 4	-0.2383***
	(0.0057)
MNE experience = (0,1]	0.0362***
	(0.0009)
MNE experience = (1,2]	0.0759***
	(0.0009)
MNE experience = (2,3]	0.1296***
	(0.0010)
MNE experience = (3,4]	0.1792***
	(0.0012)
MNE experience = (4,5]	0.2281***
	(0.0014)
MNE experience = (5,6]	0.2694***
	(0.0016)
MNE experience = (6,7]	0.3071***
	(0.0019)
MNE experience = (7,8]	0.3409***
	(0.0021)
MNE experience = (8,9]	0.3753***
	(0.0024)
MNE experience = (9,10]	0.4003***
	(0.0029)
MNE experience = (10,Inf]	0.4515***
	(0.0027)

3

International firm experience = (0,1]	0.0347***
	(0.0010)
International firm experience = (1,2]	0.0619***
	(0.0009)
International firm experience = (2,3]	0.1078***
	(0.0011)
International firm experience = (3,4]	0.1511***
	(0.0012)
International firm experience = (4,5]	0.1909***
	(0.0014)
International firm experience = (5,6]	0.2244***
	(0.0016)
International firm experience = (6,7]	0.2535***
	(0.0019)
International firm experience = (7,8]	0.2835***
	(0.0021)
International firm experience = (8,9]	0.3068***
	(0.0025)
International firm experience = (9,10]	0.3357***
	(0.0030)
International firm experience = (10,Inf]	0.3809***
	(0.0027)
Domestic firm experience = (0,1]	0.0182***
	(0.0010)
Domestic firm experience = (1,2]	0.0508***
	(0.0009)
Domestic firm experience = (2,3]	0.0901***
	(0.0011)
Domestic firm experience = (3,4]	0.1271***
	(0.0013)
Domestic firm experience = (4,5]	0.1607***
	(0.0015)
Domestic firm experience = (5,6]	0.1883***
	(0.0016)
Domestic firm experience = (6,7]	0.2179***
	(0.0019)
Domestic firm experience = (7,8]	0.2382***
	(0.0021)

Domestic firm experience = (8,9]	0.2679***
	(0.0024)
Domestic firm experience = (9,10]	0.2923***
	(0.0027)
Domestic firm experience = (10,Inf]	0.3397***
	(0.0025)
log(firm size)	0.0294***
	(0.0005)
Employer number = 2	0.1124***
	(0.0007)
Employer number = 3	0.1487***
	(0.0011)
Employer number = 4	0.1570***
	(0.0015)
Fixed-effects	
Worker (1,059,991)	✓
Firm (207,074)	✓
Observations	8,055,023
R^2	0.8288

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Estimates of eq. (3.13) based on the full sample of workers that are observed up from labor market entry. The dependent variable is the natural logarithm of hourly wage (total wage over total hours worked), detrended by industry-year fixed effects on the full sample of all workers in a first step. 'Years in firm' refers to experience accumulated while a worker is employed at the current employer of type MNE, international firm and domestic firm (reference category). MNE/international firm/domestic firm experience refers to experience accumulated before entering the current employer. Experience is calculated based on actual days worked and cut in yearly splines. Employer number refers to the cumulative number of distinct firms that a worker has been observed at, with one being the reference category. log(firm size) is the natural logarithm of the total number of (full-time) employees observed in a firm in a given year. Standard errors clustered at the worker level in parentheses.

3

3.F.2 The value of MNE experience by workers' innate ability

Table 3.12: Wage profile estimates (worker fixed effects interactions).

	log(hourly wage) (detrended)
	(1)
Years in firm = (1,2]	0.0817***
	(0.0008)
Years in firm = (2,3]	0.1434***
	(0.0013)
Years in firm = (3,4]	0.2045***
	(0.0015)
Years in firm = (4,5]	0.2605***
	(0.0018)
Years in firm = (5,6]	0.3126***
	(0.0019)
Years in firm = (6,7]	0.3609***
	(0.0025)
Years in firm = (7,8]	0.4058***
	(0.0028)
Years in firm = (8,9]	0.4478***
	(0.0035)
Years in firm = (9,10]	0.4899***
	(0.0039)
Years in firm = (10,Inf]	0.5650***
	(0.0046)
Worker fe × years in firm = (1,2]	-0.0938***
	(0.0016)
Worker fe × years in firm = (2,3]	-0.1285***
	(0.0023)
Worker fe × years in firm = (3,4]	-0.1112***
	(0.0029)
Worker fe × years in firm = (4,5]	-0.0794***
	(0.0036)
Worker fe × years in firm = (5,6]	-0.0402***
	(0.0038)
Worker fe × years in firm = (6,7]	-0.0021

	(0.0047)
Worker fe × years in firm = (7,8]	0.0301***
	(0.0056)
Worker fe × years in firm = (8,9]	0.0602***
	(0.0068)
Worker fe × years in firm = (9,10]	0.0814***
	(0.0075)
Worker fe × years in firm = (10,Inf]	0.1149***
	(0.0091)
International firm × years in firm = (1,2]	0.0217***
	(0.0010)
International firm × years in firm = (2,3]	0.0293***
	(0.0013)
International firm × years in firm = (3,4]	0.0381***
	(0.0016)
International firm × years in firm = (4,5]	0.0520***
	(0.0020)
International firm × years in firm = (5,6]	0.0667***
	(0.0022)
International firm × years in firm = (6,7]	0.0790***
	(0.0027)
International firm × years in firm = (7,8]	0.0893***
	(0.0031)
International firm × years in firm = (8,9]	0.0993***
	(0.0043)
International firm × years in firm = (9,10]	0.1027***
	(0.0047)
International firm × years in firm = (10,Inf]	0.1085***
	(0.0056)
MNE × years in firm = (1,2]	0.0370***
	(0.0010)
MNE × years in firm = (2,3]	0.0823***
	(0.0015)
MNE × years in firm = (3,4]	0.1228***
	(0.0021)
MNE × years in firm = (4,5]	0.1631***
	(0.0028)
MNE × years in firm = (5,6]	0.1976***

3

	(0.0032)
MNE × years in firm = (6,7]	0.2299***
	(0.0041)
MNE × years in firm = (7,8]	0.2571***
	(0.0049)
MNE × years in firm = (8,9]	0.2735***
	(0.0054)
MNE × years in firm = (9,10]	0.2807***
	(0.0061)
MNE × years in firm = (10,Inf]	0.3032***
	(0.0062)
Years in firm = (1,2] × Employer number = 2	-0.0077***
	(0.0009)
Years in firm = (1,2] × Employer number = 3	-0.0165***
	(0.0011)
Years in firm = (1,2] × Employer number = 4	-0.0237***
	(0.0012)
Years in firm = (2,3] × Employer number = 2	-0.0119***
	(0.0013)
Years in firm = (2,3] × Employer number = 3	-0.0288***
	(0.0013)
Years in firm = (2,3] × Employer number = 4	-0.0489***
	(0.0016)
Years in firm = (3,4] × Employer number = 2	-0.0290***
	(0.0015)
Years in firm = (3,4] × Employer number = 3	-0.0534***
	(0.0017)
Years in firm = (3,4] × Employer number = 4	-0.0817***
	(0.0022)
Years in firm = (4,5] × Employer number = 2	-0.0422***
	(0.0018)
Years in firm = (4,5] × Employer number = 3	-0.0793***
	(0.0022)
Years in firm = (4,5] × Employer number = 4	-0.1078***
	(0.0030)
Years in firm = (5,6] × Employer number = 2	-0.0568***
	(0.0021)
Years in firm = (5,6] × Employer number = 3	-0.1013***

	(0.0030)
Years in firm = (5,6] × Employer number = 4	-0.1356***
	(0.0041)
Years in firm = (6,7] × Employer number = 2	-0.0669***
	(0.0027)
Years in firm = (6,7] × Employer number = 3	-0.1235***
	(0.0036)
Years in firm = (6,7] × Employer number = 4	-0.1582***
	(0.0048)
Years in firm = (7,8] × Employer number = 2	-0.0800***
	(0.0033)
Years in firm = (7,8] × Employer number = 3	-0.1375***
	(0.0047)
Years in firm = (7,8] × Employer number = 4	-0.1813***
	(0.0071)
Years in firm = (8,9] × Employer number = 2	-0.0816***
	(0.0046)
Years in firm = (8,9] × Employer number = 3	-0.1461***
	(0.0069)
Years in firm = (8,9] × Employer number = 4	-0.1868***
	(0.0086)
Years in firm = (9,10] × Employer number = 2	-0.0933***
	(0.0044)
Years in firm = (9,10] × Employer number = 3	-0.1695***
	(0.0069)
Years in firm = (9,10] × Employer number = 4	-0.2145***
	(0.0099)
Years in firm = (10,Inf] × Employer number = 2	-0.1223***
	(0.0060)
Years in firm = (10,Inf] × Employer number = 3	-0.1902***
	(0.0083)
Years in firm = (10,Inf] × Employer number = 4	-0.2569***
	(0.0158)
Worker fe × International firm × years in firm = (1,2]	0.0226***
	(0.0021)
Worker fe × International firm × years in firm = (2,3]	0.0081**
	(0.0027)
Worker fe × International firm × years in firm = (3,4]	0.0028

3

	(0.0032)
Worker fe × International firm × years in firm = (4,5]	0.0079·
	(0.0040)
Worker fe × International firm × years in firm = (5,6]	0.0183***
	(0.0041)
Worker fe × International firm × years in firm = (6,7]	0.0355***
	(0.0051)
Worker fe × International firm × years in firm = (7,8]	0.0527***
	(0.0063)
Worker fe × International firm × years in firm = (8,9]	0.0605***
	(0.0088)
Worker fe × International firm × years in firm = (9,10]	0.0704***
	(0.0094)
Worker fe × International firm × years in firm = (10,Inf]	0.0797***
	(0.0117)
Worker fe × MNE × years in firm = (1,2]	0.0492***
	(0.0022)
Worker fe × MNE × years in firm = (2,3]	0.1080***
	(0.0029)
Worker fe × MNE × years in firm = (3,4]	0.1640***
	(0.0036)
Worker fe × MNE × years in firm = (4,5]	0.2185***
	(0.0052)
Worker fe × MNE × years in firm = (5,6]	0.2627***
	(0.0057)
Worker fe × MNE × years in firm = (6,7]	0.3079***
	(0.0073)
Worker fe × MNE × years in firm = (7,8]	0.3490***
	(0.0093)
Worker fe × MNE × years in firm = (8,9]	0.3740***
	(0.0105)
Worker fe × MNE × years in firm = (9,10]	0.3902***
	(0.0116)
Worker fe × MNE × years in firm = (10,Inf]	0.4410***
	(0.0119)
Worker fe × years in firm = (1,2] × Employer number = 2	0.0749***
	(0.0020)
Worker fe × years in firm = (1,2] × Employer number = 3	0.0790***

	(0.0021)
Worker fe × years in firm = (1,2] × Employer number = 4	0.0788***
	(0.0023)
Worker fe × years in firm = (2,3] × Employer number = 2	0.1191***
	(0.0025)
Worker fe × years in firm = (2,3] × Employer number = 3	0.1274***
	(0.0026)
Worker fe × years in firm = (2,3] × Employer number = 4	0.1166***
	(0.0031)
Worker fe × years in firm = (3,4] × Employer number = 2	0.1162***
	(0.0031)
Worker fe × years in firm = (3,4] × Employer number = 3	0.1220***
	(0.0035)
Worker fe × years in firm = (3,4] × Employer number = 4	0.1065***
	(0.0039)
Worker fe × years in firm = (4,5] × Employer number = 2	0.1067***
	(0.0037)
Worker fe × years in firm = (4,5] × Employer number = 3	0.0971***
	(0.0042)
Worker fe × years in firm = (4,5] × Employer number = 4	0.0870***
	(0.0054)
Worker fe × years in firm = (5,6] × Employer number = 2	0.0863***
	(0.0043)
Worker fe × years in firm = (5,6] × Employer number = 3	0.0686***
	(0.0055)
Worker fe × years in firm = (5,6] × Employer number = 4	0.0500***
	(0.0078)
Worker fe × years in firm = (6,7] × Employer number = 2	0.0700***
	(0.0054)
Worker fe × years in firm = (6,7] × Employer number = 3	0.0326***
	(0.0069)
Worker fe × years in firm = (6,7] × Employer number = 4	0.0161·
	(0.0091)
Worker fe × years in firm = (7,8] × Employer number = 2	0.0494***
	(0.0070)
Worker fe × years in firm = (7,8] × Employer number = 3	0.0092
	(0.0087)
Worker fe × years in firm = (7,8] × Employer number = 4	-0.0192

	(0.0130)
Worker fe × years in firm = (8,9] × Employer number = 2	0.0379***
	(0.0097)
Worker fe × years in firm = (8,9] × Employer number = 3	-0.0045
	(0.0127)
Worker fe × years in firm = (8,9] × Employer number = 4	-0.0269·
	(0.0164)
Worker fe × years in firm = (9,10] × Employer number = 2	0.0197*
	(0.0092)
Worker fe × years in firm = (9,10] × Employer number = 3	-0.0356**
	(0.0120)
Worker fe × years in firm = (9,10] × Employer number = 4	-0.0596**
	(0.0188)
Worker fe × years in firm = (10,Inf] × Employer number = 2	-0.0148
	(0.0118)
Worker fe × years in firm = (10,Inf] × Employer number = 3	-0.0465**
	(0.0154)
Worker fe × years in firm = (10,Inf] × Employer number = 4	-0.0988***
	(0.0277)
Domestic firm experience = (0,1]	0.0171***
	(0.0029)
Domestic firm experience = (1,2]	0.0813***
	(0.0027)
Domestic firm experience = (2,3]	0.1413***
	(0.0033)
Domestic firm experience = (3,4]	0.1965***
	(0.0044)
Domestic firm experience = (4,5]	0.2423***
	(0.0043)
Domestic firm experience = (5,6]	0.2840***
	(0.0055)
Domestic firm experience = (6,7]	0.3199***
	(0.0055)
Domestic firm experience = (7,8]	0.3474***
	(0.0075)
Domestic firm experience = (8,9]	0.3829***
	(0.0072)
Domestic firm experience = (9,10]	0.4042***

	(0.0086)
Domestic firm experience = (10,Inf]	0.4640***
	(0.0096)
Worker fe × Domestic firm experience = (0,1]	-0.0052
	(0.0058)
Worker fe × Domestic firm experience = (1,2]	0.0599***
	(0.0056)
Worker fe × Domestic firm experience = (2,3]	0.1037***
	(0.0072)
Worker fe × Domestic firm experience = (3,4]	0.1436***
	(0.0092)
Worker fe × Domestic firm experience = (4,5]	0.1717***
	(0.0086)
Worker fe × Domestic firm experience = (5,6]	0.2034***
	(0.0107)
Worker fe × Domestic firm experience = (6,7]	0.2202***
	(0.0115)
Worker fe × Domestic firm experience = (7,8]	0.2367***
	(0.0149)
Worker fe × Domestic firm experience = (8,9]	0.2486***
	(0.0143)
Worker fe × Domestic firm experience = (9,10]	0.2441***
	(0.0170)
Worker fe × Domestic firm experience = (10,Inf]	0.2706***
	(0.0191)
International firm experience = (0,1]	0.0208***
	(0.0022)
International firm experience = (1,2]	0.0910***
	(0.0020)
International firm experience = (2,3]	0.1531***
	(0.0028)
International firm experience = (3,4]	0.2178***
	(0.0028)
International firm experience = (4,5]	0.2799***
	(0.0036)
International firm experience = (5,6]	0.3285***
	(0.0037)
International firm experience = (6,7]	0.3727***

3

	(0.0049)
International firm experience = (7,8]	0.4130***
	(0.0056)
International firm experience = (8,9]	0.4464***
	(0.0060)
International firm experience = (9,10]	0.4743***
	(0.0080)
International firm experience = (10,Inf]	0.5389***
	(0.0064)
Worker fe × International firm experience = (0,1]	-0.0333***
	(0.0047)
Worker fe × International firm experience = (1,2]	0.0563***
	(0.0037)
Worker fe × International firm experience = (2,3]	0.0929***
	(0.0055)
Worker fe × International firm experience = (3,4]	0.1429***
	(0.0060)
Worker fe × International firm experience = (4,5]	0.1988***
	(0.0071)
Worker fe × International firm experience = (5,6]	0.2334***
	(0.0083)
Worker fe × International firm experience = (6,7]	0.2660***
	(0.0112)
Worker fe × International firm experience = (7,8]	0.2886***
	(0.0114)
Worker fe × International firm experience = (8,9]	0.3108***
	(0.0123)
Worker fe × International firm experience = (9,10]	0.3069***
	(0.0155)
Worker fe × International firm experience = (10,Inf]	0.3500***
	(0.0118)
MNE experience = (0,1]	0.0589***
	(0.0025)
MNE experience = (1,2]	0.1493***
	(0.0022)
MNE experience = (2,3]	0.2568***
	(0.0027)
MNE experience = (3,4]	0.3461***

	(0.0035)
MNE experience = (4,5]	0.4212***
	(0.0043)
MNE experience = (5,6]	0.4768***
	(0.0048)
MNE experience = (6,7]	0.5404***
	(0.0059)
MNE experience = (7,8]	0.5857***
	(0.0061)
MNE experience = (8,9]	0.6386***
	(0.0073)
MNE experience = (9,10]	0.6709***
	(0.0087)
MNE experience = (10,Inf]	0.7650***
	(0.0080)
Worker fe × MNE experience = (0,1]	0.0607***
	(0.0051)
Worker fe × MNE experience = (1,2]	0.1652***
	(0.0044)
Worker fe × MNE experience = (2,3]	0.2838***
	(0.0053)
Worker fe × MNE experience = (3,4]	0.3782***
	(0.0063)
Worker fe × MNE experience = (4,5]	0.4437***
	(0.0075)
Worker fe × MNE experience = (5,6]	0.4815***
	(0.0100)
Worker fe × MNE experience = (6,7]	0.5479***
	(0.0110)
Worker fe × MNE experience = (7,8]	0.5800***
	(0.0113)
Worker fe × MNE experience = (8,9]	0.6252***
	(0.0130)
Worker fe × MNE experience = (9,10]	0.6497***
	(0.0178)
Worker fe × MNE experience = (10,Inf]	0.7562***
	(0.0153)
log(firm size)	0.0300***

3

	(0.0005)
Employer number = 2	0.0188***
	(0.0021)
Employer number = 3	0.0269***
	(0.0030)
Employer number = 4	0.0073*
	(0.0037)
Worker fe × Employer number = 2	-0.1958***
	(0.0042)
Worker fe × Employer number = 3	-0.2536***
	(0.0061)
Worker fe × Employer number = 4	-0.3241***
	(0.0075)
Fixed-effects	
Worker (1,059,991)	✓
Firm (207,074)	✓
Observations	8,055,023
R^2	0.8331

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Estimates of eq. (3.14) based on the full sample of workers that are observed up from labor market entry. Estimated using the iterative procedure of De la Roca and Puga (2017); see Section 3.4.3. The dependent variable is the natural logarithm of hourly wage (total wage over total hours worked), detrended by industry-year fixed effects on the full sample of all workers in a first step. 'Years in firm' refers to experience accumulated while a worker is employed at the current employer of type MNE, international firm and domestic firm (reference category). MNE/international firm/domestic firm experience refers to experience accumulated before entering the current employer. Experience is calculated based on actual days worked and cut in yearly splines. Employer number refers to the cumulative number of distinct firms that a worker has been observed at, with one being the reference category. log(firm size) is the natural logarithm of the total number of (full-time) employees observed in a firm in a given year. Standard errors in parentheses are block-bootstrapped at the worker level (re-estimating worker fixed effects until convergence in all 100 iterations).

3.F.3 Selection within the multinational

Table 3.13: MNE employment probabilities.

	MNE
	(1)
Labor market experience = (0,1]	-0.0014***
	(0.0002)
Labor market experience = (1,2]	-0.0021***
	(0.0004)
Labor market experience = (2,3]	-0.0025***
	(0.0006)
Labor market experience = (3,4]	-0.0027***
	(0.0008)
Labor market experience = (4,5]	-0.0026**
	(0.0010)
Labor market experience = (5,6]	-0.0027*
	(0.0011)
Labor market experience = (6,7]	-0.0034**
	(0.0013)
Labor market experience = (7,8]	-0.0042**
	(0.0015)
Labor market experience = (8,9]	-0.0047**
	(0.0017)
Labor market experience = (9,10]	-0.0053**
	(0.0019)
Labor market experience = (10,Inf]	-0.0070***
	(0.0021)
log(firm size)	0.0474***
	(0.0005)
Employer number = 2	0.0032***
	(0.0003)
Employer number = 3	0.0040***
	(0.0004)
Employer number = 4	0.0062***
	(0.0006)
Fixed-effects	

Worker (1,059,991)	✓
Firm (207,074)	✓
Industry-year (332)	✓
Observations	8,055,023
R^2	0.9331

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Estimates of eq. (3.15) based on the full sample of workers that are observed up from labor market entry. The dependent variable is an indicator that identifies observations at MNEs. 'Labor market experience' refers to the number of years since a worker entered the labor market. Employer number refers to the cumulative number of distinct firms that a worker has been observed at, with one being the reference category. log(firm size) is the natural logarithm of the total number of (full-time) employees observed in a firm in a given year. Standard errors clustered at the worker level in parentheses.

Table 3.14: MNE employment probabilities (worker fixed effect interactions).

	MNE
	(1)
Labor market experience = (0,1]	0.0000
	(0.0005)
Labor market experience = (1,2]	0.0011
	(0.0008)
Labor market experience = (2,3]	0.0028*
	(0.0011)
Labor market experience = (3,4]	0.0042**
	(0.0015)
Labor market experience = (4,5]	0.0057**
	(0.0018)
Labor market experience = (5,6]	0.0059**
	(0.0021)
Labor market experience = (6,7]	0.0065**
	(0.0022)
Labor market experience = (7,8]	0.0057*
	(0.0024)
Labor market experience = (8,9]	0.0063*
	(0.0027)
Labor market experience = (9,10]	0.0071*
	(0.0031)
Labor market experience = (10,Inf]	0.0073*
	(0.0033)
Worker ability × Labor market experience = (0,1]	0.0032***
	(0.0008)
Worker ability × Labor market experience = (1,2]	0.0072***
	(0.0012)
Worker ability × Labor market experience = (2,3]	0.0120***
	(0.0014)
Worker ability × Labor market experience = (3,4]	0.0157***
	(0.0017)
Worker ability × Labor market experience = (4,5]	0.0188***
	(0.0019)
Worker ability × Labor market experience = (5,6]	0.0194***
	(0.0021)

3

Worker ability × Labor market experience = (6,7]	0.0224***
	(0.0024)
Worker ability × Labor market experience = (7,8]	0.0224***
	(0.0026)
Worker ability × Labor market experience = (8,9]	0.0250***
	(0.0025)
Worker ability × Labor market experience = (9,10]	0.0283***
	(0.0027)
Worker ability × Labor market experience = (10,Inf]	0.0325***
	(0.0032)
log(firm size)	0.0472***
	(0.0008)
Employer number = 2	0.0030***
	(0.0005)
Employer number = 3	0.0040***
	(0.0007)
Employer number = 4	0.0067***
	(0.0009)
Fixed-effects	
Worker (1,059,991)	✓
Firm (207,074)	✓
Industry-year (332)	✓
Observations	8,055,023
R^2	0.9332

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. Estimates of eq. (3.16) based on the full sample of workers that are observed up from labor market entry. The dependent variable is an indicator that identifies observations at MNEs. 'Worker ability' refers to the worker fixed effects of an estimation of eq. (3.14); see Section 3.5.2. 'Labor market experience' refers to the number of years since a worker entered the labor market. 'Employer number' refers to the cumulative number of distinct firms that a worker has been observed at, with 1 being the reference category. log(firm size) is the natural logarithm of the total number of (full-time) employees observed in a firm in a given year. Standard errors in parentheses are block-bootstrapped at the worker level (re-estimating worker fixed effects until convergence in all 100 iterations).

Chapter 4

Post-Automation Workforce Dynamics in (Non-)Multinationals

Abstract: We estimate the impact of automation on worker separations and wages across Multinationals (MNEs) and domestic firms within the matched employer-employee dataset of the Netherlands. In MNEs, spikes in automation costs lead to a 24% separation rate, with the remaining workforce experiencing 2% wage growth. Higher-educated workers, those with a background in ICT and Sciences, and managers gain 5-8% wage growth. In contrast, domestic firm automation spikes lead to a 11% separation rate, with remaining workers experiencing a 1.4% wage decline, mostly confined to lower-educated workers. The difference is not explained by flexible contracts in MNEs, exports, imports, or industry. Firm-level hires reduce in both types of firms. Additionally, we document that firms' spikes in ICT investments raise MNE wage growth, while they lower wage growth in domestic firms. Machinery investments show no differential impact between MNEs and domestic firms.

4.1 Introduction

Globalization has accelerated the integration of automation technologies such as industrial robots, machine learning, and artificial intelligence into business operations, raising concerns about increasing inequalities in job opportunities and wages (e.g., Autor et al., 2013; Helpman et al., 2017). At the same time, the global technological shift prompts governments to extend large budgets towards attracting and retaining Multinational Enterprises (MNEs), in the hope that these firms benefit local workers. Yet, systematic evidence on how automation in MNEs affects local workers is lacking.

While some studies indicate productivity gains and employment growth from automation (e.g., Koch et al., 2021), others highlight concerns about worker displacements and growing wage disparities (e.g., Acemoglu and Restrepo, 2019). Persistent differences in productivity levels across firms (Syverson, 2011) suggest that firm-specific characteristics, such as workforce composition (Bender et al., 2018) and management practices (Bloom and Van Reenen, 2007; Giorcelli, 2019) relate to the firm's capacity to adopt and benefit from new technologies (Acemoglu et al., 2022). MNEs, which are characterized by higher productivity and a more skilled workforce (e.g., Girma and Görg, 2007a; Koch and Smolka, 2019), often adopt more standardized production processes than domestic firms (e.g. Bircan, 2019), potentially leading to distinct automation impacts on their workers. In this chapter, we explore the differential impact of automation on worker separations and wages within MNEs and domestic firms, documenting that automating MNEs separate more and pay higher wages.

We employ unique employer-employee matched data from the Netherlands, which allows us to track the employment and hourly wage trajectories of workers in automating firms for the years 2010 to 2021. Our measure of firm-level automation derives from a survey by Statistics Netherlands, which specifically collects details on firms' costs for third-party automation services. Compared to automation measures based on imports, automation costs offer a broad measure of automation across all industries and firm sizes. Drawing on additional administrative records, we comprehensively identify foreign and Dutch multinationals within the data. Our dataset covers approximately 22,000 firms and 2.1 million workers annually, capturing about 30% of the Dutch labor market. This extensive coverage allows us to discern the nuanced effects of automation on workers in MNEs and domestic firms.

We study the impact of significant, lumpy investments in firms' automation costs on worker separations and wages. Extending the methodology from Bessen et al. (2023), we identify an automation event as the initial substantial spike in a firm's automation costs relative to its usual operational costs. Exploiting the automation event, we employ a triple difference-in-differences regression model that isolates the effects of automation simultaneously for MNEs and domestic firms. Our approach compares the change within incumbent workers at automating firms to the change within their matched counterparts in firms that automate later, with matching based on 2-digit industries, employer size, and wage trajectories before automation.

Our results highlight distinct differences in the automation response of MNEs and domestic firms. In MNEs, automation leads to about 10% of the incumbent workforce separating in the automation year and 24% by the fourth year. This represents a 70% increase in separations, compared to the baseline separation rate of one third among matched control workers in later-automating firms. Workers who remain in the MNE accumulate a 1.6% wage premium over the same time, equating to around 16% higher wage growth than the control workers. In stark contrast, domestic firms separate less after automation, with about 11% of incumbents leaving eventually. Furthermore, the wage trajectory of non-separated workers diverges negatively, as they experience a 1.4% wage decrease in domestic firms by the fourth year post-automation. These trends are consistent across both manufacturing and service sectors and broadly extend to hiring practices. In a complementary firm-level analysis, we show the number of new hires contracts by about half in both MNEs and domestic firms post-automation, while new hires in domestic firms earn up to 6% less on average.

Several plausible differences between MNEs and domestic firms do not account for the differential automation effect. Higher flexibility in MNE workers' contracts offers one possible explanation, as temporary contracts are easier to terminate than permanent contracts under Dutch labor regulations. Yet, only 1.5% of the MNE workforce exits under temporary contracts, accounting for just 6% of total MNE separations, whereas domestic firm workers experience temporary contract separations at a rate of 3.4%, representing about a third of their total separations. Furthermore, we test whether firms' international trade activity, the intensity of automation investments, and 2-digit industry explain the difference. However, controlling for these factors in our estimation produces quantitatively similar results on the differential impact of automation on separations and wages in MNEs and domestic firms.

In dissecting the automation effect within MNEs and domestic firms, we document that automation impacts different segments of the workforce. The stylized facts for domestic firms align with skill-biased technological change theories that predict productivity and labor demand increases for high-skilled workers relative to lower-skilled workers (Autor et al., 2006; Katz and Murphy, 1992). We find that domestic firms separate less from their high-educated workers relative to their lower-educated workers, and pay high-educated workers about 2.7% more wage growth over time.

In contrast, automation-induced separations in MNEs are uniformly distributed among high- and low-educated workers, suggesting greater organizational change in MNEs than domestic firms. Aligning broadly with studies that highlight organizational change around automation (e.g., Dauth et al., 2021; Dixon et al., 2021; Humlum, 2022), we find that high-educated workers and those with a technical background reach up to 5% higher wage growth post-automation in MNEs, while workers of lower education see no change in wage growth. Moreover, managers in MNEs benefit from up to 8% higher wage growth and relatively lower separation rates post-automation than non-managers, contrasting domestic firm automation, which leaves managers noticeably unaffected.

Within an additional dataset on firms' investments in Information and Communications Technology (ICT) and Machinery, we contrast the impact of general firm-level investments in such technologies to the investments in automation costs. While spikes in firms' ICT investments lead to a slight increase in wage growth in

MNEs, they decrease wages in domestic firms, analogous to the overall differential wage dynamics of automation costs. However, ICT investments impact separations less, as they remain comparably low and similar between MNEs and domestic firms. Spikes in Machinery investments show a uniform effect across both types of firms, raising worker separations slightly, without significantly affecting wage trajectories.

Only few studies document the impact of automation on individual workers, often focusing on the effects of industrial robots within manufacturing (e.g., Acemoglu et al., 2023; Dauth et al., 2021; Humlum, 2022). For instance, Acemoglu and Restrepo (2019) find modest wage increases and stable separation rates among a small sample of Dutch workers in robot-adopting firms, highlight wage compression for directly-affected blue-collar workers and wage increases for indirectly-affected workers. Similarly, Dauth et al. (2021) observe that German workers transition to higher-wage positions within their firms post-robot adoption, rather than separating.

This chapter extends these findings by analysing automation costs within the comprehensive matched employer-employee data from the Netherlands. We identify higher separation probabilities across all workers and document significant differences in the impacts of automation on workers in MNEs and domestic firms. The broad spectrum of technologies beyond just industrial robots that automation costs capture may partially explain the higher separation rates that we find. In complementary analyses, we show that firms' investments in Machinery lead to lower separation rates than automation costs. The discrepancy may also stem from the related studies' counterfactual of workers in non-automating firms. Instead, we closely match incumbent workers in automating firms to those in firms that automate later, recognizing that the trajectories of automating and non-automating firms diverge significantly. Moreover, by employing the full matched employer-employee data of the Netherlands, we track all workers of automating firms. This allows for a novel examination of the distinct distributional consequences of automation within and across firm types. Our findings show that automation in MNEs leads to substantial worker separations but also higher wage gains for remaining high-skilled employees, including managers. Conversely, domestic firms experience fewer separations but wage declines, especially among lower-skilled workers.

Most related to our study is Bessen et al. (2023), who examine the broad labor market implications of automation cost spikes in Dutch firms, highlighting important general effects such as total earnings losses and increased non-employment. Building on this, we employ newer, more detailed employer-employee matched data, enabling us to dissect within-firm impacts on hourly wages, a more direct measure of productivity effects than workers' total earnings. Our within-firm perspective reveals that while remaining workers in MNEs experience wage growth, potentially reflecting productivity increases, those in domestic firms face wage declines. Particularly, automation in MNEs disproportionately benefits managers, suggesting significant organizational changes in MNEs' response to automation. Moreover, our findings uncover that separation rates in domestic firms align with those found by Bessen et al. (2023), but MNEs exhibit substantially higher separations of up to 24%. In an additional firm-level analysis, we show that these elevated separations are not offset by corresponding increases in hiring.

Our findings on automation in MNEs also contribute to the literature on technological change within multinationals (e.g., Guadalupe et al., 2012; Koch et al., 2021). Guadalupe et al. (2012) show that foreign-acquired firms in Spain increase innovation and adopt foreign technologies. Using a similar sample, Koch et al. (2021) document that foreign acquisitions raise the level of technology and average skill of the firms' workforce. By leveraging variations in firms' automation events, we provide causal evidence at the worker level, indicating that automation leads to job displacement but also productivity increases, as evident in the higher wages that MNEs pay to managers and high-skilled workers post-automation.

4.2 Data

We construct a yearly employer-employee matched dataset for the Netherlands covering the period from 2010 to 2021. Our dataset merges a firm-level survey on firms' investments in automation with detailed administrative databases from

Statistics Netherlands. The data allows us to track worker movements in and out of employment across all firms, combined with the employers' MNE and domestic firm status, and importantly, their investments in automation.

Data on firm-level automation derives from the "Production Statistics" (Productiestatistiek), an annual survey of non-financial private firms that is available from the year 2000. This survey captures all firms with more than 50 employees and a sample of smaller firms, including a question on automation costs. Automation costs refer to payments for external automation services, including equipment and software not recorded as assets on the firm's balance sheet. While the exact automation technologies are not directly observed, automation costs serve as an official book-keeping entry that allows us to accurately identify firms' investments in automation. Using data from another small firm-level survey by Statistics Netherlands, Bessen et al. (2023) demonstrate that these costs correlate with process innovations within firms.[1] This includes a wide-range of technologies such as Customer Relationship Management (CRM) and Enterprise Resource Planning (ERP), as well as technologies for electronic data processing and big data analysis. Importantly, automation costs contrast with measures based on the import of automation technologies, which only capture imports of physical automation technologies.

The design of the automation cost survey implies that larger firms are sampled more frequently. We focus on firms that report automation costs at least every third year over the period 2010 to 2021, mitigating the influence of reporting differences when we compare lumpy investments in automation costs between MNEs and domestic firms. In addition, we remove firms that automated for the first time before 2010 (see Section 4.3.1). We link automation costs with other firm-level information, such as ownership structure, total exports, imports, and industry classification, using firms' unique identifiers. We define a firm as an MNE if a non-Dutch entity controls it or if it reports foreign affiliates. In addition, we omit firms affected by events like mergers, spin-offs, or changes in MNE status that could lead to inconsistent firm-worker linkages over time.

[1]Bessen et al. (2023) provide detailed descriptive statistics on automation costs. We discuss differences between MNEs and domestic firms in Section 4.3.1.

At the worker level, we draw our primary data from the Polisadministratie, which is a comprehensive dataset compiled from all Dutch employers' mandatory reports to the Employee Insurance Agency (Uitvoeringsinstituut Werknemersverzekeringen) and tax authorities. Alongside workers' contract type (temporary or permanent), the dataset provides details on total income and hours worked, allowing us to calculate hourly wages that include base, overtime, and bonus pay. We augment the worker data with information on demographics, socio-economic status, and highest education. Education is observed for about 60% of workers, providing details on education level and broad field according to the International Standard Classification of Education (ISCED) 2013 framework.[2] We identify workers that are ever employed at a firm with recorded automation costs and select their full earnings history across all firms, including those without recorded automation costs. For workers with multiple employers within a year, we determine the primary employer based on continuous employment at a firm with observed automation cost, highest income, most hours worked, and whether the workers holds a permanent contract. We exclude any firm-worker relationships with gaps, instances where workers' socio-economic status indicates they are students, and the full earnings history of workers with annual log hourly wage changes outside the -1 to 1 range.

The final dataset includes 22,344 firms that employ around 2.1 million workers annually, covering roughly 30% of the Dutch labor market. Although MNEs constitute only about 20% of these firms, they account for approximately 50% of the annual employment captured in the data. Table 4.1 in Appendix 4.A shows that MNEs not only pay higher hourly wages - approximately 37% more on average than domestic firms - but also employ a higher proportion of workers with a University degree. Despite similar shares of automation costs relative to total costs between MNEs and domestic firms (around 0.55%), MNEs spend nearly 50% more on automation per worker. About 40% of MNEs operate in industries such as Manufacturing, Transportation and Storage, and Construction that often employ physical automation technologies, while 60% operate in Service industries that more often focus more on

[2]Availability of education data is skewed towards younger and Dutch workers.

software-based automation technologies. For domestic firms, the share of firms in Manufacturing- or Service-related industries is similar at 35% and 65%.

4.3 Difference-in-differences framework

We employ a triple difference-in-differences regression to assess the effects of automation within MNEs and domestic firms on two worker-level outcomes: the probability of firm separation and the trajectory of hourly wages among those remaining with the firm. The main empirical challenges are identifying automation events and estimating the unobserved counterfactual outcomes for individual workers had their employer not automated, while distinguishing between MNEs and domestic firms. Extending the approach of Bessen et al. (2023), our regressions leverage lumpy investments in automation costs, so-called automation cost spikes, to closely compare incumbent workers at automating firms to their matched counterparts at firms that automate later. Sections 4.3.1 and 4.3.2 detail the definition of spikes and the matching procedure in our setting. Our empirical specification is

$$\begin{aligned} y_{igt} = {} & \alpha_i + \alpha_{gt} \\ & + \sum_{c \in C} \sum_{s=-3, s \neq -1}^{4} \delta_s^c \times auto_{igs}^c \times treated_i \\ & + \sum_{c \in C} \sum_{s=-3, s \neq -1}^{4} \gamma_s^c \times auto_{igs}^c + \mathbf{x}_{it}\beta + \epsilon_{igt}, \end{aligned} \tag{4.1}$$

where i, g, and t denote worker, matched group, and calendar year, respectively; y_{igt} is the outcome of interest (worker separation and log hourly wage of non-separated workers). The indicators $auto_{igs}^c$ identify the relative timing s from three years before to four years post the automation year, separately by firm type c: MNE or domestic. The year before the automation year serves as the reference. Similarly, the indicator $treated_i$ identifies treated workers. Vector $\mathbf{x}_{it}$ includes the control variables age and its square, delineated by the workers' contract type in the year before automation.

Finally, ϵ_{igt} is an error term.

We employ specification (4.1) to isolate the impact of automation relative to the control workers by the type of the firm c: MNE or domestic. The coefficients δ_s^{MNE} measure the automation effect on MNE workers, while the $\delta_s^{Domestic}$'s measure the impact on domestic firm workers. The differential effect on MNE workers compared to domestic firm workers is $\delta_s^{MNE} - \delta_s^{Domestic}$. We prevent these estimates from capturing firm-type-specific variation in outcome developments unrelated to automation through the coefficients γ_s^c (De Chaisemartin and D'haultfœuille, 2023; Olden and Møen, 2022).

We use two distinct outcomes: an indicator whether worker i in year t has left employment at the automating firm, and the log hourly wage of workers who stay with the firm over the entire post-automation period.[3] The former examines the cumulative probability of worker separation due to automation, while the latter captures changes in wage growth.

The difference-in-differences comparison is conditional on two fixed effects: First, α_i is a worker-specific effect that accounts for unobserved, time-invariant differences between workers. Second, α_{gt} is a time-varying effect for each group g of matched treated and control workers. With this fixed effect, treated workers' outcome developments are estimated relative to the developments in the matched control workers. This prevents biases from time-varying omitted variables, such as common demand shocks, from contributing to the identification of the impact of automation. As we match workers from firms undergoing automation to those in firms that automate later, the fixed effect also avoids "forbidden comparisons" between workers treated at different times (de Chaisemartin and D'Haultfœuille, 2020). The matching procedure is detailed in Section 4.3.2.

When estimating specification (4.1), our interest lies in the effect of automation on treated workers. To account for many-to-many matching, we assign weights to control workers according to the ratio of treated to control workers within group g

[3]Specifically, we require the worker to remain with the firm until at least the fifth year after the automation year.

(Stuart, 2010).[4] Additionally, we cluster standard errors at the level of the firm where a worker is employed before the automation event.

4.3.1 Automation events

Specification (4.1) requires the definition of an automation event. We adapt the concept of automation cost spikes of Bessen et al. (2023) to the comparison between MNE and domestic firm workers. According to their approach, a firm experiences an automation cost spike in a given year τ if its real automation costs AC, relative to its average operating costs across all years $\overline{TC}$ (excluding the automation costs), are at least three times the firm's average ratio of automation costs to total operating costs across all other years. As such a spike is defined as

$$spike_{j\tau} = \mathbb{1}\left\{\frac{AC_j}{\overline{TC}_j} \geq 3 \times \frac{1}{T-1}\sum_{t\neq\tau}\frac{AC_j}{\overline{TC}_j}\right\}, \tag{4.2}$$

where $\mathbb{1}$ is the indicator function.

Leveraging automation cost spikes in the difference-in-differences comparison assumes that equation (4.2) captures the timing of firms' automation events. We focus on a firm's first automation spike by identifying its spikes on the full automation cost data, which ranges from 2000 to 2021.[5] As additional firm- and worker-level data is only available starting in 2010, we use the pre-2010 data to filter out firms that had an automation event before 2010. Furthermore, the automation cost survey captures larger firms more frequently, whereby the timing of MNEs' automation events may be estimated more precisely. To mitigate the influence of reporting frequency on our estimates, we include only those MNEs and domestic firms that report automation costs at least every third year over the period 2010 to 2021.[6] This strategy ensures that we observe a firm's automation costs at least three times during the eight-year event window in specification (4.1).

4

[4]The distribution of OLS weights among control workers is shown in Table 4.5 in Appendix 4.C.
[5]Figure 4.7 in Appendix 4.B shows a visual depiction of the average spike in automation cost shares.
[6]Roughly 63% of MNEs report automation costs yearly, 91% at least every second year, and 97% at least every third year. For domestic firms, the shares are 18%, 63%, and 86%.

About 40% of the 22,343 firms in the post-2010 data spike at least once. Table 4.2 in Appendix 4.B shows the distribution of spikes, differentiated by MNEs and domestic firms. While the share of automating firms is similar, there are differences in the distribution of serial spikes between the two firm types. Within the group of domestic firms, 29% spike exactly once and 11% feature more than one spike. In contrast, MNEs exhibit a higher frequency of serial spikes, with only 22% of MNEs exhibiting a single spike and 18% featuring multiple spikes in the data. This complicates the event study design in specification (4.1) if firms with serial spikes respond differently to automation. Hence, when estimating the specification, we difference out the common automation effect for workers that experience multiple spikes during the event window.

MNEs and domestic firms also differ in automation expenditures and industry at the automation event. The average ratio of MNEs' automation cost to their usual automation costs is 10.4 (SD = 40.8), which significantly exceeds the average ratio of 8.2 (SD = 18.7) among domestic firms; $t(2144) = 2.2$, $p = 0.03$. Similarly, Figure 4.8 in Appendix 4.B depicts the automation cost shares and expenditures per worker. While the distributions of automation cost shares in Panel (a) largely overlap, the distribution of automation costs per worker, depicted in Panel (b), is clearly rightward shifted. This suggests that MNEs may invest more heavily in automation than domestic firms.

Regarding industry, most automation events occur in Wholesale and Retail Trade for both MNEs and domestic firms (see Figure 4.9 in Appendix 4.B). Manufacturing also shows a considerable number of automation events in MNEs, while domestic firms' automation events are more skewed towards the Construction and Administrative and Support Activities industries. In Section 4.4.4, we show that differences in automation intensity and 2-digit industry do not impact our findings.

4.3.2 Causality and event data

The causal interpretation of the difference-in-differences estimates in (4.1) relies on the assumptions that workers do not anticipate automation and that, without automation, outcomes for treated and control workers would have followed parallel trends.

To mitigate anticipation bias, we focus on incumbent workers who have been with their current firm for at least three years prior to an automation event, excluding recent hires. Recent hires might have selected into the firm in anticipation of the automation event, while anticipatory effects are less likely for incumbents, as their employment predates the firm's decision to automate by a significant margin (Bessen et al., 2023).

We address parallel trends in several ways. First, in Appendix 4.D we show that automating and non-automating firms are on different growth paths, similar to the results in Bessen et al. (2023). Consequently, workers in non-automating firms are not a good counterfactual for workers in automating firms. Instead, we rely on incumbent workers in firms that automate later as a more suitable proxy for the counterfactual. Hence, our difference-in-differences approach leverages only the variation in the timing of automation events for identification. By fixing the comparisons, this approach also circumvents issues of negative weights in difference-in-differences regressions with staggered adoption of treatment, such as ours (for an overview see e.g., Roth et al., 2023).

To operationalize the approach, we construct a stacked dataset that aligns incumbent workers by automation year cohorts (Baker et al., 2022): Firms (and workers) undergoing an automation event in year p are matched with those that will automate in years $p + c$ (where $c \geq 5$). We require each treated and each control firm to be observed over the entire event period, from three years before to four years after the automation year. In addition, we require the firm to survive in the fifth year to avoid picking up firm closures with our estimates. Given the potential for workers to be included in the dataset multiple times due to their presence in different automation year cohorts, we introduce unique identifiers for each worker-cohort combination. The inclusion of a time-invariant fixed effect at the worker-cohort level, α_i, in specification (4.1) adjusts for repeated observations across different treatment cohorts.

Second, we match incumbent workers on pre-automation outcomes, combining exact and coarsened exact matching (Iacus et al., 2012). Different from earlier literature that studies general labor market implications of automation, we employ specification (4.1) to estimate the adjustment margins within firms, which requires a close counterfactual at both the firm- and worker-level. At the firm-level, we control for business cycle fluctuations and industry-specific automation technologies by matching on calendar year and 2-digit NACE industry. To control for firm-size related differences in wages and employment, we also match on every 10th percentile of the firm employment size distribution, including a separate group specifically for the 99th percentile. Within each group of matched firms, we further refine the control group by matching individual workers on every 10th percentile and the 99th percentile of the log hourly wage distribution and its two preceding years. As specification (4.1) includes matched-group-year fixed effects, our strategy closely compares workers who are on similar wage trajectories in comparable firms prior to automation.

The descriptives of the matched data are in Table 4.3 in Appendix 4.C. Table 4.4 in the same Appendix additionally shows descriptive statistics for treated MNE and domestic firm workers separately. The table highlights that MNEs and domestic firms differ in their involvement in international trade, automation costs, and industry distribution. In Section 4.4.4, we check whether these observable differences explain differences in the impact of automation on MNE and domestic firm workers.

4.4 Main Results

In our main set of results, we estimate the impact of automation on MNE and domestic firm workers' probability to separate, and their hourly wages, conditional on staying in the firm. We also document heterogeneity by broad sectors. We further examine the potential influences of workers' contract flexibility and firms' international trade activities, automation investment intensity or industry in Sections 4.4.3 and 4.4.4. Finally, Section 4.4.5 presents estimates on the number of new hires and their average wage at the firm-level.

4.4.1 Separations and wages

Figure 4.1 depicts the results of a difference-in-differences comparison (as specified in equation (4.1)) on the impact of automation on workers' cumulative separation probability and hourly wages, compared with the matched control workers. Black estimates depict a difference-in-differences comparison for workers employed at an MNE before the automation event, while gray estimates show those for workers at domestic firms. Full regression results, including estimates on the difference between MNE and domestic firm workers, are in Table 4.7 in Appendix 4.E.

Panel (a) of Figure 4.1 depicts the cumulative separation probability after the automation event, differentiated by firm type (MNE or domestic). The estimates reveal an increase in separations of MNE workers post-automation compared to the matched controls. Specifically, in the year of automation, the probability for MNE workers to separate is about 10 percentage points higher than for the control worker.[7] Across the first to fourth year post-automation, this gap stabilizes at about 24 percentage points, implying nearly immediate separations of about 24% of the MNE workforce due to automation. In contrast, the estimates for domestic firm workers are about half the size of those for MNE workers: They rise from 3 percentage points in the automation year to 11 percentage points by the fourth year.[8] All estimates are statistically significant. Given that 34% of control workers separate by the fourth year, these results suggest that automation in MNEs leads to about a 70% increase in separations over five years, while automation in domestic firms leads to an increase in separations by about a third.

Panel (b) of Figure 4.1 examines the effect of automation on hourly wages for workers remaining with the firm post-automation. The estimates for MNE workers are positive and statistically significant in the third and fourth year post-automation, where they indicate that automation leads to about 1.6% ($\approx exp(0.016) - 1$) higher wage growth in MNEs, compared to the control group. For domestic firm workers,

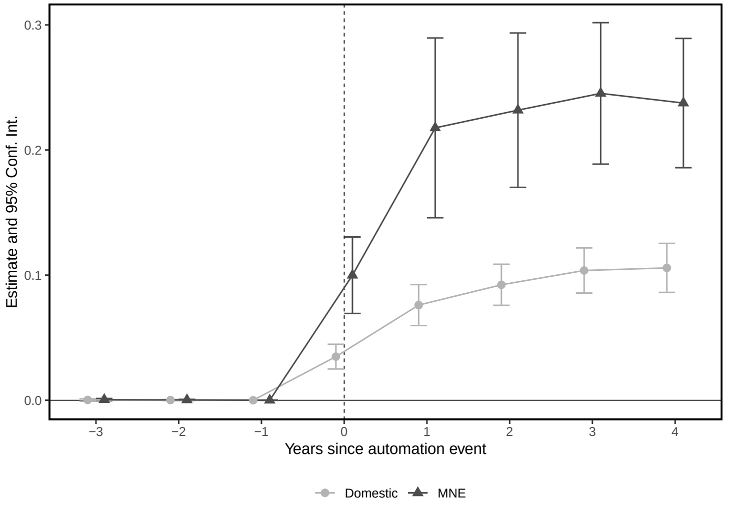
[7]This estimate could capture a partial treatment effect as we do not observe the exact timing of the automation event within the automation year.

[8]This trend is consistent with Bessen et al. (2023)'s findings, which imply a cumulative separation rate of about 8% by the fourth year for a sample of automation spikes between 2003-2011 in the Netherlands. Bessen et al. (2023) do not distinguish between MNEs and domestic firms.

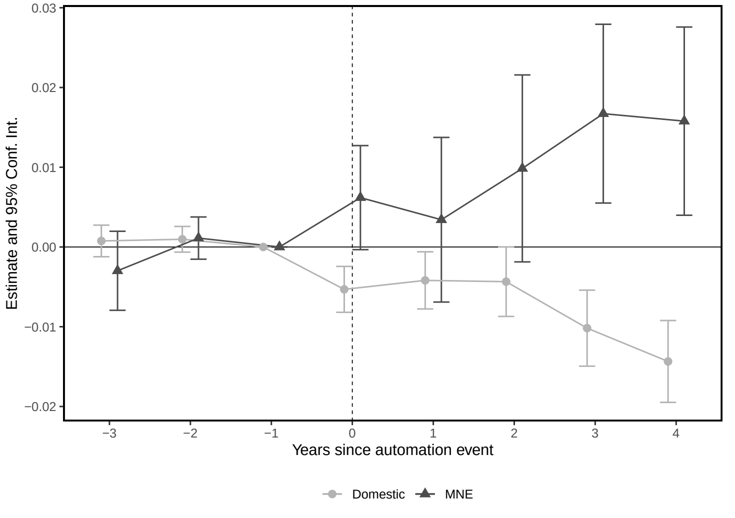
wage growth declines after an automation event. Specifically, there is a 0.5% reduction in wage growth in the automation year, deepening to a 1.4% decrease by the fourth year post-automation. Over the same period, control workers' wages increase by about 10%, suggesting that automation in MNEs increases wage growth by 16%, while it decreases wage growth in domestic firms by about 14%.

Taken together, these results imply MNEs and domestic firms adjust to automation differently: MNEs separate from a larger part of the incumbent workforce, while workers that stay within the MNE experience higher wage growth. On the other hand, domestic firms separate less but staying workers experience declining wage growth. Table 4.7 in Appendix 4.E confirms statistically significant differences, amounting to a 13 percentage-point higher probability of separation in MNEs (Column 1) and a 3% wage difference (Column 2) by the fourth year post-automation.

Figure 4.1: Separations and staying workers' wages.



(a) Separation probability.



(b) Log hourly wages of staying workers.

Notes: This plot presents difference-in-differences coefficients for MNE and domestic firm workers, see eq. (4.1). Full regression results are in Table 4.7 in Appendix 4.E. Dependent variables are an indicator whether a worker has left the automating firm (Panel (a)) and the log hourly wage of stayers (Panel (b)). MNEs include foreign firms and domestic firms with foreign subsidiaries. The regressions compare incumbent workers at automating firms with matched controls at firms that automate later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. Automation events are based on firms' costs on third-party services; see Section 4.2. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

4.4.2 Sector level heterogeneity

We document how the impact of automation on MNE and domestic workers differs within Manufacturing and Service industries. We adopt a broad definition for the Manufacturing sector that includes the NACE industries Manufacturing, Construction, and Transport and Storage (Acemoglu et al., 2023). Automation within this sector typically involves physical technologies, such as industrial robots, aiming to enhance production efficiency. Conversely, the Service sector leans towards software-based automation technologies, focusing on automating administrative tasks, information processing, and customer service interactions.[9]

Panel (a) of Figure 4.2 highlights a common trend across both sectors: MNE workers face higher separation probabilities compared to workers in domestic firms. Specifically, in Manufacturing, MNE workers face a separation probability of about 20 percentage points over the control workers, contrasted with approximately 13 percentage points for domestic firm workers. Similarly, in the Service sector, MNE workers face a separation probability of around 27 percentage points, significantly higher than the roughly 9 percentage points for workers in domestic firms.

The wage developments in Panels (b) and (c) of Figure 4.2 also reflect divergent impacts of automation. In both sectors, MNE workers experience wage increases after automation that range from approximately 1.3 to 2.0%, with a notable delay in wage increases within the Service sector. Automation in domestic firms within the Manufacturing sector does not result in significant wage adjustments (Panel (b)). Conversely, in the Service sector, wages decrease by about 2% for workers in domestic firms post-automation, aligning with the overall trends depicted in Figure 4.1.

These results highlight two insights: First, the differential impact of automation in MNEs compared to domestic firms persists across sectors. Second, this differential effect is more pronounced in the Service sector, which may employ software-based automation more often.

[9]Service sectors include the NACE industries Wholesale and Retail Trade; Administrative and Support Activities; Information and Communication; Accommodation and Food Services; Professional, Scientific and Technical activities.

4.4.3 Separations of temporary and permanent contract workers

Dutch labor market regulations tightly control the termination of permanent contracts, leaving most flexibility in separations for temporary contracts.[10] This regulatory environment suggests that MNEs might leverage a more flexible workforce to adjust after automation, which may explain their higher separation rates in Figure 4.1.

We decompose separations by workers' temporary or permanent contract before automation by scaling the contract-type-specific separation probabilities by the respective share of contracts across MNE and domestic firm workers.[11] As a result, the estimates in Figure 4.3 measure the share of the workforce leaving with a particular contract type, and they add up to the total separation rates depicted in Figure 4.1 by construction.

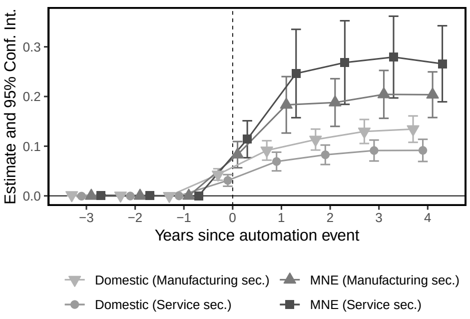
The estimates in Panel (a) of Figure 4.3 imply that about 1.5% of MNE workers separate with a temporary contract, explaining only 6% ($\approx 1.5/24$) of total separations in MNEs in Figure 4.1. This contrasts with the 3.4% of domestic firm workers, implying that separations of temporary contract workers explain about one third of total domestic firm separations. Conversely, the estimates in Panel (b) show higher separations of permanent contract workers in MNEs than in domestic firms: While 22% of MNE workers separate under a permanent contract, only 7% of domestic firm workers do so.

These findings rule out flexibility in workers' contract types as an explanation for the overall higher separation rates after automation in MNEs. This is not entirely surprising, as our focus on incumbent workers tends to select permanent contract workers. Moreover, only about 4% of MNE workers in the matched sample are on temporary contracts, compared to 20% in domestic firms (see Table 4.4 in Appendix 4.C).

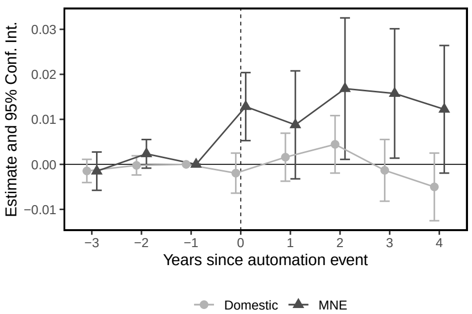
[10]Temporary contracts in the Netherlands are usually set for a period of a maximum of two years and can be terminated under a 30-day notice, depending on the collective labor agreement of the industry. For permanent contracts, obtaining approval from the Employee Insurance Agency (UWV) is a required step for terminations based on economic reasons, including organizational or technological changes like automation, see: https://www.uwv.nl/werkgevers/werkgever-en-ontslag/ik-wil-ontslag-aanvragen/detail/ontslag-via-uwv/ontslagaanvraag-wegens-bedrijfseconomische-redenen/onderbouwen-redenen-ontslag.

[11]We drop the interaction between age and contract type for this analysis.

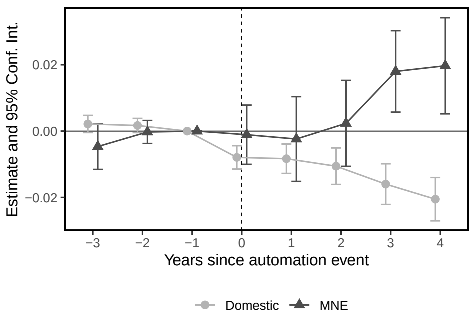
Figure 4.2: Manufacturing vs Service Sectors.



(a) Separations.



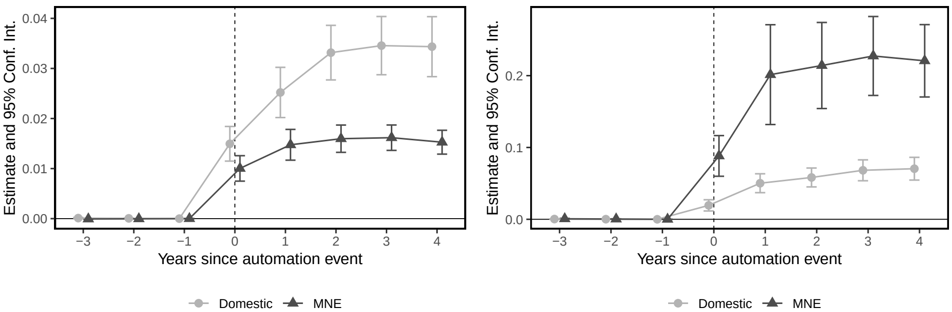
(b) Manufacturing sector: Stayer log wage.



(c) Service sector: Stayer log wage.

Notes: This plot presents difference-in-differences coefficients for MNE and domestic firm workers, based on eq. (4.1) with interactions for sectors. Manufacturing includes the NACE industries Manufacturing, Construction, and Transport and Storage. Service includes the NACE industries Wholesale and Retail Trade; Administrative and Support Activities; Information and Communication; Accommodation and Food Services; Professional, Scientific and Technical activities. Dependent variables are an indicator whether a worker has left the automating firm (Panel (a)) and the log hourly wage of stayers (Panels (b) and (c)). MNEs include foreign firms and domestic firms with foreign subsidiaries. The regressions compare incumbent workers at automating firms with matched controls at firms that automate later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. Automation events are based on firms' costs on third-party services; see Section 4.2. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

Figure 4.3: Separations by contract type.



(a) Temporary contracts.

(b) Permanent contracts.

Notes: This plot presents difference-in-differences coefficients for MNE and domestic firm workers, based on eq. (4.1) with interactions for workers' contract types (temporary or permanent) at 'Years since automation event' = -1. The dependent is both panels is an indicator whether a worker has left the automating firm. MNEs include foreign firms and domestic firms with foreign subsidiaries. The regressions compare incumbent workers at automating firms with matched controls at firms that automate later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. Automation events are based on firms' costs on third-party services; see Section 4.2. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

4.4.4 International trade, automation intensity and industries

We test three plausible explanations for the varying impacts of automation between MNEs and domestic firms in Figure 4.1: firms' international trade activities, the intensity of automation investments, and industry-specific automation technologies.

The related literature suggests that firms exposed to stronger competition in export and import markets invest more in automation (Kromann and Sørensen, 2019). Similarly, firms that adopt automation technologies are often more integrated into global supply chains, sometimes increasing offshoring activities to lower-income countries (Stapleton and Webb, 2020). Such dynamics, along with the distinct patterns of automation investment intensity and industry-specific deployment discussed in Section 4.3.1, might explain why MNEs exhibit distinct automation effects.

To difference out these related automation effects, we apply a variation of specification (4.1) that estimates the differential effect of automation in MNEs conditional on these common automation effects among MNEs and domestic firms. The adjusted specification is

$$
\begin{aligned}
y_{igt} = {} & \alpha_i + \alpha_{gt} \\
& + \sum_{s=-3, s\neq -1}^{4} \sigma_s^{MNE} \times auto_{igs}^{MNE} \times treated_i + \sum_{s=-3, s\neq -1}^{4} \gamma_s^{MNE} \times auto_{igs}^{MNE} \\
& + \sum_{g\in G} \sum_{s=-3, s\neq -1}^{4} \delta_s^{g} \times auto_{igs}^{g} \times treated_i + \sum_{g\in G} \sum_{s=-3, s\neq -1}^{4} \gamma_s^{g} \times auto_{igs}^{g} \\
& + \mathbf{x}_{it}\beta + u_{igt},
\end{aligned}
\tag{4.3}
$$

where the definitions in Section 4.3 apply.

Specification (4.3) includes a group-specific automation effect δ_s^g. This effect captures the impact of automation according to different groupings of all workers (and firms) in the data. We apply three types of groupings across different estimations. In a first set, we assign groups according to every 10th percentile of the export and import distributions before the automation event. In a second set, we assign groups according to every 10th percentile of the automation cost per worker and automation cost share distributions in the automation year. The final grouping captures automation effects at the granular 2-digit NACE industry level. If the MNE effect differs from the common automation effects captured by these groups, we should see this reflected in the MNEs' differential treatment effect σ_s^{MNE}.

We present aggregated automation effects across the trade and automation intensity groups in Tables 4.10 and 4.11 in Appendix 4.E. For expositional ease, we separately aggregate the estimates of groups below the median and those of groups above the median. Overall, the results in both tables show increased separations and declining wages. Moreover, these results suggest that higher trade volumes before the automation event associate with relatively lower separations and wage declines. Conversely, higher automation intensities associate with higher separations and wage declines.

Tables 4.8 and 4.9 in Appendix 4.E show the automation effect on separations and wages in MNEs, conditional on the related automation effects. In each table, Column 1 presents the main difference estimate using only domestic firms as reference. Columns 2 and 3 correct the difference estimate for automation effects related to firms' exports and imports. Columns 4 and 5 control for differences in automation intensity, and Column 6 for 2-digit industry effects.

The estimates show positive and significant differences for the MNE effect across all groupings. Controlling for treatment effects related to firms' imports and automation cost expenditures per worker leads to marginally smaller estimates on separations, while the difference estimates for wage growth of staying workers are very similar to the main estimate. Controlling for treatment effects related to firms' exports, automation cost shares, and 2-digit industry does not impact the estimates for both separations and wages.

4.4.5 Firm-level hires

Increased separations of incumbent workers may coincide with changes in firms' hiring practices. In particular, automating firms may increase their hires, potentially replacing incumbent workers with new workers of higher skill levels (Humlum, 2022). Moreover, adjustments in hiring may happen in anticipation of automation, or following automation. In this section, we provide descriptive evidence on the change in the count of new hires and their average wage around the automation event.

To examine hiring dynamics, we apply difference-in-differences specification (4.1) at the firm level within the matched data, with the reference year set to three years prior to the automation event to evaluate pre-automation adjustments in hiring. We exclude firms that automate in 2013 from this analysis, as we cannot identify their hires three years prior to automation. We account for zeros in the firm-level hiring data by estimating the model using Poisson regression.

Figure 4.10 in Appendix 4.E shows estimates on the change in the number of hires and their average wage at firm entry, relative to the levels three years before automation. The results on the numbers of hires in Panel (a) suggest a small increase in hires just before the automation event in MNEs, although the standard errors are large. This is followed by a substantial decline that amounts to about 50% fewer hires by the fourth year post-automation. Domestic firms show no sign of increased hiring before automation and a similar decline post-automation.

The wage trends for new hires in Panel (b) do not show significant trends in MNEs, indicating that wages for workers hired before and after the automation event are similar. However, in domestic firms, there is a notable decline in the average wages of new hires by about 6% post-automation. This wage reduction could result from overall wage adjustments within domestic firms, as evidenced by the wage decline of incumbent workers in Figure 4.1, or it may indicate a shift towards hiring lower-productivity workers.

Overall, these results do not indicate that separated incumbent workers are replaced by new hires. Three years before automation, treated MNEs average 21 new hires, compared to 12 in domestic firms, implying an overall modest influence of hiring on workforce dynamics within automating firms.

4.5 Dissecting the automation effects

While automation costs closely identify firms' expenditures on third-party automation services, the specific technologies used and their integration into the production processes of MNEs and domestic firms are unobserved. To address this, we analyze how automation impacts workers differently based on observed characteristics, such as managerial status and educational background. In Section 4.5.3, we employ a complementary dataset on firms' investments in ICT and Machinery to compare the impact of such investments with those inferred from the broader automation costs.

4.5.1 Firm hierarchy

Internationally-oriented firms are likely to adopt more standardized organizational structures (Caliendo and Rossi-Hansberg, 2012). Such standardization might come through automation and disproportionately benefit managers by enhancing their role in coordinating standardized processes (Mariscal, 2020). Hence, automation could lead to wage disparities and a shift towards higher hierarchical levels within firms, potentially affecting workers in MNEs more than those in domestic firms. We dissect the impact of automation on workers based on their managerial status to understand these dynamics.

We employ two distinct sources to identify managers. First, through the firm's Chamber of Commerce listing, we comprehensively identify members of the board of directors, owners, and upper management of each firm. Second, for a small sample of workers in each year, we observe ISCO08 occupations, which we use to complement the identification of managers. We classify a worker as a manager if, at any point in the firm-worker match, the worker is identified as a manager by either source. Only a few workers in the data are classified as managers: About 2% of MNE workers and 3% of domestic firm workers (see Table 4.4 in Appendix 4.C).

Figure 4.4 segments the impact of automation by workers' managerial status. Panels (a) and (b) show the separation rates of managers and non-managers, respectively. Both managers and non-managers separate from MNEs after automation.

Compared to the share of 23% of workers with a non-manager status separating from an MNE, separations of managers are small at about 0.23%, reflecting the small share of managers in the MNEs workforce. The estimates imply that managers face a 12% risk of separation ($\approx 0.23/2$), while non-managers face a substantially higher separation risk of 23%. In domestic firms, no impact of automation on separations of managers is visible, suggesting that separations in domestic firms are fully driven by non-managers.

Panels (c) and (d) of Figure 4.4 highlight clear differences in the impact of automation on managers' hourly wages in MNEs as compared to domestic firms. Post-automation in MNEs, managers that stay experience immediate and increasing wage growth, reaching 8% by the fourth year post-automation. There is a role for non-managers in MNEs, too. Wage effects of non-managers are visibly delayed but reach about 1.4% higher wage growth in third and fourth year. In domestic firms, managers' wages are unaffected by automation, suggesting that overall wage growth decline in domestic firms is driven by non-managers (see Panel (d)).

In sum, automation in MNEs benefits those higher up in the organizational hierarchy, through enhanced wage growth and relatively lower separation rates. Non-managerial workers, on the other hand, face higher layoff risks and see little wage growth. Domestic firms exhibit a different pattern, where automation impacts are largely confined to non-managerial workers.

4.5.2 Education level and background

Theories of skill-biased technological change imply that automation improves the productivity of higher-skilled workers, while it replaces that of lower-skilled workers. As workers' occupations and their skill content are unobserved in our data, we proxy skill level by educational attainment, which is observed for 50% of the workers in the matched data. We begin by analyzing the impact of automation on separation and wages, categorizing workers based on whether they hold a University degree. Subsequently, we categorize workers by their broad educational backgrounds in ICT and Sciences, Engineering, or non-technical fields. Following theories of skill-biased change, we expect that workers with university degrees and in technical fields experience fewer separations and higher wage growth post-automation (Acemoglu et al., 2023; Dauth et al., 2021; Katz and Murphy, 1992).

Separation rates, depicted in Panels (a) and (b) of Figure 4.5, vary by education level. To compare the relative impact on MNE and domestic firm workers with and without a university degree, we scale the separation probabilities by the respective share among workers with observed education. The estimates reveal a stark contrast: While about 12% of MNE workers with a University degree separate, the share is only about 3% for domestic firm workers. For those without University degrees, the separation rates are closer, at 12% for MNEs and 10% for domestic firms. This implies a uniform distribution of separations across educational levels in MNEs, whereas in domestic firms, separations are tilted towards workers without a University degree.

The wage impacts of automation also show significant variation by education and firm type, as indicated in Panels (d) and (c) of Figure 4.5. Post-automation, workers with a University degree experience higher wage growth of up to 5% at an MNE, while those in domestic firms see a modest 1% increase. Conversely, the negative wage impact of automation is predominantly felt by workers without a University degree, who face a 1.7% decline in wage growth in domestic firms, but no change in MNEs.

We further classify workers' educational backgrounds into ICT and Sciences, Engineering, or non-technical, following the International Standard Classification of Education (ISCED) 2013 framework.[12] Figure 4.6 details the automation effect differentiated by these educational backgrounds.

Panels (a), (c), and (e) present scaled separation rates per educational background.[13] They show that MNEs have higher separations across all educational backgrounds compared to domestic firms. The distribution of separations by education background

[12]Specifically, we categorize the ISCED2013 groups "Natural Sciences, Mathematics, and Statistics" and "Information and Communication Technologies" as "ICT and Sciences"; "Engineering, Manufacturing, and Construction" as "Engineering"; and the remaining groups as "non-technical". See https://www.cbs.nl/nl-nl/onze-diensten/methoden/classificaties/onderwijs-en-beroepen/standaard-onderwijsindeling–soi–/standaard-onderwijsindeling-2021 for an overview of the groups.

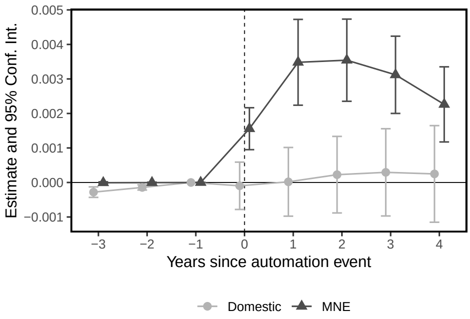
[13]The distribution of educational backgrounds is similar between MNE and domestic firm workers, with 8% (6%) of MNE (domestic firm) workers in ICT and Sciences, and 27% (26%) in Engineering.

within each firm type reveals similar shares relative to total separations: ICT and Sciences workers constitute 9% of total separations in MNEs (5% in domestic firms) and Engineering workers make up 23% (26% in domestic firms).

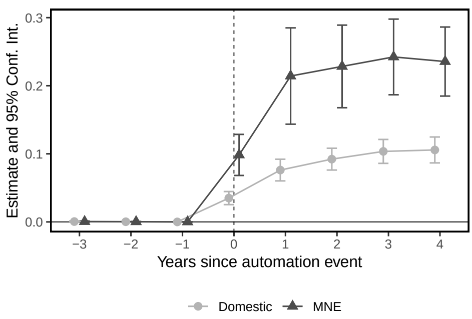
We examine wage impacts in Panels (b), (d), and (f). MNE workers with an ICT and Sciences background experience a significant wage growth of about 4.6% by the fourth year post-automation. Workers with Engineering or non-technical backgrounds in MNEs also see wage increases of 2.2% and 1.6%. Conversely, domestic firm workers in non-technical fields face a wage growth decline of about 1%, with minimal impacts on the wages of workers with ICT and Sciences or Engineering backgrounds.

These insights suggest that automation in MNEs predominantly benefits high-skilled workers, particularly those with a background in ICT and Sciences. This is reflected in higher wage growth of staying workers, but not separation rates, implying that skill-biased automation does not fully explain the impact of automation on MNE workers. Conversely, the evidence for domestic firms aligns more closely with theories of skill-biased technological change, as domestic firm automation displaces low-educated workers and benefits high-educated workers, both in relative wages and separations.

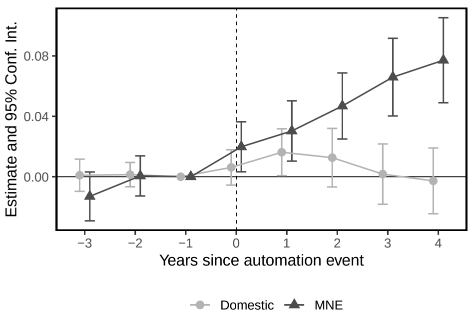
Figure 4.4: Managers vs. Non-managers.



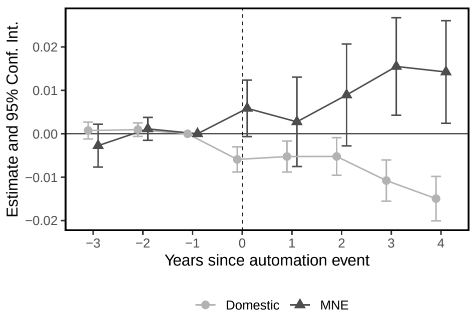
(a) Managers: Separations.



(b) Non-Managers: Separations.



(c) Managers: Stayer log wage.



(d) Non-Managers: Stayer log wage.

Notes: This plot presents difference-in-differences coefficients for MNE and domestic firm workers, based on eq. (4.1) with interactions for workers' manager status. Manager status is determined based on firm's Chamber of Commerce listing and workers' ISCO08 occupations; see Section 4.5.1 for details. Dependent variables are an indicator whether a worker has left the automating firm (Panels (a) and (b)) and the log hourly wage of stayers (Panels (c) and (d)). MNEs include foreign firms and domestic firms with foreign subsidiaries. The regressions compare incumbent workers at automating firms with matched controls at firms that automate later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. Automation events are based on firms' costs on third-party services; see Section 4.2. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

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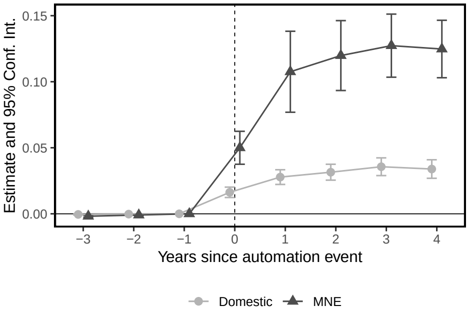
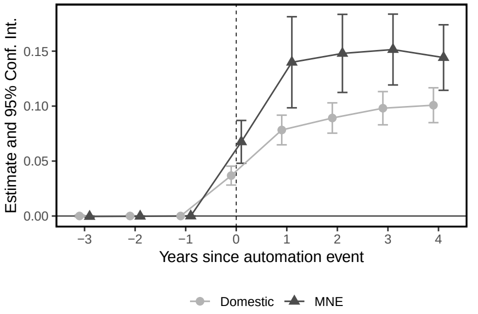
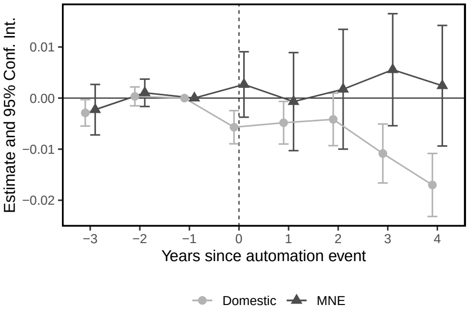
Figure 4.5: University vs. non-University educated workers.



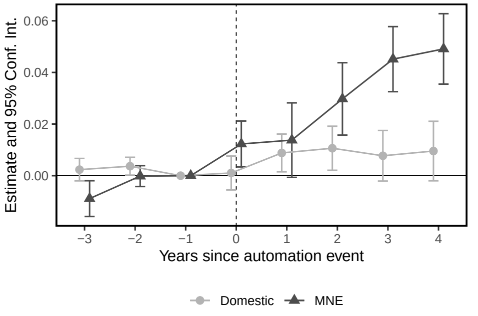
(a) University degree: Separations.



(b) No University degree: Separations.



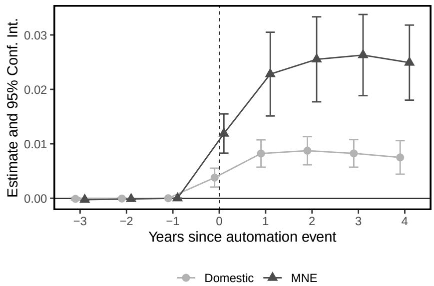
(c) No University degree: Stayer log wage.



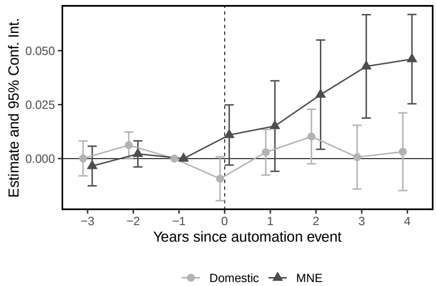
(d) University degree: Stayer log wage.

Notes: This plot presents difference-in-differences coefficients for MNE and domestic firm workers, based on eq. (4.1) with interactions for workers' education level (University, no University or unobserved). Estimates for workers' with unobserved education are omitted from the plot. Dependent variables are an indicator whether a worker has left the automating firm (Panels (a) and (b)) and the log hourly wage of stayers (Panels (d) and (c)). MNEs include foreign firms and domestic firms with foreign subsidiaries. The regressions compare incumbent workers at automating firms with matched controls at firms that automate later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. Automation events are based on firms' costs on third-party services; see Section 4.2. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

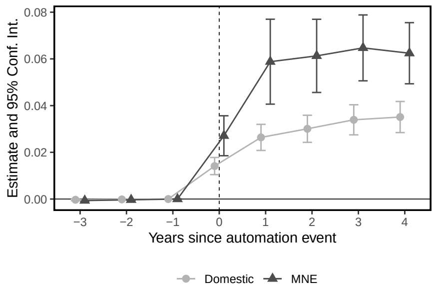
Figure 4.6: Technical vs. non-technical educational backgrounds.



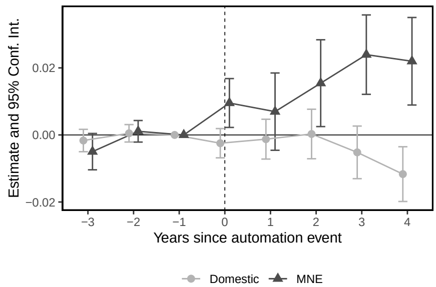
(a) ICT and Sciences: Separations.



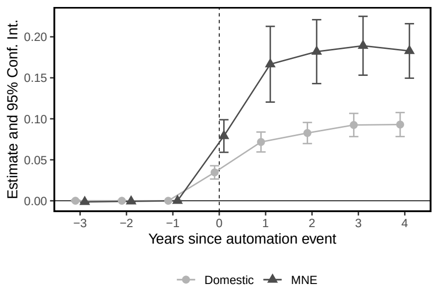
(b) ICT and Sciences: Stayer log wage.



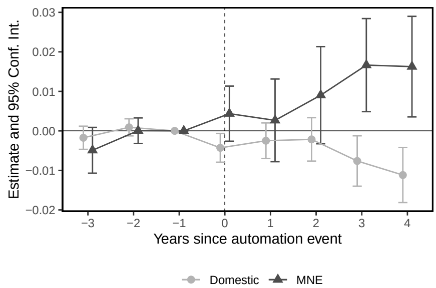
(c) Engineering: Separations.



(d) Engineering: Stayer log wage.



(e) Non-technical: Separations.



(f) Non-technical: Stayer log wage.

Notes: This plot presents difference-in-differences coefficients for MNE and domestic firm workers, based on eq. (4.1) with interactions for workers' educational background (ICT and Sciences, Engineering, Non-technical or unobserved). Estimates for workers' with unobserved educational background are omitted from the plot. "ICT and Sciences" comprises the ISCED2013 groups "Natural Sciences, Mathematics, and Statistics" and "Information and Communication Technologies (ICTs)"; "Engineering" comprises the ISCED2013 group "Engineering, Manufacturing, and Construction"; and "non-technical" all remaining ISCED2013 groups. Dependent variables are an indicator whether a worker has left the automating firm (Panels (a), (c) and (e)) and the log hourly wage of stayers (Panels (b), (d) and (f)). MNEs include foreign firms and domestic firms with foreign subsidiaries. The regressions compare incumbent workers at automating firms with matched controls at firms that automate later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. Automation events are based on firms' costs on third-party services; see Section 4.2. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

4.5.3 ICT and Machinery investments

The discussion in Section 4.4.2 indicates that the impact of automation on workers in MNEs and domestic firms differs most in the Service sector, which may employ ICT- rather than Machinery-related automation technologies more often. Although our automation cost data provides a comprehensive measure of automation, it does not distinguish between ICT and Machinery investments at the firm level. To gauge the differential effect of ICT and Machinery automation in MNEs and domestic firms, we analyze firms' investments in these technologies.

We employ another firm-level survey from Statistics Netherlands, which for the years 2012 to 2020 details firms' investments in Machinery, Computers, Communication equipment, and Software. The items include newly acquired, second-hand and in-house manufactured assets, as well as, standard, custom and self-developed software. This survey encompasses approximately two-thirds of the firms from the automation cost dataset, with a skew towards larger firms. We group investments in Computers, Communication assets, and Software as ICT investments, while investigating Machinery investments separately.

Following the methodology outlined in Section 4.3, we identify spikes in ICT and Machinery investments when a firm's investment share in these areas exceeds three times its average investment share in other years. We match incumbent workers at firms with investment spikes to those at firms that spike later. Then, we employ difference-in-differences specification (4.1) to assess the impact of these investment spikes on worker separations and hourly wages, focusing on spikes during the years 2013 to 2015.

The results in Figures 4.11 and 4.12 in Appendix 4.E show the dynamics of worker separations and wage growth surrounding ICT and Machinery investment spikes, differentiated by MNE and domestic firms. Specifically, ICT investments are associated with an approximate 6% increase in worker separations by the fourth year post-investment for both MNE and domestic firm workers. The impact on wage growth diverges between the two firms: ICT investments in domestic firms decrease hourly wage growth by about 1%, whereas in MNEs, the investment spikes lead to a

1% increase in wage growth. The difference is statistically significant, as shown in Table 4.12 in Appendix 4.E.

Conversely, spikes in Machinery investments do not exhibit a significant differential impact on MNE and domestic firm workers (Table 4.13 in Appendix 4.E). Both types show similar separation rates, with around 5% of the workforce separating by the fourth year post-investment, and no notable effect on the hourly wages of remaining workers.

These findings indicate that ICT investments, unlike Machinery investments, echo the broad pattern observed in automation costs in Figure 4.1. However, ICT investments do not account for the differing separation rates. In contrast, Machinery investments uniformly impact worker separations across both MNEs and domestic firms, while leaving wage trajectories largely unaffected.

4.6 Conclusion

We examine the role of a firm's multinational or domestic identity in shaping the consequences of automation for its workforce. Tracking workers within the matched employer-employee data of the Netherlands from 2010 to 2021, we leverage spikes in firms' third-party automation costs for identification (Bessen et al., 2023). Our triple difference-in-differences regressions compare incumbent workers at automating firms to their matched control workers at firms that automate later.

Our findings highlight that MNEs and domestic firms respond distinctly to automation. Specifically, automation in MNEs leads to a substantial increase in worker separations, with 24% of the incumbent workforce separating due to automation by the fourth year. This rate is significantly higher than the 11% separation rate among domestic firm workers. In addition, the remaining workforce in MNEs benefits from a wage premium of up to 1.6% on average, contrasting with the 1.4% wage decreases experienced by non-separated workers in domestic firms. We show that these patterns persist across Manufacturing and Service industries and are not offset by increased hiring in automating firms.

Our results resonate with those documented by Bessen et al. (2023) on the Dutch data, which imply a separation rate of around 8% over four years, similar to our estimate for domestic firms. The higher separation rate of 24% in MNEs aligns more closely with the literature on mass layoffs that highlights substantial separations rates in some firms (e.g., Dauth et al., 2021; Davis and Von Wachter, 2011). However, they contrast those of studies on foreign acquisitions, which often find negligible impacts on separations (e.g., Hijzen et al., 2013; Roesch et al., 2022), suggesting complex interactions between a firm's MNE status, its automation strategy, and labor market outcomes.

Domestic firms' responses to automation largely mirror the patterns predicted by theories of skill-biased technological change: The firm lays off relatively more lower educated workers and remaining workers' wages decline. In turn, high-educated domestic firm workers experience wage increases and relatively lower separations rates. In contrast, the adjustment process in MNEs is more complex, as they pay higher wages to high-educated workers but also separate from them frequently.

The distinct separation rates and wage adjustments in MNEs may hint at stronger selection effects within the MNE. MNEs may retain only a more productive subset of their workforce that complements the automation technology. This select group, in turn, is possibly in a better position to negotiate higher wages. In line with this, a plausible explanation for the observed divergence is that MNEs automate to standardize the organization of production (Caliendo and Rossi-Hansberg, 2012). Particularly in the context of ICT technologies, technological change may raise relative demand for managers' skills within the firm, while displacing non-managers (Mariscal, 2020). Within subsamples, we indeed document that managers wages rise disproportional compared to non-managers wages in MNEs, while managers' separation probabilities are relatively lower. The results also imply increased demand for ICT-related skills within the MNEs, as the wage of workers with an educational background in ICT and Sciences increases sharply. Finally, we find that spikes in firms' ICT investments increase wages in MNEs, while they decrease wages in domestic firms, suggesting that ICT investments lead to different adjustment mechanisms between MNEs and domestic firms.

There are various other possible explanations. One explanation is that MNEs may leverage a more flexible workforce. We find no evidence for this hypothesis, as only a minor fraction of the MNE workforce exits under temporary contracts, in stark contrast to domestic firms, which use temporary contracts more frequently for workforce adjustments. Another set of explanations suggests that MNEs may offshore more, invest more intensely in automation or are more active in industries that employ disruptive automation technologies. However, controlling for firms' exporter-, importer-, automation-intensity- or industry-specific automation effects does not account for the differential automation effect in MNEs and domestic firms. A related but unobserved explanation is that MNEs may leverage their global value chains to offshore services around automation.

Finally, the size of MNEs offers another layer of explanation. The presence of strong unions and collective labor agreements in larger firms in the Netherlands, such as MNEs, implies standardized salary scales and wage increases. While MNEs may not be able to downgrade workers to lower salary scales, productive workers can ascend quicker to higher levels. Unfortunately, our data does not allow us to test this hypothesis either.

Our findings highlight how globalization and automation relate to inequalities in local employment and wages. Post-automation, MNEs typically offer higher wages but retain fewer workers, primarily benefiting highly skilled workers, whereas domestic firm automation mostly affects low skilled workers. These observations suggest that policies aimed at attracting MNEs should not only focus on expected technology transfers but also consider strategies to protect less-skilled workers when firms automate, such as through enhanced training programs. Moreover, we find that observable differences do not fully account for MNE and domestic firms' differential response to automation. Data detailing the application of different automation technologies within firms would be invaluable for discerning the nuanced effects of automation on workforce dynamics and firm organization in relation to globalization.

Appendix to Chapter 4

4.A Summary statistics of the data

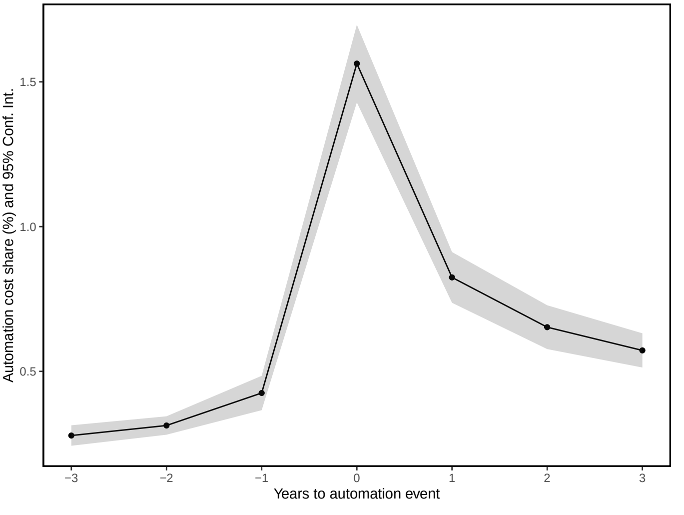
Table 4.1: Firm-level summary statistics by MNE and domestic firm status.

	MNE		Domestic	
	Mean	SD	Mean	SD
Firms	4,921		17,423	
Automation adopter	0.41	0.49	0.41	0.49
Automation cost share	0.61	1.12	0.52	0.77
Automation cost per worker real (1,000 EURs)	2.57	29.54	1.70	100.19
Sales per worker real (1,000 EURs)	1,264.84	13,014.28	267.35	449.00
Employment (full time equivalent)	234.24	1,140.36	58.56	201.62
Mean hourly wage real	32.05	11.50	23.45	6.06
Mean worker age	42.62	4.43	40.62	5.17
University Education Level (%)	18.86	14.32	10.96	15.31
Medium Education level (%)	19.21	9.86	21.19	13.12
Low Education level (%)	6.32	6.14	7.69	7.73
Unobserved Education level (%)	42.51	15.81	38.94	19.58
Wholesale and Retail Trade (%)	39.99		31.09	
Manufacturing (%)	25.91		11.47	
Information and Communication (%)	7.86		7.40	
Transportation and Storage (%)	8.92		7.51	
Prof., Scientific and Technical Activities (%)	7.01		10.78	
Administrative and Support Activities (%)	5.39		11.84	
Construction (%)	3.92		16.29	
Accommodation and Food Service (%)	1.00		3.61	

Notes: This table shows summary statistics of the full data; see Section 4.2 for details. Real values refer to 2021 EURs.

4.B Automation spikes

Figure 4.7: Automation cost share spike.



Notes: This plot depicts the average spike in the automation cost share in data. Automation cost shares are calculates as firms' real automation in a given year relative to its total operations costs (excluding automation costs) averaged across all years. Automation cost share spikes identify years that exceed three times the average cost share across all years; see Section 4.3.1 for details.

Table 4.2: Distribution of automation spikes in MNEs and domestic firms.

Domestic firms

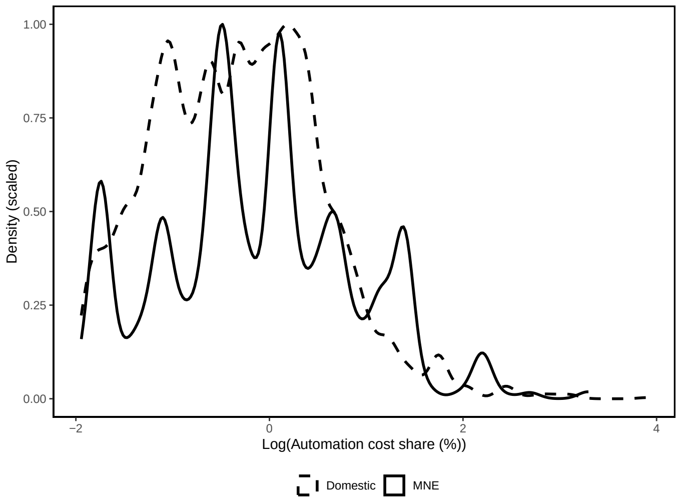
Automation spikes	Firms	Share of firms
0	10,335	59.32
1	5,063	29.06
2	1,590	9.13
3	355	2.04
≥ 4	80	0.46

Multinationals

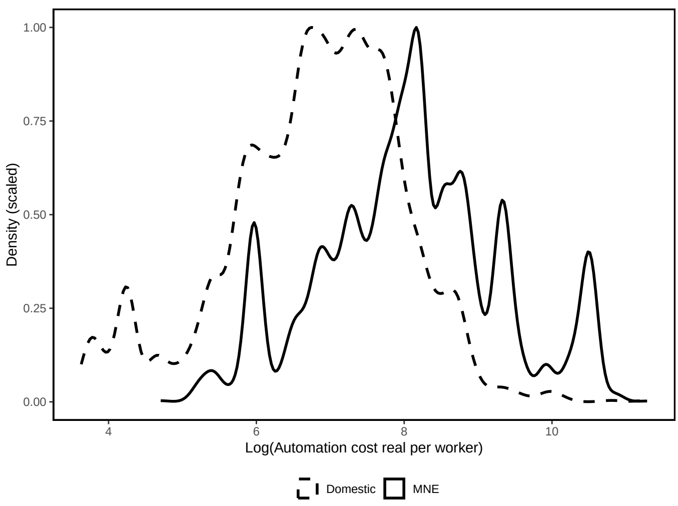
Automation spikes	Firms	Share of firms
0	2,912	59.17
1	1,100	22.35
2	619	12.58
3	223	4.53
≥ 4	67	1.36

Notes: The tables shows the distribution of the number of automation cost share spikes, differentiated by firms' MNE and domestic status. Automation cost shares are calculates as firms' real automation in a given year relative to its total operations costs (excluding automation costs) averaged across all years. Automation cost share spikes identify years that exceed three times the average cost share across all years; see Section 4.3.1 for details.

Figure 4.8: Automation costs at the automation event.



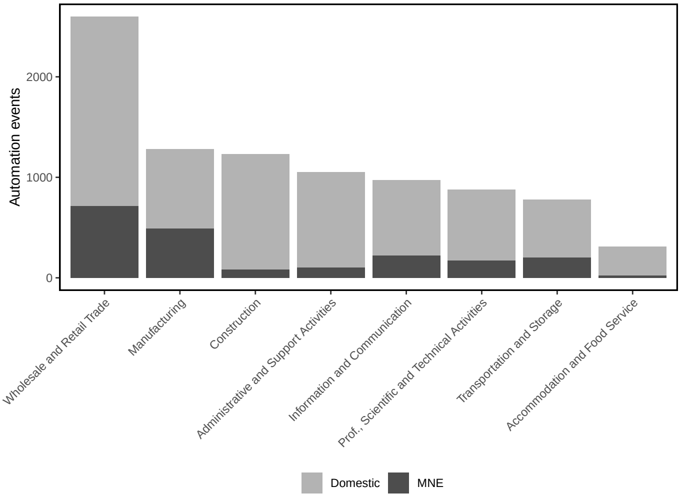
(a) Automation cost share.



(b) Automation costs per worker.

Notes: This plot depicts the distribution of automation cost shares and real automation costs per worker at the automation event, differentiated by firms' MNE and domestic status. Automation events constitute firms' first automation cost share spike. Automation cost shares are calculates as firms' real automation in a given year relative to its total operations costs (excluding automation costs) averaged across all years. Automation cost share spikes identify years that exceed three times the average cost share across all years; see Section 4.3.1 for details. Real values are in 2021 EURs.

Figure 4.9: Automation events in 2010 - 2021 by NACE sector.



Notes: This plot depicts the distribution of automation events - defined as the first spike in a firms' automation cost share - by NACE industry and firms' MNE and domestic status. Automation cost shares are calculates as firms' real automation in a given year relative to its total operations costs (excluding automation costs) averaged across all years. Automation cost share spikes identify years that exceed three times the average cost share across all years; see Section 4.3.1 for details.

4.C Matched sample

Table 4.3: Summary statistics, before and after matching.

	Before matching		Matched Control		Matched Treated	
	Mean	SD	Mean	SD	Mean	SD
N (workers)	667,391		184,548		53,259	
Unique workers	345,116		119,064		53,257	
N (firms)	131,36		7,439		1,605	
Unique firms	6,149		3,540		1,605	
N (matching groups)	667,391		13,526		13,526	
Female	0.31	0.46	0.27	0.44	0.27	0.45
Foreign born or foreign-born parents	0.14	0.35	0.12	0.32	0.14	0.34
Age	43.74	10.85	43.14	10.75	43.70	10.48
Temporary contract	0.12	0.32	0.12	0.32	0.11	0.31
Manager	0.02	0.14	0.03	0.17	0.02	0.15
Hourly wage real	25.81	18.01	28.52	18.58	28.55	19.08
Hourly wage growth	0.02	0.11	0.02	0.08	0.02	0.08
MNE	0.49	0.50	0.55	0.50	0.59	0.49
Exporter	0.70	0.46	0.72	0.45	0.72	0.45
Importer	0.81	0.39	0.84	0.36	0.83	0.38
Wholesale and Retail Trade (%)	24.57		27.06		27.06	
Manufacturing (%)	26.43		21.53		21.53	
Information and Communication (%)	4.87		7.09		7.09	
Transportation and Storage (%)	7.99		9.46		9.46	
Prof., Sci. and Tech. Activities (%)	6.08		6.62		6.62	
Admin. and Support Activities (%)	16.69		11.67		11.67	
Construction (%)	11.67		13.15		13.15	
Accom. and Food Service (%)	1.70		3.42		3.42	

Notes: This table shows summary statistics of the matched data in the pre-automation year; see Section 4.3.2 for details. Exporter and Importer are identifiers that take the value one if a firm's average real exports and imports exceed 10K EUR. Real values refer to 2021 EURs.

Table 4.4: Summary statistics, matched sample, treated MNE vs. domestic firm workers.

	MNE		Domestic	
	Mean	SD	Mean	SD
N (workers)	31,378		21,881	
Unique workers	31,378		21,880	
N (firms)	384		1,221	
Unique firms	384		1,221	
N (matching groups)	6037		8514	
Female	0.24	0.43	0.32	0.47
Foreign born or foreign-born parents	0.15	0.36	0.11	0.31
Age	44.17	9.99	43.02	11.12
Temporary contract	0.04	0.21	0.20	0.40
Manager	0.02	0.13	0.03	0.18
Hourly wage real	32.47	22.24	22.93	11.09
Hourly wage growth	0.02	0.09	0.02	0.07
MNE	1.00	0.00	0.00	0.00
Exporter	0.94	0.24	0.41	0.49
Importer	0.98	0.14	0.61	0.49
Wholesale and Retail Trade (%)	33.64		26.91	
Manufacturing (%)	29.72		13.94	
Information and Communication (%)	8.05		7.04	
Transportation and Storage (%)	10.97		7.83	
Prof., Scientific and Technical Activities (%)	6.71		6.31	
Administrative and Support Activities (%)	3.01		16.74	
Construction (%)	7.77		15.82	
Accommodation and Food Service (%)	0.13		5.41	

Notes: This table shows summary statistics of matched treated workers in the pre-automation year, differentiated by their MNE or domestic firm status; see Section 4.3.2 for details. Exporter and Importer are identifiers that take the value one if a firm's average real exports and imports exceed 10K EUR. Real values refer to 2021 EURs.

Table 4.5: Cumulative distribution of OLS weights among control workers.

	Weight
0%	0.004
10%	0.032
20%	0.055
30%	0.077
40%	0.102
50%	0.136
60%	0.179
70%	0.250
80%	0.333
90%	0.597
95%	1.000
99%	2.782
99.99%	17.000
100%	37.000

Notes: This table shows the cumulative distribution of weights assigned to control workers in the matched data. Weights are assigned according to the ratio of treated to control workers within a group of matched workers. Matching is based on 2-digit NACE industry, employment size, and log hourly wages (including two lags) in the pre-atuomation year; see Section 4.3 for details.

4.D Comparing automating to non-automating firms

We compare the firm employment, sales and wage growth trajectories of MNEs and domestic firms with and without an automation event. Following the approach in Bessen et al. (2023), we consider variations of the specification

$$
\begin{aligned}
\Delta lnY_{jts} = {} & \gamma_t + \gamma_s \\
& + \beta^{MNE} \times \text{automator}_j^{MNE} + \beta^{Domestic} \times \text{automator}_j^{Domestic} \\
& + \beta^{l} \times MNE_j + \mathbf{k_{jt}}\zeta + e_{jts},
\end{aligned} \tag{4.4}
$$

where j, t and s index firms, calendar year and two-digit sector; Y_{jt} is the outcome of interest; γ_t is a year fixed effect; and γ_s is a two-digit industry fixed effect. The dummies automator_j^{MNE} and $\text{automator}_j^{Domestic}$ identify MNEs and domestic firms with an automation cost spike, respectively. The dummy MNE_j dentifies MNEs. We include firms' initial (log) values of Y in the vector of control variables $\mathbf{k_{jt}}$. Finally, e_{jt} is an error term.

The estimation results are in Table 4.6. Our estimates confirm the results in Bessen et al. (2023) for automation events over the years 2010 - 2021: Automating compared to non-automating firms grow faster in employment. In addition, our data allows us to consider firm sales and firm-average hourly wages. We find that sales of automating firms grow faster than those of non-automating firms, while hourly wages do not grow faster at the firm level.

Taken together, the results in Table 4.6 imply that automating and non-automating firms are on different growth paths. This is why we use firms that automate later to proxy the counterfactual in specification (4.1); see Section 4.3 for details.

Table 4.6: Automating vs. non-automating firms.

	Employment		Sales		Hourly Wage	
	(1)	(2)	(3)	(4)	(5)	(6)
Automating & MNE	0.0134***	0.0134***	0.0137***	0.0095***	-0.0001	-0.0007
	(0.0024)	(0.0023)	(0.0038)	(0.0036)	(0.0007)	(0.0006)
Automating & Domestic	0.0114***	0.0123***	0.0152***	0.0114***	0.0000	-0.0009***
	(0.0014)	(0.0013)	(0.0018)	(0.0017)	(0.0003)	(0.0003)
MNE	-0.0140***	-0.0685***	-0.0100***	-0.0763***	0.0043***	-0.0306***
	(0.0018)	(0.0057)	(0.0028)	(0.0239)	(0.0005)	(0.0073)
Controls		✓		✓		✓
Fixed-effects						
Year	✓	✓	✓	✓	✓	✓
Industry (2 digit)	✓	✓	✓	✓	✓	✓
Observations	232,415	232,415	232,415	232,415	232,415	232,415
R^2	0.0190	0.0434	0.0079	0.0136	0.0083	0.0244

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. The table shows estimations of eq. (4.4). The regressions compare automating MNEs and domestic firms to non-automating firms. MNEs include foreign firms and domestic firms with foreign subsidiaries. 'Automating' identifies firms with an automation event; see Section 4.3.1. Dependent variables are log employment growth (Columns one, two), log sales growth (Coumns three, four), Log hourly wage growth (Columns five, six). Controls are initial log values of the dependent variables (year 2010). Standard errors are clustered at the firm level.

4

4.E Supporting tables and figures

Table 4.7: The effect of automation on workers in MNEs vs. domestic firms.

	Separation prob.	Stayer log wage
	(1)	(2)
event time = -3 × MNE × treated	0.0003	-0.0037
	(0.0007)	(0.0028)
event time = -2 × MNE × treated	0.0001	0.0001
	(0.0003)	(0.0016)
event time = 0 × MNE × treated	0.0651***	0.0115***
	(0.0170)	(0.0038)
event time = 1 × MNE × treated	0.1417***	0.0076
	(0.0393)	(0.0058)
event time = 2 × MNE × treated	0.1396***	0.0142**
	(0.0343)	(0.0067)
event time = 3 × MNE × treated	0.1416***	0.0269***
	(0.0320)	(0.0065)
event time = 4 × MNE × treated	0.1318***	0.0301***
	(0.0297)	(0.0069)
event time = -3 × treated	0.0002	0.0008
	(0.0005)	(0.0010)
event time = -2 × treated	0.0001	0.0010
	(0.0002)	(0.0008)
event time = 0 × treated	0.0348***	-0.0053***
	(0.0050)	(0.0015)
event time = 1 × treated	0.0761***	-0.0042**
	(0.0084)	(0.0018)
event time = 2 × treated	0.0923***	-0.0044*
	(0.0084)	(0.0022)
event time = 3 × treated	0.1037***	-0.0102***
	(0.0092)	(0.0024)
event time = 4 × treated	0.1058***	-0.0144***
	(0.0100)	(0.0026)
Controls	✓	✓
Fixed-effects		

Matched-group-year	✓	✓
Worker-cohort	✓	✓
event time-additional spikes-(MNE | Domestic)	✓	✓
Observations	1,902,456	1,252,686
R^2	0.6408	0.9768

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. This table presents difference-in-differences coefficients for MNE and domestic firm workers, see eq. (4.1). MNEs include foreign firms and domestic firms with foreign subsidiaries. Dependent variables are an indicator whether a worker has left the automating firm (Column one) and the log hourly wage of stayers (Column two). Controls include age and its square, delineated by the workers' contract type in the pre-automation year. The regressions compare incumbent workers at automating firms with matched controls at firms that automate later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. Automation events are based on firms' costs on third-party services; see Section 4.2. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

Table 4.8: Separation probability in MNEs, conditional on other automation effects.

Reference group	Main	Trade		Automation cost		Industry
	Domestic firms	Export	Import	per worker	share	
	(1)	(2)	(3)	(4)	(5)	(6)
event time = -3 × MNE × treated	0.0003	-0.0006	-0.0015*	-0.0003	0.0003	0.0000
	(0.0007)	(0.0008)	(0.0009)	(0.0008)	(0.0007)	(0.0008)
event time = -2 × MNE × treated	0.0001	-0.0003	-0.0007*	-0.0001	0.0001	0.0000
	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
event time = 0 × MNE × treated	0.0651***	0.0818***	0.0602***	0.0607***	0.0682***	0.0812***
	(0.0170)	(0.0169)	(0.0159)	(0.0181)	(0.0151)	(0.0184)
event time = 1 × MNE × treated	0.1417***	0.1530***	0.1047***	0.1200***	0.1405***	0.1685***
	(0.0393)	(0.0302)	(0.0278)	(0.0332)	(0.0309)	(0.0384)
event time = 2 × MNE × treated	0.1396***	0.1535***	0.1103***	0.1128***	0.1453***	0.1596***
	(0.0343)	(0.0289)	(0.0299)	(0.0302)	(0.0293)	(0.0342)
event time = 3 × MNE × treated	0.1416***	0.1493***	0.1017***	0.1109***	0.1452***	0.1429***
	(0.0320)	(0.0276)	(0.0301)	(0.0278)	(0.0279)	(0.0316)
event time = 4 × MNE × treated	0.1318***	0.1413***	0.0956***	0.0989***	0.1329***	0.1325***
	(0.0297)	(0.0252)	(0.0281)	(0.0250)	(0.0257)	(0.0290)
event time × group × treated	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Fixed-effects						
Matched-group-year	✓	✓	✓	✓	✓	✓
Worker-cohort	✓	✓	✓	✓	✓	✓
event time-MNE	✓	✓	✓	✓	✓	✓
event time-additional spikes-group		✓	✓	✓	✓	✓
Observations	1,902,456	1,902,456	1,902,456	1,902,456	1,902,456	1,902,456
R^2	0.6408	0.6442	0.6432	0.6455	0.6451	0.6475
Adjusted R^2	0.5609	0.5649	0.5637	0.5666	0.5661	0.5687

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. This table presents difference-in-differences coefficients for MNE workers, see eq. (4.3). Column one shows the main difference estimate; Columns two to five adjusts the reference group by export, import, automation cost per worker, automation cost share percentile groups; Column six adjusts the reference group at the 2-digit NACE industry level. MNEs include foreign firms and domestic firms with foreign subsidiaries. The dependent variable is an indicator whether a worker has left the automating firm. Controls include age and its square, delineated by the workers' contract type in the pre-automation year. The regressions compare incumbent workers at automating firms with matched controls at firms that automate later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. Automation events are based on firms' costs on third-party services; see Section 4.2. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

Table 4.9: Log wage of stayers in MNEs, conditional on other automation effects.

Reference group	Main	Trade		Automation cost		Industry
	Domestic firms	Export	Import	per worker	share	
	(1)	(2)	(3)	(4)	(5)	(6)
event time = -3 × MNE × treated	-0.0037	-0.0063**	-0.0070**	-0.0048**	-0.0041*	-0.0044
	(0.0028)	(0.0031)	(0.0028)	(0.0023)	(0.0025)	(0.0030)
event time = -2 × MNE × treated	0.0001	0.0006	0.0007	-0.0008	0.0000	0.0017
	(0.0016)	(0.0019)	(0.0020)	(0.0017)	(0.0019)	(0.0017)
event time = 0 × MNE × treated	0.0115***	0.0052	0.0135***	0.0118***	0.0136***	0.0073*
	(0.0038)	(0.0044)	(0.0043)	(0.0037)	(0.0039)	(0.0039)
event time = 1 × MNE × treated	0.0076	0.0032	0.0123*	0.0110*	0.0091	0.0045
	(0.0058)	(0.0067)	(0.0071)	(0.0059)	(0.0059)	(0.0053)
event time = 2 × MNE × treated	0.0142**	0.0112	0.0148*	0.0171***	0.0164**	0.0161**
	(0.0067)	(0.0076)	(0.0078)	(0.0066)	(0.0069)	(0.0064)
event time = 3 × MNE × treated	0.0269***	0.0246***	0.0272***	0.0310***	0.0307***	0.0308***
	(0.0065)	(0.0073)	(0.0073)	(0.0062)	(0.0067)	(0.0068)
event time = 4 × MNE × treated	0.0301***	0.0298***	0.0279***	0.0333***	0.0350***	0.0326***
	(0.0069)	(0.0067)	(0.0081)	(0.0068)	(0.0073)	(0.0071)
event time × group × treated	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Fixed-effects						
Matched-group-year	✓	✓	✓	✓	✓	✓
Worker-cohort	✓	✓	✓	✓	✓	✓
event time-MNE	✓	✓	✓	✓	✓	✓
event time-additional spikes-group		✓	✓	✓	✓	✓
Observations	1,252,686	1,252,686	1,252,686	1,252,686	1,252,686	1,252,686
R^2	0.9768	0.9769	0.9769	0.9770	0.9769	0.9772
Adjusted R^2	0.9715	0.9717	0.9717	0.9717	0.9717	0.9720

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. This table presents difference-in-differences coefficients for MNE workers, see eq. (4.3). Column one shows the main difference estimate; Columns two to five adjusts the reference group by export, import, automation cost per worker, automation cost share percentile groups; Column six adjusts the reference group at the 2-digit NACE industry level. MNEs include foreign firms and domestic firms with foreign subsidiaries. The dependent variable is the log hourly wage of stayers. Controls include age and its square, delineated by the workers' contract type in the pre-automation year. The regressions compare incumbent workers at automating firms with matched controls at firms that automate later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. Automation events are based on firms' costs on third-party services; see Section 4.2. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

Table 4.10: Controlling for other treatments, aggregated control coefficients (exporter and importer groups).

	Export groups		Import groups	
	Separation prob.	Stayer wage	Separation prob.	Stayer wage
	(1)	(2)	(3)	(4)
event time = -3 × above med. × treated	0.0019**	0.0031	0.0021**	0.0062**
	(0.0007)	(0.0028)	(0.0008)	(0.0026)
event time = -3 × below med. × treated	-0.0007	-0.0018	0.0008	-0.0005
	(0.0008)	(0.0022)	(0.0007)	(0.0015)
event time = -3 × none × treated	0.0002	0.0008	-0.0014	-0.0020
	(0.0007)	(0.0013)	(0.0010)	(0.0018)
event time = -2 × above med. × treated	0.0009**	-0.0011	0.0010**	0.0006
	(0.0004)	(0.0019)	(0.0004)	(0.0019)
event time = -2 × below med. × treated	-0.0003	0.0025	0.0004	0.0014
	(0.0004)	(0.0016)	(0.0003)	(0.0012)
event time = -2 × none × treated	0.0001	0.0010	-0.0007	0.0006
	(0.0003)	(0.0010)	(0.0005)	(0.0015)
event time = 0 × above med. × treated	0.0134	0.0053	0.0430***	-0.0027
	(0.0158)	(0.0039)	(0.0147)	(0.0033)
event time = 0 × below med.× treated	0.0595***	-0.0124***	0.0443***	-0.0116***
	(0.0160)	(0.0041)	(0.0085)	(0.0025)
event time = 0 × none × treated	0.0334***	-0.0056***	0.0164**	-0.0067**
	(0.0070)	(0.0019)	(0.0081)	(0.0027)
event time = 1 × above med. × treated	0.0647*	0.0038	0.1155***	-0.0038
	(0.0336)	(0.0060)	(0.0315)	(0.0046)
event time = 1 × below med. × treated	0.1068***	-0.0131***	0.0903***	-0.0142***
	(0.0276)	(0.0049)	(0.0140)	(0.0031)
event time = 1 × none × treated	0.0761***	-0.0030	0.0552***	-0.0020
	(0.0119)	(0.0023)	(0.0127)	(0.0032)
event time = 2 × above med. × treated	0.0752***	-0.0017	0.1221***	-0.0021
	(0.0271)	(0.0070)	(0.0293)	(0.0056)
event time = 2 × below med. × treated	0.1337***	-0.0117*	0.1145***	-0.0118***
	(0.0272)	(0.0062)	(0.0136)	(0.0039)
event time = 2 × none × treated	0.0852***	-0.0047	0.0651***	-0.0027
	(0.0111)	(0.0030)	(0.0130)	(0.0039)
event time = 3 × above med. × treated	0.0976***	-0.0084	0.1439***	-0.0105*
	(0.0252)	(0.0064)	(0.0292)	(0.0055)
event time = 3 × below med. × treated	0.1432***	-0.0188***	0.1239***	-0.0134***
	(0.0266)	(0.0057)	(0.0143)	(0.0038)
event time = 3 × none × treated	0.0922***	-0.0095***	0.0740***	-0.0088**
	(0.0117)	(0.0032)	(0.0136)	(0.0045)
event time = 4 × above med. × treated	0.1004***	-0.0129**	0.1427***	-0.0103*
	(0.0237)	(0.0065)	(0.0279)	(0.0061)
event time = 4 × below med. × treated	0.1372***	-0.0202***	0.1206***	-0.0187***

	(0.0252)	(0.0056)	(0.0140)	(0.0042)
event time = 4 × none × treated	0.0965***	-0.0151***	0.0839***	-0.0148***
	(0.0123)	(0.0036)	(0.0155)	(0.0048)
event time = -3 × MNE × treated	-0.0006	-0.0063**	-0.0015*	-0.0070**
	(0.0008)	(0.0031)	(0.0009)	(0.0028)
event time = -2 × MNE × treated	-0.0003	0.0006	-0.0007*	0.0007
	(0.0004)	(0.0019)	(0.0004)	(0.0020)
event time = 0 × MNE × treated	0.0818***	0.0052	0.0602***	0.0135***
	(0.0169)	(0.0044)	(0.0159)	(0.0043)
event time = 1 × MNE × treated	0.1530***	0.0032	0.1047***	0.0123*
	(0.0302)	(0.0067)	(0.0278)	(0.0071)
event time = 2 × MNE × treated	0.1535***	0.0112	0.1103***	0.0148*
	(0.0289)	(0.0076)	(0.0299)	(0.0078)
event time = 3 × MNE × treated	0.1493***	0.0246***	0.1017***	0.0272***
	(0.0276)	(0.0073)	(0.0301)	(0.0073)
event time = 4 × MNE × treated	0.1413***	0.0298***	0.0956***	0.0279***
	(0.0252)	(0.0067)	(0.0281)	(0.0081)
Controls	✓	✓	✓	✓
Fixed-effects				
Matched-group-year	✓	✓	✓	✓
Worker-cohort	✓	✓	✓	✓
event time-MNE	✓	✓	✓	✓
event time-additional spikes-group	✓	✓	✓	✓
Observations	1,902,456	1,252,686	1,902,456	1,252,686
R^2	0.6442	0.9769	0.6432	0.9769
Adjusted R^2	0.5649	0.9717	0.5637	0.9717

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. This table presents difference-in-differences coefficients for MNE workers and aggregated coefficients for the reference groups, see eq. (4.3). Columns one and two adjust the reference group by percentile group of the log real export distribution in the pre-automation year; Columns three and four by the log real import distribution. The coefficients presented here are aggregated according to whether the reference group features no exports/imports ("none"), lies below the median (groups 1-5; "below median") or above the median (groups 6-10; "above median"). MNEs include foreign firms and domestic firms with foreign subsidiaries. Dependent variables are an indicator whether a worker has left the automating firm (Columns one, three) and the log hourly wage of stayers (Columns two, four). Controls include age and its square, delineated by the workers' contract type in the pre-automation year. The regressions compare incumbent workers at automating firms with matched controls at firms that automate later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. Automation events are based on firms' costs on third-party services; see Section 4.2. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

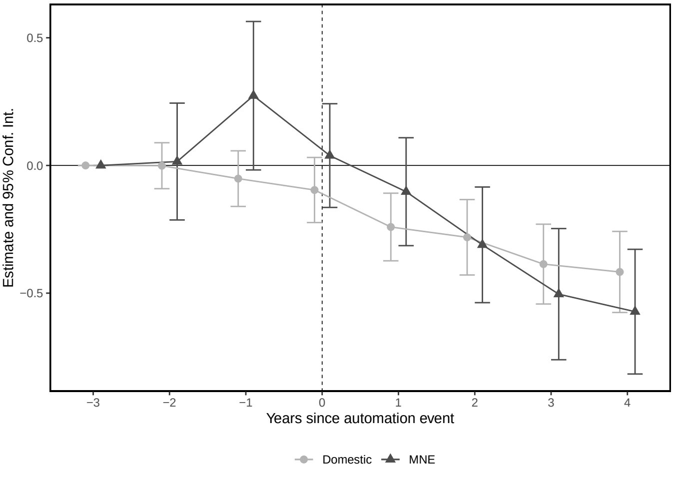
Table 4.11: Controlling for other treatments, aggregated control coefficients (automation intensity groups).

	Automation cost per worker		Automation cost share	
	Separation prob.	Stayer wage	Separation prob.	Stayer wage
	(1)	(2)	(3)	(4)
event time = -3 × above med. × treated	0.0004	0.0007	0.0008	0.0006
	(0.0007)	(0.0022)	(0.0007)	(0.0016)
event time = -3 × below med. × treated	0.0002	-0.0007	-0.0005	0.0017
	(0.0006)	(0.0014)	(0.0005)	(0.0015)
event time = -2 × above med. × treated	0.0002	0.0014	0.0004	0.0009
	(0.0003)	(0.0016)	(0.0003)	(0.0012)
event time = -2 × below med. × treated	0.0001	0.0007	-0.0002	0.0016
	(0.0003)	(0.0010)	(0.0003)	(0.0012)
event time = 0 × above med. × treated	0.0488***	-0.0061*	0.0255***	-0.0086***
	(0.0127)	(0.0032)	(0.0092)	(0.0025)
event time = 0 × below med. × treated	0.0282***	-0.0061***	0.0421***	-0.0030
	(0.0075)	(0.0020)	(0.0083)	(0.0022)
event time = 1 × above med. × treated	0.1265***	-0.0074*	0.0663***	-0.0123***
	(0.0244)	(0.0043)	(0.0175)	(0.0034)
event time = 1 × below med. × treated	0.0593***	0.0000	0.0899***	0.0040
	(0.0132)	(0.0027)	(0.0163)	(0.0028)
event time = 2 × above med. × treated	0.1532***	-0.0023	0.0909***	-0.0084**
	(0.0233)	(0.0049)	(0.0172)	(0.0042)
event time = 2 × below med. × treated	0.0747***	-0.0009	0.0962***	-0.0003
	(0.0127)	(0.0033)	(0.0157)	(0.0039)
event time = 3 × above med. × treated	0.1649***	-0.0073	0.1141***	-0.0124***
	(0.0228)	(0.0050)	(0.0168)	(0.0042)
event time = 3 × below med. × treated	0.0894***	-0.0043	0.0983***	-0.0051
	(0.0128)	(0.0033)	(0.0154)	(0.0040)
event time = 4 × above med. × treated	0.1655***	-0.0104*	0.1164***	-0.0180***
	(0.0214)	(0.0055)	(0.0161)	(0.0045)
event time = 4 × below med. × treated	0.0924***	-0.0091**	0.1022***	-0.0081*
	(0.0126)	(0.0036)	(0.0154)	(0.0042)
event time = -3 × MNE × treated	-0.0003	-0.0048**	0.0003	-0.0041*
	(0.0008)	(0.0023)	(0.0007)	(0.0025)
event time = -2 × MNE × treated	-0.0001	-0.0008	0.0001	0.0000
	(0.0004)	(0.0017)	(0.0004)	(0.0019)
event time = 0 × MNE × treated	0.0607***	0.0118***	0.0682***	0.0136***
	(0.0181)	(0.0037)	(0.0151)	(0.0039)
event time = 1 × MNE × treated	0.1200***	0.0110*	0.1405***	0.0091
	(0.0332)	(0.0059)	(0.0309)	(0.0059)
event time = 2 × MNE × treated	0.1128***	0.0171***	0.1453***	0.0164**
	(0.0302)	(0.0066)	(0.0293)	(0.0069)
event time = 3 × MNE × treated	0.1109***	0.0310***	0.1452***	0.0307***

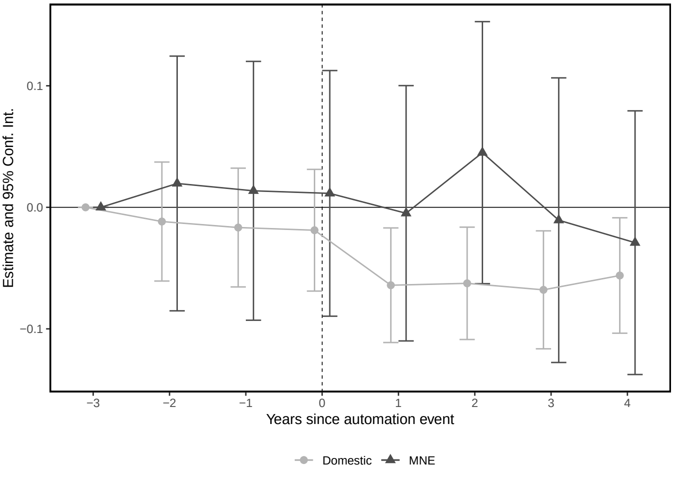
	(0.0278)	(0.0062)	(0.0279)	(0.0067)
event time = 4 × MNE × treated	0.0989***	0.0333***	0.1329***	0.0350***
	(0.0250)	(0.0068)	(0.0257)	(0.0073)
Controls	✓	✓	✓	✓
Fixed-effects				
Matched-group-year	✓	✓	✓	✓
Worker-cohort	✓	✓	✓	✓
event time-MNE	✓	✓	✓	✓
event time-additional spikes-group	✓	✓	✓	✓
Observations	1,902,456	1,252,686	1,902,456	1,252,686
R^2	0.6455	0.9770	0.6451	0.9769
Adjusted R^2	0.5666	0.9717	0.5661	0.9717

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. This table presents difference-in-differences coefficients for MNE workers and aggregated coefficients for the reference groups, see eq. (4.3). Columns one and two adjust the reference group by percentile groups of the log real automation cost per worker distribution in the automation year; Columns three and four by the automation cost share distribution. The coefficients presented here are aggregated according to whether the reference group features no lies below the median (groups 1-5; "below median") or above the median (groups 6-10; "above median"). MNEs include foreign firms and domestic firms with foreign subsidiaries. Dependent variables are an indicator whether a worker has left the automating firm (Columns one, three) and the log hourly wage of stayers (Columns two, four). Controls include age and its square, delineated by the workers' contract type in the pre-automation year. The regressions compare incumbent workers at automating firms with matched controls at firms that automate later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. Automation events are based on firms' costs on third-party services; see Section 4.2. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

Figure 4.10: Firm-level hires.



(a) Number of hires.



(b) Average wage of hires.

Notes: This plot presents difference-in-differences coefficients for MNE and domestic firms, using a firm-level equivalent of eq. (4.1). The coefficients are estimated using Poisson regression. Dependent variables are the firm-level number of new hires (Panel (a)) and the mean hourly wage of new hires (Panel (b)). MNEs include foreign firms and domestic firms with foreign subsidiaries. The regressions compare automating firms with matched control firms that automate later, matched on 2-digit NACE industry, and employment size; see Sections 4.3 and 4.4.5. Automation events are based on firms' costs on third-party services; see Section 4.2.

Table 4.12: The effect of ICT investments on workers in MNEs vs. domestic firms.

	Separation prob. (1)	Stayer log wage (2)
event time = -3 × MNE × treated	-0.0025*	0.0005
	(0.0013)	(0.0020)
event time = -2 × MNE × treated	-0.0013*	-0.0002
	(0.0007)	(0.0019)
event time = 0 × MNE × treated	-0.0130*	-0.0025
	(0.0078)	(0.0047)
event time = 1 × MNE × treated	-0.0175	-0.0004
	(0.0157)	(0.0051)
event time = 2 × MNE × treated	-0.0108	0.0124***
	(0.0174)	(0.0045)
event time = 3 × MNE × treated	0.0063	0.0147***
	(0.0207)	(0.0054)
event time = 4 × MNE × treated	0.0159	0.0224***
	(0.0221)	(0.0058)
event time = -3 × treated	0.0007	0.0009
	(0.0005)	(0.0011)
event time = -2 × treated	0.0003	-0.0004
	(0.0002)	(0.0007)
event time = 0 × treated	0.0120**	-0.0017
	(0.0054)	(0.0019)
event time = 1 × treated	0.0387***	-0.0032*
	(0.0116)	(0.0018)
event time = 2 × treated	0.0460***	-0.0043*
	(0.0116)	(0.0025)
event time = 3 × treated	0.0527***	-0.0074**
	(0.0114)	(0.0030)
event time = 4 × treated	0.0552***	-0.0113***
	(0.0106)	(0.0031)
Controls	✓	✓
Fixed-effects		
Matched-group-year	✓	✓
Worker-cohort	✓	✓
event time-additional spikes-(MNE | Domestic)	✓	✓

4

Observations	2,178,888	1,469,132
R^2	0.6276	0.9769

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. This table presents difference-in-differences coefficients for MNE and domestic firm workers, see eq. (4.1). MNEs include foreign firms and domestic firms with foreign subsidiaries. Dependent variables are an indicator whether a worker has left the automating firm (Column one) and the log hourly wage of stayers (Column two). Controls include age and its square, delineated by the workers' contract type in the pre-automation year. The regressions compare incumbent workers at firms with an ICT investment spike with matched controls at firms that spike later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. ICT investment spikes are based on firms' investments in ICT-related technologies: Computers, Communication equipment, and Software; see Section 4.5.3. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

Table 4.13: The effect of Machinery investments on workers in MNEs vs. domestic firms.

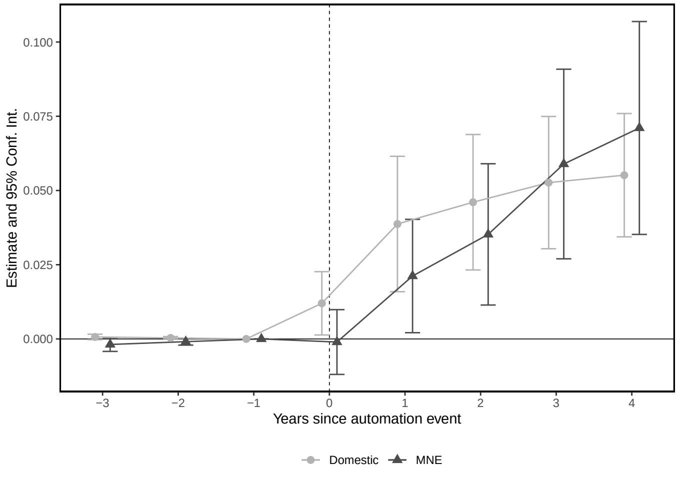
	Separation prob. (1)	Stayer log wage (2)
event time = -3 × MNE × treated	-0.0029***	-0.0060***
	(0.0009)	(0.0020)
event time = -2 × MNE × treated	-0.0014***	-0.0010
	(0.0004)	(0.0017)
event time = -1 × MNE × treated	-0.0066	0.0221**
	(0.0060)	(0.0101)
event time = 0 × MNE × treated	-0.0071	-0.0019
	(0.0127)	(0.0062)
event time = 1 × MNE × treated	0.0013	0.0056
	(0.0156)	(0.0060)
event time = 2 × MNE × treated	0.0024	0.0051
	(0.0176)	(0.0055)
event time = 3 × MNE × treated	0.0037	0.0095*
	(0.0182)	(0.0057)
event time = -3 × treated	0.0023***	0.0018*
	(0.0007)	(0.0011)
event time = -2 × treated	0.0011***	-0.0004
	(0.0003)	(0.0010)
event time = 0 × treated	0.0091**	0.0002
	(0.0043)	(0.0041)
event time = 1 × treated	0.0206**	0.0056
	(0.0091)	(0.0036)
event time = 2 × treated	0.0312***	0.0027
	(0.0102)	(0.0023)
event time = 3 × treated	0.0435***	0.0005
	(0.0101)	(0.0028)
event time = 4 × treated	0.0461***	-0.0020
	(0.0099)	(0.0031)
Controls	✓	✓
Fixed-effects		
Matched-group-year	✓	✓
Worker-cohort	✓	✓

4

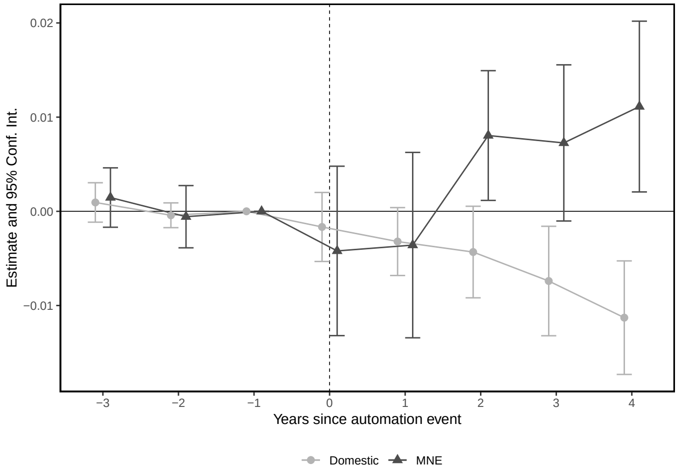
event time-additional spikes-(MNE | Domestic)	✓	✓
Observations	1,894,184	1,346,698
R^2	0.6000	0.9773

Notes: ***Significant at the 0.1% level; **significant at the 1% level; *significant at the 5% level; .significant at the 10% level. This table presents difference-in-differences coefficients for MNE and domestic firm workers, see eq. (4.1). MNEs include foreign firms and domestic firms with foreign subsidiaries. Dependent variables are an indicator whether a worker has left the automating firm (Column one) and the log hourly wage of stayers (Column two). Controls include age and its square, delineated by the workers' contract type in the pre-automation year. The regressions compare incumbent workers at firms with a Machinery investment spike with matched controls at firms that spike later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. Machinery investment spikes are based on firms' investments in Machinery; see Section 4.5.3. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

Figure 4.11: Spikes in ICT investments: Separations and staying workers' wages.



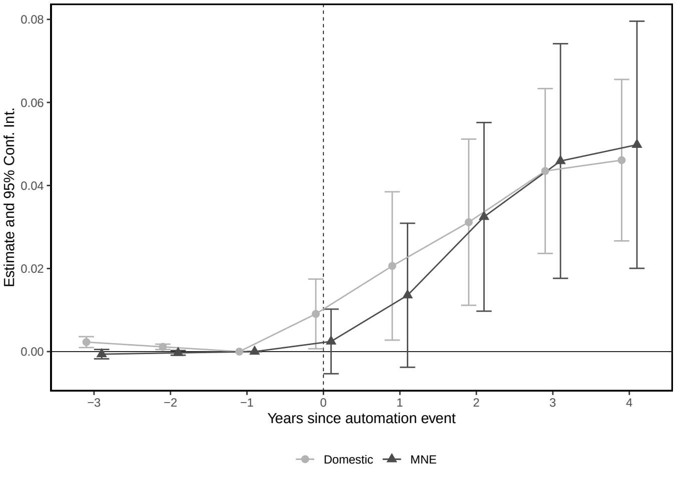
(a) Separation probability.



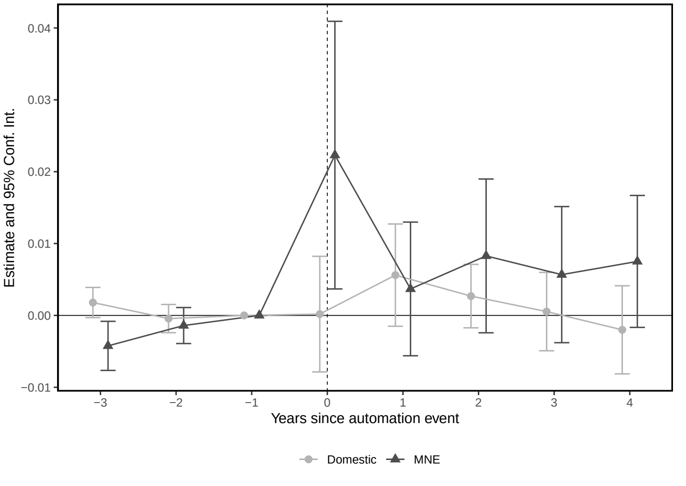
(b) Log hourly wages of staying workers.

Notes: This plot presents difference-in-differences coefficients for MNE and domestic firm workers, see eq. (4.1). Full regression results are in Table 4.12 in Appendix 4.E. ICT refers to Information and Communications Technology. Dependent variables are an indicator whether a worker has left the automating firm (Panel (a)) and the log hourly wage of stayers (Panel (b)). MNEs include foreign firms and domestic firms with foreign subsidiaries. The regressions compare incumbent workers at firms with an ICT investment spike with matched controls at firms that spike later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. ICT investment spikes are based on firms' investments in ICT-related technologies: Computers, Communication equipment, and Software; see Section 4.5.3. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

Figure 4.12: Spikes in Machinery investments: Separations and staying workers' wages.



(a) Separation probability.



(b) Log hourly wages of staying workers.

Notes: This plot presents difference-in-differences coefficients for MNE and domestic firm workers, see eq. (4.1). Full regression results are in Table 4.13 in Appendix 4.E. Dependent variables are an indicator whether a worker has left the automating firm (Panel (a)) and the log hourly wage of stayers (Panel (b)). MNEs include foreign firms and domestic firms with foreign subsidiaries. The regressions compare incumbent workers at firms with a Machinery investment spike with matched controls at firms that spike later, matched on 2-digit NACE industry, employment size, and log hourly wages (including two lags); see Section 4.3. Machinery investment spikes are based on firms' investments in Machinery; see Section 4.5.3. Standard errors at clustered at the level of the firm where the worker is employed in the pre-automation year.

Chapter 5

Conclusion

This thesis investigates the impact of multinational firms on local workers. I use highly detailed employer-employee matched data from the Netherlands to follow workers in and out of employment at MNEs and domestic firms. Across three self-contained studies, I explore how MNEs influence local workers' wages and employment opportunities.

Policymakers in the Netherlands and worldwide invest significantly in attracting MNEs, hoping these firms' productivity advantages will benefit the local economy and its workers. High wage levels among MNE workers are often seen as a key indicator of these benefits. However, earlier literature often attributes the MNEs' wage premia to the high skill level of their workforce (e.g., Heyman et al., 2007; Hijzen et al., 2013; Setzler and Tintelnot, 2021). This raises doubts about the true benefits of MNEs for local workers. In contrast, I show that employment at an MNE, as opposed to a domestic firm, leads the same local worker to command a higher wage.

In Chapter 2, I show that acquisitions by foreign MNEs increase the acquired firms' average wage level. By decomposing the wage increase, I demonstrate that firm-level changes, rather than workforce adjustments, are the primary driver. These high firm contributions to wage are not specific to the Dutch setting. They result from adjusting the estimates for selection effects, as MNEs target high-wage and high-skill domestic firms for acquisition. The dominance of firm contributions indicates that foreign ownership changes firms beyond workforce adjustments. These changes benefit local workers, as the same worker receives a higher wage at an acquired firm than at a non-acquired firm.

Interpreting wage as the marginal product of labor, high firm contributions to wage after an acquisition are consistent with productivity improvements. Post-acquisition, MNEs may transfer advanced technologies, management, and production practices to acquired firms. These productivity advantages plausibly raise local wages and output.

Chapter 3 focuses on the dynamic accumulation of wage throughout workers' careers. To do so, I track the careers of all labor market entrants in the Netherlands. Compared to domestic firm careers, workers at MNEs accrue considerable wage premia over time. The wage premia of MNE careers increase with MNE tenure and transfer with the worker across employers, both to MNEs and to non-MNEs. Moreover, they persist even when employment experience among workers varies due to mass layoffs. In line with the results in Chapter 2, I show that the sorting of high-wage workers to MNEs does not explain the career premia. When adjusting for experience, estimates of workers' earnings capabilities are very similar between MNEs and domestic firms. Instead, it is the experience within an MNE that transforms workers into high-wage earners. This implies that MNE employment allows workers to build human capital or to signal quality. Back-of-the-envelope calculations suggest that the value of MNE experience is significant in the aggregate: Around 6% of total labor income in the Netherlands in 2021 is associated with MNE experience, compared to a counterfactual situation where this experience had been accumulated in domestic firms.

The career wage premia of MNEs may signal that, through worker mobility, the productivity advantage of MNEs spills over to domestic firms. While I do not show spillovers directly, my results align with the idea that workers acquire skills in MNEs that they would not have acquired in a domestic firm. Intangible assets of the MNEs' productivity advantage may transport with workers across employers, which may explain the wage premia of earlier MNE experience.

However, MNEs may also derive part of their productivity advantage by adapting to local labor markets. Chapter 3 presents a standard trade model, augmented with hires and promotions. In the model, junior workers accept low entry wages in exchange for valuable MNE experience that pays off later. This leads the MNE to hire more junior workers, learn their firm-specific productivity, and promote only the best workers to high-wage senior positions. Several stylized facts align with the predictions of this model. This suggests that higher productivity in MNEs may not only result from the MNEs' inherent productivity advantages but also from MNEs' strategic management of their workforce.

In Chapter 4, I analyze spikes in firms' third-party automation costs, comparing separations and wages in MNEs and domestic firms. Post-automation, workers in MNEs face significantly higher separation risks compared to those in domestic firms. The separation risks in domestic firms align with earlier findings in the literature, reflecting a moderate impact of automation on job instability (Bessen et al., 2023). However, in MNEs, separation risks are considerably larger. Moreover, workers who remain in MNEs post-automation experience wage increases, whereas those in domestic firms often face wage declines. This indicates that MNEs may use automation to streamline production processes more than domestic firms, benefiting remaining workers but at the cost of higher separations.

Additional results across the three chapters highlight that MNEs benefit high-skilled workers significantly more than low-skilled workers. Foreign acquisitions in Chapter 2 lead managers' wages to rise twice as fast as those of non-managers, with higher firm contributions explaining most of the difference. In Chapter 3, workers with higher initial ability also benefit more from MNE careers than workers with lower initial ability. In light of automation, Chapter 4 shows that the wage increases for remaining workers benefit primarily managers and technically proficient employees, while lower-skilled workers face greater job instability and limited wage growth.

Overall, this thesis demonstrates the nuanced effects that MNEs have on local labor markets. MNE employment benefits local workers and their careers through higher wages. However, it also introduces potential challenges, such as wage inequality and job instability, particularly in the context of automation.

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English Summary

High wages, advanced technology, and productivity in multinational firms (MNEs) inspire policymakers in the Netherlands and globally to attract these firms to their local economies. While the productivity effects of MNEs are well-documented, less is known about their impact on individual workers, despite employment being a primary reason for attracting MNEs. This thesis comprises three self-contained studies that assess the impact of MNE employment on local workers' wages and career trajectories, using detailed matched employer-employee data from the Netherlands.

In Chapter 2, I examine the drivers of the wage gap that arises when a domestic firm is acquired by a foreign MNE. I introduce a novel method to decompose this wage gap into contributions from i) the firm, such as increased productivity, and from ii) changes in the workforce composition. My findings reveal that firm-level changes, rather than workforce adjustments, drive most of the post-acquisition wage increases. This suggests that foreign acquired firms enhance workers' earnings, beyond a mere reshuffling of the workforce. These changes benefit local workers, as the same worker receives a higher wage at an acquired firm than at a non-acquired firm.

Chapter 3 studies the career outcomes of workers in MNEs. By tracking the career paths of all Dutch graduates entering the labor market, I demonstrate that workers accumulate considerable wage premia during their tenure within an MNE. These wage premia are highly portable to subsequent employers and rise in earlier MNE tenure. The wage premia explain the diverging wage patterns between careers in MNEs and domestic firms, as both types of firms hire workers of similar initial earnings potential. Hence, employment at an MNE improves a worker's earnings potential within the MNE and at other firms, suggesting that MNE experience allows workers to build human capital or to signal quality.

Additionally, this chapter introduces a model where MNEs leverage their value for careers to enhance productivity. Consistent with this model, I show that MNEs recruit more junior staff, pay lower entry wages, and apply stricter selection criteria for senior positions than domestic firms. Moreover, the wage premia of MNE careers explain almost the entire productivity advantage of MNEs that translates into pay.

Chapter 4 focuses on the impact of automation on wages and job separations within MNEs and domestic firms. Automation in domestic firms typically leads to wage reductions and few separations, largely affecting low-skilled workers. In contrast, automation in MNEs results in substantial separations, but also in wage increases for remaining workers. In MNEs, wage increases are concentrated among high-skilled workers, such as managers and those with a technical educational background. However, all types of workers face increased separation risks, indicating significant organizational changes within MNEs post-automation.

Overall, this thesis demonstrates that while MNE employment raises local wages and fosters skill development, it can also lead to greater job instability and wage inequality.

Nederlandse Samenvatting

Hoge lonen, geavanceerde technologie en productiviteit in multinationale ondernemingen (MNE's) inspireren beleidsmakers in Nederland en wereldwijd om deze bedrijven naar hun lokale economieën te trekken. Hoewel de productiviteitseffecten van MNE's goed gedocumenteerd zijn, weten we minder over hun daadwerkelijke impact op individuele werknemers. Dat terwijl werkgelegenheid een voornaamste reden is om MNE's aan te trekken. Dit proefschrift omvat drie onafhankelijke studies die de impact van werkgelegenheid bij MNE's op de lonen en carrièrepaden van lokale werknemers beoordelen. De analyses zijn gebaseerd op gedetailleerde gekoppelde werkgever-werknemer gegevens uit Nederland.

In Hoofdstuk 2 onderzoek ik de factoren achter de loonkloof die ontstaat wanneer een binnenlands bedrijf wordt overgenomen door een buitenlandse MNE. Ik introduceer een nieuwe methode om deze loonkloof uiteen te zetten in bijdragen van i) het bedrijf, zoals verhoogde productiviteit, en ii) van veranderingen in de samenstelling van het personeel. Mijn bevindingen wijzen uit dat vooral veranderingen op bedrijfsniveau de belangrijkste factor vormen voor de loonstijgingen na de overname. Dit suggereert dat buitenlands verworven bedrijven de lonen van werknemers actief verhogen, en dat dit effect verder gaat dan slechts een verandering in de samenstelling van het personeel. Deze veranderingen zijn gunstig voor lokale werknemers, aangezien dezelfde werknemer een hoger loon ontvangt bij een overgenomen bedrijf dan bij een niet-overgenomen bedrijf.

Hoofdstuk 3 bestudeert de carrière-uitkomsten van werknemers bij MNE's. Door de carrièrepaden van alle afgestudeerden in Nederland die toetreden tot de arbeidsmarkt te volgen, toon ik aan dat werknemers aanzienlijke loonpremies opbouwen tijdens hun dienstverband bij een MNE. Deze loonpremies zijn overdraagbaar naar andere werkgevers en nemen toe met eerdere MNE-ervaring. De loonpremies verklaren de divergerende loonpatronen tussen carrières bij MNE's en binnenlandse bedrijven, aangezien beide soorten bedrijven werknemers met vergelijkbaar aanvangsloonpotentieel aannemen. Het dienstverband bij een MNE verbetert het verdienpotentieel van een werknemer binnen de MNE en bij andere bedrijven, wat suggereert dat ervaring bij een MNE werknemers in staat stelt om menselijk kapitaal op te bouwen of kwaliteit te signaleren.

Daarnaast introduceert dit hoofdstuk ook een model waarin MNE's hun waarde voor carrières benutten om de productiviteit te verhogen. In overeenstemming met dit model werven MNE's meer junior personeel, bieden zij lagere aanvangslonen en hanteren zij strengere selectiecriteria voor hogere posities dan binnenlandse bedrijven. Bovendien verklaren de loonpremies bijna het gehele productiviteitsvoordeel van MNE's dat zich vertaalt in betere betaling van werknemers.

Hoofdstuk 4 richt zich op de impact van automatisering op lonen en baanafscheidingen binnen MNE's en binnenlandse bedrijven. Automatisering bij binnenlandse bedrijven leidt typisch tot loonverlagingen en tot weinig ontslagen, voornamelijk bij laaggeschoolde werknemers. Daarentegen leidt automatisering bij MNE's juist niet alleen tot aanzienlijke ontslagen, maar ook tot loonstijgingen voor de overgebleven werknemers. Bij MNE's zijn loonstijgingen geconcentreerd onder hooggeschoolde werknemers, zoals managers en degenen met een technische opleiding. Echter, alle soorten werknemers ervaren een verhoogd ontslagrisico, wat wijst op significante organisatorische veranderingen binnen MNE's na automatisering.

Over het geheel genomen toont dit proefschrift aan dat werkgelegenheid bij MNE's weliswaar de lokale lonen verhoogt en vaardigheidsontwikkeling bevordert, maar ook grotere baaninstabiliteit en loonongelijkheid introduceert.

About the Author

Marcus A. Rösch was born on February 3, 1994, in Mainz, Germany. He holds a Bachelor of Science in Economics and Business, with distinction, from the University of Konstanz, and a Master of Science in Law and Economics, cum laude, from Utrecht University. Marcus pursued a PhD in Economics at the Erasmus School of Economics, Erasmus University, under the supervision of Prof. Dr. Frank van Oort, Dr. Bas Karreman, and Dr. Michiel Gerritse.

During his PhD studies, Marcus was affiliated with the globalization group at Statistics Netherlands (Centraal Bureau voor de Statistiek) and the Tinbergen Institute. In 2023, he spent four months as a visiting scholar at Bocconi University in Milan, Italy. He also completed various courses and summer schools in Economics, Econometrics, and Machine Learning.

Marcus' research uses large matched employer-employee data to study the impact of multinational firms on local labor markets. His work has been presented at numerous prestigious international conferences, including meetings of the European Economics Association, the Urban Economics Association, and the International Association for Applied Econometrics.

Portfolio

Website

https://mrcsrsch.github.io

Working papers and publications

Roesch, M. (2024). Post-Automation Workforce Dynamics in (Non-)Multinationals.

Roesch, M., Gerritse, M., & Karreman, B. (2024). Careers in Multinational Enterprises. *Tinbergen Institute Working Paper (No. 24-005/V)*.

Roesch, M., Gerritse, M., Karreman, B., van Oort, F., & Loog, B. (2022). Do firms or workers drive the foreign acquisition wage premium? *Tinbergen Institute Working Paper (No. 22-014/V)*.

Potrafke, N., Roesch, M., & Ursprung, H. (2020). Election systems, the "beauty premium" in politics, and the beauty of dissent. *European Journal of Political Economy*, 64, 101900.

Research visits and other experience

External Researcher — *since November 2019*
Centraal Bureau voor de Statistiek (Statistics Netherlands)

Research visit — *February - June 2023*
Bocconi University

Conference presentations

International Association for Applied Econometrics Annual Conference — *2024*
Xiamen, China

International Economic Association World Congress — *2023*
Medellín, Colombia

Urban Economics Association Meeting — *2023*
Toronto, Canada

Meeting of the European Economic Association and the Econometric Society — *2023*
Barcelona, Spain

International Association for Applied Econometrics Annual Conference — *2023*
Oslo, Norway

European Trade Study Group Meeting — *2022*
Groningen, the Netherlands

Teaching activities

Supervision of BSc and MSc theses	*2019, 2020, 2021, 2022, 2023, 2024*
Supervision of corporate internships	*2021, 2022, 2023, 2024*
Guest lecturer at the Universities of Ancona and Verona	*2024*
Teaching Assistant for "Firm Location Strategy" (Master level)	*2020, 2021, 2022*
Guest lecturer in "Quantitative Spatial Analysis" (Master level)	*2020*

PhD coursework

Tinbergen Institute Research Qualification

Core requirement

Fundamental Mathematics, Statistics, Econometrics I, Econometrics II, Econometrics III, Macroeconomics III, Microeconomics IV

Field requirement

Supervised Machine Learning, Unsupervised Machine Learning, Deep Learning, International Economics, Economic Policy Research Workshop, Multinational Firms and Global Value Chains (external course at Kiel Institute for the World Economy)

Summer schools

Machine Learning for Business *2024*
Business Data Science Graduate Program, Amsterdam

Econometrics of Networks *2023*
Vrije Universiteit Amsterdam

Firm Dynamics and Economic Growth *2022*
Center for Monetary and Financial Studies, Madrid

Foundations of Data Analysis and Machine Learning in Python *2022*
Business Data Science Graduate Program, Amsterdam

Previous Education

Master of Science in Law and Economics *2017 - 2018*
Utrecht University

Bachelor of Science in Economics *2014 - 2017*
University of Konstanz

Tinbergen Institute Research Series

The Tinbergen Institute is the Institute for Economic Research, which was founded in 1987 by the Faculties of Economics and Econometrics of the Erasmus University Rotterdam, University of Amsterdam and Vrije Universiteit Amsterdam. The Institute is named after the late Professor Jan Tinbergen, Dutch Nobel Prize laureate in economics in 1969. The Tinbergen Institute is located in Amsterdam and Rotterdam. For a full list of PhD theses that appeared in the series we refer to List of PhD Theses – Tinbergen.nl. The following books recently appeared in the Tinbergen Institute Research Series:

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