



# Reassessing the foreign ownership wage premium in Germany

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## Funding information

Deutsche Forschungsgemeinschaft, Grant/Award Number: 322072184

## KEYWORDS

difference-in-difference estimation, foreign ownership wage premium, impact versus adjustment effects, propensity-score matching

## 1 | INTRODUCTION

Empirical evidence reported by Aitken, Harrison, and Lipsey (1996) that foreign direct investment leads to higher wage payments has sparked a lot of interest among economists and has started a new strand of empirical research in international trade, documenting the existence of a foreign ownership wage premium (Girma, Greenaway, & Wakelin, 2001; Lipsey & Sjöholm, 2004; Velde & Morrissey, 2003). With access to detailed firm-level data, the causal relationship between foreign ownership and wage premia has become an important topic in this strand of research over the last decade (Balsvik, 2011; Girma & Görg, 2007; Heyman, Sjöholm, & Tingvall, 2007). This paper provides new evidence for a foreign ownership wage premium in Germany.

We contribute to the literature in two important ways. First, we estimate the foreign ownership wage premium in Germany, using a longitudinal linked employer–employee data set that covers an observation window spanning the years 2003–14. This long time span not only allows us to separate the impact effect of foreign takeover in the period of ownership change from lagged adjustment effects in subsequent years, but also to estimate the foreign ownership wage premium by skill groups. Second, we test three channels put forward by previous research, which give rise to a foreign ownership wage premium. In particular, we consider *rent appropriation by managers*, *technology protection* and *training on new technology* as distinct arguments justifying the existence of a foreign ownership wage premium. Finally, we control for an *export platform* motive of foreign takeover to ensure that our estimates do not accidentally pick up an exporter wage premium.

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To estimate a causal effect of foreign takeover on wages paid by German establishments, we combine propensity-score matching with a difference-in-difference estimator. We associate the probability of a worker in our sample to be treated—defined by staying in an establishment acquired by a foreign investor—with the product of two probabilities: the probability that the employer is target of a foreign takeover and the probability of the worker to remain employed in the same establishment. As both, worker and establishment characteristics, matter for the treatment, we follow Martins (2004) and Hijzen, Martins, Schank, and Upward (2013) and combine establishment information with data on worker characteristics in the propensity-score matching. To construct our control group, we match to each treated worker the nearest observational neighbour from establishments that are not subject to foreign takeover.

Using a difference-in-difference approach, we then shed light on the effect of treatment on wages distinguishing between an impact effect in the year after takeover and a lagged adjustment effect arising in the second and the third year after takeover. We use data from the year prior to acquisition to determine the control group and build our analysis on a four-year window around the takeover. Because we rely on takeover events from different years, choosing the same four-year observation window for all acquisitions is important (see Egger, Egger, & Greenaway, 2008). In our baseline specification, we estimate an average wage premium from foreign takeover of 4.0 log points, which further grows to 6.3 log points 3 years after ownership change. When analysing the impact of ownership change for different skill groups, we find that the wage premium is larger for high-skilled than for low- and medium-skilled workers. We also provide evidence that foreign takeover exerts a lagged adjustment effect particularly on the wages of medium- and high-skilled workers.

To shed light on the economic mechanisms that can explain a foreign ownership wage premium, we rely on insights from previous research and distinguish three arguments. First, managers receive a higher remuneration after a successful acquisition (see Heyman, Sjöholm, & Tingvall, 2011). If rent appropriation by managers plays a role, we would expect to find a transitory increase in manager wages. Second, as a result of knowledge transfer by the investor, foreign takeover may lead to the use of new technology requiring training of workers (see Fosfuri, Motta, & Rønnde, 2001; Görg, Strobl, & Walsh, 2007). If training of workers requires time, we would expect to observe a lagged adjustment of wages. Third, foreign-owned plants may have an incentive to protect their technology by reducing knowledge dissipation through worker turnover (see Glass & Saggi, 2002). Workers should thus receive wage premia as long as foreign-owned plants possess a technology advantage.

Using information on manager status and *product* as well as *process innovation*, we analyse these different explanations empirically and find support for the relevance of all three channels. Our results also indicate that none of the considered channels can simultaneously explain the immediate wage premium in the year after takeover and the lagged adjustment effect in consecutive periods. Because previous research suggests that an important motive for foreign investment can be market access in nearby countries, we also account for the initial export status of acquired plants to control for an *export platform* motive. We thus ensure that the estimated wage premia do not incorrectly pick up an exporter wage premium due to an expansion of trade with Eastern Europe after the millennium (cf. Dauth, Findeisen, & Suedekum, 2014).<sup>1</sup> Since the estimated wage premium from foreign takeover does not differ for initial exporters and non-exporters, this concern seems not to be justified.

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<sup>1</sup>The platform argument has been put forward by Motta and Norman (1996), Yeaple (2003), Ekholm et al. (2007), and Neary (2009) as an important motive for foreign investment. It has been used to explain the finding that foreign investment increased in times of falling trade costs. Bernard and Bradford Jensen (1999, 1995) were the first providing evidence that exporters pay higher wages than non-exporters. The existence of an exporter wage premium has been empirically confirmed by Schank et al. (2007) and Wagner (2007).

We complement our empirical analysis by three extensions. First, we analyse whether wages increase because it takes time before the full effect of foreign takeover materialises, or because foreign takeover affects not only the wage level but also the wage growth of workers in German plants. To distinguish between these two interpretations, we expand the observation window around the takeover events to 6 years. Our results indicate that the growth of wages, which we observe after ownership change, is temporary and washes out after 4 years. Moreover, our results indicate that reaching the full effect of foreign takeover on wages takes longer for high-skilled workers. Second, we investigate whether foreign takeover causes a persistent wage increase that lasts even after divestment and (re-) acquisition of a foreign-owned plant by a German investor. We find support for a persistent effect, consistent with the view that foreign investment provides a one-time knowledge transfer and a permanent improvement of technology. In a third extension, we conduct a placebo test and estimate the effect of a takeover of German plants by German investors. For low- and medium-skilled workers, we find negative and mostly insignificant effects. For high-skilled workers, we only document insignificant estimates. Thus, a wage premium from takeover only exists in our data if the investor comes from abroad. Finally, in two sensitivity tests, we show that our results are robust to changes in the matching procedure.

The remainder of this paper is organised as follows. In Section 2, we introduce the data set and present descriptive statistics. In Section 3, we describe our empirical methodology, and in Section 4, we present and discuss our estimation results. Section 5 presents extensions of our analysis and robustness checks, and Section 6 concludes.

## 2 | DATA AND DESCRIPTIVE STATISTICS

For our empirical analysis, we rely on two data sources provided by the Institute for Employment Research (IAB). The first data set is the IAB Establishment Panel, which consists of a stratified 1% random sample of establishments that employ at least one employee covered by the social security system at 30 June each year. Since 1993, the IAB Establishment Panel surveys the same establishments from all industries in West Germany and since 1996 in East Germany. Response rates of repeatedly interviewed establishments—which account for about 7% of the German workforce—are above 80%. From the survey questions, we use information about (change in) plant size, industry affiliation, exporting share, location and profitability. Crucial to our analysis, the IAB Establishment Panel also provides information on majority ownership of establishments, differentiating between East German, West German, foreign and public owners as well as a residual group, for which no majority owner is found. Relying on this information, we identify foreign takeover as a change in the majority ownership from German to non-German in two subsequent years.

As a second data set, we use the Integrated Employment Biographies (IEB) of the IAB, which cover about 80% of the German workforce. This data set contains administrative data on all employees who are subject to social security contributions. The IEB provides information about age, gender, nationality, tenure, occupation, education and the daily wage of workers employed in the plants of the IAB Establishment Panel.

The IEB can be linked to the IAB Establishment Panel by a unique identifier, which allows constructing a linked employer–employee data set (LIAB) with highly reliable information on workers, wages and establishments.<sup>2</sup> The IEB does not contain detailed information on hours

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<sup>2</sup>For further details on the LIAB, see Alda et al. (2005) and Klosterhuber et al. (2016).

worked. In addition, since worker information comes from social security records, wages are top-coded at the social security contribution ceiling. To deal with these drawbacks, we use only full-time workers aged 16–65 years in our analysis and impute wages above the social security contribution ceiling using tobit regressions (see, e.g. Baumgarten, 2013; Dustmann, Fitzenberger, Schönberg, & Spitz-Oener, 2014; Gartner, 2004; Schafer, 1997).<sup>3</sup> Furthermore, we have information on the education level, which is missing or inconsistent for some workers. To mitigate this problem, we impute missing or implausible information on education, relying on information from previous periods.

Due to the low number of takeovers prior to 2003, we use establishment and worker information for the years 2003–14. Furthermore, we concentrate on establishments located in West Germany, because economic conditions and wages in East Germany still differ substantially from those in West Germany. Another reason for focusing on West Germany only is that we want to avoid attributing the effects of foreign takeover to more general (wage) adjustments during the ongoing transition and catch-up process of East Germany. We also drop establishments, which we do not observe over four consecutive years around the takeover. We use information from the year prior to takeover to match workers and the 3 years afterwards to distinguish the impact effect in the year after ownership change from lagged adjustment effects of foreign acquisition. Because we are interested in the wage impact of foreign takeover over a time span of 3 years, we only keep workers employed at an establishment over the 4-year time horizon (stayers). We also drop workers changing their education over time or employees with a monthly wage below the social security threshold.<sup>4</sup> Finally, we drop 1% of workers with the highest wages in each year to avoid that outliers influence our results.

Table 1 reports descriptive statistics of the key variables used when matching workers from acquired to workers in non-acquired establishments. We identify 152 foreign takeovers and 24,946 stayers in the acquired establishments over our sample period. The descriptive statistics are in line with previous findings that establishments acquired by foreign investors differ from non-acquired establishments in various dimensions (see Gelübcke, 2013). Targeted establishments are bigger, more prevalent in manufacturing and less prevalent in services than in other industries. Moreover, foreign takeover targets exhibit better profits. At the worker level, Table 1 shows that acquired and non-acquired establishments differ slightly with regard to gender and skill composition and that employees in establishments that are target of foreign takeover receive higher wages. Overall, the descriptive statistics suggest that foreign investors do not choose establishments and their workforce randomly, which is why we perform propensity-score matching to deal with selection.

### 3 | ESTIMATION STRATEGY

#### 3.1 | Baseline specification

To estimate the effect of foreign acquisition on wages, we use information from a 4-year window around a takeover event. Specifically, to construct our control group, we use data one year prior to the change in ownership, whereas we use information from 3 years after foreign takeover to distinguish the impact effect of ownership change from lagged adjustment effects. Thus, we consider foreign

<sup>3</sup>In 2003, the social security contribution ceiling was 167.67 euro for daily wages and 12.5% of the wages of full-time workers are top-coded.

<sup>4</sup>The limit was 400 Euro per month from 2003 to 2012 and 450 Euro in 2013 and 2014.

TABLE 1 Descriptives of key covariates

	Acquired	(SD)	Non-acquired domestic	(SD)
(a) Plant characteristics				
Plants	152		6,219	
Plant-years	608		56,052	
Log employment	4.485	(1.726)	3.400	(1.580)
$\Delta$ Log employment	-0.013	(0.207)	0.014	(0.179)
Agriculture and energy	0.018	(0.133)	0.031	(0.173)
Manufacturing	0.479	(0.500)	0.276	(0.447)
Construction	0.033	(0.179)	0.107	(0.310)
Retail and repair	0.192	(0.395)	0.185	(0.388)
Services and finance	0.278	(0.448)	0.400	(0.490)
Profitability	0.493	(0.500)	0.413	(0.492)
(b) Worker characteristics				
Workers	24,946		489,686	
Worker-years	99,784		3,502,568	
Log wage	4.851	(0.463)	4.810	(0.504)
Age	42.8	(9.1)	42.8	(9.5)
Female	0.187	(0.390)	0.197	(0.398)
Low-skilled	0.142	(0.349)	0.123	(0.329)
Medium-skilled	0.744	(0.436)	0.759	(0.427)
High-skilled	0.114	(0.317)	0.117	(0.322)

*Notes:* Plant characteristics are plant-year averages which are taken from the IAB Establishment Panel survey. Worker characteristics are worker-year averages which come from the IEB.  $\Delta$ Log Employment is the difference in log employment before ownership change. We classify workers with no vocational training, no high-school degree (Abitur) or workers lacking education information and conducting simple tasks as low-skilled; workers with a high-school degree and/or vocational training or workers lacking education information and conducting specialised tasks as medium-skilled; and workers with a degree from a university or a university of applied sciences or workers lacking education information and conducting highly complex tasks as high-skilled. The dummy variable profitability is one if a plant evaluates its previous profits as very good or good. Further matching variables (not listed) are eight West German federal state dummies for Schleswig-Holstein (including Hamburg), Lower Saxony (including Bremen), North Rhine-Westphalia, Hesse, Rhineland-Palatinate (including Saarland), Baden-Württemberg, Bavaria and Berlin.

acquisitions in the years 2004–12. Despite the long observation period, we observe only 152 foreign takeovers. To avoid problems from small sample size, we therefore follow Martins (2004), Heyman et al. (2007) and Hijzen et al. (2013) and build our analysis on individual worker data. All workers in an establishment subject to a foreign takeover are part of the *treatment group* if they are continuously employed in the same establishment over the four-year window around ownership change. Accordingly:

$$D_{ij} = \begin{cases} 1 & \text{if } i \text{ employed in } j \text{ from } t = 0 \text{ to } t = 3 \text{ and } j \text{ foreign - acquired} \\ 0 & \text{if } i \text{ employed in } j \text{ from } t = 0 \text{ to } t = 3 \text{ and } j \text{ domestic \& non - acquired} \end{cases}$$

defines the treatment indicator  $D_{ij}$  which is equal to one if worker  $i$  from establishment  $j$  that has been acquired by a foreign investor between  $t = 0$  and  $t = 1$  stays in this establishment over the period  $t = 0$

to  $t = 3$ .<sup>5</sup> In contrast, the treatment indicator is zero if worker  $i$ 's plant  $j$  remains domestically owned over the 4-year observation window, which defines our *control group*. However, to identify a causal effect of foreign takeover on wages, we have to take into account that acquired and non-acquired establishments differ in several dimensions, including their workforce (see Table 1). Since previous research shows that establishment characteristics and worker characteristics are determinants of both foreign takeover (see Bandick & Görg, 2010; Girma & Görg, 2007; Heyman et al., 2007) and wage payments (Bayard & Troske, 1999; Idson & Oi, 1999; Winter-Ebmer & Zweimüller, 1999), estimates of the effects of foreign takeover that do not account for pre-existing establishment and worker differences are vulnerable to a selection bias.

To overcome the problem of selection bias, we use nearest-neighbour propensity-score matching to define a suitable control group (see e.g. Rosenbaum & Rubin, 1983). We use the variables reported in Table 1 to construct the treatment and control group. To determine the nearest neighbour of a worker in the treatment group, we proceed in two steps. We first estimate the probability that a worker  $i$  is employed in plant  $j$  in the year prior to foreign takeover ( $t = 0$ ) and stays in the plant until year  $t = 3$ . We then match workers from the treatment group to workers with the smallest absolute distance in their propensity scores in the control group.

We model the probability of workers to be treated:

$$P(D_{ij} = 1) = \Phi(\beta \cdot \mathbf{X}_{j,0} + \gamma \cdot \mathbf{X}_{i,0}), \quad (1)$$

as a function of a vector of establishment-level covariates,  $\mathbf{X}_{j,0}$ , and a vector of worker-level covariates,  $\mathbf{X}_{i,0}$ , measured in period  $t = 0$  (with  $\beta$ ,  $\gamma$  the respective vectors of coefficients). Establishment-level covariates include the log of employment to control for plant size and the change in log employment prior to takeover to capture business conditions, as well as a dummy that is one if a plant reports very good or good profits as proxy for profitability, five sector dummies indicating establishments' industry affiliation and eight federal state dummies determining establishments' location.<sup>6</sup>

Worker-level covariates include information on log wage, age and gender to control for worker heterogeneity. Conditioning on worker characteristics prior to an ownership change ensures that we compare two workers—one in a foreign-acquired and the other in a domestic establishment—with similar earnings potential before the takeover event. To minimise differences in workers' education level, we also match on workers' skill levels. Finally, we match observations from the same year to minimise the risk that estimates of the foreign ownership wage premium pick up macroeconomic changes that have been quite substantial over the observation period.

Table A1 in the Appendix reports the mean values of the covariates in the treatment and control group before and after matching. We also show two diagnostics for evaluating the matching quality based on individual covariates (see Balsvik & Saethre, 2016; Girma & Görg, 2007; Hijzen et al., 2013). The first one is the standardised (percentage) bias put forward by Rosenbaum and Rubin (1985). The reduction of the mean standardised bias from 14.8% in the unmatched to 4.2% in the matched sample indicates a fairly good matching result. As a second diagnostic, we report the normalised difference of covariate means introduced by Imbens and Wooldridge (2009), who suggest an upper limit of one quarter to consider a variable as sufficiently balanced. This criterion is fulfilled for all of our covariates.

<sup>5</sup>Since we do not know the exact date of ownership change, we refer to period  $t = 0$  as the year prior to takeover and to period  $t = 1$  as the year after takeover.

<sup>6</sup>Due to the low number of affected workers in some federal states, we assign Bremen (538 stayers) to Lower Saxony, Saarland (11 stayers) to Rhineland-Palatinate and Hamburg (554 stayers) to Schleswig-Holstein.

We estimate the causal effect of foreign takeover on the wages of stayers using a difference-in-difference (DID) approach. We estimate the treatment effect separately for the year after a foreign takeover and the two subsequent years using a baseline DID model of the following form:

$$w_{ijt} = \alpha_i + \lambda_t + \sum_{s=1}^3 \nu_s \cdot d_t^s \cdot D_{ij} + \varepsilon_{ijt}, \quad (2)$$

where  $w_{ijt}$  is the log daily wage of worker  $i$  in plant  $j$  and year  $t$  after ownership change,  $\alpha_i$  is a worker-fixed effect to control for time-invariant unobserved heterogeneity, and  $\lambda_t$  is a time-fixed effect.<sup>7</sup> Furthermore,  $d_t^s$  is a time dummy equal to one if  $t = s$  and  $D_{ij}$  is the treatment indicator equal to one for each stayer, whose plant  $j$  has been acquired between  $t = 0$  and  $t = 1$ , and zero otherwise. Parameters  $\nu_s$  are the coefficients for the interaction term of the time dummies with the treatment indicator. Finally,  $\varepsilon_{ijt}$  is the error term. Since the estimates of  $\nu_s$  represent the wage premium  $s$  periods after a worker experiences a foreign takeover, Equation (2) allows us to estimate the effect of ownership change at three points in time. Hence, we can determine whether the effect of foreign takeover on wages is immediate or takes time to develop (see Balsvik & Haller, 2010; Hijzen et al., 2013).

The coefficients  $\nu_s$  capture a causal average treatment effect of foreign takeover on workers if the following three assumptions hold. The first one is the *conditional independence assumption* (CIA), which states that conditional on the covariates in Table 1 the assignment of workers into treatment and control groups is random. We take the CIA into account by matching on establishment characteristics, such as establishment size, employment growth, industry, establishments' location and profitability, as well as worker characteristics, such as age, gender, skill dummies and the log daily wage.

The second assumption is the *stable unit treatment value assumption* (SUTVA). In our context, this assumption requires that untreated workers' wages are not affected by other workers staying in a foreign-acquired establishment. We are confident that the SUTVA holds because the number of foreign takeovers compared to the total German establishment population is small (see Table 1). Thus, we do not expect an effect of a foreign takeover on the wages of untreated workers, for example due to an increase in equilibrium wages for all workers.

Third, in the absence of treatment, the wages of both treated and untreated workers have to follow the same path, which is referred to as the *common trend assumption* (CTA). With data from only one pre-acquisition period, it is not possible to test whether the CTA holds prior to takeover (see e.g. Mora & Reggio, 2012). However, our matching approach ensures that workers in the treatment and control group are similar in their skills as well as their wages prior to acquisition. To the extent that wages of workers with similar characteristics follow a common trend in a competitive labour market, it is likely that our matching approach does not violate the CTA. As suggested by Pischke (2005), we investigate pre-treatment trends graphically. Since examining a pre-trend requires at least two observations for each worker prior to a foreign takeover, we restrict the sample to all matched worker pairs, which we observe from period  $t = -1$  to period  $t = 3$ . This decreases the sample to 15,581 stayers in 117 foreign-acquired plants.

Figure 1 depicts the wage trend of treated and untreated workers. The graph shows that wages grow similarly prior to foreign acquisition and that our matching is successful in eliminating differences in average wages between treatment and control group. A test on the equality of means confirms that average wages in period  $t = 0$  do not differ significantly between groups. Finally, a simple linear regression of the wage increase between  $t = -1$  and  $t = 0$  additionally shows that the growth in wages prior to acquisition is similar. These results suggest that the wage premium from foreign takeover does

<sup>7</sup>Since all workers in our sample are stayers, we cannot estimate a separate establishment fixed effect. Put differently, the average worker fixed effect within an establishment is the establishment fixed effect.

not pick up pre-existing differences in the compositions of treatment and control group. As illustrated by Figure 1, workers in establishments that are subject to foreign takeover experience a considerably steeper wage profile after ownership changes.

### 3.2 | Heterogeneity of the foreign wage premium

Combining propensity-score matching with a difference-in-difference approach allows to estimate a causal effect of a foreign takeover on wages. However, if the wage premium from foreign takeover differs between heterogeneous workers or plants, estimates from the pooled sample are less informative. From a policy perspective, heterogeneity in the wage premium is particularly relevant if some workers benefit from a foreign investment while others lose. There are two approaches to analyse if the wage premium varies across workers. First, we can split the sample by observable differences of workers and estimate the model in Equation (2) separately for the sub-samples. We follow this approach to analyse whether pooling over skill groups is justified in our model.

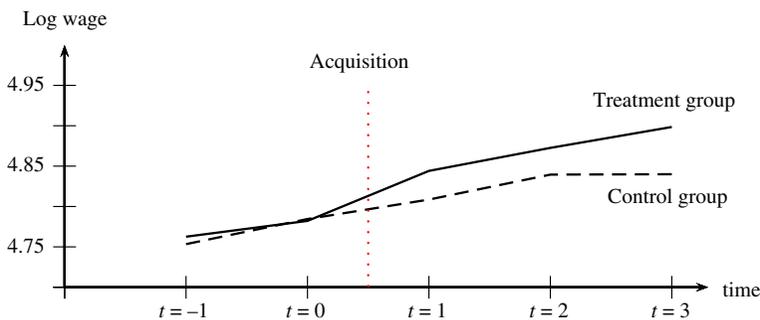
Second, we can include interaction terms in the difference-in-difference regression and estimate a model of the following form:

$$w_{ijt} = \alpha_i + \lambda_t + \sum_{s=1}^3 v_s \cdot d_t^s \cdot D_{ij} + \sum_{s=1}^3 \eta_s \cdot d_t^s \cdot I_i + \sum_{s=1}^3 \pi_s \cdot d_t^s \cdot D_{ij} \cdot I_i + \rho_{ijt}, \quad (3)$$

where  $I_i$  is an indicator variable for worker  $i$ , which is one if worker  $i$  belongs to a certain subgroup and zero otherwise, and  $\rho_{ijt}$  is an error term. Accordingly, coefficients  $\eta_s$  measure the wage differential  $s$  years after ownership change for workers with  $I_i = 1$  compared to workers with  $I_i = 0$ .  $\pi_s$  is the additional wage gain  $s$  years after acquisition from foreign takeover between years  $t = 0$  and  $t = 1$  for workers from this group. In general,  $I_i$  can vary across workers differing in individual characteristics within an establishment (such as the manager status) or can be the same for all workers from the same establishment but differ between establishments (such as the export status of the employer).

## 4 | RESULTS

This section presents results from our difference-in-difference estimation for the matched sample of treated and untreated workers. The estimates reported in Table 2 rely on the baseline specification



**FIGURE 1** Average wages in treated and matched control group [Colour figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com)]

**TABLE 2** The foreign ownership wage premium

	<b>All workers</b>	<b>Low-skilled</b>	<b>Medium-skilled</b>	<b>High-skilled</b>
	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>
Acquisition $t = 1$	0.040*** (0.010)	0.031*** (0.011)	0.031*** (0.009)	0.113*** (0.022)
Acquisition $t = 2$	0.042*** (0.012)	0.008 (0.014)	0.030*** (0.010)	0.167*** (0.028)
Acquisition $t = 3$	0.063*** (0.014)	0.026* (0.016)	0.052*** (0.012)	0.181*** (0.030)
Constant	4.780*** (0.004)	4.641*** (0.005)	4.763*** (0.004)	5.062*** (0.009)
Observations	199,384	28,368	148,352	22,664

*Notes:* Dependent variable is the log daily wage. The estimation includes time dummies and worker-fixed effects. Standard errors in parentheses are clustered at the establishment-level. \*\*\*, \*\* and \* denote significance at the 1, 5 and 10% levels, respectively.

in Equation (2). In the first column, we report our finding for the pooled sample of all workers. The estimates show that workers receive a wage premium of 4.0 log points in the year after ownership change, with the wage premium further increasing to 6.3 log points after 3 years. A simple Wald test, which compares the equality of coefficients, shows that the difference between the premia in  $t = 3$  and  $t = 1$  is significant. These results are similar in size to previous estimates for Germany (see Andrews, Bellmann, Schank, & Upward, 2009; Hijzen et al., 2013) and in line with findings from other studies arguing that the impact of ownership change takes time to develop (see Balsvik & Haller, 2010; Hijzen et al., 2013).

Columns (2) to (4) of Table 2 show the estimates of foreign takeover by skill groups. We find that the immediate wage impact in the period of ownership change with 3.1 log points is less pronounced for low- and medium-skilled workers than for high-skilled workers, who obtain a sizeable wage premium of 11.3 log points right after a foreign takeover. This indicates that foreign takeover exerts a stronger positive wage effect if workers are better educated. The wage premium of low-skilled workers is positive but insignificantly different from zero in  $t = 2$  and reaches 2.6 log points in the third year after takeover. The wage premium for medium-skilled workers increases to 5.2 log points while the wage premium for high-skilled workers noticeably rises to 18.1 log points after 3 years. The differences in the wage premia between periods  $t = 3$  and  $t = 1$  for medium- and high-skilled workers are significant. Thus, wages even grow stronger after the ownership change—particularly for workers with higher skill levels.

With the results in Table 2 at hand, we investigate three main channels highlighted in the literature explaining positive wage effects of a foreign takeover. A first potential explanation is the *rent appropriation by managers* hypothesis. Evidence for this hypothesis has been put forward by Heyman et al. (2011). Using Swedish linked employer–employee data, they find that the foreign ownership wage premium for high-skilled workers disappears when additionally controlling for individuals' manager status. Manasse and Turrini (2001) discuss a theoretical argument for managers' rent appropriation by arguing that manager income is directly linked to profits, implying that this group benefits disproportionately from cross-border activity (see Kong, Kong, Panga, & Zhang, 2018, for supportive evidence). In the context of foreign acquisition, incentives for rent

appropriation by managers exist in particular if performance-based contracts stipulate higher remuneration upon successful takeover.<sup>8</sup>

To investigate to what extent the skill bias in the foreign takeover effect reported in Table 2 captures differences in the positions of workers with different skill levels in a plant's hierarchy, we estimate the model outlined in Equation (3), in which we additionally interact the treatment indicators with the manager status of workers. For this purpose, we use occupation codes from the German nomenclature in 1988 (KldB 1988) available in the LIAB.<sup>9</sup> We convert these codes into the international standard classification of occupations (ISCO-88 COM) and, following Caliendo, Monte, and Rossi-Hansberg (2015), assign employees to three hierarchical layers: *workers*, *supervisors* and *managers or directors*. We then refer to *managers or directors* simply as *managers*.<sup>10</sup> Of course, formal education and hierarchical position in a plant are highly correlated. Still, distinguishing between skill groups and management status is not the same, because low-skilled workers can be managers of an establishment, while high-skilled university graduates can end up working in the production process. In our sample, about 13.4% of workers are managers, with the fraction varying between 1.3% among low-skilled workers, 7.9% among medium-skilled workers and 63.4% among high-skilled workers. Table 3 summarises the estimation results when additionally taking into account the manager status of workers.

Column (1) in Table 3 shows that the wage premium of 3.1 log points for non-managers is considerably lower than for managers who receive a wage premium of 10.3 log points in the year after acquisition. Similar to the baseline results in Table 2, the wage premium increases for non-managers to 5.1 log points and for managers to 14.8 log points three periods after the ownership change. This result is well in line with the *rent appropriation by managers* hypothesis.<sup>11</sup>

Columns (2) to (4) show that for non-managers the initial ranking of wage gains from our baseline specification is unaffected with the wage premia ranging from 2.7 to 10.0 log points in the first and 2.7 to 15.4 log points in the third year after ownership change. Moreover, we find an additional and immediate wage gain of 3.5 log points for medium-skilled managers, which stays fairly stable over the three-year, post-treatment observation window. Our results for the low-skilled group indicate that in the first year after ownership change low-skilled managers receive an additional positive wage premium of 6.0 log points. In the second and third year after takeover for low-skilled workers and over the whole post-treatment observation window for the group of high-skilled workers, we estimate a positive effect of manager status on the foreign ownership wage premium, which is, however, not statistically significant. Summing up our results, we find that after separating the wage gain by manager status, wage premia for non-managers decrease in all skill groups—with high-skilled workers still receiving the largest wage increase—but remain economically and statistically significant. This differentiates our results from Heyman et al. (2011) and indicates that the *rent appropriation by*

<sup>8</sup>The *rent appropriation by managers* hypothesis is also supported by findings that top executives experience a strong income increase after a successful acquisition (cf. Schmidt & Fowler, 1990; Datta et al., 2002; Girma et al., 2006). Guest (2009) extends the analysis to international acquisitions and shows that the payment increase for managers is temporary. Whereas studies on the link between acquisition and manager remuneration typically focus on the effects in acquiring firms, one may argue that if there are rents to share, managers of acquired plants should also benefit from a takeover.

<sup>9</sup>The occupation codes capture the job a worker currently performs.

<sup>10</sup>Accordingly, our manager category comprises workers from ISCO-88 COM codes 111–131 and 211–247.

<sup>11</sup>Since most managers are high-skilled (about 55% in our sample), our estimated manager premia could in fact simply control for high skills. To make sure that this is not the case, we have estimated an additional specification controlling for high-skilled workers and their interaction with the acquisition dummy. This lowers the estimated manager premium but does not eliminate it. Also, including a further interaction between acquisition, high-skilled workers, and manager status does not change this result.

**TABLE 3** The foreign ownership wage premium for managers

	All workers	Low-skilled	Medium-skilled	High-skilled
	(1)	(2)	(3)	(4)
Acquisition $t = 1$	0.031*** (0.009)	0.027** (0.011)	0.028*** (0.008)	0.100*** (0.028)
Acquisition $t = 2$	0.032*** (0.010)	0.008 (0.013)	0.030*** (0.009)	0.140*** (0.031)
Acquisition $t = 3$	0.051*** (0.012)	0.027* (0.015)	0.049*** (0.012)	0.154*** (0.029)
Manager $\times$ acquisition $t = 1$	0.072*** (0.019)	0.060* (0.036)	0.035* (0.020)	0.014 (0.034)
Manager $\times$ acquisition $t = 2$	0.087*** (0.021)	0.033 (0.032)	0.015 (0.018)	0.039 (0.039)
Manager $\times$ acquisition $t = 3$	0.097*** (0.026)	0.023 (0.039)	0.036* (0.022)	0.042 (0.046)
Constant	4.780*** (0.004)	4.641*** (0.005)	4.763*** (0.004)	5.062*** (0.009)
Observations	199,384	28,368	148,352	22,664

Notes: Dependent variable is the log daily wage. The estimation includes time dummies and worker-fixed effects. Standard errors in parentheses are clustered at the establishment-level. \*\*\*, \*\* and \* denote significance at the 1, 5 and 10% levels, respectively.

*manager* hypothesis does not provide a satisfying explanation for the skill bias in the wage effects reported in Table 2.

In a next step, we investigate to what extent the wage effects reported in Table 2 capture an incentive of firms to reduce job turnover and technology dissipation (*technology protection*), as argued for instance by Glass and Saggi (2002). An incentive to reduce worker turnover exists in particular if the acquired plant developed an innovation around the event of foreign takeover. Otherwise, competitors could have targeted workers with valuable knowledge from the respective plant already prior to foreign acquisition. Such an innovation can be triggered by the investor, because after ownership change firm assets are jointly used, which gives the acquired plant access to the know-how of the foreign owner (see Barba Navaretti & Vernables, 2004). This technology transfer may be associated with *process innovation* and evidence for it is provided by Bloom, Genakos, Sadun, and Van Reenen (2012) and Bloom, Sadun, and Van Reenen (2012). However, there could also be a *product innovation* in the acquired plant.

To test this *technology protection* hypothesis, we investigate both *product* and *process innovation*. We capture *product innovation* by a survey question asking if the plant introduced a completely new activity or product over the two preceding years. From this question, we derive a dummy variable, which is one if a plant reports *product innovation* in the 2 years prior to takeover and zero otherwise. Thirty of 152 acquired plants (accounting for 9,975 stayers) report *product innovation* according to this criterion. Table 4 reports the results after adding the interaction of this dummy with the treatment indicators to the model outlined in Equation (3). In Column (1), we see little evidence for an impact of *product innovation* on the foreign ownership wage premium when considering the pooled sample of all workers—although the estimates of the acquisition dummies are somewhat reduced. This picture does not change, when zooming in on the subgroups of low- and medium-skilled workers. However, for high-skilled workers the estimated foreign ownership premium increases by 7.6–12.6 log point in plants reporting *product innovation*, with

**TABLE 4** The foreign ownership wage premium with product innovation

	All workers	Low-skilled	Medium-skilled	High-skilled
	(1)	(2)	(3)	(4)
Acquisition $t = 1$	0.028*** (0.007)	0.019* (0.010)	0.024*** (0.007)	0.068*** (0.015)
Acquisition $t = 2$	0.034*** (0.008)	-0.000 (0.011)	0.028*** (0.008)	0.113*** (0.020)
Acquisition $t = 3$	0.046*** (0.012)	0.010 (0.013)	0.042*** (0.012)	0.118*** (0.023)
Product innovation × acquisition $t = 1$	0.019 (0.017)	0.013 (0.021)	0.009 (0.019)	0.076** (0.030)
Product innovation × acquisition $t = 2$	0.005 (0.020)	-0.002 (0.026)	-0.010 (0.018)	0.095*** (0.036)
Product innovation × acquisition $t = 3$	0.030 (0.022)	0.022 (0.029)	0.015 (0.021)	0.126*** (0.038)
Constant	4.780*** (0.003)	4.641*** (0.004)	4.763*** (0.003)	5.062*** (0.005)
Observations	199,384	28,368	148,352	22,664

Notes: Dependent variable is the log daily wage. The estimation includes time dummies and worker-fixed effects. Standard errors in parentheses are clustered at the establishment-level. \*\*\*, \*\* and \* denote significance at the 1, 5 and 10% levels, respectively.

the estimated effects being highly statistically significant. This suggests that concerns of job turnover and technology dissipation can explain part of the skill bias in the estimated wage effects of foreign takeover.

To investigate the role of *process innovation*, we rely on information about new investment and create a dummy variable, which is one if a plant reports positive investment in the period of foreign takeover and zero otherwise. The idea is that firms with modern technology are more receptive to the know-how provided by the new owner. Two-thirds of the acquired plants (accounting for 20,893 stayers) report *process innovation* according to this definition. Table 5 displays the results from estimating Equation (3) when controlling for the interaction of the *process innovation* dummy with the treatment indicators. From these estimates, we see that in acquired plants *process innovation* exerts a statistically significant effect only 3 years after ownership change for the subgroup of high-skilled workers. However, the small insignificant coefficient in  $t = 2$  and the following increase in  $t = 3$  for high-skilled workers are in line with installment of new technology and training of the workforce after foreign takeover. This is because according to the *training on new technology* hypothesis, foreign takeover and technology transfer should lead to lagged positive wage effects as training to use technology productively takes time (see Fosfuri et al., 2001; Görg et al., 2007).<sup>12</sup>

<sup>12</sup>The lack of strong supportive evidence for *technology protection* in the context of *process innovation* could come from using a rather imprecise proxy for technology transfer. Therefore, we have run an additional specification, in which we associate positive investment with *process innovation* only if the plant additionally reports expenditures for IT infrastructure. This captures the idea that foreign-owned plants use IT more productively or that the foreign owner implements new management practices that may require intensive communication with the parent (cf. Bloom et al., 2012; Javorcik & Poelhekke, 2017; Bloom & Van Reenen, 2010). Using this alternative measure as a proxy for *process innovation* gives slightly stronger results and significant effects of the interaction term for medium- and high-skilled workers. The general picture from Table 5 does not change after this refinement.

**TABLE 5** The foreign ownership wage premium with process innovation

	All workers	Low-skilled	Medium-skilled	High-skilled
	(1)	(2)	(3)	(4)
Acquisition $t = 1$	0.027*	0.018	0.013	0.097***
	(0.015)	(0.016)	(0.015)	(0.036)
Acquisition $t = 2$	0.057***	0.014	0.039***	0.168***
	(0.014)	(0.021)	(0.014)	(0.034)
Acquisition $t = 3$	0.045**	0.009	0.034*	0.120***
	(0.019)	(0.024)	(0.019)	(0.035)
Process innovation $\times$ acquisition $t = 1$	0.016	0.015	0.022	0.025
	(0.019)	(0.020)	(0.018)	(0.044)
Process innovation $\times$ acquisition $t = 2$	-0.017	-0.006	-0.011	0.007
	(0.020)	(0.026)	(0.018)	(0.047)
Process innovation $\times$ acquisition $t = 3$	0.021	0.020	0.021	0.086*
	(0.025)	(0.030)	(0.024)	(0.047)
Constant	4.780***	4.641***	4.763***	5.062***
	(0.004)	(0.005)	(0.004)	(0.008)
Observations	199,384	28,368	148,352	22,664

Notes: Dependent variable is the log daily wage. The estimation includes time dummies and worker-fixed effects. Standard errors in parentheses are clustered at the establishment-level. \*\*\*, \*\* and \* denote significance at the 1, 5 and 10% levels, respectively.

In a final step, we analyse to what extent the estimated foreign ownership premium actually captures a wage premium from exporting (see Bernard & Jensen, 1995; Schank, Schnabel, & Wagner, 2007) because the main reason for the investment decision is a platform motive (*export platform hypothesis*). This concern is of particular relevance for the years after the millennium, because the trade literature points to the opening up of Eastern Europe as a historic event providing evidence for platform investment (see Ekholm, Forslid, & Markusen, 2007; Motta & Norman, 1996; Neary, 2009). To account for the *export platform hypothesis*, we consider the initial exporter status of establishments and interact it with the treatment indicator. The exporter dummy is one if the plant generated positive revenues abroad in the year prior to acquisition. Seventy acquired plants (accounting for 17,539 stayers) report positive exports prior to the takeover event.

Table 6 displays the wage effects for workers employed by initial exporters and non-exporters. Our results indicate that the wage premium is not significantly different between workers employed by exporters and non-exporters. Dividing the sample by skill groups shows that we observe the same ranking of wage premia as in our baseline specification, that is high-skilled workers receiving the highest premium and low-skilled workers the lowest one. From the results in Table 6, we therefore conclude that the estimated wage premia do not pick up an exporter effect.

Summing up, our estimation results illustrate that *rent appropriation* (Heyman et al., 2011), *technology protection* (Glass & Saggi, 2002) and *training on technology* (Fosfuri et al., 2001; Görg et al., 2007) are relevant channels for explaining the wage effects of foreign takeover in German plants. However, a single hypothesis does not explain the whole pattern of the observed wage effects and our finding of a positive impact and a lagged adjustment effect therefore suggests that different theoretical hypotheses have to be combined to better understand wage effects of foreign acquisition. As discussed, platform investment to access Eastern European markets may also be an important motive for foreign

**TABLE 6** The foreign ownership wage premium for exporters

	All workers	Low-skilled	Medium-skilled	High-skilled
	(1)	(2)	(3)	(4)
Acquisition $t = 1$	0.057*** (0.021)	0.038* (0.023)	0.049** (0.023)	0.132*** (0.028)
Acquisition $t = 2$	0.053*** (0.017)	0.010 (0.016)	0.045** (0.018)	0.157*** (0.041)
Acquisition $t = 3$	0.064*** (0.021)	0.036* (0.019)	0.055** (0.023)	0.157*** (0.031)
Exporter $\times$ acquisition $t = 1$	-0.023 (0.024)	-0.010 (0.026)	-0.025 (0.025)	-0.029 (0.040)
Exporter $\times$ acquisition $t = 2$	-0.015 (0.023)	-0.002 (0.025)	-0.023 (0.021)	0.013 (0.053)
Exporter $\times$ acquisition $t = 3$	-0.002 (0.028)	-0.013 (0.028)	-0.005 (0.027)	0.033 (0.048)
Constant	4.780*** (0.004)	4.641*** (0.005)	4.763*** (0.004)	5.062*** (0.009)
Observations	199,384	28,368	148,352	22,664

Notes: Dependent variable is the log daily wage. The estimation includes time dummies and worker-fixed effects. Standard errors in parentheses are clustered at the establishment-level. \*\*\*, \*\* and \* denote significance at the 1, 5 and 10% levels, respectively.

takeover. However, there is no evidence from our analysis that we falsely attribute an exporter premium to a foreign ownership effect.

Whereas we consider different channels for explaining the wage premium of foreign takeover above, lacking the necessary data we cannot shed light on two further channels that may be similarly important. The first one is rooted in a general rent-sharing argument, stressing that employers as well as employees benefit from increasing profits. In a study for the UK, Conyon, Girma, Thompson, and Wright (2004) shows that mergers lead to higher profitability and higher wages, providing support for this argument in the context of acquisitions. However, they exclude international mergers from their data set. Rent sharing in the context of international ownership has been put forward by Budd, Konings, and Slaughter (2005) who provide evidence for international rent sharing in multinational firms. According to their results, wages in German plants should increase if the acquiring firm has higher profits than its target. Egger and Kreickemeier (2013) additionally shows that international rent sharing can rationalise evidence on residual wage premia in the context of North-South investment.<sup>13</sup>

A second channel we cannot address in this paper is the changing demand for skill types after foreign takeover, which can be an important factor for explaining the skill bias in the wage effects reported in Table 2. Hummels, Jørgensen, Munch, and Xiang (2014) show a differential effect of offshoring on skilled and unskilled workers in Danish firms and argue that such a differential effect can exist if firms face skill-specific, upward-sloping labour supply curves. Egger, Kreickemeier, Moser, and Wrona (2019) elaborate on this argument in a general equilibrium framework with monopsonistic

<sup>13</sup>Relying on Chinese data, Greaney and Li (2017) find support that foreign-owned plants pay higher wages because of rent-sharing and to reduce worker turnover.

**TABLE 7** The foreign ownership wage premium over 5 years

	<b>All workers</b>	<b>Low-skilled</b>	<b>Medium-skilled</b>	<b>High-skilled</b>
	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>
Acquisition $t = 1$	0.055*** (0.017)	0.060*** (0.019)	0.043** (0.017)	0.135*** (0.032)
Acquisition $t = 2$	0.063*** (0.017)	0.041* (0.022)	0.047*** (0.015)	0.219*** (0.043)
Acquisition $t = 3$	0.095*** (0.022)	0.065*** (0.021)	0.080*** (0.022)	0.251*** (0.038)
Acquisition $t = 4$	0.117*** (0.026)	0.071** (0.029)	0.103*** (0.025)	0.300*** (0.037)
Acquisition $t = 5$	0.100*** (0.035)	0.050 (0.040)	0.081** (0.034)	0.323*** (0.067)
Constant	4.799*** (0.009)	4.679*** (0.010)	4.786*** (0.009)	5.075*** (0.017)
Observations	111,468	15,468	85,188	10,812

*Notes:* Dependent variable is the log daily wage. The estimation includes time dummies and worker-fixed effects. Standard errors in parentheses are clustered at the establishment-level. \*\*\*, \*\* and \* denote significance at the 1, 5 and 10% levels, respectively.

labour market competition and demonstrate that in such a setting one can also explain that a major part of vertical foreign investment is observed between similar countries. However, due to not having information on the acquirer's profitability and lacking the data necessary to estimate labour supply at the firm-level, we cannot further analyse these two additional channels.

## 5 | EXTENSIONS

Section 4 documents a sizeable effect on wages in the year after foreign takeover as well as evidence for a lagged adjustment effect, suggesting additional wage growth in later years for medium- and high-skilled workers. There are two possible interpretations of the lagged adjustment in wages. On the one hand, additional positive wage effects may arise because it takes some time before the full effect of ownership change materialises—for instance, due to increased training in the period right after ownership change (see Fosfuri et al., 2001; Görg et al., 2007). On the other hand, the lagged adjustment in wages is also consistent with the idea that foreign takeover does not only increase the level but also the growth rate of wages.

To discriminate between these two interpretations, we expand the time window around the ownership change to 6 years. Expanding the observation window reduces the number of takeover events to 50 and the number of treated stayers to 9,289. It also somewhat increases the mean standardised bias of matching. Still, this exercise has the advantage of showing a more long-run perspective of wage adjustments due to ownership change. Table 7 reports the results and shows that—despite the reduction in the number of observations—the general picture regarding the effects of foreign takeover on wages in the first 3 years after ownership change remains by and large unaffected. Furthermore, looking at periods four and five after ownership change reveals that the wage growth is temporary and washes out after 4 years. For low- and medium-skilled workers, we even observe a decline in the estimated wage

**TABLE 8** The wage effect of foreign divestments

	All workers	Low-skilled	Medium-skilled	High-skilled
	(1)	(2)	(3)	(4)
Divestment $t = 1$	-0.005 (0.009)	-0.014 (0.009)	0.000 (0.011)	-0.028 (0.023)
Divestment $t = 2$	0.002 (0.011)	-0.021* (0.012)	0.007 (0.012)	0.001 (0.034)
Divestment $t = 3$	0.008 (0.012)	-0.014 (0.012)	0.009 (0.013)	0.036 (0.037)
Constant	4.778*** (0.003)	4.578*** (0.003)	4.769*** (0.004)	5.127*** (0.010)
Observations	70,016	11,640	50,328	8,048

Notes: Dependent variable is the log daily wage. The estimation includes time dummies and worker-fixed effects. Standard errors in parentheses are clustered at the establishment-level. \*\*\*, \*\* and \* denote significance at the 1, 5 and 10% levels, respectively.

premium after 4 years. High-skilled workers receive a higher wage premium in  $t = 5$  as compared to  $t = 4$ , but the estimates do not differ significantly, indicating that the wage premia for high-skilled workers also stop increasing after 4 years. This suggests that foreign takeover affects the level and not the growth rate of wages, with the level effect requiring about 4 years before materialising fully.

Another question related to the timing of wage effects recently addressed by Javorcik and Poelhekke (2017, p. 501) is whether the benefit from foreign ownership is ‘due to a one-time knowledge transfer or [due to] continuous injections of knowledge.’ To rephrase the question in our context: Are higher wages due to foreign ownership temporary or do they persist and continue to exist even after foreign investors sell back domestic plants? To elaborate on this question, we investigate the wage effect of foreign divestments, that is (re-)acquisitions of foreign-owned plants by German investors. During the sample period, we identify 97 foreign divestments with 8,752 stayers. Using our propensity-score matching approach with employees in continuously foreign-owned plants as controls, we can estimate an equation similar to (2) to shed light on the effect of foreign divestments.

Column (1) in Table 8 shows that foreign divestment does not have a significant effect on wages. The estimates in column (2) suggest that low-skilled workers are worse off after foreign divestment. However, the wage loss is small and only significant in  $t = 2$ . Medium- and high-skilled wages are not affected significantly in any period. Overall, these results imply that at least for medium- and high-skilled workers the positive wage effect of foreign ownership does not vanish within 3 years after takeover by a domestic owner.

The results in Table 8 also indicate that the wage premia reported in Table 2 are due to foreign takeover and not the result of a general acquisition effect. To further elaborate on this finding, we consider as a Placebo test takeovers of West German plants by East or West German investors. Thereby, we use the IAB Establishment Panel to identify whether the majority owner of an establishment is from West or East Germany and classify a German takeover as an event, in which ownership in two consecutive years switches from West German to East German or vice versa. We identify 81 intra-German takeovers with 2,721 stayers over our sample period. The matching for this placebo is similarly successful as for our main specification. Table 9 presents the results.

To the extent that our estimates so far only capture a general takeover effect, stayers in domestic-acquired establishments should experience a wage increase similar to stayers in foreign-acquired establishments. However, Table 9 shows that this is clearly not the case. For low- and high-skilled

**TABLE 9** The wage premium from intra-German takeovers

	<b>All workers</b>	<b>Low-skilled</b>	<b>Medium-skilled</b>	<b>High-skilled</b>
	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>
Acquisition $t = 1$	-0.004	-0.003	-0.009	0.023
	(0.012)	(0.018)	(0.012)	(0.025)
Acquisition $t = 2$	-0.022*	-0.017	-0.027**	-0.003
	(0.012)	(0.023)	(0.013)	(0.033)
Acquisition $t = 3$	-0.050	-0.088	-0.047	0.003
	(0.036)	(0.070)	(0.031)	(0.027)
Constant	4.366***	4.298***	4.351***	4.581***
	(0.005)	(0.010)	(0.005)	(0.009)
Observations	21,736	4,424	14,896	2,416

*Notes:* Dependent variable is the log daily wage. Estimation includes relative time dummies and controls for worker-fixed effects. Standard errors in parentheses are clustered at the establishment-level. \*\*\*, \*\* and \* denote significance at the 1, 5 and 10% levels, respectively.

workers, we find no significant effect of a German takeover. Medium-skilled workers even lose from a takeover by a German investor after 2 years. Summing up, the insignificant and negative coefficients in Table 9 support the findings from foreign divestment that the wage premia in Table 2 do not capture a general takeover, but rather a genuine foreign takeover effect.

To ensure that our results are robust to changing the set of covariates used for matching, we control for three additional variables in the propensity-score estimation. The first one is a dummy variable that is one if the establishment has already existed prior to 1990. This dummy controls for selection of older targets by foreign investors, which can pay higher wages because they are already well-established in the market (see Heyman et al., 2007). As a second matching variable, we add a dummy that is one if wages paid by the establishment are subject to collective labour agreements. Accounting for this dummy helps ruling out a selection bias because foreign investors aim to avoid wage pressure from unions.<sup>14</sup> As a final control variable, we add the share of female workers because recent research by Vahter and Masso (2018) suggests that foreign investors require stronger commitment and higher flexibility from workers, which potentially leads to selection of establishments with fewer women, who might be less flexible due to family responsibilities. We compute the share of female workers in  $t = 0$  as the share of females among the total workforce in an establishment. If foreign investors acquire establishments, which are older, covered by collective agreements and/or employ an below-average share of women, our matching would suffer from omitted variable bias when not controlling for these covariates.<sup>15</sup>

Table 10 shows the results after adding the three additional matching variables and confirms our initial findings. Thus, selection by firm age, coverage by collective agreements or gender composition does not drive our results. Matching with a more extensive set of covariates leaves the initial ranking of wage premia unaffected. Moreover, the wage premia are similar in size, ranging from 2.8 to 10.7 log points in the period after takeover and 2.7 to 17.4 log points after 3 years.

<sup>14</sup>The turmoil after the takeover of the German automotive supplier Grohmann Engineering by Tesla is a prominent example in this case.

<sup>15</sup>Despite adding three new covariates, matching is similarly successful as in our main specification. The mean standardised bias slightly increases to 6.5%, while normalised differences are smaller than one quarter for all covariates.

**TABLE 10** Additional matching covariates

	All workers	Low-skilled	Medium-skilled	High-skilled
	(1)	(2)	(3)	(4)
Acquisition $t = 1$	0.037*** (0.010)	0.028** (0.011)	0.028*** (0.009)	0.107*** (0.022)
Acquisition $t = 2$	0.038*** (0.012)	0.011 (0.015)	0.024** (0.010)	0.165*** (0.028)
Acquisition $t = 3$	0.060*** (0.014)	0.027* (0.015)	0.049*** (0.012)	0.174*** (0.030)
Constant	4.757*** (0.004)	4.626*** (0.005)	4.740*** (0.004)	5.031*** (0.009)
Observations	199,496	28,400	148,432	22,664

*Notes:* Nearest-neighbour propensity-score matching includes a dummy indicating if the plant has already existed prior to 1990, a dummy for workers under collective labour agreements and the establishment share of females in addition to baseline covariates in Table A1. The dependent variable in the difference-in-difference estimation is the log daily wage. The estimation includes time dummies and worker-fixed effects. Standard errors in parentheses are clustered at the establishment-level. \*\*\*, \*\* and \* denote significance at the 1, 5 and 10% levels, respectively.

In a further extension reported in Table 11, we refine our matching approach by introducing a caliper of 0.05. This implies that stayers are matched to controls only within a range of 0.05 of their respective propensity scores and excludes observations for which no match is found within this range.<sup>16</sup>

Table 11 confirms our baseline results that foreign takeover leads to an immediate wage increase for all workers. The reported coefficients vary between 2.1 and 12.2 log points in  $t = 1$  and 1.5 to 18.7 log points in  $t = 3$  and are comparable in size to the estimates relying on nearest-neighbour matching without a caliper reported in Table 2. However, the estimated low-skilled wage premium in period  $t = 3$  is no longer significant. Overall, we nonetheless conclude that the finding of a significant foreign ownership wage premium is robust to refinements of the matching procedure.<sup>17</sup>

## 6 | CONCLUSION

This paper provides new evidence on the foreign ownership wage premium from a large German employer–employee data set. Using information on ownership change of German establishments,

<sup>16</sup>The standardised bias of caliper matching is 6.1%, and normalised differences are less than 0.13 for all variables. The number of establishments subject to foreign takeover remains unaffected, whereas the number of treated workers falls to 21,857, which implies a decrease by 3,066 observations.

<sup>17</sup>The finding of a significant foreign ownership wage premium also remains unchanged when conducting matching with replacement, when matching separately for each skill group or after excluding workers lacking reliable education information. Since we cannot rule out that plants in the control group are multinationals themselves, our estimated wage premia may be compressed due to a comparison of foreign-owned multinationals with domestic-owned multinationals. To make sure that this is not the case, we have conducted a robustness check, in which we confine the control group to standalone plants only. This modification does hardly affect our results and is available upon request. Furthermore, the results are robust to including part-time workers or to restricting the sample to establishments with more than ten full-time workers, which is the threshold at which employment protection legislation in Germany limits the scope of establishments to lay off workers.

**TABLE 11** The foreign ownership wage premium with caliper matching

	All workers	Low-skilled	Medium-skilled	High-skilled
	(1)	(2)	(3)	(4)
Acquisition $t = 1$	0.036*** (0.010)	0.021* (0.011)	0.024** (0.010)	0.122*** (0.030)
Acquisition $t = 2$	0.035*** (0.011)	-0.005 (0.010)	0.020** (0.009)	0.173*** (0.035)
Acquisition $t = 3$	0.057*** (0.013)	0.015 (0.013)	0.043*** (0.012)	0.187*** (0.037)
Constant	4.752*** (0.004)	4.611*** (0.004)	4.735*** (0.004)	5.033*** (0.012)
Observations	174,856	25,240	128,704	20,912

*Notes:* Propensity-score matching is conducted with a caliper of 0.05. The dependent variable in the difference-in-difference estimation is the log daily wage. The estimation includes time dummies and worker-fixed effects. Standard errors in parentheses are clustered at the establishment-level. \*\*\*, \*\* and \* denote significance at the 1, 5 and 10% levels, respectively.

we observe 152 foreign takeover events over the period 2003 to 2014. To identify a causal effect of foreign acquisition, we combine propensity-score matching with a difference-in-difference estimator. We then analyse workers' wages in the year after ownership change as well as in two subsequent periods to distinguish impact effects from lagged adjustment effects. Similar to previous studies, we conduct our analysis at the worker level and consider employees staying in the same establishment over a four-year window around ownership change.

Based on this data set, we provide evidence for the existence of a foreign ownership wage premium in the year after takeover. On average, this wage premium amounts to 4.0 log points and varies considerably by skill group. We find evidence for a lagged adjustment effect, in particular for medium- and high-skilled workers. Moreover, high-skilled workers experience the highest wage stimulus from ownership change over the whole observation period. In further estimations, we distinguish possible explanations for the existence of a foreign ownership wage premium. We find support for the *rent appropriation by managers* hypothesis, the *technology protection* hypothesis and the *training on new technology* hypothesis. We also show that the wage premium is of similar size for initial exporters and non-exporters, indicating that our results do not falsely pick up an exporter wage premium. Expanding the observation window around takeover events reveals that foreign takeover increases the level but not the growth rate of wages, with the level effect requiring roughly 4 years before materialising fully. We also show that the wage gains from a foreign takeover are persistent and do not vanish after divestment. In a placebo test, we analyse takeovers by German investors and show that intra-German takeovers—if at all—affect wages negatively. Finally, we also document that our results are robust to changes in the matching procedure.

Drawing a nuanced picture of the foreign ownership wage premium, we think that our results are of interest to policymakers, who set the rules and conditions for foreign investment. In this respect, our finding that workers' positions in the hierarchy of establishments explain part of the wage premium is disconcerting, as it suggests that foreign takeover gives the management of acquired establishments scope for rent appropriation. At the same time, it is comforting that the wage increase after foreign takeover seems to be persistent and is not eliminated when the foreign owner decides to sell back the plant to German investors. Moreover, we believe that distinguishing immediate impact and lagged adjustment effects of foreign takeover within a single framework is a promising avenue for future research.

## ACKNOWLEDGEMENTS

We are grateful to two anonymous reviewers for helpful comments and suggestions. We also would like to thank participants of two research seminars at the University of Bayreuth, the Bavarian Graduate Program in Economics (BGPE)-Workshop in Regensburg and the 21st Annual Conference of the European Trade Study Group (ETSG) in Bern for valuable discussion.

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**How to cite this article:** Egger H, Jahn E, Kornitzky S. Reassessing the foreign ownership wage premium in Germany. *World Econ.* 2020;43:302–325. <https://doi.org/10.1111/twec.12910>

## APPENDIX

TABLE A1 Balancing test for the matching procedure

Variable	Sample	Mean		Stand. bias %	Bias reduction	Normal diff.
		Treated	Control			
(a) Plant characteristics						
Log employment	Unmatched	6.841	6.982	-8.3		
Log employment	Matched	6.841	6.569	16.0	-92.3	0.133
$\Delta$ Log employment	Unmatched	-0.012	0.009	-17.3		
$\Delta$ Log employment	Matched	-0.012	-0.013	0.4	97.6	0.002
Profitability	Unmatched	0.580	0.531	9.8		
Profitability	Matched	0.580	0.555	5.0	49.2	0.035
Manufacturing	Unmatched	0.758	0.681	17.1		
Manufacturing	Matched	0.758	0.783	-5.6	67.2	-0.041
Construction	Unmatched	0.005	0.031	-19.2		
Construction	Matched	0.005	0.008	-2.2	88.5	-0.025
Trade and repair	Unmatched	0.051	0.076	-10.3		
Trade and repair	Matched	0.051	0.045	2.1	79.3	0.017
Services and finance	Unmatched	0.180	0.183	-1.0		
Services and finance	Matched	0.180	0.157	5.9	-498.3	0.041
Lower Saxony	Unmatched	0.173	0.136	10.5		
Lower Saxony	Matched	0.173	0.149	6.8	35.0	0.048
North Rhine-Westphalia	Unmatched	0.094	0.231	-37.9		
North Rhine-Westphalia	Matched	0.094	0.107	-3.6	90.4	-0.031
Hesse	Unmatched	0.217	0.072	42.0		
Hesse	Matched	0.217	0.203	3.9	90.7	0.022
Rhineland-Palatinate	Unmatched	0.042	0.073	-13.5		
Rhineland-Palatinate	Matched	0.042	0.054	-5.2	61.5	-0.040
Baden-Württemberg	Unmatched	0.278	0.137	35.3		
Baden-Württemberg	Matched	0.278	0.312	-8.3	76.5	-0.051
Bavaria	Unmatched	0.104	0.265	-42.5		
Bavaria	Matched	0.104	0.086	4.5	89.4	0.041
Berlin	Unmatched	0.028	0.022	3.9		
Berlin	Matched	0.028	0.021	4.3	-9.5	0.031
(b) Worker characteristics						
Log Wage	Unmatched	4.779	4.761	4.1		
Log Wage	Matched	4.779	4.782	-0.5	86.8	-0.006
Age	Unmatched	41.3	41.3	-0.7		
Age	Matched	41.3	41.3	-0.4	43.6	-0.003
Female	Unmatched	0.187	0.197	-2.7		

(Continues)

**TABLE A1** (Continued)

Variable	Sample	Mean		Stand. bias %	Bias reduction	Normal diff.
		Treated	Control			
Female	Matched	0.187	0.180	1.6	40.6	0.012
Medium skill	Unmatched	0.744	0.759	-3.6		
Medium skill	Matched	0.744	0.740	1.0	71.5	0.007
High skill	Unmatched	0.114	0.117	-1.2		
High skill	Matched	0.114	0.120	-2.1	-73.4	-0.014
Sample			Mean bias	Median bias		
Unmatched			14.8	10.3		
Matched			4.2	3.9		

*Notes:* All variables are measured in  $t = 0$  and averaged at the worker level in the treated and control group, respectively. Due to the low number of takeovers in small Federal States, we assign the city state of Bremen to Lower Saxony, Saarland to Rhineland-Palatinate and the city state of Hamburg to Schleswig-Holstein. The omitted Federal State is Schleswig-Holstein including Hamburg; the omitted sector is agriculture, hunting and forestry.