Importing Automation and Wage Inequality through Foreign Acquisitions*

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April 2023

ABSTRACT

Is technology or trade driving increases in wage inequality? We propose that technology interacts with trade in the form of foreign direct investments to widen domestic wage inequality. We show that foreign acquisitions of domestic firms disproportionately affect wages for workers who perform tasks sensitive to the technology specialization (software or robotics) of the acquiring firm. Based on Swedish matched employer-employee data covering two decades and staggered difference-in-differences methods we find wages to decline by up to 5.2% annually over an eight-year post period. Our results suggest that a trade policy aimed at attracting foreign companies with high technological capabilities can help countries advance technologically, but this may come at the cost of increased domestic wage inequality.

Keywords: Foreign Direct Investments, Automation, Inequality, AI, Robots, Technology, Trade, Mergers and Acquisitions, Multinational firms, Wages.
JEL Codes: F23, G34, J30, R10.

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1 Introduction

Is technology or trade driving increases in wage inequality? This fundamental question is central to understanding the rise of wage inequality all over the world. This paper proposes a novel approach to examine the mechanism through which technology interacts with trade to widen domestic wage inequality. We posit that multinational enterprises (MNEs) transfer technologies for automation across borders through foreign acquisitions and that these acquisitions directly affect wage inequality in the recipient country. Focusing on exposure to software and robotics, we show that foreign acquisitions of domestic firms disproportionately affect the wages of workers that perform tasks sensitive to the technological intensity of the foreign 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 multinational firm source country heterogeneity. 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 a foreign acquisition with similar workers performing similar tasks in firms that foreign firms do not acquire.

For identification, we run stacked difference-in-differences and triple-differences regressions along various dimensions of worker and firm source country heterogeneity. We focus on worker heterogeneity regarding occupational exposure to software, robotics, and AI using the occupation exposure measures of Webb (2020). For firm source country and industry 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 foreign acquisitions over time.

We have the following five key results. First, foreign acquisitions overall have *no effect* on domestic wages. The lack of a wage effect holds for all foreign acquisitions, independent of the technological intensity of the foreign acquirer. That foreign acquisitions do not affect individual wages is consistent with earlier micro evidence from Sweden (Heyman, Sjöholm, and Tingvall, 2007).

Second, 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 foreign acquisition and ending at around a 5% relative decline at the end of the eight years. This relative decline in wages can be traced to acquisitions by software-intense foreign 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 foreign 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 foreign acquisition. Again, we do not observe this pattern if the foreign acquirer was less software-intensive.

Third, 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 and between an occupation's robot exposure, and software exposure 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 foreign 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.

Fourth, the patterns reverse when we examine the relationship between software-intense foreign 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 foreign 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 foreign acquisitions.

Finally, we also investigate firm-level observables relating to software and telecommunications use and ask if technology investments increase relative to when the foreign 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 foreign acquisitions. However, the relative difference to low intense foreign acquisitions is not statistically significant.

Our paper contributes to the literature in international economics on how foreign direct investment and technologies affect wage inequality (Helpman, 2017). Most closely related is work on how foreign acquisitions affect domestic wages through various dimensions of worker and foreign firm source heterogeneity (Hale and Xu, 2020). On worker heterogeneity, research has shown that foreign acquisitions can increase wages for CEOs and managers and high-skilled workers (Heyman, Sjöholm, and Tingvall, 2011; Setzler and Tintelnot, 2021). On foreign firm source heterogeneity, there is evidence that individual countries, management practices, and membership in transnational production networks matter (Conyon, Girma, Thompson, and Wright, 2003; Griffith and Simpson, 2004; Heyman, Norbäck, and Hammarberg, 2019; Setzler and Tintelnot, 2021). Our contribution highlights worker task heterogeneity across exposure to automation in terms of software and robot exposure to job tasks and its interaction with firm source heterogeneity in technological intensity. We thus provide a novel link between the literature on trade and inequality and technological change and inequality. We also bring insights from the firm-level literature on foreign ownership, productivity, IT, and innovation down to the worker level (Branstetter, 2006; Guadalupe, Kuzmina, and Thomas, 2012; Bloom, Sadun, and Reenen, 2012).

Our paper also contributes to the labor and finance literature. It is closely related to Olsson and Tåg (2017, 2018), who show how private equity buyouts of low productive firms can spur job polarization by increasing unemployment for workers performing routine or offshorable tasks, and to Ma, Ouimet, and Simintzi (2022), who show similar findings for mergers and acquisitions. Related is also Agrawal and Tambe (2016), who show that workers' careers in IT-related tasks benefit from private equity buyouts. Our contribution emphasizes how *foreign* mergers and acquisitions can have differential effects on incumbent worker careers depending on the acquiring firm's expertise and intangible capital utilization. We also show how firms can transfer technologies across borders to impact within-firm wage inequality in the destination country, thereby tying the cross-border merger and acquisitions finance literature (see, for instance, the recent survey by Erel, Jang, and Weisbach (2022)) to the labor and finance literature.

Our paper has several important implications. First, an extensive literature has established that foreign multinational firms positively affect local firm productivity and wages paid to local workers (Hale and Xu, 2020; Setzler and Tintelnot, 2021). These results are one reason governments try to attract foreign multinational firms with various policies. Our paper points to another key reason to attract multinational firms: spurring technological upgrading of local firms, but only if the multinational firm originates from a technologically intensive country and sector. There may thus be a rationale for directed trade policy that interacts with government policies designed to achieve international leadership in technology.

Second, our findings imply that a trade policy towards attracting foreign multinationals risks increasing income inequality due to detrimental wage effects on workers exposed to transplanted new technologies. We thus highlight the relevance of linking trade policy with policies designed to help workers mitigate technological change.

Third, our empirical findings suggest the need for more theoretical work investigating how trade and technology jointly affect inequality. In particular, integrating the task-based technological change models (Acemoglu and Autor, 2011) with models of multinational firms (Antràs and Yeaple,

2014) and models of technology transfer across borders (Guadalupe et al., 2012) seems like a fruitful avenue for further research. Such work would enhance our understanding of the linkages of technology and trade and provide a basis for more detailed policy guidance.

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 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 Occupations and exposure to different technologies

Our analysis hinges on classifying job tasks or occupations at risk of becoming obsolete due to technological upgrades by foreign 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 Foreign acquisitions and technological intensity

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 multinational enterprises 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 foreign 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 The final sample

Figure 1 displays how foreign 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 foreign 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 foreign 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 foreign acquisition (Olsson and Tåg, 2017, 2021; 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 foreign 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 foreign 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 foreign 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 foreign 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 foreign 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 foreign 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 foreign acquirer. More specifically, we then estimate a triple difference estimator model specified as:

$$w_{ikt} = \alpha_0 + \alpha_1 T_i + \alpha_2 Post_k + \beta_1 DID_{ik} + \mu_1 High_i + \mu_2 High_i \times T_i$$
$$+ \mu_3 High_i Post_k + \beta_2 DIDID_{ik} \omega_t + X_i + X_f + \epsilon_{ikt}, \quad (3)$$

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 foreign 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 foreign 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 foreign 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 foreign 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 foreign 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 foreign acquisitions on wages

In Figure 2, we investigate the effect of foreign 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 foreign acquisition. Our sample's lack of a foreign acquisition wage premium aligns with earlier evidence from Sweden in Heyman et al. (2007).

Panels B and C present estimates for foreign acquisitions by more or less software intense foreign 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 foreign 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 foreign acquirer (Panels B and C in Figure 3). Interestingly, the relative wage decline can only be attributed to software-intensive foreign 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 foreign 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 foreign acquirers (Panel D) and less software intense foreign 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 foreign 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 foreign 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 foreign acquisition (panel A) and after a less software-intense foreign 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 foreign 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 foreign 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 foreign 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 foreign acquisitions with high software intensity and no statistically significant effect on wages for less software-intense foreign 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 foreign 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 foreign 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 foreign 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 foreign 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 foreign acquirers with high and low-intensity software and database use, respectively. Following high-intensity foreign 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 foreign 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 foreign 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 foreign 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 foreign acquisitions, but the relative difference to low-intensity foreign 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 foreign 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 foreign 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 foreign acquirer subsamples (columns 3 and 5). We still find no wage effects following the foreign 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 Conclusion

In this paper we analyze the relationship between technology, trade, and wage inequality, and propose a novel mechanism through which technology interacts with trade to widen domestic wage inequality. We suggest that multinational enterprises transfer technologies for automation across borders through foreign acquisitions and that these acquisitions directly affect wage inequality in the recipient country. Specifically, we focus on exposure to software and robotics and show that foreign acquisitions of domestic firms disproportionately affect the wages of workers that perform tasks sensitive to the technological intensity of the foreign firm that undertakes the acquisition.

To provide evidence, we utilize matched employer-employee panel data that allows us to distinguish between worker heterogeneity in tasks and multinational firm source country heterogeneity.

We construct a data set covering nearly 500 firms and about 160,000 workers over nearly two decades. We then we match workers that are part of a foreign acquisition with similar workers performing similar tasks in firms that foreign firms do not acquired and run stacked difference-in-differences regressions.

We have five key results. First, foreign acquisitions overall have no effect on domestic wages. The lack of a wage effect holds for all foreign acquisitions, independent of the technological intensity of the foreign acquirer. Second, 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 foreign acquisition and ending at around a 5% relative decline at the end of the eight years. This relative decline in wages can be traced to acquisitions by software-intense foreign acquirers. In contrast, there are no changes in relative wages for workers in firms acquired by less software-intense foreign acquirers. Third, 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. Fourth, the patterns reverse when we examine the relationship between software-intense foreign acquirers and workers' AI exposure. Finally, we investigate firm-level observables relating to software and telecommunications use and show that technology investments increase when the foreign acquiring firm is more software intense.

The paper makes a contribution to the labor and finance literature by emphasizing how foreign mergers and acquisitions can have differential effects on domestic workers, and to the international economics literature by highlighting the ways in which technology, trade, and foreign acquisitions can intersect to create wage inequality.

Our results highlight benefits of trade and investment policies that attracts foreign firms to invest domestically in terms of technology transfers to domestic firms. At the same time, we show how foreign investments can increase inequality among workers. The latter shows the need for support programs that help workers adjust to the technological changes brought about through foreign acquisitions. We thus point to the important linkages between trade and investment policies to attract foreign firms to invest locally, and labor market policies to help cushion the shock for the most exposed workers.

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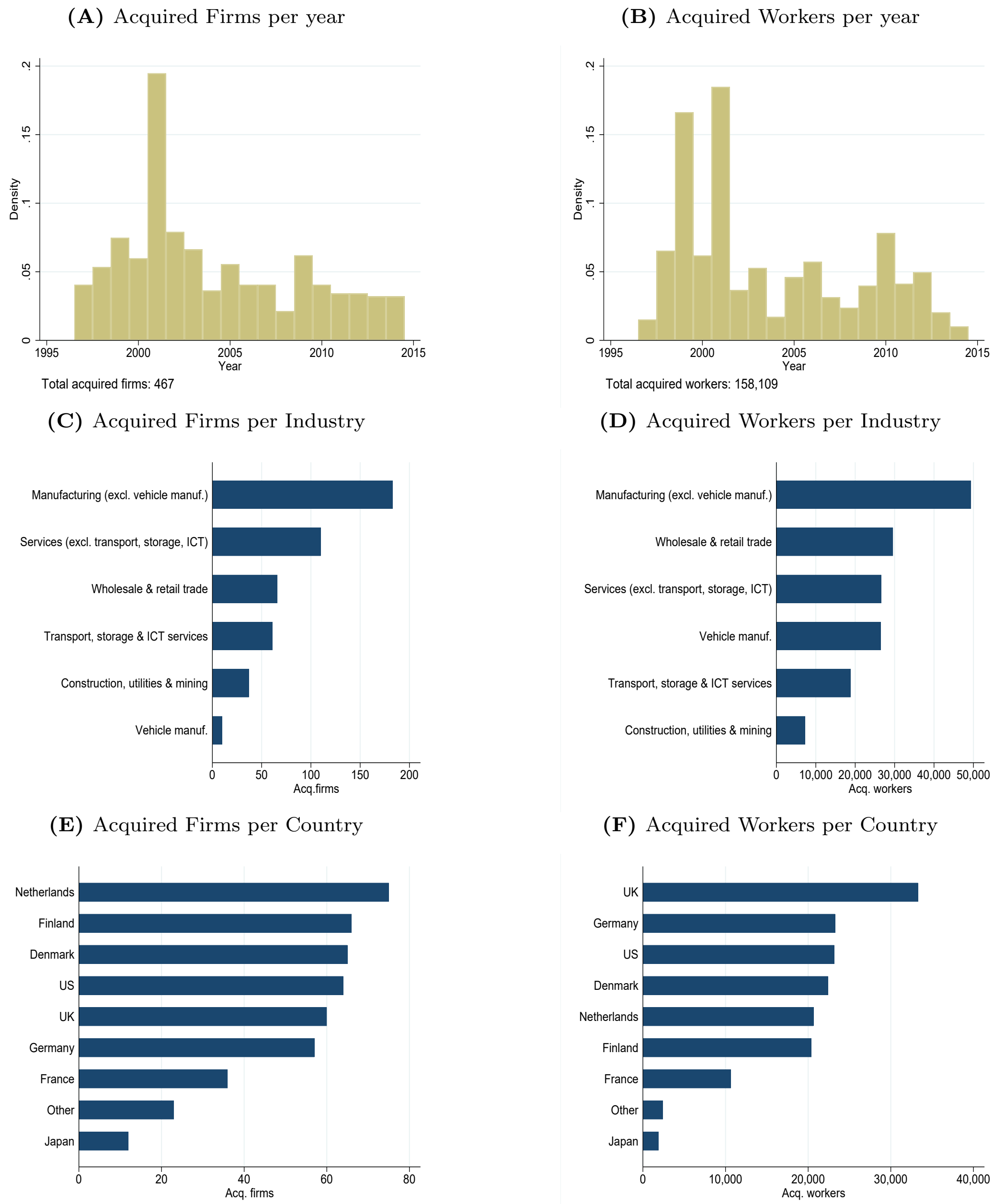
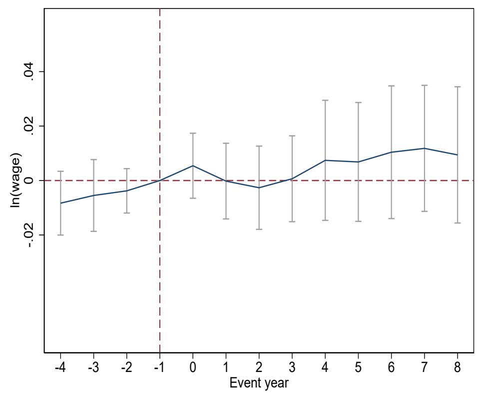
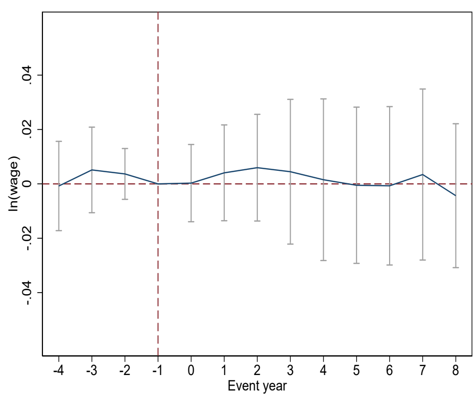


Figure 1: Foreign acquisitions over time, across industries and countries
Panels A and B depict the temporal distribution of our foreign acquisition sample at both the firm and worker levels. Panels C and D illustrate the distribution across various industry groups at the firm and worker levels, respectively.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

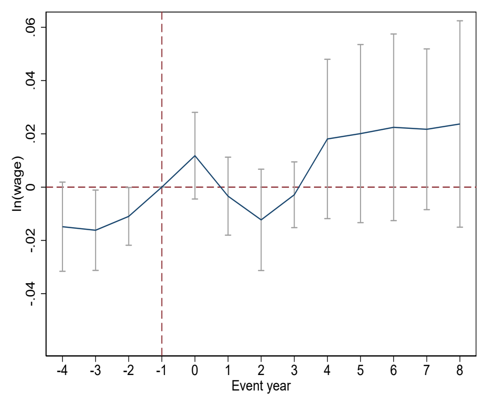
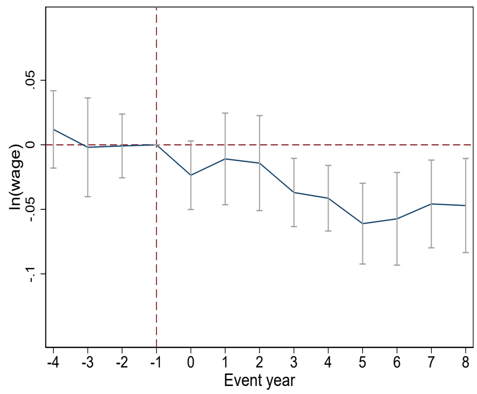
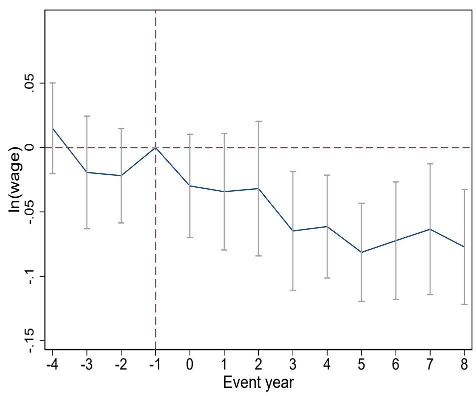


Figure 2: Wage effects of foreign acquisitions in the EU KLEMS sample
The figures depict annual difference-in-differences estimates relative to the year preceding the foreign acquisition (event time -1), using dynamic variants of the regressions presented in columns 1-3 in Table 2. The acquisitions are 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.

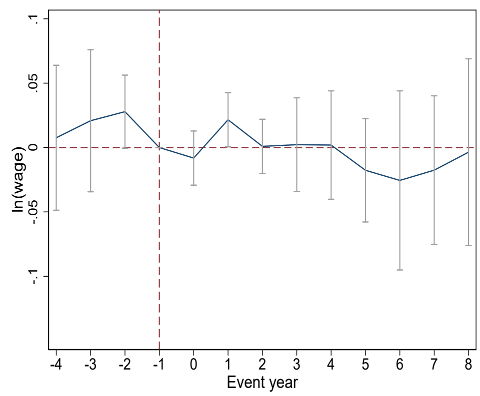
(A) DDD High Software Exposed



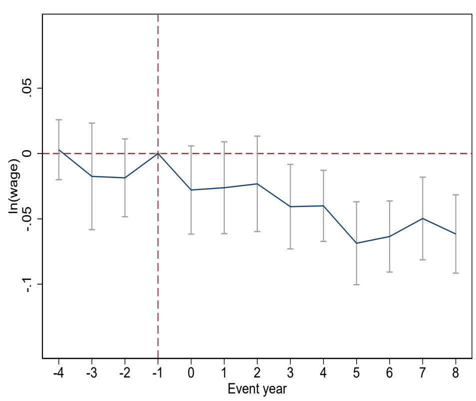
(B) High Intensity: DDD High Exposed



(C) Low Intensity: DDD High Exposed



(D) High Intensity & High Exposed



(E) Low Intensity & High Exposed

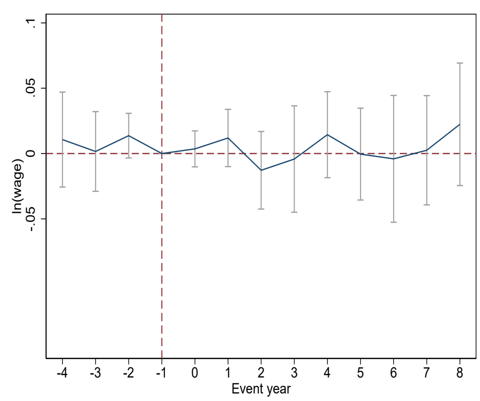
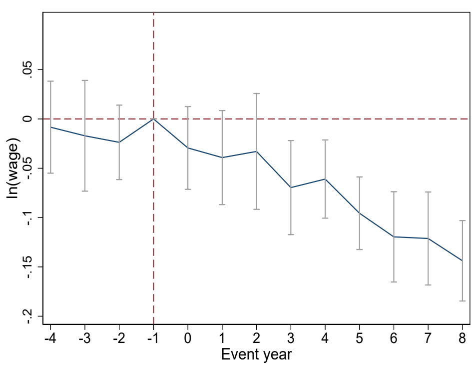
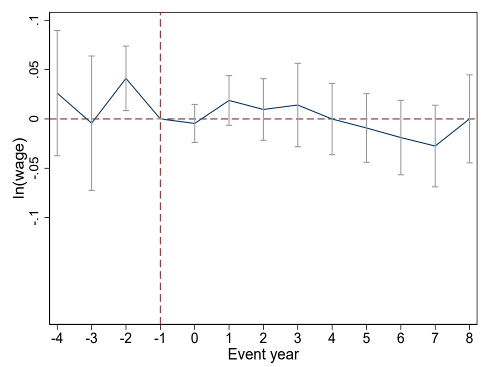


Figure 3: Wage effects of foreign acquisitions for high software exposed occupations
The figures show yearly estimates for difference-in-difference-in-difference (DDD) and difference-in-difference effects relative to the year preceding the foreign acquisition (event time -1), using dynamic variants of 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.

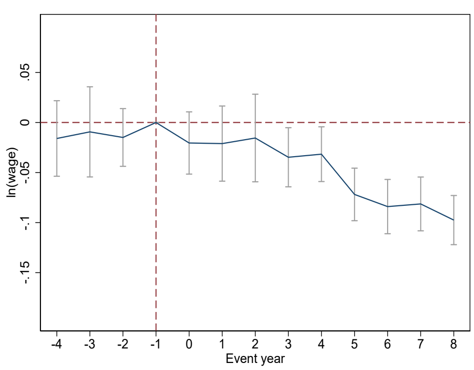
(A) High Intensity: DDD High Exposed



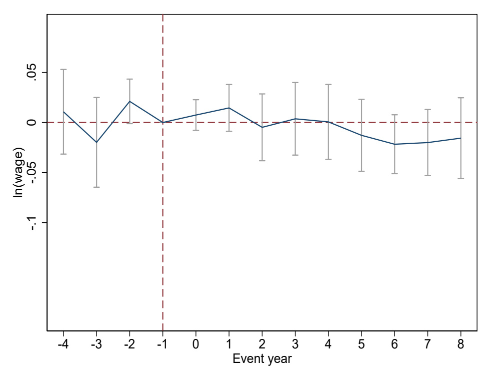
(B) Low Intensity: DDD High Exposed



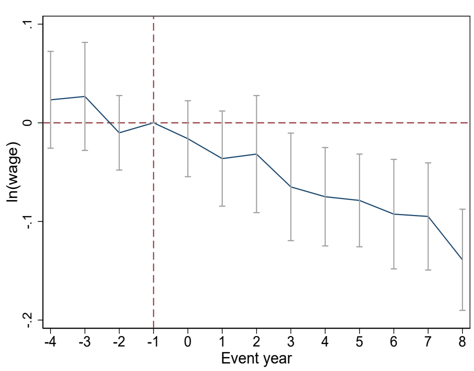
(C) High Intensity & High Exposed



(D) Low Intensity & High Exposed



(E) High Intensity: DDD High vs Low Exposed



(F) Low Intensity: DDD High vs Low Exposed

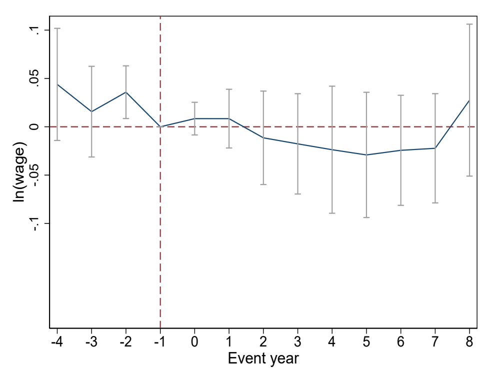


Figure 4: Stayers: Wage effects of foreign acquisitions for high software exposed occupations
The figures show yearly estimates of difference-in-difference-in-difference (DDD) and difference-in-difference effects relative to the year prior to the foreign acquisition (event time -1), using dynamic variants 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.

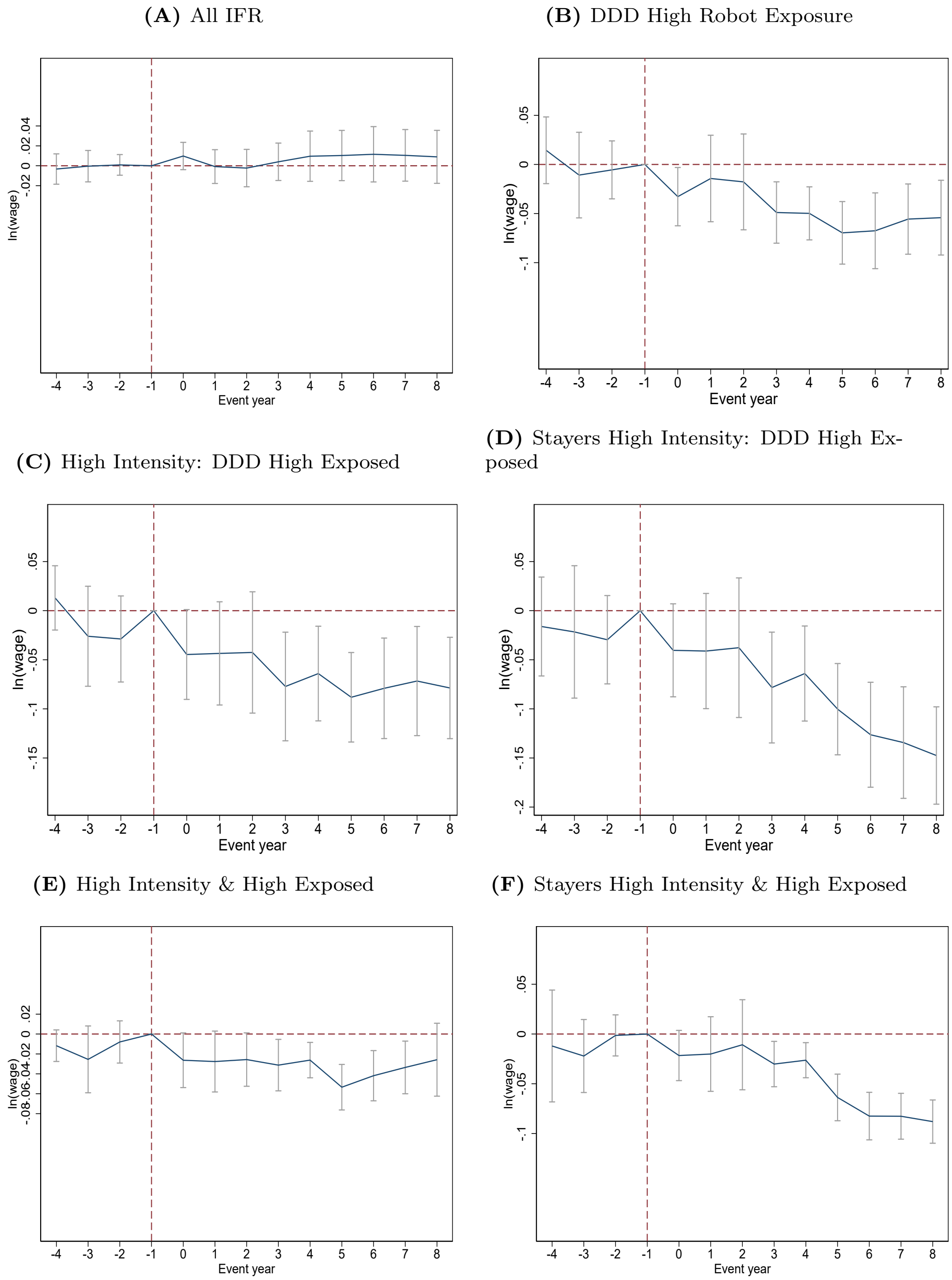
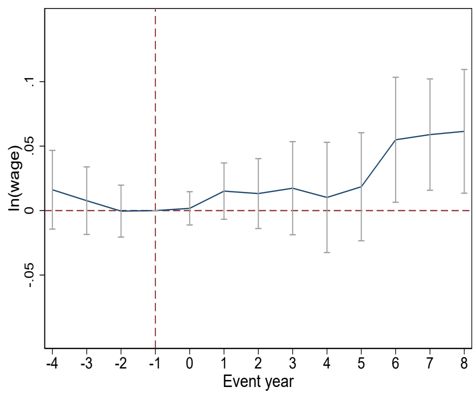
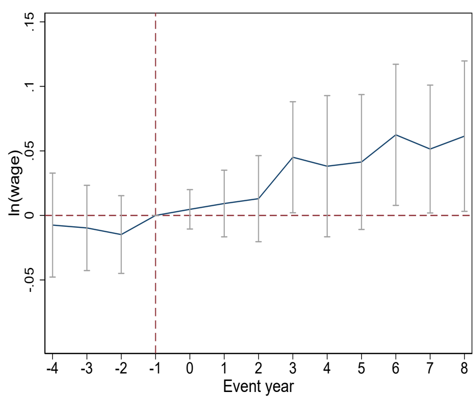


Figure 5: Wage effects of foreign acquisitions for high robot exposed occupations in the IFR sample
The figures illustrate yearly difference-in-difference-in-difference (DDD) and difference-in-difference estimates relative to the year before the foreign acquisition (event time -1), using dynamic variants 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.

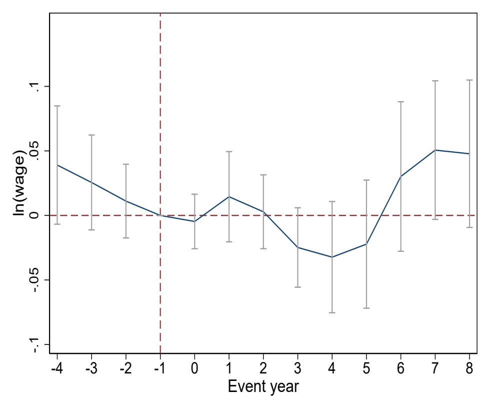
(A) DDD High AI Exposed



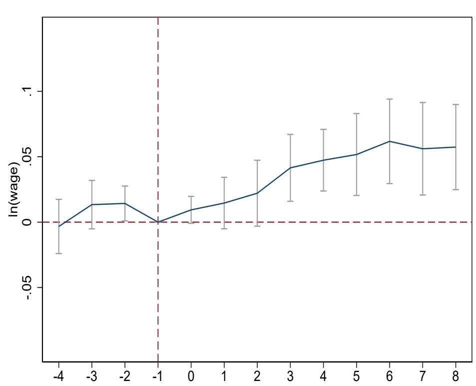
(B) High Intensity: DDD High Exposed



(C) Low Intensity: DDD High Exposed



(D) High Intensity & High Exposed



(E) Low Intensity & High Exposed

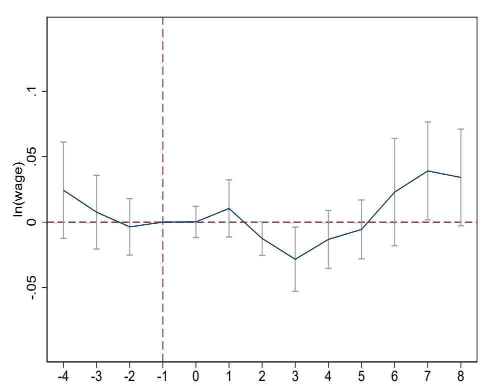


Figure 6: Wage effects of foreign acquisitions for high AI exposed occupations
The figures present yearly estimates of difference-in-difference-in-difference (DDD) and difference-in-difference relative to the year prior to the foreign acquisition (event time -1), based on dynamic versions of the regressions 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.

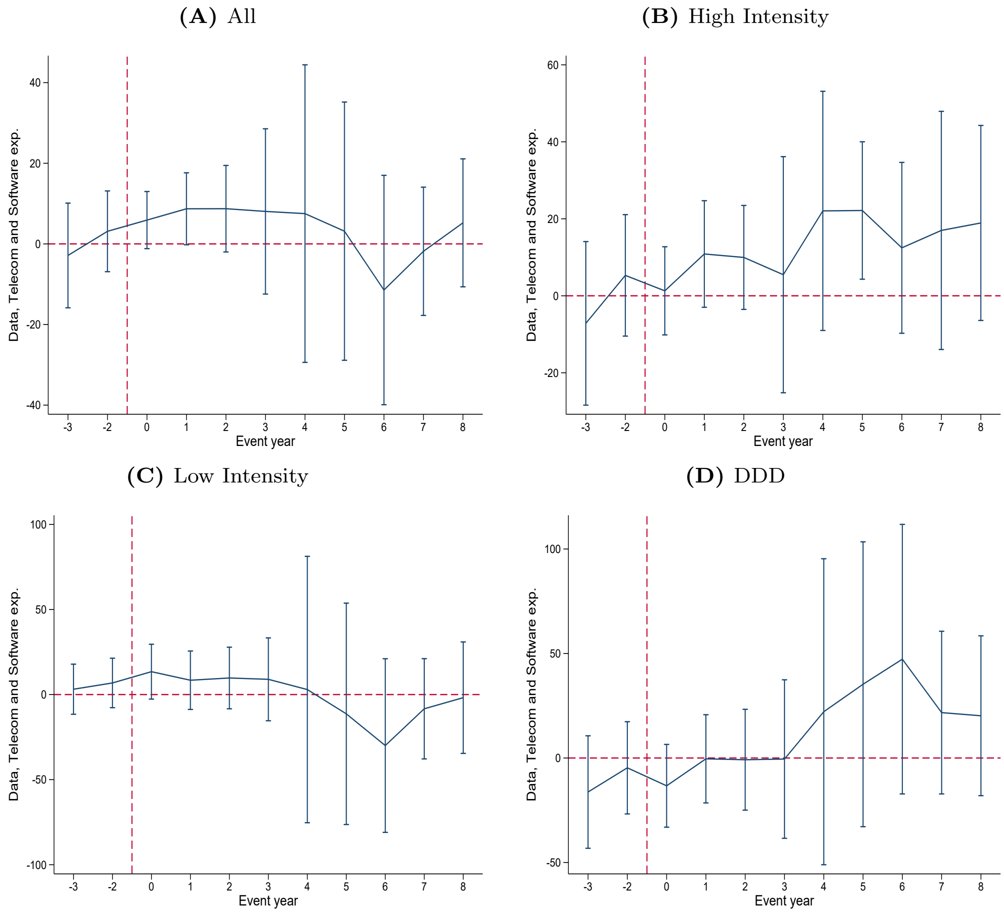


Figure 7: Firm level outcomes
The figures illustrate the yearly difference-in-difference-in-difference (DDD) and difference-in-difference estimates, relative to the year before the foreign acquisition (event time -1), based on dynamic variants of the regressions in columns 1, 2, 3, and 4 in Table 7. 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 (1)	High Intensity (2)	Low Intensity (3)	All (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	High intensity Professionals	Low intensity Professionals	All Managers	High intensity Managers	Low intensity Managers
	(1)	(2)	(3)	(4)	(5)	(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	All	High Exposed	All	High Exposed	All
	(1)	(2)	(3)	(4)	(5)	(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: Importing Automation and Wage Inequality through Foreign Acquisitions

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April 2023

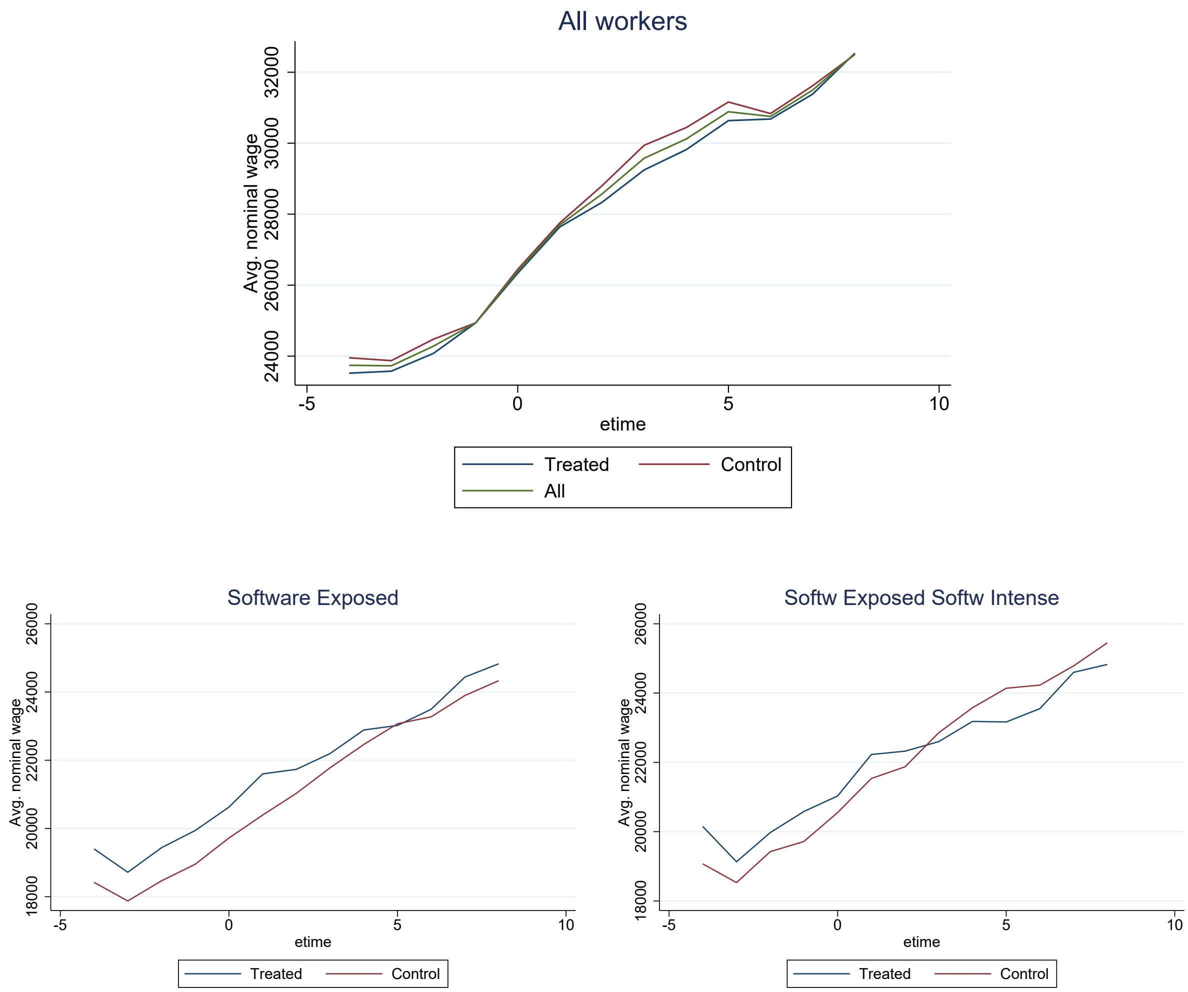


Figure A1: Nominal wage trends on average in different samples
The figures display average nominal wages around the acquisition year. Panel A shows wages for all, treated and controls, Panel B for workers in software exposed occupations only, and Panel C for workers in software exposed occupations that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

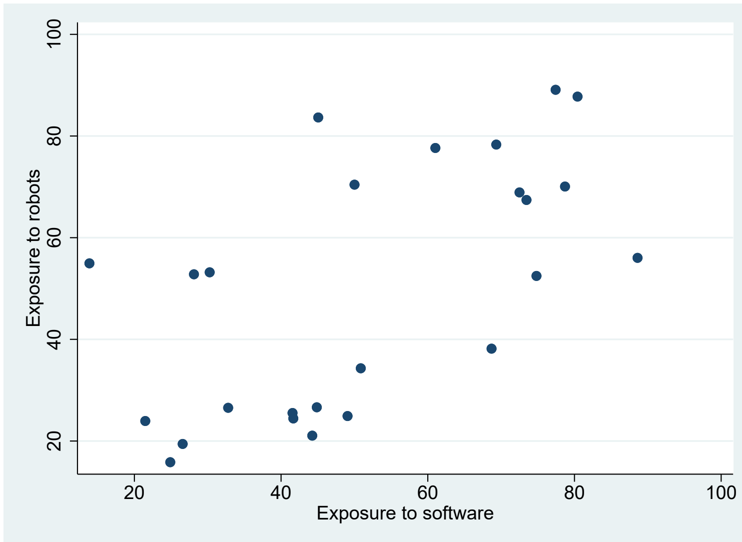
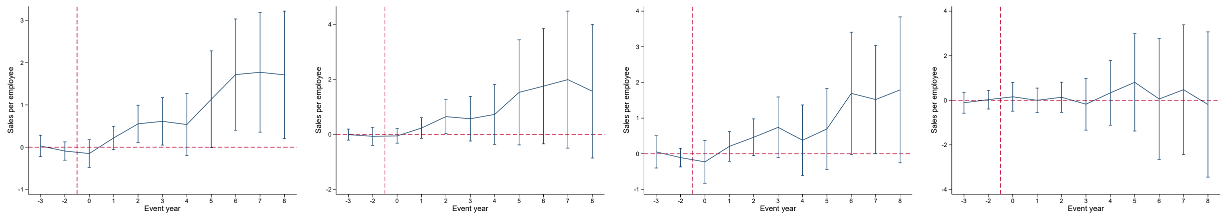
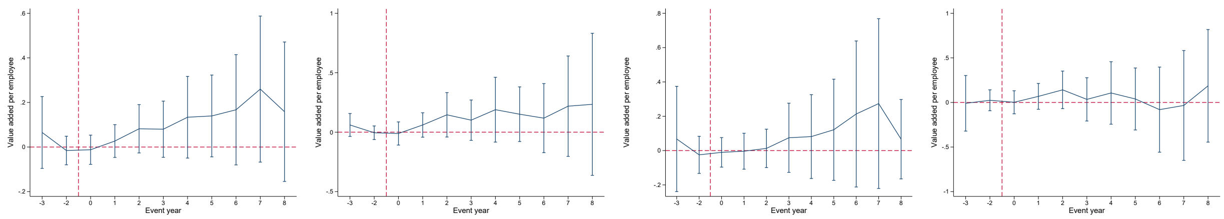


Figure A2: Correlation between exposure to software and exposure to robots
The figure displays a scatter plot between exposure to software and exposure to robots at the occupation level.

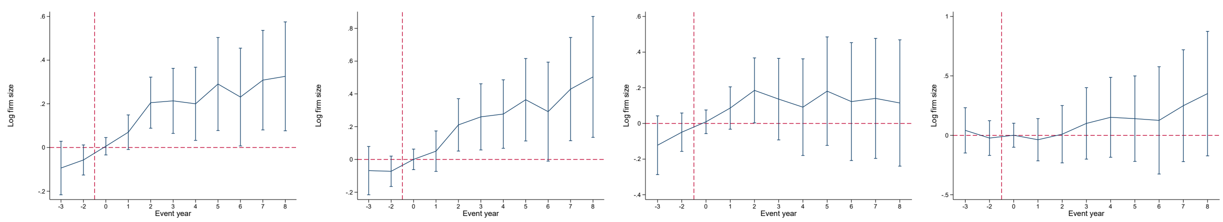
Panel A: Sales (logged)



Panel B: Value added per employee (logged)



Panel C: Number of employees (logged)



Panel D: Wage bill of the firm (logged)

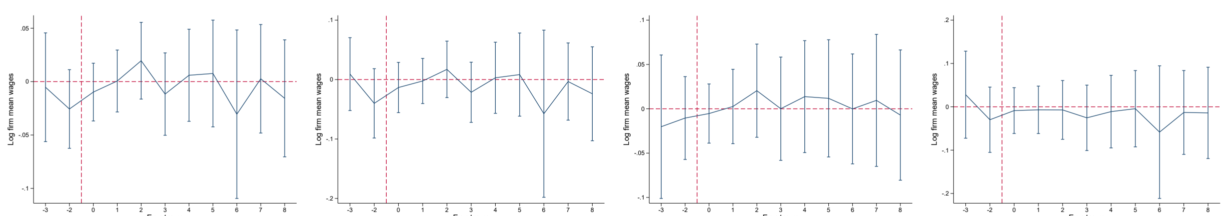


Figure A3: Effects on other firm level outcomes

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.

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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