Technology Transfer in Mergers and Acquisitions and the Careers of Workers*

Malin Gardberg
IFN

Fredrik Heyman
IFN and Lund University

Joacim Tåg
IFN and Hanken

March 2024

PRELIMINARY DRAFT

ABSTRACT

Mergers and acquisitions (M&As) are often catalysts for change in target firms. We analyze a novel economic mechanism through which M&As interact with human capital inside the firm: that M&As are a vehicle for transferring technologies across the boundaries of firms and such transfers have spillovers on workers long-run careers. Using Swedish registry data covering two decades, we find that acquisitions by technologically more advanced firms disproportionately affects workers who perform tasks sensitive to the technology specialization of the acquiring firm. These spillovers can be substantial: the long run relative wage impacts are between -15% and +3.5% percent.

Keywords: Careers, human capital, technology transfer, mergers and acquisitions, wages.
JEL Codes: G34, J30, J31, O39.

*E-mails: malin.gardberg@ifn.se, fredrik.heyman@ifn.se, joacim.tag@ifn.se. We thank Ashwini Agrawal, Alex Xi He, Ernst Maug, Andreas Moxnes, Pehr-Johan Norbäck, Oskar Nordström Skans, Spyridon Lagaras, Martin Olsson, Anna Salomons, Per Strömberg, Joonas Tuhkuri, Ramona Westermann and seminar and conference participants at the CSEF-RCFS Conference on Finance, Labor and Inequality 2023, IFN, NICE 2022 and the Swedish conference in Economics at the Stockholm School of Economics for valuable comments and suggestions. We are grateful for financial support from the Jan Wallanders och Tom Hedelius stiftelse samt Tore Browaldhs stiftelse (W19-0030,P22-0094), Marianne and Marcus Wallenberg Foundation (2020.0049) and Torsten Söderbergs Stiftelse (ET2/20). This paper replaces our previous working paper entitled ”Importing Automation and Wage Inequality Through Foreign Acquisitions”.

1 Introduction

Mergers and acquisitions (M&As) are often a catalyst for restructuring, labor reallocation and technology adoption within the acquired firm . In this paper, we investigate if technology transfer across firm boundaries in M&As can have spillovers on the long run careers of workers in target firms.

We document substantial heterogeneity in technology diffusion, and show that the spread of technology across firm borders is contingent on the technological specialization of the acquiring firm. Technology transfer occurs in acquisitions where the acquirer has superior technological intensity compared to the target firm, but not in acquisitions where the acquirer is from an industry with inferior technological intensity. Focusing on exposure to software, robotics and artificial intelligence (AI), we show that acquisitions disproportionately affect the wages of workers that perform tasks sensitive to the technological intensity of the firm that undertakes the acquisition (be it software or robots).

To provide evidence for our proposed mechanism, we need matched employer-employee panel data to distinguish between worker heterogeneity in tasks, and heterogeneity in acquirer technological sophistication. We also need an identification strategy. For data availability, we turn to Sweden as a laboratory, and for identification, we rely on the recent literature on difference-in-differences estimation (Baker, Larcker, and Wang, 2022). We construct a data set covering firms and workers over nearly two decades. We then match workers that are part of M&As with similar workers performing similar tasks in firms that are not acquired.

For identification, we run stacked difference-in-differences and triple-differences regressions along various dimensions of worker and acquiring firm heterogeneity. We focus on worker heterogeneity regarding occupational exposure to software, robotics, and AI using the occupation exposure measures of Webb (2020). For acquiring firm technological specificity heterogeneity, we rely on software and database capital to total capital in the EU KLEMS database and information on robot adoption from the International Federation of Robotics (IFR). The stacked difference-in-differences estimation strategy allows us to avoid econometric problems related to time-invariant unobservables and staggered acquisitions over time.

We have the following four key results. First, we show that workers in software-exposed occupations see *relative wage declines* of 3.2% over an eight-year post period. This decline is monotonic, starting after the acquisition and ending at around a 5% relative decline at the end of the eight years. This relative decline in wages can be entirely traced to acquisitions by software-intense acquirers. In this sub-sample, the relative wage declines are, on average, 4.2% over the full post-period, ending at around 8% lower at the end of the eight years. In contrast, we do not observe any changes in relative wages for workers in firms acquired by less software intense acquirers. The wage effects are even more dramatic for workers that remain with the firm. Workers with high software exposure that stay with the firm after a software-intense acquisition experience a relative wage decline of 5.2% on average. This decline is monotonic during the entire post-period, with relative wages ending up almost 15% lower eight years after the acquisition. Again, we do not observe this pattern if the acquirer was less software-intensive.

Second, we find strikingly similar patterns in the data when we focus on the robot intensity of the acquirer and robot exposure to the job tasks that workers in the acquired firm perform. This finding provides strong evidence of a causal transfer effect since the correlation between robot and software intensity is relatively low. Using robot intensity and exposure, the relative monotonic decline in wages after the acquisition for workers in occupations exposed to robots and acquired by robot-intense firms reaches almost 10% for all workers and 12% for stayers at the end of the eight-year post-period. On average, the relative decline for all workers is 2.1%, and it is 3.1% for workers that stay with the firm.

Third, the patterns reverse when we examine the relationship between software-intense acquirers and workers' AI exposure. AI-exposed workers acquired by software-intense industries see relative wage increases of 3.5% over the post-period. Thus, AI exposure is quite different from software exposure in that AI-exposed occupations complement rather than substitute the software intensity of the acquirer. These positive wage effects on AI-exposed workers likely combine with the adverse wage effects on software and robot-exposed workers to produce the economically small and statistically insignificant overall effect on wages from acquisitions.

Finally, we also investigate firm-level observables relating to software and telecommunications use and ask if technology investments increase relative to when the acquiring firm is more software intense. We find a relative increase of about 14.8 MSEK in higher software, data, and telecommunications expenditures following highly intense acquisitions. However, the relative difference to low intense acquisitions is not statistically significant. These collective sets of findings support the hypothesis that technology transfer in M&As have spillovers on the long-run careers of workers.

Our paper contributes to the labor and finance literature, and more specifically to the literature on the worker level outcomes of M&As.[1] Closest to our work here is Lagaras (2021) and Ma, Ouimet, and Simintzi (2022). Lagaras (2021) uses data from M&As in Brazil to study post-merger restructuring of the work force. One of the mechanisms he highlights is how mergers can lead to technological upgrading of the workforce with routine jobs becoming fewer inside the firm, a difference that is also larger the relatively more advanced the acquirer is pointing to the existence of knowledge transfers. Ma et al. (2022) uses establishment level data from the US to show that M&As overall implement technological change and thus change the occupational composition of the work force. This leads to fewer routine-based jobs inside the firm. These papers do not, however, study spillovers on the long run careers of workers nor how transfers of specific technologies affect workers with different exposures differently. Rather, they focus on the composition of workers

[1] There is also an extensive literature on firm and establishment level reallocation of workers following M&As (see Gehrke, Maug, Obernberger, and Schneider (2022) for an excellent survey), on the worker effects of privatizations of state owned enterprises (Arnold, 2022; Olsson and Tåg, 2023), on the worker effects of foreign acquisitions (Heyman, Sjöholm, and Tingvall, 2007, 2011; Setzler and Tintelnot, 2021), and on the worker effects of private equity buyouts (Agrawal and Tambe, 2016; Olsson and Tåg, 2017, 2018; Antoni, Maug, and Obernberger, 2019; Cohn, Nestoriak, and Wardlaw, 2021; Garcia-Gomez, Maug, and Obernberger, 2022; Fang, Goldman, and Roulet, 2022). Though they study private equity buyouts rather than M&As, both Agrawal and Tambe (2016) and Olsson and Tåg (2017) show evidence that IT investments and technological modernisation have spillovers on long run worker careers (the evidence is less clear in Antoni et al. (2019) and Fang et al. (2022)).

inside the firm and thus on short-run employment. Related papers that do study worker outcomes independently on if they stay with the firm or not is Siegel and Simons (2010), Prager and Schmitt (2021), Arnold (2021), He and le Maire (2022), Lagaras (2023), Gehrke, Maug, Obernberger, and Schneider (2023), and Bach, Baghai, Bos, and Silva (2023). These paper do not, however, focus on the novel economic mechanism in our paper: that technology transfer across firm boundaries in M&As can have spillovers on the long run careers of workers.

Our paper is also related to the literature on technology transfers across borders through the activities of multinational firms (Branstetter, 2006; Keller, 2010; Guadalupe, Kuzmina, and Thomas, 2012; Bloom, Sadun, and Reenen, 2012).[2] This literature has emphasized knowledge spillovers from technology transfer, but has so far been confined to firm-level studies and contains no mention of spillover effects from technology transfer on long-run worker careers.

Our paper specifically complements these two existing literatures in at least three ways. First, a key insight from our work is documenting *long-run spillover effects on worker careers* from technology transfer that are both economically and statistically significant. Second, as emphasized by Lagaras (2023) among others, understanding the heterogeneity in the worker effects of M&As is important from a policy perspective if we wish to consider policies that could potentially mitigate negative spillovers on worker careers. Our paper shows how that *technological specificity* is key, and that workers exposed in software, robots, and AI all have different long run exposures depending on the acquirers technological edge. Importantly, we also document that not all M&As are coupled with technological upgrading that leads to spillovers on worker careers. This occurs only when the acquirer is more technologically advanced in a direction that lines up with how exposed workers in the firm are to this specific technology type. Finally, we plan to investigate several *barriers to technology transfer* that mitigate the extent to which technology transfer has spillovers on the long run careers of workers. In particular, we investigate technological distance, cultural similarity, financing capabilities (business cycle variation), and internal agency costs (CEO turnover events).

[2]This literature is part of a much broader literature on international trade and innovation: see Keller (2010) and Akcigit and Melitz (2022).

By developing and documenting the existence of such barriers, our research provides guidelines for policymakers interested in encouraging technology transfer and mitigating adverse effects on long-run worker careers.

We have organized the paper as follows. Section 2 details our data sources and provides descriptive statistics on the sample. Section 3 outlines our empirical strategy. Section 4 contains our key empirical results, while Section 5 discusses additional analyses and the robustness checks we perform. Section 6 concludes.

2 Data

2.1 Data on firms and workers

We analyze data from Statistics Sweden, which provides matched employer-employee data covering 1996-2015. The firm data is comprehensive, including detailed information on all Swedish firms, such as value-added, capital stock (book value), number of employees, wages, ownership status, sales, and industry. We also use Regional Labor Market Statistics (RAMS) to obtain plant-level information on education and demographics, which we aggregate to the firm level. This data covers all Swedish plants and adds to the richness of the analysis.

The worker data originates from the Salary Structure Statistics (Lönestrukturstatistiken), a survey of all firms with more than 500 workers and a representative sample of 8,000 to 11,000 firms with more than ten employees. The worker data includes information on approximately 50% of all private-sector workers. The data includes full-time equivalent monthly real wages, education, occupation, and gender. Occupation data is collected using the Swedish Standard Classification of Occupations (SSYK96), based on the International Standard Classification of Occupations (ISCO-88). Firms are legally obligated to respond to the survey, ensuring excellent coverage of occupation data.

2.2 Measuring occupational exposure to different technologies

Our analysis hinges on classifying job tasks or occupations at risk of becoming obsolete due to technological upgrades by acquirers. To do this, we utilize measures developed by Webb (2020) that gauge an occupation's exposure to software, robots, and AI. These measures use data from the O*NET database of occupations and tasks and patent descriptions in the Google Patents Public Data. They measure the extent to which patents in each technology class (software, robots, and AI) have targeted the tasks of particular occupations. An occupation's overall score is the average of its task scores. A higher score for software exposure indicates that the occupation's tasks overlap highly with software patents, which implies that the tasks can be automated.

The occupations in Webb (2020) are based on the American standard occupational code SOC2010, which we map to ISCO08, and then to the two-digit SSYK96 occupational code in our data. The exposure measures are expressed as score percentiles for each occupation. We define a worker as highly (low) exposed to a particular technology if the worker has an occupation that places her in our sample's 90th (10th) percentile of workers exposed to that technology.

2.3 Measuring the technological intensity of the acquirer

To examine the role of the nationality of foreign-owned firms, we match our firm-level data with information from the Swedish Agency for Economic and Regional Growth (Tillväxtanalys), which indicates the nationalities of foreign MNEs operating in Sweden. A firm is classified as foreign-owned if more than 50% of the equity is foreign-owned, and the primary owner's place of origin defines the nationality. We define a *foreign acquisition* as the foreign ownership dummy switching from zero to one between two years. Some firms in our sample are re-acquired by a Swedish firm or acquired by another foreign firm. To ensure that we only measure the effect of the first acquisition, we exclude a firm (and all of its workers) from our sample once it changes nationality a second time.

We supplement this data with information from the EU KLEMS 2019 database. This data source contains annual industry-level capital and labor statistics for all EU countries, US, UK, and Japan. We also add annual data on industry-level robot stocks from the IFR Robot Database. The primary source of robot data in the IFR data is data on robot installations by industry, country, and applications that all major industrial robot suppliers report to the IFR in combination with information from national robot associations.

To measure the technological intensity of the acquiring firm, we construct a measure of the software and database capital to total capital at the country-, industry-, and annual level for the acquiring countries and the host country Sweden. We define an acquiring firm as software intense if the software and database capital to total capital in the acquiring country, industry and year is higher or equal to the same level in the target industry that year in Sweden. As we only have information on the nationality of the acquiring firm but not the industry, we assume that the acquiring firm is in the same industry as the target firm. While this assumption adds some noise to the subset of regressions that incorporate the acquirer's technological intensity, there is no apparent reason to expect that this assumption biases the difference-in-difference and triple-differences estimates.

We also construct a measure of the stock of robots to total industry employment at the start of the IFR sample at the country-, industry-, and annual level for the acquiring countries and the target industry. We define an acquiring firm as robot intense if the robot stock to employment in the acquiring country, industry, and year is higher or equal to the same level in the domestic target industry. The IFR robot data is available for a restricted set of industries compared to the EU KLEMS data because IFR excludes industries with a very low prevalence of robots. Table A1 in the Appendix provides more details on the variables we use.

2.4 Details of the final sample

Figure 1 displays how acquisitions are distributed in Sweden over time, by industry and by country, for the acquired firms and workers in our sample. Our sample includes 467 acquisitions spanning the period of 1997-2015, and these firms employ 158,109 workers in the year before the acquisition. The most common industry for acquisitions is manufacturing, both in terms of the number of acquisitions and the number of affected workers. Foreign acquisitions in Sweden show a high level of pro-cyclicality, with a clear spike in 2001 following the spectacular bust of the dot-com bubble (Lerner and Tåg, 2013).

3 Empirical strategy

3.1 Design

To estimate the effects of acquisitions on worker wages, we employ a stacked difference-in-differences and triple difference design. This methodology enables us to create a control group of workers who are similar to the treated group in terms of key observable variables, both in pre-treatment trends and in levels in the year prior to the acquisition (Olsson and Tåg, 2017; **?**; Baker et al., 2022). In addition to being a widely used approach, the stacked design also addresses issues related to heterogeneous treatment effects that can be problematic in standard staggered two-way fixed effects models (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Baker et al., 2022).

3.2 Constructing the control group

To create the control group, we begin with all workers who were employed in the target firms and were between 25 and 55 years of age in the year before the acquisition. For each cohort, we conduct exact cell matching to find comparable workers in similar firms that were not part of any acquisitions. Within each cell, workers are randomly matched based on occupation, location (residence in a major city or not), firm type (Swedish MNE vs. Swedish local firm), and calendar year.

Next, we collect panel data for both treated and control workers in each cohort, creating cohort-specific panels. These panels are then stacked into a single panel and we align the timing of all treated and control workers to the year of matching/treatment. This enables us to use this normalized time to run standard difference-in-differences and triple difference regression models as if treatment occurred contemporaneously for all cohorts.

3.3 Comparing treated and control workers

Table 1 presents a comparison of the background, educational, and career characteristics of the treated and control workers. To test for mean differences between the two groups, we use the normalized t-value, which is necessary because standard t-values are affected by sample size and will decrease as sample size decreases. The normalized t-value divides the difference between the means of the two groups by the square root of the sums of their variances, eliminating this mechanical relationship (Imbens and Wooldridge, 2009). An absolute normalized t-value greater than 0.25 suggests significant differences in means.

Our analysis indicates that treated and control workers have similar characteristics on average, with normalized t-values well below 0.25. The majority of workers are male, reside in urban areas, and have more than three years of tenure at the firm prior to the acquisition.

3.4 Regression model

We use regression analysis to examine the effect of acquisitions on log wages for worker i at event year k and calender year t. Our baseline regression is a standard difference-in-difference (DID) model, which we estimate using the following equation:

$$w_{ikt} = \alpha_0 + \alpha_1 T_i + \alpha_2 Post_k + \beta DID_{ik} + \omega_t + X_i + X_f + \epsilon_{ikt}. \tag{1}$$

Here, $Post_k$ takes the value one in the year of the acquisition (k=0) and all years after. T_i takes the value one for workers who are employed in a firm that is acquired by a firm one year later (the treatment group) and zero for workers who, in the same year, are employed in a firm that is never subject to a acquisition (the control group). The interaction term DID_{ik} takes a value of one for treated workers in the year of the acquisition and all years after and zero otherwise. The key coefficient is β, which captures the average intention-to-treat effect.

We also control for other factors that could affect wages, such as calendar-year fixed effects ω_t, and worker and firm characteristics X_i and X_f, respectively. X_i includes controls for age, gender, education, experience, experience^2, a dummy if the person has been unemployed in the 2-4 years prior to the acquisition, a dummy for three or more years of tenure at the target firm, and municipality fixed effects. X_f includes log firm size, value added to employment, the share of high skilled workers, a dummy for Swedish MNE status and industry fixed effects. We measure all individual and firm level controls in the year prior to the acquisition.

To capture dynamic effects, we replace $Post_k$ with event time dummies and estimate the following dynamic model:

$$w_{ikt} = \alpha_0 + \tau_k + \alpha_1 T_i + \beta_k \sum_{k=-4}^{k=8} \tau_k \times T_i + \omega_t + X_i + X_f + \epsilon_{ikt} \tag{2}$$

Here, τ_k denotes event year fixed effects ranging from $k-4$ to $k+8$. We set $k-1$ as the baseline event year and β_k captures the average intention-to-treat effect during event time k. We examine in our base estimations the effect of acquisitions on mobility by comparing β_k up to eight years after acquisitions with the years before acquisitions.

To analyze the impact of exposure to automation and firm source country heterogeneity, we augment the model in equation (1) and allow the treatment effects to vary by occupational exposure to automation and the technological intensity of the acquirer. More specifically, we then estimate a triple difference estimator model specified as:

$$\begin{aligned} w_{ikt} = {} & \alpha_0 + \alpha_1 T_i + \alpha_2 Post_k + \beta_1 DID_{ik} + \mu_1 High_i + \mu_2 High_i \times T_i \\ & + \mu_3 High_i Post_k + \beta_2 DIDID_{ik} \omega_t + X_i + X_f + \epsilon_{ikt}, \end{aligned} \tag{3}$$

The triple difference estimator accounts for differences between the before-after period, the treated-control groups, and high-low occupational exposure groups. $High_i$ is an indicator variable equal to one if a worker is in a high-exposure occupation, zero otherwise. As described in Section 2.2, a worker is defined as being highly exposed to a particular technology if the worker has an occupation that places her in our sample's 90th percentile of workers exposed to that technology. The main variable of interest in equation (3) is β_2, the estimated coefficient for the the triple difference term $DIDID_{ik}$, equal to $Post_k \times T_i \times High_i$. This triple interaction term captures how wage differences between treated and non treated employees vary by exposure to different technologies. To take into account the impact of source country heterogeneity in technological intensity, we also estimate equation (3) separately by the technological intensity of the acquiring firm.

Finally, we also estimate a modified version of equation (3) to examine the dynamic effects, again as in equation (2), replacing $Post_k$ with event time dummies. In all regressions, we cluster the standard errors at the targeted firm level at the baseline event year to account for common within-firm shocks to workers, and at the target industry and baseline event year to account for common within-industry shocks.

3.5 Internal validity

The internal validity of the difference-in-difference estimator and the triple difference estimator is dependent on several factors. Firstly, the parallel trend assumption requires that the treated and control groups have similar trends in the absence of the acquisition. While it is impossible to formally test this assumption, we can assess its plausibility by comparing trends in outcome variables before treatment. Parallel pre-trends suggest that past shocks have affected the two groups similarly, making it likely that the same will hold true in the future. However, it is important to note that the key identifying assumption for the triple difference estimator is parallel trends in the triple difference and not parallel trends in the two difference-in-differences estimators that make up the triple difference estimator, as pointed out by Olden and Møen (2022).

Secondly, no coincidental events should be affecting workers at the same time as acquisition (i.e., there should be no significant time-varying unobservables). To account for this, we include industry, municipality, and year-fixed effects in the regressions, which control for yearly macroeconomic industry and regional shocks that could affect worker outcomes independently of the acquisition.

It is worth noting that unobservables at the individual level not captured by our matching procedure or control variables must be time-varying and correlate with the acquisition timing to be a concern for identification. For example, if our match fails to capture underlying characteristics related to ability that positively correlate with wage developments, we would only be concerned about these unobservables leading to a positive wage bias if these characteristics differentially affect career developments after a software/robot intense acquisition relative to a less software/robot intense acquisition, and these trend-shifts would be unrelated to the acquisition itself. However, it is difficult to conceive of such unobservables, particularly since our sample of acquisitions is spread out over time and across industries.

Finally, the stable unit treatment value assumption (SUTVA) states that there should be no spillovers between the treatment and control groups. To ensure this, we selected controls from the entire population of non-acquired workers, rather than relying on controls from the same narrow geographical area. This makes it unlikely that a acquisition in one part of Sweden will affect control workers in another part of Sweden.

3.6 External validity

To interpret the external validity of our results, it is important to consider that our study was conducted in a highly developed country. Sweden's GDP per capita is above the OECD average, the government enjoys a high level of public trust, corruption is relatively rare, and labor market protections are robust.

However, there are several factors that suggest our findings may be applicable in other contexts as well. First, Swedish employment law imposes no specific regulations regarding employment conditions after ownership changes. Instead, the rights and obligations towards workers transfer to the new owner, with existing employment contracts remaining in place unless workers opt to renegotiate. Additionally, ownership changes alone do not constitute grounds for terminating employment contracts, unless substantial organizational or economic restructuring occurs (LAS 1994:1685, paragraphs 6b and 7). Severance pay agreements are not guaranteed and are typically negotiated on an individual basis.

Second, collective bargaining agreements govern wage setting in nearly 90% of the Swedish labor market (Saez, Schoefer, and Seim, 2019). While these agreements are renegotiated every three years, the vast majority of workers are covered by contracts that leave firms free to adjust individual wages as they see fit. This flexibility is notable given the country's strong labor market protections.

Finally, evidence suggests that labor markets in Sweden behave similarly to those in other developed countries, including other Scandinavian nations, Belgium, France, Germany, Italy, the Netherlands, and the United States (Lazear and Shaw, 2009). This similarity further suggests that our findings may have broader applicability beyond Sweden.

4 Results

4.1 The overall effects of acquisitions on wages

In Figure 2, we investigate the effect of acquisitions on worker wages. Panel A displays the difference-in-differences estimates (β_k) relative to the year before the acquisition using Equation 2. Treated and control workers had similar wage developments before the acquisition, and this trend continued after the acquisition. Our sample's lack of a acquisition wage premium aligns with earlier evidence from Sweden in Heyman et al. (2007).

Panels B and C present estimates for acquisitions by more or less software intense acquirers. In both subsamples, there are no differential pre-trends in treatment, and wages remain unchanged after the acquisition. Table 2 presents key regression coefficients using the model specified in equation 1. Overall, and in the two subsamples, the difference-in-difference estimates are economically small in magnitude and far from statistically significant.

4.2 Software intensity and software exposure

We now turn to the effects on workers more likely to be replaced by technological changes implemented by software-intense acquirers. We proceed in five steps. First, in Figure 3, Panel A displays coefficients from a dynamic extension of the triple difference regression (equation 3) that compares treated-control, before-after, and high-low software exposure of workers employed in the target firm prior to the acquisition. Before the acquisition, relative wages evolve similarly, but after the acquisition, workers in occupations with high software exposure begin to see their relative wages deteriorate. The decline starts immediately after the acquisition and accelerates three years after, stabilizing around a 5% relative decline. Column 2 in Table 3 shows that the triple difference point estimate over the entire period is a 3.2% decline in wages. Figure A1 shows that this decline is driven by nominal wage decreases for the treated workers, with the trend for control workers remaining the same.

Second, we rerun the triple difference regressions, differentiating between the tech intensity of the acquirer (Panels B and C in Figure 3). Interestingly, the relative wage decline can only be attributed to software-intensive firms' acquisitions (Panel B). In contrast, we see no effects on relative wages for less software-intense firms (Panel C). Column 4 in Table 3 shows that the point estimate for software intense acquirers is a 4.2% decline in wages. Column 6 shows that less software-intense acquirers have no statistically significant impact on relative wages.

Third, we focus only on workers with high exposure to software working in firms acquired by software-intense acquirers (Panel D) and less software intense acquirers (Panel E). The panels now display difference-in-differences estimates, just comparing treated-control and before-after. Again, all of the relative declines in wages can be attributed to being acquired by software-intense firms (a 3.3% relative decline from column 3 in Table 3). In contrast, the relative wages of workers with jobs with high software exposure and acquired by less software intense firms remain unchanged (column 5 in Table 3). Again, Figure A1 shows that this decline is driven by nominal wage decreases for the treated workers, with the trend for control workers remaining the same.

Fourth, we now focus on wage changes within the firm by examining workers who remain with the company. Previous wage estimates have incorporated the effects of both in-firm wage changes and changes due to worker transitions between firms. Results from estimating a dynamic triple difference model to isolate the impact of within-firm changes are presented in panels A and B in Figure 4. These panels illustrate the coefficients for workers with high software exposure who continue to work for the company after a software-intense acquisition (panel A) and after a less software-intense acquisition (panel B) . The results show a significant decrease in relative wages for high software exposure workers following a software-intense acquisition, with the decline continuing throughout the entire post-period. After eight years, their relative wages are nearly 15% lower. Conversely, there is no decline in relative wages for less tech-intensive acquirers. Table 4 columns 2 and 5 present the triple difference point estimates of a 5.2% decline for tech-intense acquisitions over the entire period and no decline for less tech-intense acquisitions. These results highlight that the effects on high-exposure workers in intense software acquisitions drive the decline in relative wages. Next, we separately examine workers with high software exposure following high and low software-intense acquisitions. Panels C and D in Figure 4 show that the effect on relative wages arises from high-exposure workers in high software-intense acquisitions (Panel C). We here observe a 3.5% relative wage decline (as seen in column 1 in Table 4).

Finally, using a triple difference estimator, we investigate the relative wage changes between the most (at the 90th percentile of the exposure distribution) and least exposed (at the 10th percentile of the exposure distribution) workers who remain with the firm. Panels E and F in Figure 4 reveal a similar pattern, with the effects on wages concentrated among software-intense acquisitions. Specifically, columns 3 and 6 in Table 4 show a 5.7% relative wage decline for the most exposed workers and no relative wage decline for the least exposed workers in software-intense acquisitions over the entire period.

4.3 Robot intensity and robot exposure

So far, we have focused on the software intensity of the acquiring firm and the software exposure of workers in the acquired firm. A natural question to ask is if our argument holds for technologies other than software use.

To shed light on this issue, we focus on the robot intensity of the acquiring firm and the robot exposure to the job tasks performed by workers in the acquired firm. Figure A2 shows that the correlation between exposure to robots and exposure to software at the occupation level is relatively low. Panel A in Figure 5 displays coefficients from a dynamic difference-in-differences model run on the sample of firms in industries that use robots abroad. Similar to the software sample, we do not find any overall effects of acquisitions on relative wages, and there are no apparent pre-trends. The point estimate in column 1 of Table 5 is economically small and statistically insignificant. However, we observe relative wage declines of 2.7% for workers with high robot exposure (column 2 in Table 5). Panel B in Figure 5 and column 3 in Table 5 show that the triple difference point estimate for highly exposed workers over the entire period is a 3.7% wage decline.

Next, Panels C and D focus only on acquiring firms with high robot-intensity and report coefficient estimates from dynamic triple difference regressions comparing treated-control, before-after, and high and low robot exposure. Panel C investigates wages independently of staying with the firm, while Panel D restricts attention to stayers. Similar to the software intensity and exposure results, we observe a relative monotonic wage decline after the acquisition, reaching almost 10% for all workers and 15% for stayers. There are no statistically significant differences in relative wages in the pre-period. Columns 5 and 7 in Table 5 show that the point estimate for all workers is a relative 3.5% decline in wages, and for stayers, relative wages decline by 4.2% over the full post-period. Finally, Panels E and F display coefficients from dynamic difference-in-differences regressions that show that the effects in the triple difference specification are driven by workers in high robot-exposed occupations being acquired by firms with high robot exposure. From columns 4 and 6 in Table 5, the point estimates for the full post-period are a 2.1% relative decline for all workers and a 3.1% relative decline for stayers.

In summary, these results show that our argument that acquisitions can transfer technologies across borders extend to robot use as well.

4.4 Software intensity and AI exposure

Webb (2020) measures occupations' software exposure, robot exposure, and AI exposure. A natural question is whether our findings apply when using AI instead of software exposure. However, it is essential to note that AI exposure is distinct from software exposure. A high AI exposure does not necessarily mean that workers' job tasks are substitutes for software and databases. Therefore, it is unclear ex ante whether we should expect workers in AI-exposed jobs to experience relative wage increases or decreases.

Figure 6 Panel A presents coefficients from a dynamic triple difference model that compares treated-control, before-after, and high AI exposure workers to low AI exposure workers. Before the acquisition, relative wages evolve similarly, but after the acquisition, relative wages begin to increase and ended up just over 5% higher six to eight years later.

Panels B and C report coefficients from a dynamic triple difference regression focusing on high software-intensive and low-intensive acquirers. Similar to the findings regarding software exposure, the relative wage effects stem from high software-intense acquirers. Columns 3 and 5 in Table 6 show that the point estimates for the full post-period indicate a 3.5% relative increase in wages for acquisitions with high software intensity and no statistically significant effect on wages for less software-intense acquisitions.

Panels D and E in Figure 6 report coefficients obtained from a dynamic difference-in-differences model that focuses only on high AI-exposed occupations and software-intense acquirers (Panel D) and less software-intense acquirers (Panel E). The results indicate that the effect in the triple difference model is driven by workers with high AI exposure who are part of software-intense acquisitions. Columns 2 and 4 in Table 6 report the point estimates for the full period: a 2.3% relative increase for AI-exposed workers who are part of a software-intense acquisition and no effects for AI-exposed workers who are part of a less software-intense acquisition.

Thus, these findings suggest that working in an AI-exposed occupation is complementary to having a firm with high software intensity as a new owner. These positive wage effects on AI-exposed workers likely combine with the negative wage effects on software and robot-exposed workers to produce the economically small and statistically insignificant overall effect on wages from acquisitions reported in Figure 2 and Table 2.

5 Additional analyses and discussion

5.1 Firm level outcomes

In this subsection, we investigate firm-level observables related to the use of software and telecommunications. We aim to determine whether technology investments increase in relative terms following software-intensive acquisitions, compared to less software-intensive ones, when the acquiring firm is more tech-intense.

To investigate this question, we implement a firm-level match following the procedure outlined in Section 3. We also add information on firm-level expenditures related to software, data, and telecommunications from Statistics Sweden's official annual survey on expenditures on IT and marketing (data is available from 2009-2016).

Panel A in Figure 7 shows coefficients from a dynamic difference-in-differences model that compares treated-control and before-after an acquisition by a firm. Before the acquisition, firm-level expenditures related to data, telecommunications, and software evolve similarly, and this pattern continued after the acquisition. Column 1 in Table 7 confirms that the difference-in-differences coefficient is statistically insignificant. Panels B and C report coefficients from subsamples focusing on acquirers with high and low-intensity software and database use, respectively. Following high-intensity acquisitions, there is a slight increase in data, telecommunications, and software use (Panel B). Column 2 in Table 7 shows a statistically significant increase of 14 MSEK in expenditures. However, this is not the case for low-intensity acquisitions (Panel C in Figure 7, column 3 in Table 7). Finally, Panel D in 7 shows coefficients from a dynamic triple-difference model comparing high-low intensity acquisitions. There are some indications of a relative increase from four years after the acquisition, but the coefficients are not statistically significant. The lack of statistically significant relative effects for the full post-period is verified in column 4 in Table 7.

In the Appendix, we also analyze additional firm outcomes, such as the number of employees, sales, value added per employee, and the wage bill of the firm. These results are displayed in Figure A3 and Table A3. We do not observe any statistically significant triple-difference results between acquirers with high and low software intensity. There is also no evidence of pre-trends in firm-level observables.

In summary, higher expenditures on software, data, and telecommunications follow high-intensity acquisitions, but the relative difference to low-intensity acquisitions is not statistically significant.

5.2 Other winners from automation

This section investigates two other groups of workers who might possibly benefit from software or robot-intense acquisitions. First, existing literature shows that automation increases wages for high-skill workers/professionals and managers (Aghion, Antonin, Bunel, and Jaravel, 2022). Thus, we replicate Table 3 for professionals and managers only. Panel A in Table 8 displays the results. Columns 1-3 show that employees in the professionals category experience overall wage increases of 1.6% (column 1). The professionals who are part of high-intensity acquisitions drive these results: for them, the wage increase is 2.6% (column 2). We observe no change for professionals who are part of low-intensity acquisitions. A similar pattern can be observed for managers (columns 4-6), except that the positive coefficient for the entire sample is not statistically significant (column 4). Managers who are part of high-intensity acquisitions experience a 2.1% increase in wages (column 5), while there are no effects for managers who are part of low-intensity acquisitions (column 6).

Second, workers with stronger employment protections may benefit as they are harder to fire and are thus more likely to be re-positioned or retrained to work with newer technologies. Sweden's labor regulations stipulate a "last-in-first-out" rule, meaning workers with longer tenure have stronger employment protections. To take this into account, we replicate Table 3 for workers with more than five years of tenure. Panel B in Table 8 displays the results. The results are now significantly muted. Comparing the two tables, we no longer find any adverse wage effects overall (column 1) or for workers who are part of high-intensity acquisitions (column 3). The triple-difference estimates in columns 2 and 4 are reduced by up to 50%.

5.3 Software intensity and offshoring exposure

One may be concerned that offshoring activities by the new owners drive our results. This concern is particularly relevant if occupations with high software and robot exposure are also highly likely to be offshored. To address this concern, we replicate our analysis using the offshorability exposure measure developed by Blinder and Krueger (2013) in place of the software exposure measure.

In the Appendix, we present Table A2, which displays the results of our analysis. We find no evidence of visible wage effects for workers with high offshoring exposure, either for the entire sample (column 1) or for the high or low software intense acquirer subsamples (columns 3 and 5). We still find no wage effects following the acquisition when we focus on workers with high offshoring exposure (columns 2 and 4). These results suggest that offshoring is not driving our findings.

6 Concluding summary

In this paper we posit that multinational enterprises transfer technologies for automation across borders through acquisitions, and that the adoption of technology directly affect workers' careers. We document heterogeneity in the patterns through which cross border technology diffusion occurs and show that the spread of technology is contingent on the technological intensity of the acquiring firm. Technology adoption occurs in acquisitions where the acquirer has superior technological intensity compared to the target firm, but not in acquisitions where the acquirer is from an industry with inferior technological intensity. Specifically, we focus on exposure to software and robotics and show that acquisitions of firms disproportionately affect the wages of workers that perform tasks sensitive to the technological intensity of the firm that undertakes the acquisition.

References

Aghion, P., C. Antonin, S. Bunel, and X. Jaravel (2022, June). *The Effects of Automation on Labor Demand* (First ed.)., pp. 15–39. London: Routledge.

Agrawal, A. and P. Tambe (2016, September). Private Equity and Workers' Career Paths: The Role of Technological Change. *The Review of Financial Studies 29*(9), 2455–2489.

Akcigit, U. and M. Melitz (2022). International trade and innovation. In *Handbook of International Economics*, Volume 5, pp. 377–404. Elsevier.

Antoni, M., E. Maug, and S. Obernberger (2019, September). Private Equity and Human Capital Risk. *Journal of Financial Economics 133*(3), 634–657.

Arnold, D. (2021). Mergers and Acquisitions, Local Labor Market Concentration, and Worker Outcomes. *SSRN Electronic Journal*.

Arnold, D. (2022, October). The Impact of Privatization of State-Owned Enterprises on Workers. *American Economic Journal: Applied Economics 14*(4), 343–380.

Bach, L., R. P. Baghai, M. Bos, and R. C. Silva (2023). How Do Acquisitions Affect the Mental Health of Employees? pp. 70.

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

Blinder, A. S. and A. B. Krueger (2013, April). Alternative Measures of Offshorability: A Survey Approach. *Journal of Labor Economics 31*(S1), S97–S128.

Bloom, N., R. Sadun, and J. V. Reenen (2012, February). Americans Do IT Better: US Multinationals and the Productivity Miracle. *American Economic Review 102*(1), 167–201.

Branstetter, L. (2006, March). Is foreign direct investment a channel of knowledge spillovers? Evidence from Japan's FDI in the United States. *Journal of International Economics 68*(2), 325–344.

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

Cohn, J., N. Nestoriak, and M. Wardlaw (2021, October). Private Equity Buyouts and Workplace Safety. *The Review of Financial Studies 34*(10), 4832–4875.

Fang, L., J. Goldman, and A. Roulet (2022). Private Equity and Pay Gaps Inside the Firm. pp. 32.

Garcia-Gomez, P., E. G. Maug, and S. Obernberger (2022). Private Equity Buyouts and Employee Health. *SSRN Electronic Journal*.

Gehrke, B., E. Maug, S. Obernberger, and C. Schneider (2022). Post-merger Restructuring of the Labor Force. ECGI Finance Working paper 753/2021, European Corporate Governance Institute.

Gehrke, B., E. Maug, S. Obernberger, and C. Schneider (2023). Post-merger Restructuring of the Labor Force.

Guadalupe, M., O. Kuzmina, and C. Thomas (2012). Innovation and Foreign Ownership. *The American Economic Review 102*(7), 46.

He, A. X. and D. le Maire (2022). Mergers and Managers: Manager-Specific Wage Premiums and Rent Extraction in M&As. *SSRN Electronic Journal*.

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

Heyman, F., F. Sjöholm, and P. G. Tingvall (2011, May). Multinationals, cross-border acquisitions and wage dispersion: Multinationals, cross-border acquisitions. *Canadian Journal of Economics 44*(2), 627–650.

Imbens, G. W. and J. M. Wooldridge (2009, March). Recent Developments in the Econometrics of Program Evaluation. *Journal of Economic Literature 47*(1), 5–86.

Keller, W. (2010). International Trade, Foreign Direct Investment, and Technology Spillovers. In *Handbook of the Economics of Innovation*, Volume 2, pp. 793–829. Elsevier.

Lagaras, S. (2021). Corporate Takeovers and Labor Restructuring. *SSRN Electronic Journal*.

Lagaras, S. (2023). M&As, Employee Costs, and Labor Reallocation. *Journal of Finance*.

Lazear, E. P. and K. L. Shaw (2009, January). Wage Structure, Raises and Mobility: An Introduction to International Comparisons of the Structure of Wages Within and Across Firms. In *The Structure of Wages: An International Comparison*, pp. 1–57. University of Chicago Press.

Lerner, J. and J. Tåg (2013, February). Institutions and Venture Capital. *Industrial and Corporate Change 22*(1), 153–182.

Ma, W., P. P. Ouimet, and E. Simintzi (2022). Mergers and Acquisitions, Technological Change and Inequality. *SSRN Electronic Journal*.

Olden, A. and J. Møen (2022, March). The Triple Difference Estimator. *The Econometrics Journal*.

Olsson, M. and J. Tåg (2017, July). Private Equity, Layoffs, and Job Polarization. *Journal of Labor Economics 35*(3), 697–754.

Olsson, M. and J. Tåg (2018, November). Are Foreign Private Equity Buyouts Bad for Workers? *Economics Letters 172*, 1–4.

Olsson, M. and J. Tåg (2023). What is the Cost of Privatization for Workers? *SSRN Electronic Journal*.

Prager, E. and M. Schmitt (2021, February). Employer Consolidation and Wages: Evidence from Hospitals. *American Economic Review 111*(2), 397–427.

Saez, E., B. Schoefer, and D. Seim (2019, May). Payroll Taxes, Firm Behavior, and Rent Sharing: Evidence from a Young Workers' Tax Cut in Sweden. *American Economic Review 109*(5), 1717–1763.

Setzler, B. and F. Tintelnot (2021, June). The Effects of Foreign Multinationals on Workers and Firms in the United States. *The Quarterly Journal of Economics 136*(3), 1943–1991.

Siegel, D. S. and K. L. Simons (2010). Assessing the effects of mergers and acquisitions on firm performance, plant productivity, and workers: New evidence from matched employer-employee data. *Strategic Management Journal*, n/a–n/a.

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

Webb, M. (2020). The Impact of Articial Intelligence on the Labor Market.

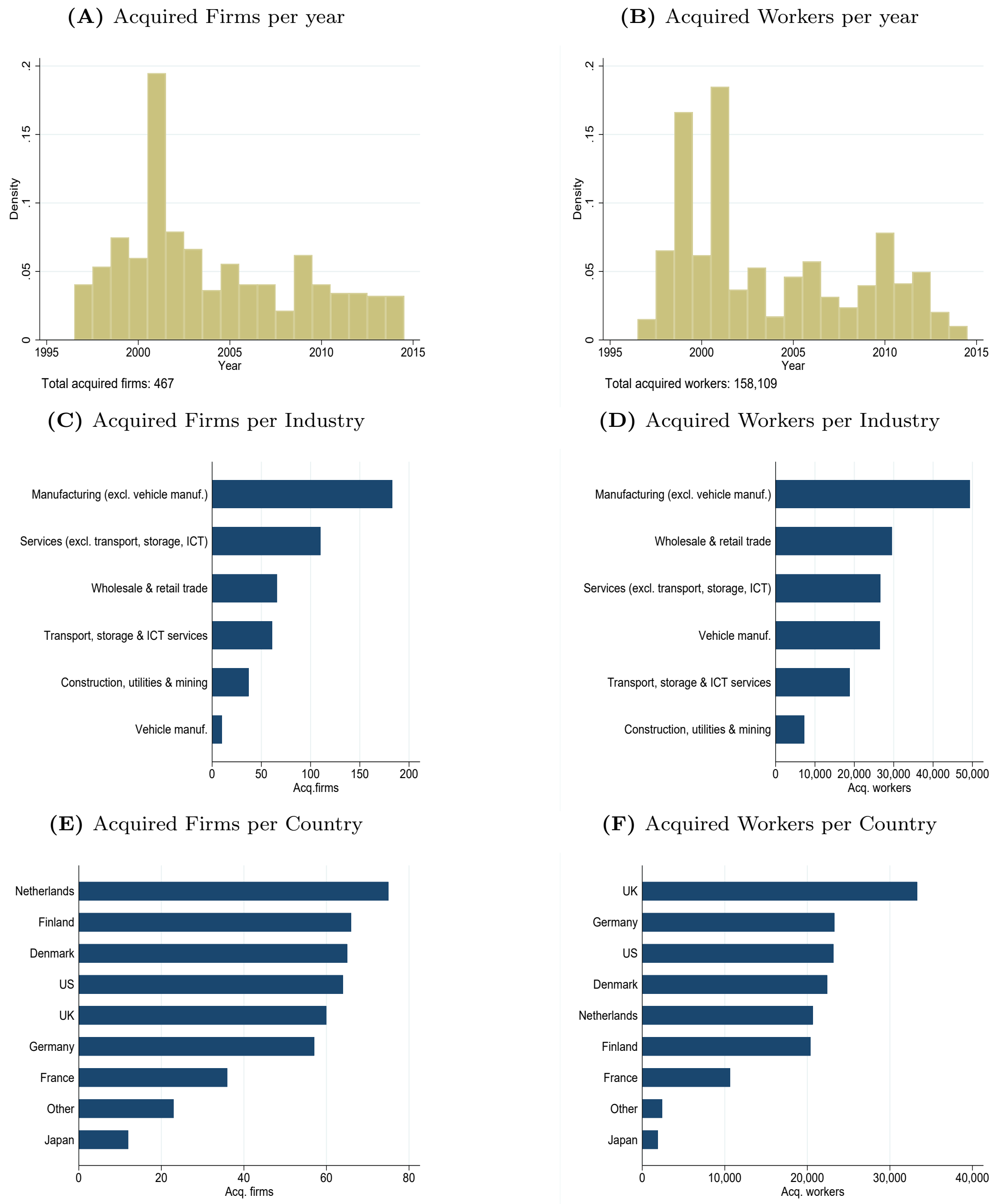
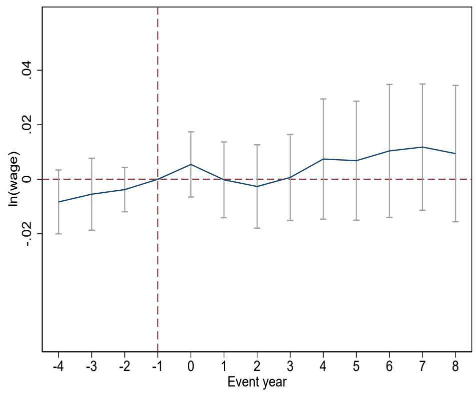
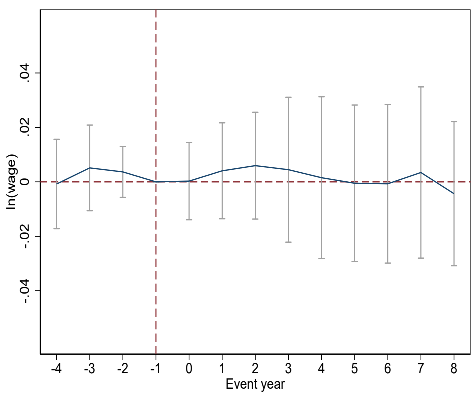


Figure 1: Foreign acquisitions over time, across industries and countries
Panels A and B depict the temporal distribution of our foreign acquisition sample at both the firm and worker levels. Panels C and D illustrate the distribution across various industry groups at the firm and worker levels, respectively. Panels E and F present the distribution across the most frequently occurring acquiring countries at the firm and worker levels, respectively.

(A) All



(B) High Software Intensity



(C) Low Software Intensity

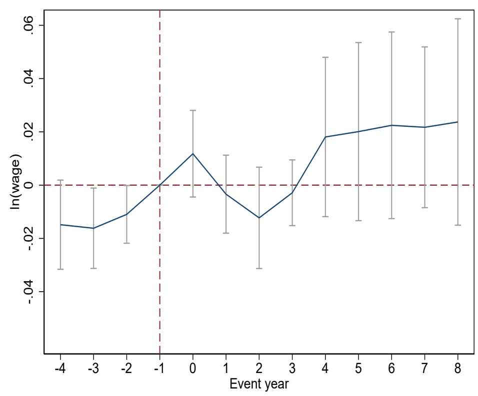
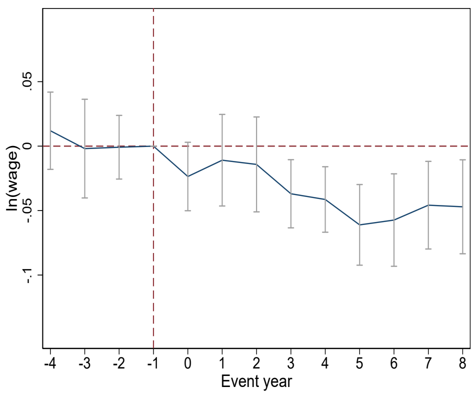
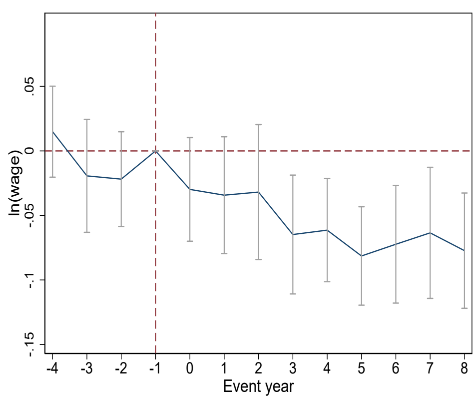


Figure 2: Wage effects of foreign acquisitions in the EU KLEMS sample
The figures depict annual difference-in-differences estimates relative to the year preceding the foreign acquisition (event time -1), using dynamic variants of the regressions presented in columns 1-3 in Table 2. The acquisitions are differentiated based on their acquirer's software and database capital intensity, with High (Low) Software Intensity denoting acquisitions with high (low) intensity. The vertical bars represent 95% confidence intervals, with robust standard errors clustered at the acquisition industry-acquisition year and acquiring firm-acquisition year levels.

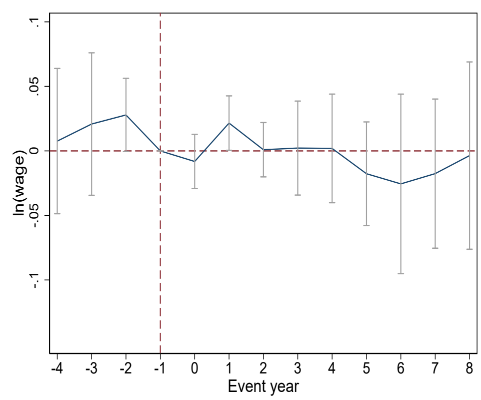
(A) DDD High Software Exposed



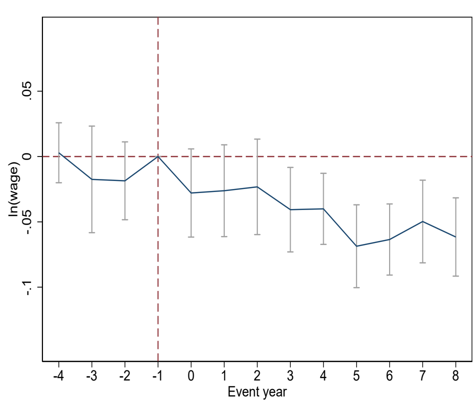
(B) High Intensity: DDD High Exposed



(C) Low Intensity: DDD High Exposed



(D) High Intensity & High Exposed



(E) Low Intensity & High Exposed

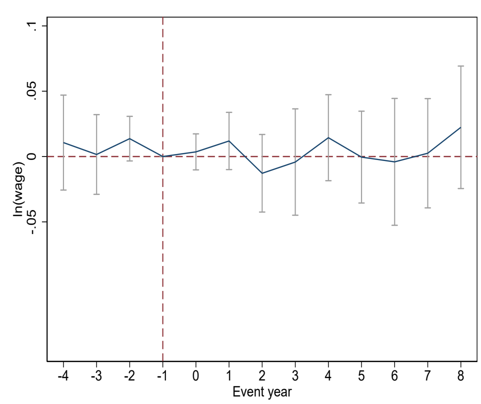
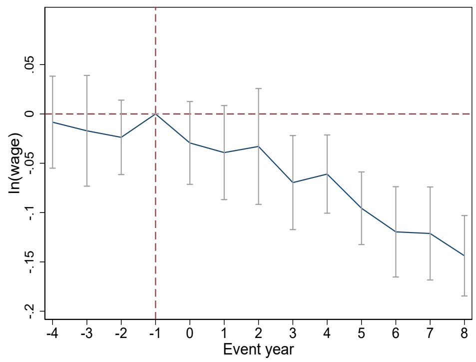
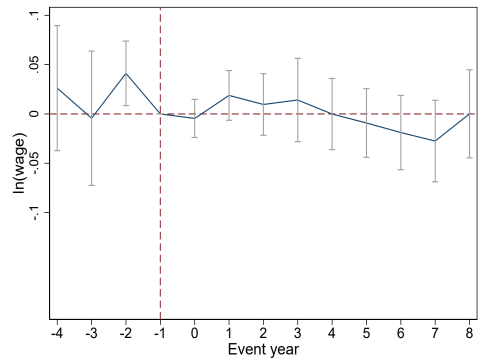


Figure 3: Wage effects of foreign acquisitions for high software exposed occupations
The figures show yearly estimates for difference-in-difference-in-difference (DDD) and difference-in-difference effects relative to the year preceding the foreign acquisition (event time -1), using dynamic variants of the regressions presented in columns 2, 4, 6, 3, and 5 in Table 3. We differentiate between acquisitions with high (low) software and database capital intensity, denoted as High (Low) Intensity, and workers in high software-exposed occupations, referred to as High Exposed. The vertical bars represent 95% confidence intervals, computed using robust standard errors clustered at the acquisition industry - acquisition year and acquiring firm - acquisition year level.

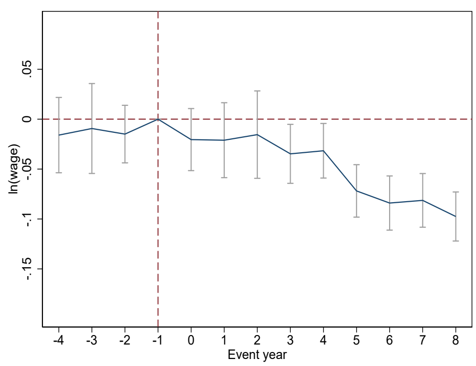
(A) High Intensity: DDD High Exposed



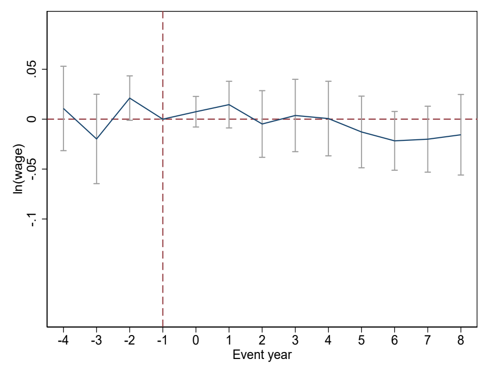
(B) Low Intensity: DDD High Exposed



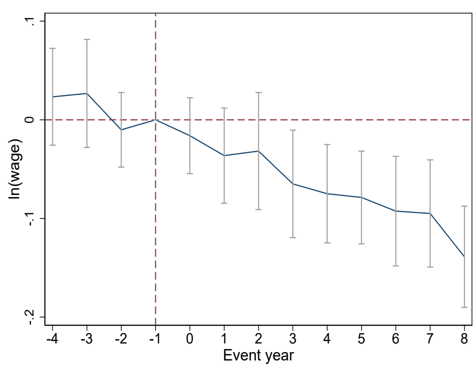
(C) High Intensity & High Exposed



(D) Low Intensity & High Exposed



(E) High Intensity: DDD High vs Low Exposed



(F) Low Intensity: DDD High vs Low Exposed

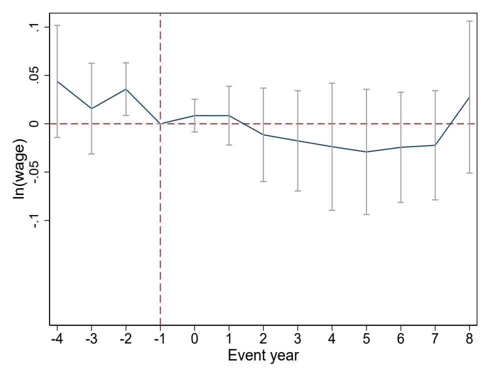


Figure 4: Stayers: Wage effects of foreign acquisitions for high software exposed occupations
The figures show yearly estimates of difference-in-difference-in-difference (DDD) and difference-in-difference effects relative to the year prior to the foreign acquisition (event time -1), using dynamic variants of the regressions in columns 2, 5, 1, 4, 3, and 6 in Table 4. The analysis focuses on a sample of workers who remain employed in the acquiring firm, with individuals leaving the sample once they leave the acquired firm. We differentiate between acquirers coming from country-industry combinations with high and low software and database capital intensity (High and Low Intensity). Additionally, we consider workers in high software-exposed occupations (High Exposed), defined as those in the top 90th percentile of the exposure distribution. The vertical bars show 95% confidence intervals based on robust standard errors clustered at the acquisition industry-year and acquiring firm-year levels.

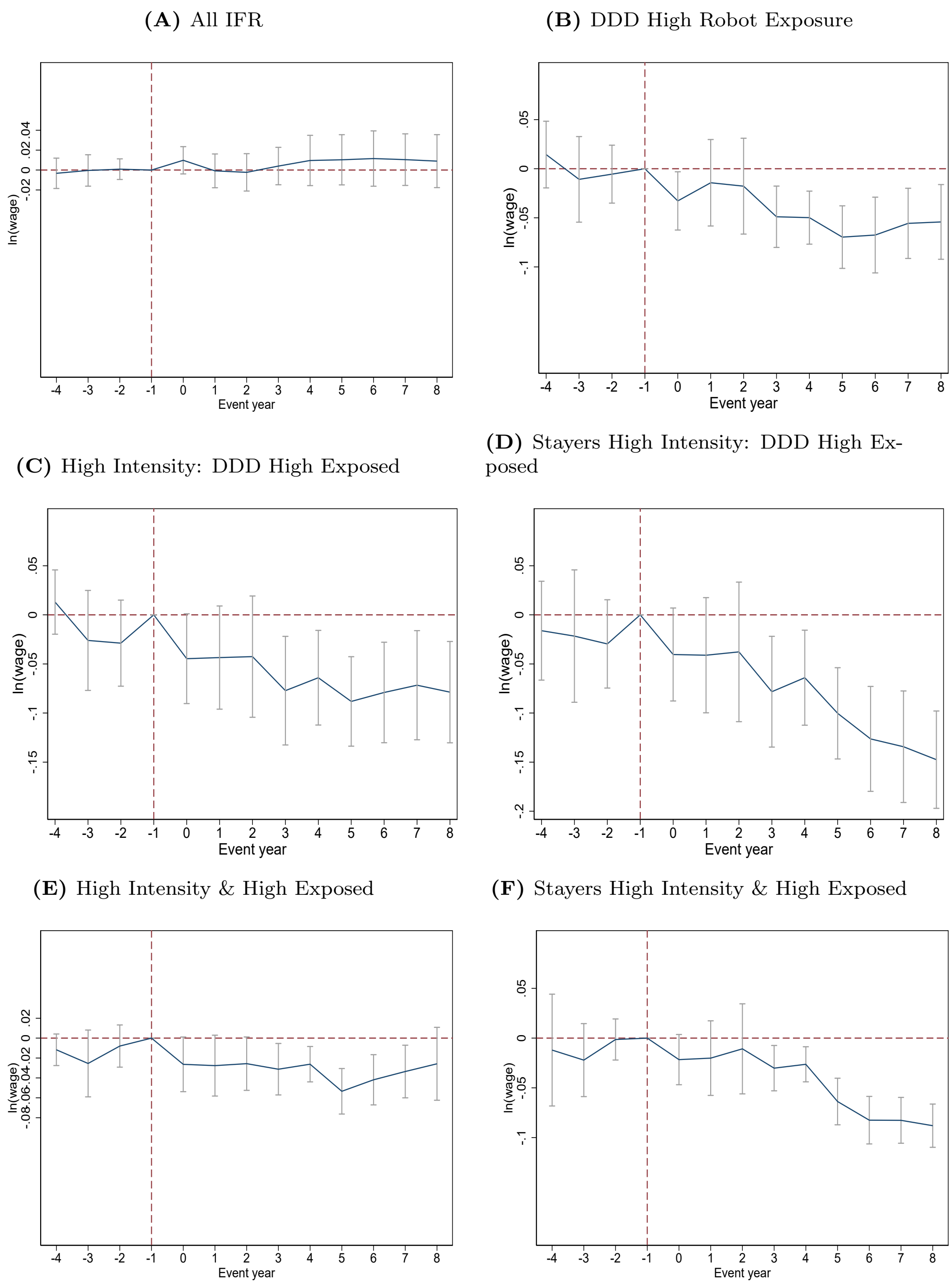


Figure 5: Wage effects of foreign acquisitions for high robot exposed occupations in the IFR sample The figures illustrate yearly difference-in-difference-in-difference (DDD) and difference-in-difference estimates relative to the year before the foreign acquisition (event time -1), using dynamic variants of the regressions in columns 1, 2, 4, 6, 3, and 5 in Table 5. We distinguish between acquisitions with high and low acquirer robot stock to employment (High/Low Intensity), and between workers in high robot exposure occupations and those in other occupations (High Exposed). The sample only includes stayers, i.e. workers who remain employed at the acquired firm. The vertical bars represent 95% confidence intervals using robust standard errors clustered at the acquisition industry - acquisition year and acquiring firm - acquisition year level.

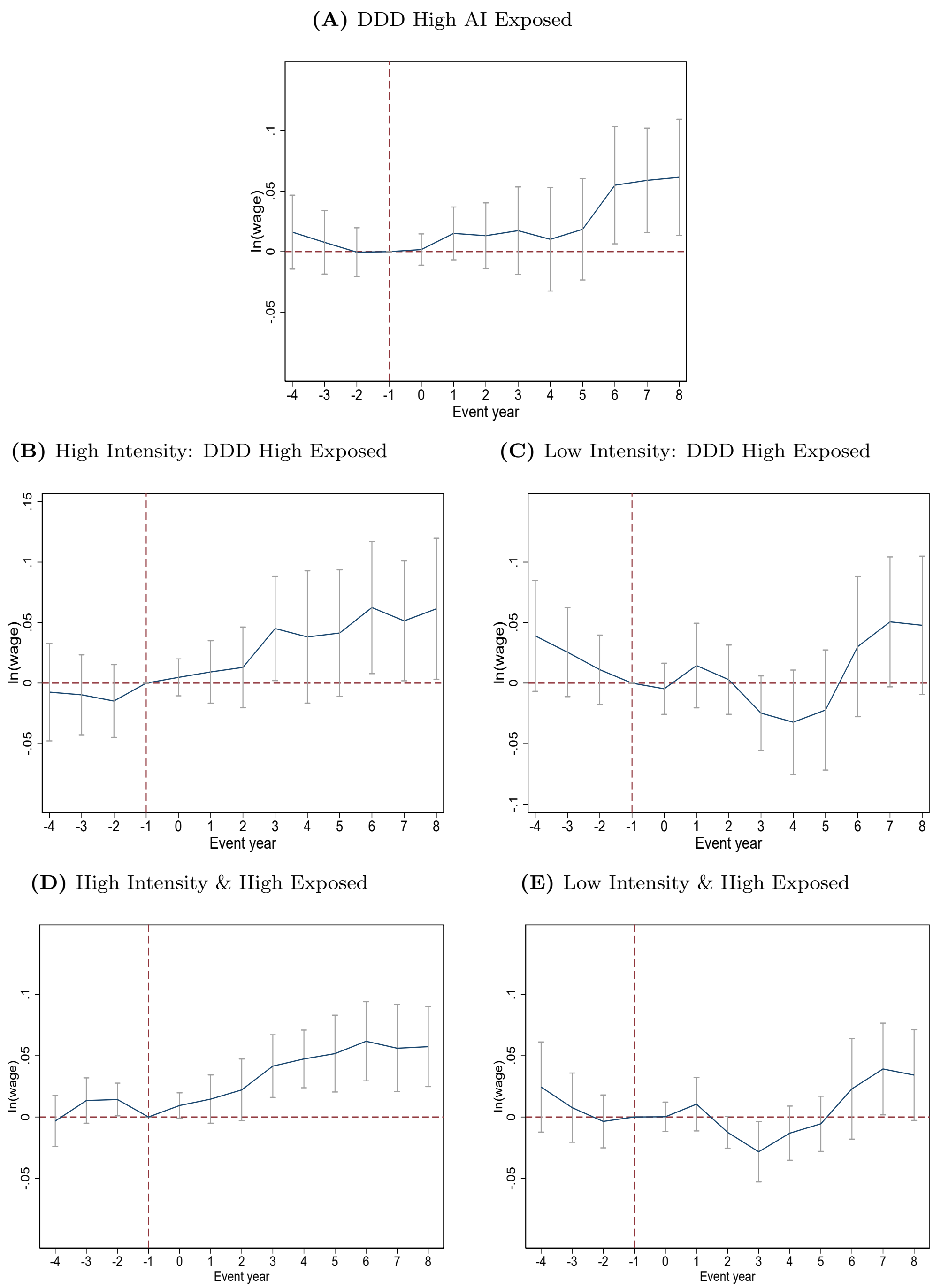


Figure 6: Wage effects of foreign acquisitions for high AI exposed occupations
The figures present yearly estimates of difference-in-difference-in-difference (DDD) and difference-in-difference relative to the year prior to the foreign acquisition (event time -1), based on dynamic versions of the regressions presented in columns 1, 3, 5, 2, and 4 in Table 6. We distinguish between acquisitions with high (low) acquirer software and database capital intensity, denoted as High (Low) Intensity, and workers in high AI-exposed occupations, denoted as High Exposed. The vertical bars represent 95% confidence intervals calculated using robust standard errors clustered at both the acquisition industry - acquisition year and acquiring firm - acquisition year levels.

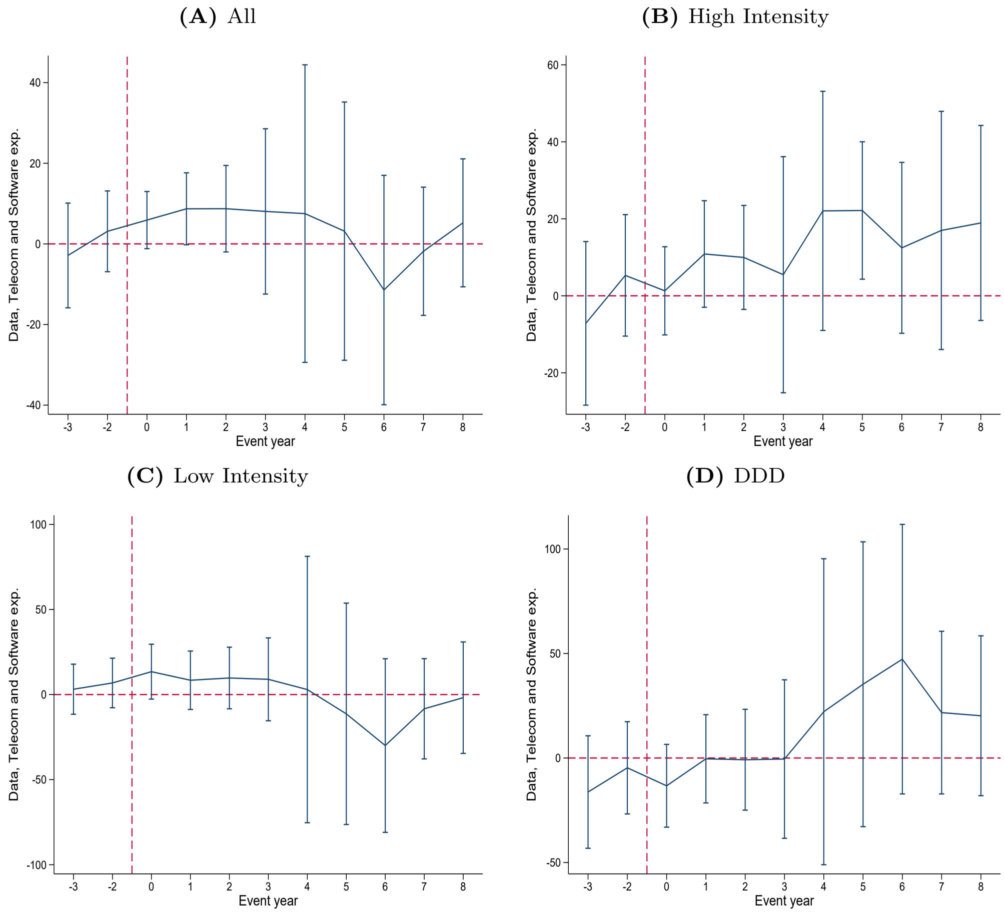


Figure 7: Firm level outcomes
The figures illustrate the yearly difference-in-difference-in-difference (DDD) and difference-in-difference estimates, relative to the year before the foreign acquisition (event time -1), based on dynamic variants of the regressions in columns 1, 2, 3, and 4 in Table 7. High (Low) Intensity refers to acquisitions with high (low) acquirer software and database capital intensity. The vertical bars represent 95% confidence intervals using robust standard errors.

Table 1: Comparison of treated and control workers

This table presents the mean characteristics of both treated and control workers one year before the foreign acquisition (columns 1 and 2), the difference between the two (column 3), and a normalized t-test for mean differences (column 4). A normalized t-test above 0.25 indicates significant differences in means (Imbens and Wooldridge, 2009). The table shows the observable characteristics we match on, such as major city resident and Swedish MNE (in addition to acquisition year and occupation). Additionally, it includes a set of observable individual and firm-level characteristics that we do not include in the match.

	Treated	**Control**	**Difference**	**Norm. T-value**
	(1)	(2)	(3)	(4)
Individual variables				
ln wage	9.988	9.980	0.008	0.018
Software exposure	0.541	0.541	0	0.000
Robot exposure	0.512	0.512	0	0.000
AI exposure	0.528	0.528	0	0.000
Age	39.39	40.97	-1.581	-0.128
Education (1-7)	3.712	3.657	0.055	0.028
Experience	20.67	22.33	-1.654	-0.125
Experience^2	513.8	588.4	-74.55	-0.126
Female (%)	0.348	0.341	0.007	0.011
Major city resident (%)	0.693	0.693	0	0.000
Prev. unemp (%)	0.117	0.104	0.013	0.030
$\geq$ 3 year tenure (%)	0.556	0.666	-0.110	-0.161
Firm variables				
ln Firm size	7.158	7.223	-0.065	-0.027
Share high skilled (%)	0.289	0.300	-0.011	-0.034
Swedish MNE (%)	0.524	0.524	0	0.000
VA/L	0.556	0.669	-0.112	-0.135
Observations	158,109	158,109	316,218	

Table 2: Foreign acquisitions and wages

This table presents key coefficients from difference-in-differences regressions that explain the changes in log wages around foreign acquisitions. We differentiate between acquirers from country-industry combinations with high and low software and database capital intensity (High vs Low Intensity). The sample comprises treated workers employed one year before acquisition and matched control workers. To control for individual and firm-level factors, we include variables such as age, gender, education, experience, experience^2, unemployment incidence in the years -4 to -2, 3 or more years of tenure, and municipality, as well as firm-level variables such as log firm size, VA/L, Swedish MNE status, industry, calendar year, and a constant. All controls, except calendar year, are measured one year before the acquisition. The standard errors are clustered at the acquisition industry and year level and acquisition firm and year level. Asterisks indicate the significance levels: *** 1%, ** 5%, * 10%.

Sample	All	High Intensity	Low Intensity
	(1)	(2)	(3)
Post	0.061***	0.067***	0.050***
	(0.004)	(0.006)	(0.005)
T	0.024*	0.008	0.031
	(0.013)	(0.012)	(0.021)
Post*T	0.006	-0.001	0.014
	(0.008)	(0.011)	(0.010)
$\bar{R}^2$	0.466	0.477	0.466
Obs	2,360,631	1,217,070	1,143,558

Table 3: Wages by technological intensity of the foreign acquirer for high software-exposed occupations

This table presents selected coefficients from both difference-in-differences and difference-in-difference-in-differences regressions, which aim to explain log wages in the context of foreign acquisitions. We differentiate between acquirers from country-industry combinations with high versus low software and database capital intensity (High vs Low Intensity). The sample comprises treated workers employed one year before the acquisition and matched control workers. We focus on two groups: all workers (All), and workers in high software-exposed occupations (High Exposed), which refers to occupations in the 90th percentile of workers exposed to software. We control for individual variables such as age, gender, education, experience, experience squared, unemployment incidence in the years -4 to -2, 3 or more years of tenure, and municipality, as well as firm-level variables including log firm size, VA/L, Swedish MNE status, and industry. We also include calendar year and a constant. All controls, except calendar year, are measured one year before the acquisition. Standard errors are clustered at the acquisition industry and year level, as well as the acquisition firm and year level. Levels of significance are denoted as follows: *** for 1%, ** for 5%, and * for 10%.

Sample	**All**		**High Intensity**		**Low Intensity**	
	High Exposed	All	High Exposed	All	High Exposed	All
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.021*	0.062***	0.017	0.069***	0.021**	0.052***
	(0.012)	(0.004)	(0.012)	(0.007)	(0.009)	(0.005)
T	0.041***	0.023*	0.048**	0.012	-0.004	0.031
	(0.014)	(0.013)	(0.021)	(0.011)	(0.014)	(0.022)
Post*T	-0.024***	0.009	-0.033***	0.004	-0.003	0.015
	(0.007)	(0.008)	(0.009)	(0.011)	(0.016)	(0.011)
High Exposed		-0.102***		-0.099***		-0.096***
		(0.010)		(0.012)		(0.018)
Post*High Exposed		-0.019***		-0.018**		-0.023**
		(0.007)		(0.008)		(0.010)
T*High Exposed		0.017		0.022		0.021
		(0.015)		(0.020)		(0.020)
Post*T*High Exposed		-0.032***		-0.042***		-0.013
		(0.011)		(0.015)		(0.024)
$\bar{R}^2$	0.482	0.475	0.462	0.486	0.543	0.471
Obs	208,171	2,360,631	129,269	1,217,070	78,899	1,143,558

Table 4: Wages by technological intensity of the foreign acquirer for stayers

This table presents selected coefficients from difference-in-differences and difference-in-difference-in-differences regressions examining log wages around acquisitions. We distinguish between acquirers from country-industry combinations with higher versus lower software and database capital intensity (High vs Low Intensity). The sample includes treated workers employed one year before the acquisition and matched control workers who remain employed by the acquiring or control firm; workers exit the sample upon leaving the firm. We analyze all workers (All) still employed by the firm, workers in high software-exposed occupations (High Exposed) still employed by the firm, or workers in both high and low software-exposed occupations (High & Low Exposed) still employed by the firm. High Exposed denotes workers in occupations in the 90th percentile of software exposure, and Low Exposed represents workers in the lowest 10th percentile. We control for individual variables such as age, gender, education, experience, experience squared, unemployment incidence in the years -4 to -2, 3 or more years of tenure, and municipality and firm-level variables, including log firm size, VA/L, Swedish MNE status, industry, calendar year, and a constant. All controls except calendar year are measured one year before the acquisition. We cluster the standard errors at the acquisition industry and year level and the acquisition firm and year level. Levels of significance are indicated as *** for 1%, ** for 5%, and * for 10%.

Sample	**High Intensity**			**Low Intensity**		
	High Exposed	All	High & Low Exp.	High Exposed	All	High & Low Exp.
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.011	0.058***	0.069***	0.024***	0.047***	0.051***
	(0.014)	(0.006)	(0.012)	(0.008)	(0.005)	(0.011)
T	0.054**	0.019*	-0.001	0.002	0.033	-0.003
	(0.022)	(0.011)	(0.017)	(0.017)	(0.022)	(0.020)
Post*T	-0.035***	0.005	0.013	-0.006	-0.008	-0.006
	(0.010)	(0.012)	(0.014)	(0.013)	(0.007)	(0.015)
High Exposed		-0.102***	-0.220***		-0.099***	-0.188***
		(0.013)	(0.020)		(0.016)	(0.015)
Post*High Exposed		-0.006	-0.035***		-0.007	-0.036***
		(0.008)	(0.009)		(0.010)	(0.011)
T*High Exposed		0.019	0.048*		0.026	0.053**
		(0.020)	(0.027)		(0.018)	(0.022)
Post*T*High Exp.		-0.052***	-0.057***		-0.006	-0.020
		(0.014)	(0.016)		(0.012)	(0.023)
$\bar{R}^2$	0.488	0.488	0.527	0.504	0.473	0.509
Obs	106,020	1,045,728	230,687	55,193	909,815	211,132

Table 5: Wages by robot intensity of the foreign acquirer for high robot-exposure occupations

This table presents selected coefficients from difference-in-differences and difference-in-difference-in-differences regressions that explain log wages around acquisitions. The analysis focuses on workers employed one year before the acquisition and matched control workers in industries covered by the IFR robot data. Specifically, we differentiate between acquirers from country-industry combinations with high robot stock to employment intensity (High Intensity). We examine three samples: all workers (All), workers in high robot-exposed occupations (High Exposed), and workers in the High Intensity sample who remain in the acquiring firm (Stayers: High Intensity). High robot-exposed refers to workers in occupations in the 90th percentile of those exposed to robotization. We control for individual variables including age, gender, education, experience, experience^2, unemployment incidence in the years -4 to -2, 3 or more years of tenure, and municipality, as well as firm-level variables such as log firm size, VA/L, Swedish MNE status, industry, calendar year, and a constant. All controls, except calendar year, are measured one year before the acquisition. Standard errors are clustered at the acquisition industry-year and acquisition firm-year levels. Three asterisks indicate levels of significance for 1%, two asterisks for 5%, and one asterisk for 10%.

Sample	**All**			**High Intensity**		**Stayers: High Intensity**	
	All	High Exp.	All	High Exposed	All	High Exposed	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post	0.061***	0.024*	0.061***	0.040***	0.069***	0.038***	0.056***
	(0.005)	(0.014)	(0.005)	(0.010)	(0.008)	(0.008)	(0.008)
T	0.010	0.036**	0.008	0.026***	0.014	0.036***	0.012
	(0.018)	(0.015)	(0.018)	(0.008)	(0.029)	(0.011)	(0.029)
Post*T	0.005	-0.027***	0.009	-0.021***	0.019	-0.031***	0.005
	(0.009)	(0.008)	(0.010)	(0.007)	(0.013)	(0.006)	(0.016)
High Exposed			-0.113***		-0.116***		-0.122***
			(0.012)		(0.016)		(0.016)
Post*High Exposed			-0.014**		-0.015		0.004
			(0.007)		(0.009)		(0.008)
T*High Exposed			0.030*		0.020		0.031
			(0.018)		(0.021)		(0.020)
Post*T*High Exp.			-0.037***		-0.035**		-0.042***
			(0.012)		(0.015)		(0.016)
$\bar{R}^2$	0.456	0.498	0.466	0.481	0.480	0.470	0.484
Obs	1,743,214	177,438	1,743,214	137,951	1,068,343	105,226	877,849

Table 6: Wages by technological intensity of the foreign acquirer for high AI-exposed occupations

This table presents selected coefficients obtained from difference-in-differences regressions that aim to explain the changes in log wages around acquisitions. The sample comprises treated workers employed one year before the acquisition and their matched control workers. We distinguish between acquirers from country-industry combinations with high or low software and database capital intensity (High and Low Intensity) and focus on either all workers (All) or workers in highly AI-exposed occupations (High Exposed), which are defined as those in the 90th percentile of workers exposed to AI. We control for individual variables such as age, gender, education, experience, experience squared, unemployment incidence in the years -4 to -2, 3 or more years of tenure, and municipality, as well as firm-level variables including log firm size, VA/L, Swedish MNE status, industry, calendar year, and a constant. All controls except calendar year are measured one year before the acquisition. Standard errors are clustered at the acquisition industry and year level, and the acquisition firm and year level. Levels of significance are denoted as *** for 1%, ** for 5%, and * for 10%.

Sample	**All**	**High Intensity**		**Low Intensity**	
	All	High Exposed	All	High Exposed	All
	(1)	(2)	(3)	(4)	(5)
Post	0.055***	0.115***	0.060***	0.086***	0.044***
	(0.004)	(0.011)	(0.006)	(0.008)	(0.005)
T	0.030**	-0.017	0.014	-0.048***	0.039*
	(0.014)	(0.011)	(0.011)	(0.010)	(0.023)
Post*T	0.005	0.023**	-0.004	-0.009	0.016
	(0.007)	(0.011)	(0.010)	(0.010)	(0.011)
High Exposed	0.079***		0.062***		0.100***
	(0.011)		(0.014)		(0.014)
Post* High Exposed	0.051***		0.054***		0.045***
	(0.009)		(0.011)		(0.010)
T*High Exposed	-0.064***		-0.059***		-0.086***
	(0.016)		(0.019)		(0.029)
Post*T*High Exposed	0.014		0.035**		-0.018
	(0.015)		(0.017)		(0.021)
$\bar{R}^2$	0.472	0.443	0.483	0.386	0.471
Obs	2,360,631	127,231	1,217,070	104,760	1,143,558

Table 7: Firm level outcomes

This table reports selected coefficients from difference-in-differences regressions explaining firm-level expenditures on data, telecommunications and software around acquisitions. We differentiate between high and low software-intense foreign acquirers. Standard errors are clustered at the acquisition industry and year level, and the acquisition firm and year level. Levels of significance are denoted as *** for 1%, ** for 5%, and * for 10%.

	All	High Intensity	Low Intensity	All
	(1)	(2)	(3)	(4)
Post	-5,334	-12,987**	2,976	2,103
	(7,691)	(6,277)	(8,553)	(11,237)
T	-18,321**	-30,455**	-5,648	590
	(7,993)	(13,784)	(6,515)	(8,217)
Post*T	6,476	14,853**	-2,659	-7,161
	(7,457)	(6,816)	(12,647)	(13,324)
High Intensity				13,849
				(11,170)
Post*High Intensity				-11,379
				(10,324)
T*High Intensity				-29,652*
				(17,758)
Post*T*High Intensity				20,496
				(14,670)
Constant	-61,329***	-58,970***	-58,238*	-70,546***
	(20,312)	(21,075)	(30,344)	(24,323)
Observations	1,037	601	436	1,037
Adjusted R-squared	0.199	0.297	0.124	0.201

Table 8: Other winners

This table presents coefficients from difference-in-differences and difference-in-difference-in-differences regressions that explain log wages around acquisitions for various groups of workers. We distinguish between acquirers from country-industry combinations with high and low software and database capital intensity (High vs Low Intensity). The sample consists of treated workers employed one year before the acquisition, matched with control workers. We focus only on all workers (All) or workers in high software-exposed occupations (High Exposed), defined as occupations in the 90th percentile of workers exposed to software. Our analysis controls for individual variables such as age, gender, education, experience, experience^2, unemployment incidence in the years -4 to -2, 3 or more years of tenure, and municipality, as well as firm-level variables including log firm size, VA/L, Swedish MNE status, and industry, calendar year, and a constant. All controls, except calendar year, are measured one year before the acquisition. Standard errors are clustered at the acquisition industry and year and acquisition firm and year level. Significance levels are denoted as *** for 1%, ** for 5%, and * for 10%.

Panel A: Professionals and Managers

	All Professionals (1)	High intensity Professionals (2)	Low intensity Professionals (3)	All Managers (4)	High intensity Managers (5)	Low intensity Managers (6)
Post	0.098***	0.110***	0.082***	0.071***	0.081***	0.060***
	(0.007)	(0.009)	(0.008)	(0.008)	(0.009)	(0.010)
T	-0.022**	-0.014	-0.036***	0.014	-0.004	0.020
	(0.010)	(0.010)	(0.010)	(0.012)	(0.016)	(0.018)
Post*T	0.016*	0.026***	0.004	0.007	0.021**	-0.004
	(0.009)	(0.010)	(0.010)	(0.009)	(0.010)	(0.011)
Observations	315,100	165,662	149,437	176,237	87,622	88,610
R-squared	0.379	0.413	0.365	0.450	0.473	0.434

Panel B: More than five years of tenure

	All		High Intensity		Low Intensity	
	High Exposed (1)	All (2)	High Exposed (3)	All (4)	High Exposed (5)	All (6)
Post	0.000	0.044***	-0.001	0.046***	-0.003	0.038***
	(0.009)	(0.005)	(0.011)	(0.006)	(0.013)	(0.009)
T	0.029*	-0.001	0.040**	-0.005	-0.034	-0.004
	(0.017)	(0.009)	(0.018)	(0.012)	(0.025)	(0.012)
Post*T	0.005	0.010*	0.007	0.014*	0.009	0.009
	(0.007)	(0.006)	(0.006)	(0.007)	(0.015)	(0.009)
High exp.		-0.161***		-0.173***		-0.119***
		(0.016)		(0.017)		(0.016)
Post*High exp.		-0.012*		-0.010		-0.025***
		(0.007)		(0.008)		(0.009)
T*High exp.		0.052***		0.054***		0.021
		(0.017)		(0.017)		(0.026)
Post*T*High exp.		-0.025**		-0.021*		-0.002
		(0.011)		(0.011)		(0.015)
Observations	31,028	549,156	25,691	321,674	5,336	227,480
R-squared	0.448	0.480	0.458	0.476	0.527	0.502

Internet Appendix:
Technology Transfer in Mergers and Acquisitions and the Careers of Workers

Malin Gardberg
IFN

Fredrik Heyman
IFN

Joacim Tåg
IFN

March 2024

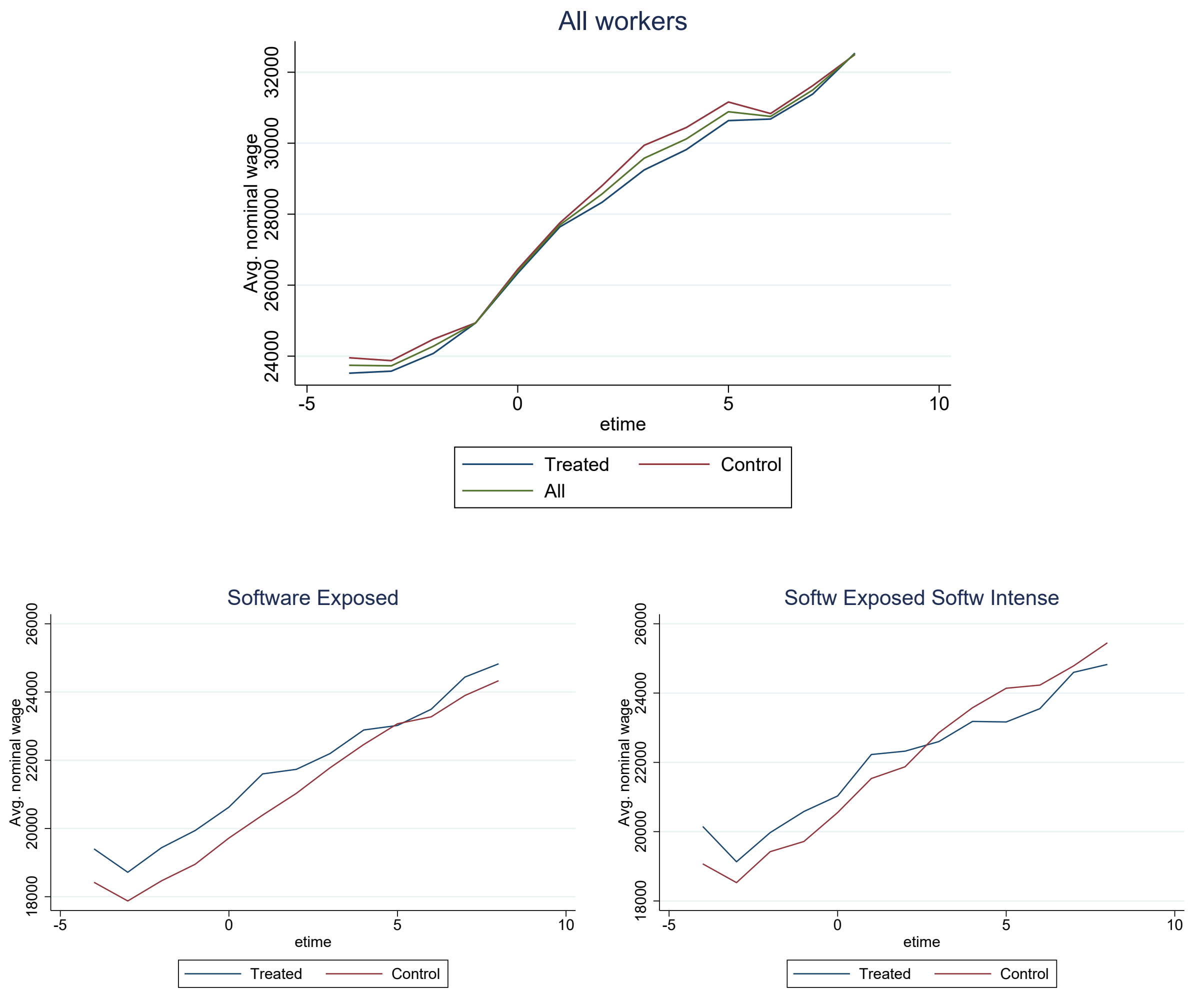


Figure A1: Nominal wage trends on average in different samples
The figures display average nominal wages around the acquisition year. Panel A shows wages for all, treated and controls, Panel B for workers in software exposed occupations only, and Panel C for workers in software exposed occupations that are part of high software intensity foreign acquisitions.

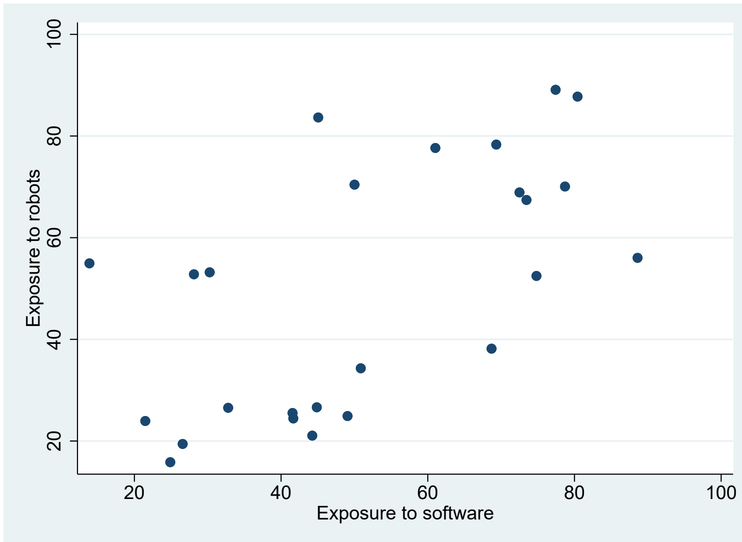
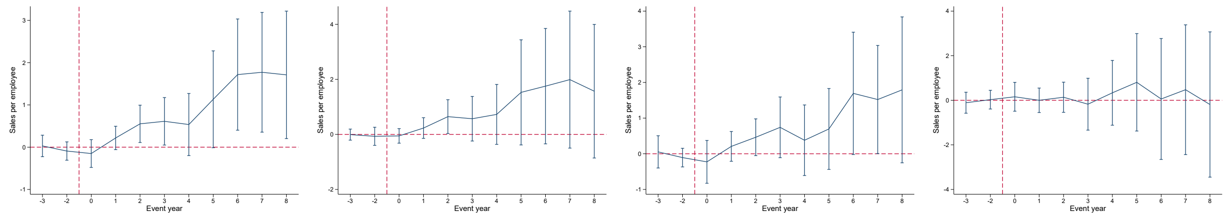
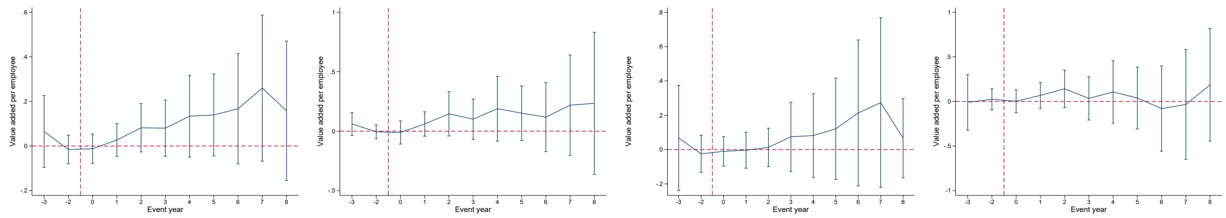


Figure A2: Correlation between exposure to software and exposure to robots
The figure displays a scatter plot between exposure to software and exposure to robots at the occupation level.

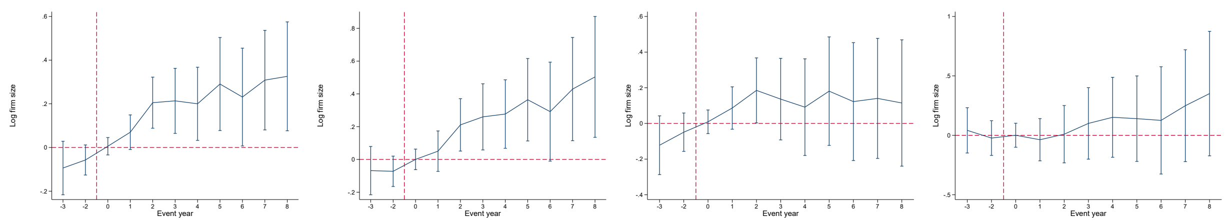
Panel A: Sales (logged)



Panel B: Value added per employee (logged)



Panel C: Number of employees (logged)



Panel D: Wage bill of the firm (logged)

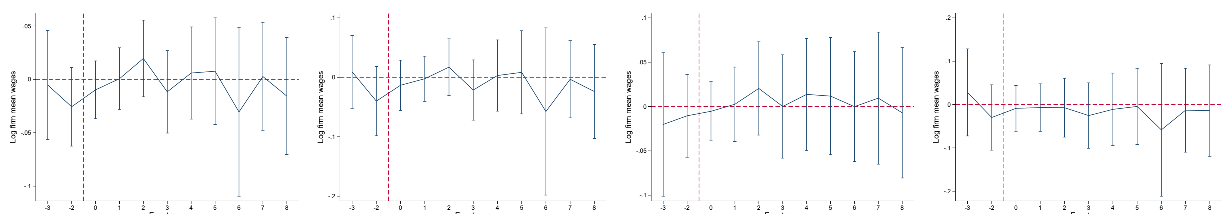


Figure A3: Effects on other firm level outcomes
The figures display yearly difference-in-difference estimates (first three figures per panel) and difference-in-difference-in-difference estimate (last figure per panel) relative to the year prior to the foreign acquisition (event time -1) for sales, value added per employee, number of employees, and the wage bill of the firm. The vertical bars display 95% confidence intervals using robust standard errors. The sample is based on the same firm level match underlying Table 7. The first figure in each panel displays results for all firms, the second figure for firms targeted by high intensity software acquirers, the third figure for low intensity acquirers and the final figure displays a triple difference estimate comparing treated-control, before-after, and high-low intensity foreign acquirers.

Table A1: Detailed variable descriptions

Variable	Notes
Panel A: Individual level variables	
Age	Original source is the population registry.
Education	Information on highest completed education level comes from the Education Register at Statistics Sweden (Utbildningsregistret). The education level is based on a graded scale from 1-7, where 1: Lower secondary education, $<$ 9 years, 2: Lower secondary education, 9 years, 3: High school, $<$ 3 years, 4: High school, 5: University, $<$ 3 years, 6: University, $\leq$ 3 years, and 7: PhD.
Employment	Employment and employer (firm) are defined in November each year.
Exposure to Software, Robotics and AI	Webb's 2020 exposure measures are available for US SOC2010 occupational classifications. We map the US classifications to the European occupational code, ISCO08, which in turn can be translated to SSYK96. The US code is more detailed than both the EU and Swedish occupational classifications, i.e. some European codes include several US occupations (and vice versa in some cases). We use occupational employment weights from the US Bureau of Labor Statistics (BLS) and Statistics Sweden when there is no 1:1 relationship between the US and European occupations. Furthermore, we use the new Swedish occupational classification SSYK2012 for mapping ISCO08 to SSYK96. While SSYK2012 is almost identical to ISCO08 differences exist; in these cases, we use different methods to convert the occupational codes.
Gender	A dummy taking the value one for females, zero otherwise. Original source is the population registry.
Major city	Residence in a major city (storstad) versus a smaller city or rural area is based on the classifications based on 4-digit municipality codes by Statistics Sweden.
Wage	Full-time equivalent monthly real wage data are from the Salary Structure Statistics (Lönestrukturstatistiken), measured in November each year.
Experience	Labor market experience is based on a person's age or year of academic degree. If highest educational level is primary education or lower (including missing information), labor market experience is defined as age minus 16. If upper secondary education, it is defined as age minus 19. If post-secondary education less than two years, age minus 20. If post-secondary education two year or loner, age minus 23. If higher educational level, as age minus year of academic degree.
Municipality	The municipality where the person is registered at the time of reference (normally December 31 each year).
Occupation	We use the 2-digit SSYK96 code. The new occupation classification SSYK2012 is mapped to SSYK96.
Offshorability	The offshorability index is available at the 2-digit ISCO-88 level.
Retired	A person is defined as retired if collecting retirement pension payments during a year, not retired otherwise. Retired workers are excluded from the sample.
Tenure	We calculate the tenure of a worker based on observing worker-firm links between the years 1990 and 2011. A worker can thus have a maximum tenure of 20 years.
Unemployment	A person is defined as having been unemployed at some point during the year if collecting unemployment benefits, not unemployed otherwise.

Continued on next page.

Continued from previous page.

Panel B: Firm level variables	
Acquirer nationality	Original source is the Swedish Agency for Economic and Regional Growth (Tillväxtanalys), and indicates the nationalities of foreign MNE firms operating in Sweden. The Agency uses definitions that are in accordance with definitions in similar data from the OECD and Eurostat.
Firm size	Number of employees as of November each year.
Industry	Information on the industry and geographical location of the firm comes from Statistics Sweden who assigns identifiers, industry, and location codes to physical places of work (the underlying databases at Statistics Sweden are the RAMS and the Företagsdatabasen databases).
Share high skilled	The share of the work force defined as high skilled. We define a worker as high skilled if holding a university degree, low skilled otherwise. Aggregated from individual level data.
Swedish MNE	A dummy variable indicating whether the Swedish firm is a multinational enterprise (MNE) as opposed to a local firm.
VA/L	Value added divided by firm size.
Panel C: Industry level variables	
Robot Intensity	The main IFR Robot Database variables are number of robots newly installed and operational stocks by country and industry. The definition of a robot is “An automatically controlled, reprogrammable, multipurpose manipulator programmable in three of more axes, which can be wither fixed in place or mobile for use in industrial automation applications”. The industry classification in the IFR data is based on 2-digit ISIC Rev. 4. We use a crosswalk to match it with our Swedish data that use NACE Rev. 1.1. Employment shares are used in case of ambiguous cases.
Software Intensity	The industry classification in the EU KLEMS database is based on 2-digit ISIC Rev. 4. We use a crosswalk to match it with our Swedish data that use NACE Rev. 1.1. Employment shares are used in case of ambiguous cases. The industry classifications have been categorized into 23 broader industry groups.
Expenditures on data, telecommunications and software	From the Statistics Sweden survey on IT use.

Table A2: Wages by technological intensity of the foreign acquirer for high offshoring-exposed occupations

This table reports selected coefficients from difference-in-differences regressions explaining log wages around acquisitions. We differentiate between acquirers coming from country-industry combinations with high and low software and database capital intensity (High and Low Intensity). The sample consists of treated workers employed one year prior to the acquisition and matched control workers. We focus either on all workers (All), or on workers in high offshoring exposed occupations (High Exposed). High Exposed refers to workers in occupations in the 90:th percentile of workers exposed to offshoring. We control for the individual variables age, gender, education, experience, experience^2, unemployment incidence in the years -4 to -2, 3 or more years of tenure, and municipality, the firm level variables log firm size, VA/L, Swedish MNE status, and industry, calendar year and a constant. All controls, except calendar year, are measured one year prior to the acquisition. The standard errors are clustered at the acquisition industry & year and acquisition firm & year level. Levels of significance: *** 1%, ** 5%, * 10%.

Sample	**All**	**High Intensity**		**Low Intensity**	
	All	High Exposed	All	High Exposed	All
	(1)	(2)	(3)	(4)	(5)
Post	0.065***	0.005	0.073***	0.022***	0.051***
	(0.004)	(0.005)	(0.007)	(0.007)	(0.005)
T	0.023*	0.039***	-0.001	-0.009	0.034
	(0.014)	(0.010)	(0.011)	(0.011)	(0.021)
Post*T	0.005	0.009	-0.004	0.005	0.015
	(0.008)	(0.010)	(0.012)	(0.010)	(0.010)
High Exposed	-0.066***		-0.083***		-0.054***
	(0.011)		(0.011)		(0.011)
Post*High Exposed	-0.042***		-0.052***		-0.031***
	(0.006)		(0.007)		(0.009)
T*High Exposed	0.000		0.029*		-0.028
	(0.016)		(0.015)		(0.021)
Post*T*High Exposed	0.006		0.017		-0.017
	(0.010)		(0.013)		(0.016)
$\bar{R}^2$	0.473	0.429	0.488	0.432	0.469
Obs	2,360,631	218,483	1,217,070	89,320	1,143,558

Table A3: Alternative firm level outcomes

This table reports selected coefficients from difference-in-differences regressions explaining various firm outcomes around acquisitions. We differentiate between acquirers coming from country-industry combinations with high and low software and database capital intensity (High and Low Intensity). The sample consists of treated workers employed one year prior to the acquisition and matched control workers. Levels of significance: *** 1%, ** 5%, * 10%.

	Employment			**Sales**			**VA/Emp**			**Wage bill**		
	All	High	Low	All	High	Low	All	High	Low	All	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post	-0.014	0.010	-0.030	0.266**	0.161	0.353**	0.022	0.013	0.013	0.048***	0.059***	0.041***
	(0.054)	(0.072)	(0.074)	(0.115)	(0.156)	(0.146)	(0.025)	(0.033)	(0.039)	(0.009)	(0.014)	(0.011)
T	0.385***	0.450***	0.323***	0.456**	0.200	0.728***	0.032	-0.045	0.114	0.027**	0.009	0.046***
	(0.073)	(0.104)	(0.109)	(0.180)	(0.271)	(0.240)	(0.043)	(0.042)	(0.069)	(0.013)	(0.019)	(0.016)
Post*T	0.199***	0.231***	0.149*	0.578**	0.677*	0.496	0.064	0.087	0.041	0.001	-0.003	0.006
	(0.056)	(0.072)	(0.088)	(0.247)	(0.388)	(0.308)	(0.070)	(0.093)	(0.105)	(0.014)	(0.020)	(0.018)
Constant	3.878***	3.846***	3.874***	1.129**	1.394***	1.038	0.485***	0.495***	0.523***	5.693***	5.721***	5.668***
	(0.077)	(0.112)	(0.092)	(0.489)	(0.518)	(0.718)	(0.080)	(0.098)	(0.116)	(0.026)	(0.037)	(0.036)
Obs.	10,023	5,102	4,921	10,020	5,099	4,921	10,023	5,102	4,921	9,988	5,084	4,904
Adj. R^2	0.255	0.269	0.273	0.144	0.178	0.119	0.132	0.158	0.124	0.396	0.390	0.400
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ind. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes