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FDI and Wages:
Evidence from Linked Employer-Employee Data in Hungary, 1986-2005

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Abstract

This paper analyzes the effects of foreign ownership on wage structure and employment composition using a linked employer-employee panel in Hungary. The data set follows workers and firms from 1986, when any foreign presence in corporations was prohibited, until 2005, the year after EU accession. Hungarian policy encouraged and frequently even subsidized foreign entry, and it has experienced an unusual volume of foreign investment, so our data contain a large number of foreign-owned firms and more foreign acquisitions and divestitures than in previous studies. To control for ownership selection, we employ fixed effects for firms, detailed worker groups, and individuals (where workers can be linked inside firms), and to focus on acquisitions we construct matched samples using multiple years of pre-acquisition data. Point estimates are highly sensitive to specification, but in all cases the results point to positive effects of foreign ownership on wages of all worker types. The only significantly larger foreign differential concerns workers with university education. We consider possible explanations for the findings, including issues of selection and measurement, and possibility of higher productivity and rent-sharing. Preliminary results suggest that the higher wages paid by foreign-owned firms are fully accounted for by higher productivity.

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1. Introduction

Economic policy in many countries seems to display a certain ambivalence towards inward foreign direct investment (FDI). On the one hand, FDI is valued as a source of finance, technologies, and markets, and governments frequently compete for the favor of foreign investors, awarding them various subsidies, preferences, and tax abatements. On the other hand, most countries completely prohibit FDI in so-called "strategic" sectors – for instance, airlines and (until recently) banking in the US – and they often impose additional regulatory burdens and uncertainties that add to the inherently higher costs of sending capital and exerting control across national boundaries. In adjusting these policies towards FDI, a major issue concerns the effects of foreign acquisitions on workers, in particular on the wages of employees in the acquired companies.

This paper addresses the impact of FDI on the level and structure of wages in Hungary, an economy that rapidly reformed and liberalized inward investment during the 1990s. The data we analyze begin during the last years of central planning when foreign involvement was completely banned, they continue through the adoption of a very liberal regime in which, despite significant opposition from the population, the government awarded special treatment to many foreign investors, and they end in 2005, just after accession to the European Union. The result of liberalization was ownership transfer from domestic to foreign owners that took place not only quickly but also broadly across nearly all sectors. At the same time, the tightly controlled wages of the centrally planned systems were abruptly liberalized, permitting organizations to set their own wages and to increase skill differentials, which had tended to be compressed under socialism (e.g., Kornai, 1990).

Our analysis of the wage effects of FDI in Hungary builds on a considerable body of previous research. Early studies tended to have small samples and short time series, and they sometimes lacked a comparison group. An important and widely recognized problem is the need to account for ownership selection effects. If firms are not randomly selected into ownership types with respect to their wage policies (for instance due to "cherry picking"), and the researcher does not take this into account, the estimated causal effect will be biased. Indeed, some recent studies of firm-level data attack this problem directly: Conyon et al. (2002) employ firm fixed effects; Brown et al. (forthcoming) employ matching, firm fixed effects, and random trend methods; and Girma and Görg (2007) combine matching with difference-in-differences. These

studies, and most others using firm-level data, tend to find a wage premium in foreign firms, although identification with fixed effects in panel data frequently relies upon small numbers of ownership switchers.[1] Moreover, a further problem with analyses based on firm-level data is that they typically contain little or no data on individual worker wages and characteristics. If worker skills change simultaneously with a foreign acquisition, then an average wage differential may represent a change in employee composition rather than a change in employee compensation.[2]

As an alternative to firm-level data, several studies employ individual data that contain information on ownership of employers. Kertesi and Kollo (2002), for example, compare the returns to education between foreign and domestic firms in Hungary, and find a foreign wage premium. While the ability to control for measures of skills and other variables is a big advantage, a potential problem with these studies is possibly inaccurate measures of ownership, which are reported by household respondents who may not be fully informed about the progress of acquisitions. More importantly, worker-level data generally do not permit controls for firm selection into ownership type, nor do they usually include much employer information, which can be useful for disentangling the possible mechanisms underlying an FDI-wage correlation.[3]

The advantages of both firm- and worker-level data can be exploited only if one combines the two data types and uses linked employer-employee data (LEED), and recently there have been several such studies. Huttunen (2007) uses LEED for Finland, and finds positive foreign wage effects, which decreases with skill. Almeida (2007) finds that foreign ownership in Portugal has a modest positive effects on the wages of workers, and the effect is higher for the skilled group. Martins (2004) uses the same data source, but finds a negative effect. Andrews et al. (2007) find small positive, sometimes insignificant effects in Germany. Heyman et al. (2006, 2007) find very small, sometimes negative average wage effects in Sweden, but this masks positive effects for the high skilled, and negative effects for the low skilled workers. The only study of a transitional country is Earle and Telegdy (2008) for Hungary, who however focus only on ownership change

[1] A related area analyzes effects of all types of ownership change on wages: e.g., Lichtenberg and Siegel (1990) on LBOs, Gokhale et al. (1995), on hostile takeovers, McGuckin and Nguyen (2001) and Siegel et al. (2009) on mergers and acquisitions. Our data, however, contain information only on transitions between domestic and foreign ownership types, not on changes of ownership within these groups.

[2] Other firm-level studies include Feliciano and Lipsey (1999) on the US; Aitken, Harrison, and Lipsey (1996) on Mexico, Venezuela, and the US; Lipsey and Sjöholm (2004) on Indonesia; and Almeida (2007) on Portugal; the latter two studies include controls for the composition of workforce at the firm level.

[3] A possible identification approach in analyzing the effects of private-public wage differentials is to examine wage changes of workers who switch sectors. The approach here is instead to examine firms that switch sectors. For a survey of studies analyzing wage differences between the public and private sector, see Gregory and Borland (1999).

through privatization in which they show a foreign ownership effect on wages of 7-12 percent. Finally, Brazil is the single developing country where the effect of foreign ownership on wages was studied with LEED. Martins and Esteves (2008) find no effect of foreign ownership on wages.

All of these studies pose selection as the central issue in studying the wage effects of foreign ownership, and most also provide empirical evidence that selection does matter. To address this issue, many studies use panel data to carry out difference-in-differences calculations, and some employ propensity score matching techniques to select a control group with similar characteristics. These methods, however, are very demanding on the data, requiring a large number of treatments and long time series. Most studies, however, face problems of relatively few ownership changes, short time series, or both, so that the ownership effect may be identified only on a relatively small sample of firms observed in only briefly before and after the treatment.[4]

Our paper builds on and contributes to this literature in a number of ways. Our data contain over 900 changes in foreign ownership, mostly acquisitions, and the time series extends over a 20-year period. Our panel data contain detailed information on firm-level financial and performance measures, including firm-level wages, which we use to condition the selection of matched control firms over multiple years. By contrast, most studies use firm characteristics only in the year before acquisition to select matched controls, which ignores any dynamics, such as possible wage growth or improving productivity, over the pre-acquisition period. Our methods combine matching with panel regression methods, using the full longitudinal structure of the data rather than selecting individual pre- and post-acquisition years for comparison. This approach has the additional benefit of permitting us to estimate dynamics of the acquisition effects both prior to acquisition, which are useful for "pre-program" specification tests, and after acquisition, which are useful for assessing the short-term versus permanence of the effects.

While we follow much of the literature in controlling for firm fixed effects in many specifications, we also exploit the detailed information on worker characteristics to construct more detailed fixed effects at the firm-worker-group level, based on cells defined by gender, educational category, number of years of experience, and county in each firm (which produces

[4] The length of the panel is 5 years or less in most of these studies, and the number of ownership switches is typically between 100 and 300. Almeida (2007) has 1,193 switches but about 20 percent have no pre-switch observations, 30 percent have no post-observations, 50 percent have at most one pre- and 50 percent have at most one post-

about 400,000 cells with at least two worker observations in a total sample size of 2.4 million). Moreover, we have linked individual workers over time within firms, which permits us to use worker fixed effect to identify the impact of foreign acquisitions on incumbent workers at the time of takeover.

Using these methods, we find consistent evidence of a positive impact of FDI, albeit one that varies considerably across specifications. We then consider a variety of explanations for this finding, including measurement and selection issues and mechanisms working through enhanced productivity, rent-sharing, and personnel policies. For addressing these issues, we rely on detailed information on both the worker side, where we can estimate the impact of FDI on workers by skill and other characteristics, and the firm side, where we can examine changes in firm performance.

The next section describes the construction of our database. In Section 3 we briefly explain the evolution of the ownership structure during the period studied and provide summary statistics for wages and worker characteristics. Section 4 describes the estimation procedure, and Section 5 presents the results. Section 6 concludes with a summary and suggested directions for further research.

2. Data Sources and Sample Selection

We study a linked employer-employee dataset that relies on two data sources. The Hungarian Wage Survey, hosted by the National Employment Office, gathered information on workers' earnings and characteristics every three years between 1986 and 1992, and on an annual basis since then. The last round we use in this analysis is 2005 so we have information on firms and their employees starting as early as 1986, well before the transition started, until 2005, one year after the country's accession to the European Union. In 1986 and 1989 the survey covered all firms, then in 1992-1995 each firm with more than 20 employees. From 1996, a random sample of smaller firms was added, including employers with 11-20 employees between 1996 and 1999, and those with 5-20 employees for the 2000-2005 period.

In 1986 and 1989, workers were selected from narrowly defined occupational and earnings groups within firms, using a systematic random design with a fixed interval of selection.

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observation. Andrews et al. (2006) also have a large number of foreign takeovers and divestments, but the data set

High-ranking managers were exempt from this rule and were surveyed comprehensively. In 1992 the sample design changed and was based on the day of birth of workers. Production workers were selected if born on the 5th or 15th of any month, while non-production workers were chosen if born on the 5th, 15th, or 25th of any month. Therefore, even though the target group of the survey was the population of firms above 20 employees, if a firm did not have an employee born on the given days in a particular year, the firm is missing from the data.

Starting with 1996, the survey scheme of workers was maintained for medium and large firms, but all employees' information were required from smaller companies. The size threshold below which all workers were sampled was 20 employees between 1996 and 2001, and 50 since 2002.

For most firms, this selection procedure oversamples non-production workers, providing a random sample of workers of about 6.6 percent of production workers and 10 percent of non-production workers. We use information on the numbers of production and non-production workers in the firm to weight the within-firm samples and adjust for the oversampling of non-production workers. In addition to this problem, a firm also drops out from the sample if none of its employees were born on the relevant dates, and the probability of being included therefore increases with employment size of the firm.[5] To overcome this problem, we also construct a firm weight which varies by firm size and adjusts the sample to the total number of employees in the relevant sectors of the Hungarian economy.

The Wage Survey data include some firm characteristics, which we use to link the worker data to firm-level data. These data contain each double-entry bookkeeping firm between 1992 and 2005, and they include the balance sheet and income statement, the proportion of share capital held by different types of owners, and some basic variables, such as employment, location and industrial branch of the firm. For the two early years – 1986 and 1989 – these universal data do not exist, and for these years we used firm information provided with the worker sample.

We cleaned firm ownership data extensively, checking for miscoding and dubious changes. We also cleaned the longitudinal linkages of the data extensively. For this procedure we used a dataset by the Hungarian Statistical Office that provides information on reregistration and boundary changes, which result in spurious entry and exit of firms from our data. As this

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contains information for only two years.

dataset is not comprehensive, we also tried to find false entries and exits by looking for matches of exits among the entries on the basis of headquarter settlement, industry, sales, and employment. While we know that spurious entry and exit is still a potential source of bias in our data, we believe we decreased the magnitude of this problem to a large extent. Finally, since our data span two decades during which radical change took place in the Hungarian political and economic system, we paid a great deal of attention to harmonizing variable definitions and classifications over time.[6]

From the resulting dataset we selected those firms which were above the respective size threshold of sample inclusion in a particular year. Companies that went through more than two changes in majority ownership during their presence in the dataset even after extensive cleaning were dropped from the data (14 companies). We restrict our attention to full-time employees only, and workers below the age of 15 and above the age of 74 are excluded from the analysis.

Our final selection rule is to drop firms from those two-digit industries where there were no foreign acquisitions or divestments.[7] After further minor decreases due to missing values, the resulting data is a large firm panel of 30,038 firms, linked with a within-firm random sample of 2.4 million workers. Table 1 shows the number of workers with full information on individual characteristics, the number of firms with information on ownership and location as well as the total employment of the relevant corporate sector. Except for the first two observed years, in each year the number of workers in our sample is between 79 and 131 thousand, making a random sample of 3,674 firms in 1986, 7,535 in 1997, and 6,411 in 2005. Our sample represents a total employment of over 3.5 million in the first year of the analysis, and around 2 million in the last year.

3. Estimation Procedures

We follow the broader literature on the effects of ownership in estimating reduced form equations, while trying to account for potential problems of heterogeneity and simultaneity bias (Djankov and Murrell, 2002; Megginson and Netter, 2001). We are able to exploit the rich set of worker characteristics as well as the longitudinal structure of the data in order to estimate panel

[5] For example, a firm with 20 production workers will have a probability of 0.11 to be excluded from the sample, while for a similar firm with 100 employees this probability is only 0.012.
[6] We thank the help of the Databank from the Institute of Economics of the Hungarian Academy of Sciences.
[7] The following 2-digit NACE industries were excluded from the analysis: 5, 12, 13, 30, 41, 62, 67, 80, 85, 91, 93.

regressions with several types of fixed effects and to construct matched samples that include a set of control firms similar to those acquired by foreigners. We combine matching and regressions in a number of specifications, and to focus on the impact of FDI on incumbent workers we employ worker fixed effects in a subsample where workers can be linked.

Our first estimating equation describes the foreign differential controlling only for region and year:

$$lnw_{ijt} = \alpha + \delta_f FOREIGN_{jt-1} + \Sigma\gamma_j REGION_j + \Sigma\lambda_t YEAR_t + u_{ijt}, \quad (1)$$

where i indexes workers, j indexes firms and t indexes time. Lnw_i is the natural logarithm of the wage, and we control in each specification for 20 year effects (*YEAR*) and 6 regional effects (*REGION*) – the latter defined at the plant level.[8] *FOREIGN* indicates whether the firm is controlled by foreign owners. As ownership is measured at the end of each year, our ownership variable is lagged. In a set of specifications we disaggregate *FOREIGN* into acquisitions and divestments to allow for non-symmetric treatment of ownership switches involving foreign presence. Observations are weighted, as described in Section 2, so that results are representative of Hungarian tax-paying, double-bookkeeping entities.

Next, we augment this simple equation (1) with standard human capital variables and a gender dummy, to account for the composition of employment (which may be correlated with ownership):

$$lnw_{ijt} = \alpha + \boldsymbol{X}_{it}\boldsymbol{\beta} + \delta_f FOREIGN_{jt-1} + \Sigma\gamma REGION_j + \Sigma\lambda YEAR_t + u_{ijt}. \quad (2)$$

$\boldsymbol{X}_i$ is a vector of individual characteristics including three educational dummies (*VOCATIONAL, HIGH SCHOOL*, and *UNIVERSITY*, the omitted category is 8 or less years of schooling), (potential) *EXPERIENCE* and its square, and a dummy variable indicating whether the worker is female or not (*FEMALE*). As education and experience may be correlated, and gender may influence both, in a third specification we add to equation (2) a full set of interactions among these variables.

In the fourth specification, we add a dummy variable indicating that the employee is manager and whether the worker is newly hired in the past year, to control more precisely for the

[8] An alternative control for non-random shocks would be to interact region and year in order to permit these shocks to vary over both dimensions simultaneously. In our data, this would imply adding 380 dummies instead of 19, which would make some of our computations very time consuming. Nevertheless, we ran most of our specifications including the full region-year interactions, and the results were little changed.

composition of the workforce. Finally, we also control for the industrial composition of firms to account for industrial wage differentials.

We fit this equation under alternative assumptions about the error term, u_{ijt}, first assuming that it is uncorrelated with each right-hand-side variable. We use this simple OLS as a benchmark for examining other specifications that can more seriously claim to estimate causal effects, under alternative assumptions about firm and worker heterogeneity.[9]

A serious problem that contaminates studies of ownership change is non-random selection of firms into ownership type. The owners of the acquiring firms are likely to select targets that have better growth prospects or a more skilled workforce, for example. If this aspect of the firm is not observed for the researcher, the estimated effect of ownership on wages will be biased if they are estimated by OLS.

We try to account for selection bias in several ways. First, we add firm fixed effects to the regression to control for all unobserved time invariant effects at the firm level. The data contain sufficient within-firm variation in ownership to allow the foreign effect to be identified: Table 2 shows numbers of firm-year observations for acquisitions and divestments as well as for always foreign and always domestic firms in the regression sample.

Unobserved heterogeneity may vary not only at the firm level but also within groups of workers in the same firm, in another specification we interact the firm fixed effect with narrowly defined groups of workers. We construct groups of workers defined by gender, four education categories, and years of experience. We also distinguish workers by county (which is defined at the plant level) and the resulting grouping is interacted with firm identifiers. In this specification therefore we allow a different intercept for each education-gender-experience-county group within each firm.

The universe of domestically owned firms may be very different from those which were acquired by foreign investors. To construct a control group that is similar to treated firms before the treatment took place, we do propensity score matching of firms before running the regressions. First, we restrict the sample to those treated firms that exist at least two years before and two after the treatment, and we drop firms that are always foreign-owned, as they are obviously not at risk

[9] We report all standard errors permitting general within-firm correlation of residuals using Arellano's (1987) clustering method. The standard errors of all our test statistics are robust to both serial correlation and heteroskedasticity. See Kézdi (2003) for a detailed analysis of autocorrelation and the robust cluster estimator in panel data models.

of being acquired. When we match, we also use only those domestic firms that satisfy this criterion for a given treatment year. To obtain the propensity score, we run a pooled probit on all years included in the data, with the dependent variable equal to one if the firm was acquired in that year by a foreign investor and zero otherwise. We prefer this to estimating separate probits each year because the relatively small number of treated firms make the estimation of the propensity score less robust than in the case of a pooled regression. In order to account for the differential proportions of treated firms across years, we construct the ratio of treated firms in a particular year to the total number of treated firms, and we use this proportion to weight the untreated firms in the probit regression (the treated firms receive a weight of one). For example, if there were relatively few treatments in a year, all observations from that year will contribute to the propensity score to a lesser extent than those from a year in which many treatments took place.[10] The independent variables include the lagged and twice lagged values of firm characteristics, among them the dependent variable, to match not only on observable, but also on unobservable characteristics. To allow for a flexible relation between the probability of being acquired and the firm characteristics pre-treatment, we include all continuous variable in levels and squared as well. More specifically, the set of variables used for matching is the following: log employment, log average wage (total personal expenditures/employment), log sales, log capital intensity (tangible assets/employment), and the first lag of log export share (export sales/total sales).[11]

Having obtained the propensity score, we match on nearest neighbors with replacement, matching exactly by year and industry. We pair each treated firm with a domestic firm that will never become foreign with the smallest distance measured by the propensity score. We allow this firm to become a control in the second stage if the difference between the two propensity scores is smaller than 10 percent of the treated firm's propensity score. If there are multiple firms in that range, we take the closest one and all domestic firms whose propensity score is less than 10 percent different from the closest firm's. One firm may become a control several times either because it is chosen for several treated firms, or because it is chosen in different years. [State

[10] This method is similar to that used by Chari et al. (2009), but they drop the sample proportionally to the proportion of treated firms in each year. Instead of dropping, we rather keep all the potentially treated firms in the sample and reweight them.

[11] In order not to lose firms because one variable is missing for a year, we dummy out the missing values.

numbers of duplicate controls, number of extra controls, number of non-matched treated firms, etc.]

4. Ownership Evolution, Variable Definitions, and Summary Statistics

Hungary got off to an early start in corporate control changes with gradual decentralization and increased autonomy provided to state-owned enterprises during the late 1980s. The first foreign acquisition took place already in 1989, when General Electric bought the majority of shares in the lighting company Tungsram. In the early 1990s, not only were constraints on foreign investment drastically eased, but tax and other preferences for foreign investors were also provided. By the mid-1990s, Hungary had the highest value of foreign direct investment per capita among the post-socialist countries (King and Varadi, 2002).

Our database provides the ownership shares of domestic and foreign owners at the end of each year (the reporting date). We define a firm to be foreign controlled if a majority of its shares are in foreign hands [because foreigners rarely take minority stakes, this classification is little altered by other thresholds – e.g., 10 percent]. The evolution of the ownership structure among the firms in our sample is presented in Figure 1, clearly reflecting the early start and the heavy presence of foreign ownership in Hungarian corporate ownership, at least compared to other former socialist countries. Already in 1992, the first post-transition year observed in these data, foreigners owned about 5 percent of all firms in our data, and their holdings increased to above ten percent by the next year. By 2005, the last year in our data, 20 percent of the companies in our sample was foreign-owned. These firms tend to be larger than the average Hungarian firm, so the proportion of employees working for foreign owners was about twice as high, about 40 percent.

The wage variable we focus on consists of the sum of the base wage, overtime pay, regular payments other than the base wage (such as language and managerial allowances), and $1/12^{th}$ of the previous year's irregular payments (such as end-of-year bonuses). If the worker was hired during the previous year, we divide this wage component by the number of months the worker spent with the company that year. Figure 1 also shows the U-shaped evolution of the average real wage over this period: an initial decline of around 30 percent and subsequent rise of

about 40 percent.[12] The steady, substantial growth in the Hungarian real wage since 1997 is unusual among the transition economies. The reliability of the real wage measure is of course strongly influenced by the quality of the deflator (in this case, the CPI), and the large changes in quality and availability of goods suggest caution should be exercised when interpreting these figures. When we estimate wage differences by ownership, however, we control for time effects, so our comparisons are not influenced by these measurement problems.

Table 3 presents the average wage and characteristics of workers by the type of owner for the whole database. Workers employed in domestic firms receive lower wages than those working in foreign-owned companies, and the difference is quite large: about 53 percent. But the average characteristics of workers also differ substantially by ownership type. The proportion female is larger in foreign firms by 6.5 percentage points. Workers with only elementary education are 10 percentage points less common, and those with university studies are 7 percentage points much more prevalent in foreign firms. The average worker in domestic firms has 22.6 years of potential work experience (age-imputed years of schooling-6), larger by almost three years than the potential experience of the typical employee of a foreign company. More workers at foreign firms have been newly hired in the previous year: 15.2 percent in foreign-owned and 10.8 percent in domestic. But somewhat fewer workers at foreign firms are managers. To sum up, workers in foreign companies tend to be more educated, female, and less experienced, and they are less likely to be managers, more likely to be new hires, and have higher wages than in state-owned companies. Of course, these are unconditional means that take no account of any other characteristics of foreign and domestic companies, but they are suggestive of the underlying heterogeneity in the population.

Some firm characteristics by ownership type, again unconditional means over the entire sample, are displayed in Table 4. Measured by the value of tangible assets (in 2005 HUF), foreign firms are 4 times larger than domestic firms on average, and their employment (measured by the yearly average number of employees) is twice as large; together these imply capital-labor intensity again twice as large. Labor productivity (measured by the ratio of real value of sales to the average number of employees) is three times as high, and the probability of exporting is four times. The industrial composition of foreign and domestic firms also differ. Relative to domestic

[12] To maintain comparability over time, the evolution of the average real wage is estimated from the year effects in a ln(real wage) equation that controls for firm fixed effects.

firms, foreign-owned firms predominate in industry, 43 compared to 26 percent, and finance, insurance and real estate (FIRE), 8 compared to 4 percent. They are less prevalent in agriculture, construction and other services. In summary, foreign-owned firms are larger, more capital-intensive, more productive, and more export-oriented than domestic firms, and they are concentrated in the industrial and FIRE sectors.

By contrast with these stark differences in domestic and foreign firms in the full sample are the tests of covariate balance in the matched samples of acquisitions, shown in Table 5. For each variable in both the year prior to acquisition and two years prior to acquisition, the normalized difference between the acquired firms and the controls is small and statistically insignificant.

5. The Effect of Foreign Ownership on Average Wages

We first discuss the results of the benchmark estimation, the pooled OLS controlling only for region and year effects in addition to the variables shown. The first column of Table 6 includes no extra controls and shows that the wages of workers employed in foreign firms is 37.1 percent higher than in domestic firms. Controlling for worker gender, education and potential experience does not change the estimated coefficient at all. The inclusion of interaction terms between worker characteristics also leaves the estimated wage coefficient unchanged (interactions are demeaned, so the main effects are measured for the average worker). When we add controls for workers in managerial position and new hires, the foreign wage effect increases by one percentage point. Finally, the inclusion of two-digit industry effects reduces the foreign wage effect by only 2 percentage points.

It is notable that the estimated wage effects of worker characteristics are always highly statistically significant and are in the conventional range. The gender wage gap is 0.17 – 0.22. Compared to workers with elementary education, the wage premium associated with vocational studies is measured to be 0.9 – 0.12, high school premium 0.26 – 0.38, and the wage difference associated with a university degree 0.73 – 0.95, depending on the controls used. The first year of potential experience increases wages by 2 – 2.5 percent on average, and the starting wage of employees is 0.9 – 0.11 less than average wages of workers with more than one year of job tenure. Managers earn 0.35 – 0.40 on average more than ordinary employees.

In Table 7, we show the estimated foreign wage premium for specifications (1) – (4) from Table 6, with further attention to the possibility os selection bias in the selection of target firms by future foreign owners. When we add firm fixed effects (FE) to the OLS specification, the foreign wage effect drops by more than half to 15 – 17 percent, showing that selection on fixed firm heterogeneity is an important phenomenon in foreign acquisitions and divestments. The inclusion of worker group effects interacted with firm effects (GFE) results in a further decline of about 5 percentage points. When we combine matching with linear estimation methods the results further decrease, but nonetheless they remain positive, sizable, and statistically highly significant. The estimated coefficients are around 9 – 10 percent, regardless of the estimation method applied in the second stage (OLS, FE, or GFE).[13]

The analysis so far does not distinguish greenfield FDI from acquisitions and divestitures, although only the latter switches identify the estimated effects in specifications with firm FE (or firm-worker GFE). The implicit assumption is that the foreign wage effect is symmetric in both directions, but an interesting question is whether this assumption is correct. To allow the foreign effect to vary by the type of ownership change, we disaggregate the foreign ownership variable into an acquisition and a divestment dummy (both being equal to one when the firm is owned by foreigners). The results, presented in Table 8, show very different effects of foreign acquisitions and divestments on the wages of workers. The effects of acquisitions are very similar to the main effect presented in the previous table, while we do not find much evidence that divestments have any effect on wages, as the estimated coefficients are small, insignificant, positive in some, and negative in other specifications. The sole exception is the GFE specification, where the estimated coefficient is 0.055 and significant at the five-percent level. But this effect disappears when controls for new hire and manager are included, and when GFE is combined with matching.

To summarize our results, we find that foreign owners increase wages in acquired firms, but divestment does not appear to result in a wage decline. The foreign acquisition effect does not vary much with the set of controls, but it does with the selection method applied. When we use the matched sample, the results do not vary much across regression methods, and the

[13] To test whether the matched sample is balanced we compute normalized differences in means of the matching variables between treated firms and their assigned pairs. All differences are small, less than 0.1 standard deviations. Also, standard t-tests accept the hypothesis of equal sample means at any conventional level.

magnitude of the coefficient is around 8 – 10 percent. This estimated effect is larger than found in other studies.

Next we analyze dynamics of the foreign acquisition effect. Specifications where we permit the FDI effect to vary by event (acquisition) time are presented in Figures 2 and 3. The OLS dynamics show that the estimated foreign effect is large even before the acquisition happened and it is constantly increasing. The addition of FEs decreases all coefficients in magnitude, and the pre-treatment difference between treated and non-treated firms four years before acquisition becomes statistically insignificant. The inclusion of GFE further reduces the coefficients and the pre-acquisition effects all become insignificant except one year before treatment, while the increasing post-acquisition profile is unchanged. Matching on pre-treatment firm characteristics results in a total loss of significance in the estimated pre-acquisition effects regardless of the regression method, while the post-acquisition profile remains positive and upward-trending. Therefore, the dynamics provide further evidence that selection is an important issue in foreign acquisitions, and that matching combined with regression help control for unobserved heterogeneity. The foreign acquisition effect is estimated to be positive and steadily increasing, even after 4 years.

Another specification adds fixed firm-worker effects, dummy variables for individual workers who we can track within firms. The foreign effects in this specification are identified from workers who are observed at a particular employer both before and after an ownership change, and thus they refer to the wage effect of foreign acquisitions (and divestments) on incumbents. Results are displayed in Table 9. Even for incumbents under this very demanding specification, and regardless of whether we control for other individual and job characteristics, the estimated effects of acquisitions are positive and statistically significant. And again the effects of foreign ownership in firms that will be divested are smaller and statistically insignificant.

Finally, we investigate whether the foreign premium varies across different types of workers. Table 10 contains the results from adding interactions of the foreign acquisition to dummy to specification (4) in Table 7. The results imply that all types of workers – defined by gender, education, experience, new hire status, and managerial or nonmanagerial occupation – receive higher wages under foreign ownership. FDI is estimated to raise the college premium in

all specifications, and the high school and managerial differentials in some specifications, but there is little difference associated with new hire status or gender.

6. Possible Explanations (preliminary)

All the evidence suggests that foreign ownership is associated with higher wages, even when we control for various forms of ownership selection and firm and worker heterogeneity. Why should foreign-owned firms pay more? Several possible explanations exist, including the measurement of wages and other job attributes, selection at the firm and worker level, productivity changes, and rent sharing. In this section we discuss each of these issues and try to test them empirically.

To start with the measurement of wages, one possible critique is that domestic firms are more likely to report lower wages to avoid taxes. The tax burden on work in Hungary is high and tax avoidance is widely considered rife. Our results may be contaminated by underreporting of wages if the proportion of this is different across ownership types. To check whether domestic firms are indeed more likely to avoid taxes than foreign-owned enterprises, we carry out several exercises, shown in Table 11. First, it is likely that underreporting is inversely proportional to the size of the company; it is harder to imagine that in a large corporation workers are paid under the table than in small firms, where managers and workers are working closely together and can trust more that this practice will not be reported to the tax authority. If our results are driven by the underreporting of domestic companies, the foreign wage premium should increase with firm size. We interact employment size with the foreign dummy to test for this possibility, and receive a positive, significant coefficient on this interaction. Second, we interact the foreign dummy with a cheating index which is defined at the industry level and shows the likeliness of cheating (Elek et al., 2008). Our results show that in industries where underreporting is less likely, the foreign wage difference is larger than in cheating industries. Both of these set of results seem to reject the hypothesis of domestic firms being less honest in terms of reporting true earnings, although they are also consistent with other differences across size and industry categories in how foreign firms operate.

As a third check, we apply specification (2), but replace wages with a dependent variable indicating whether the worker was paid very close to the minimum wage that year (defined as

being paid less than 3 percent more than the minimum wage). We find that a lower proportion of workers were paid the minimum wage in foreign-owned companies, and the estimated coefficient is significantly different from zero. This result may suggest more misreporting in domestic firms, but the magnitude of the coefficient is rather small, 2-5 percent. As only about 10 percent of workers receive the minimum wage in our sample, this wage differential cannot explain the 9 to 15 percentage points foreign wage premium.[14]

Another potential measurement problem is related to the wage variable itself. As common in many European studies, we use monthly, instead of hourly wage. If hours changed as a result of the ownership change, it is possible that workers get the same wage and the increased wages are explained by the longer hours the new foreign owner requires them to work. We have information on hours for a limited set of years (between 1999 and 2005), and we perform several checks on these years. First we put hours as a dependent variable and find an insignificant foreign coefficient. Second, we compute an hourly wage by dividing the monthly wage by the number of monthly working hours, and use this as the dependent variable. Third, we use the original specification, but we control for hours. The results do not change.

Another major issue concerns the composition of firms and workers. Although our results in the previous section control for observable characteristics of workers as well as selection of firms by unobservables, differential firm exit by ownership and unobserved worker heterogeneity may contaminate our results. To check – at least indirectly – for the effect of firm exit, we run exit regressions to check whether the exit rate differs by firm ownership [not done yet). To control for worker unobserved effects, we link those workers across years that do not change their workplace, and control for these firm-worker fixed effects in the wage regression. These controls reduce the estimated coefficient of foreign ownership, but it nevertheless remains significant and as large as 5 percent (see Table 9). It should be noted that this specification estimates the wage effect only for those workers who were employed at the firm before and after the foreign acquisition took place and does not take into account the effect on new hires.

We also investigate how the composition of workers changed after the foreigners took over the company. We run several linear probability models with the dependent variable indicating the worker's gender, her highest degree of education, her category of potential experience and whether she is in a managerial position. By adding firm fixed effects to the

[14] This result can also be interpreted as another piece of evidence for the foreign wage premium.

regression, we can check how these worker characteristics changed when the company was taken over by a foreign owner. We find only small foreign effects, except for experience, where we find that the foreign workforce gets younger. When we do the same for the new hire variable, we find that younger workers are more likely to be hired by foreigners.

Another possible cause of wage increases is the increased productivity of the firm. To test this, we run a seemingly unrelated regression model at the firm level, where the dependent variables are labor productivity and average compensation. By comparing the magnitudes and testing the difference of the two estimated coefficients of the foreign dummy, we can draw conclusions about the similarity of the productivity and wage effects. Table 12 contains the results, which show rather similar effects of 0.12 and 0.10 on labor productivity and wages, respectively. The formal test rejects equality, but that is not unexpected with such a large sample. The correlation of the residuals from the two equations is quite high (0.47), although again the formal test of equality (Breusch-Pagan) rejects. Specification (2) shows that the productivity effect of acquisitions exceeds the wage effect, while the reverse is true for the foreign effect associated with divestments: after divestment, the new domestic owners tend to reduce wages but maintain productivity (or even increase it, according to the point estimate).

An extended version of this approach examines the foreign impact on the skill differential. In the spirit of Hellerstein, Neumark, and Troske (1999), we include capital, materials, and skilled and unskilled labor (where skill is defined as university-educated) in both the productivity and wage equations and we interact the foreign variable with the two types of labor. The results are shown in Table 13. The main foreign effects are larger than in Table 12: 0.17 in the production function and 0.13 in the wage equation. The coefficients on high and low skill groups are not very similar for domestic firms but much more so for foreign. This provides some suggestive evidence that foreign owners award skilled workers more highly because such workers are in fact more productive at foreign firms.

One possibility suggested in previous research is that foreign owners provide more on the job training to their employees than domestic owners, and the increased productivity is transformed into higher wages. [to be added].

Finally, rent sharing may also be of different magnitude among domestic and foreign-owned firms. To test this, we add a union dummy to the regressions. The results do not change.

7. Conclusions

[To be added]

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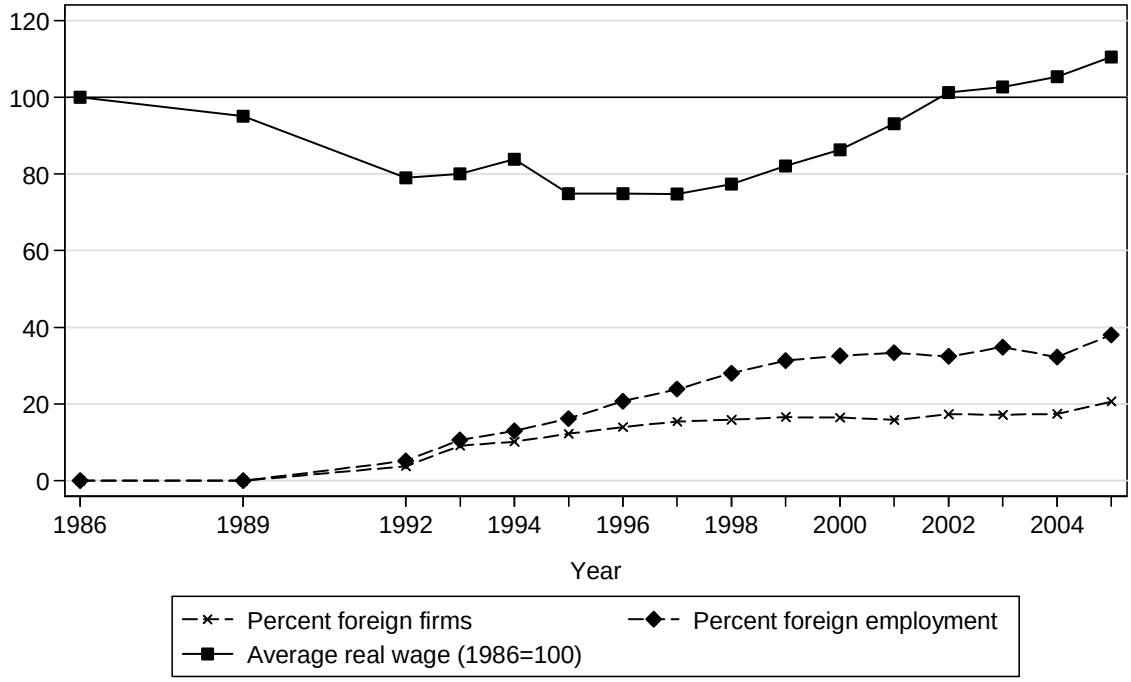
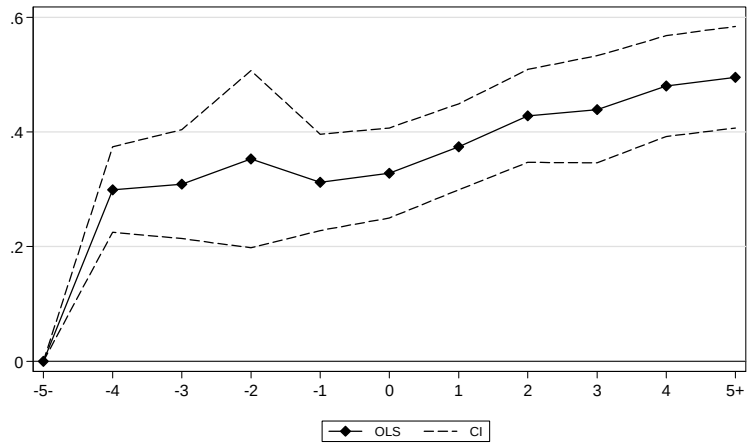
Tables and Figures

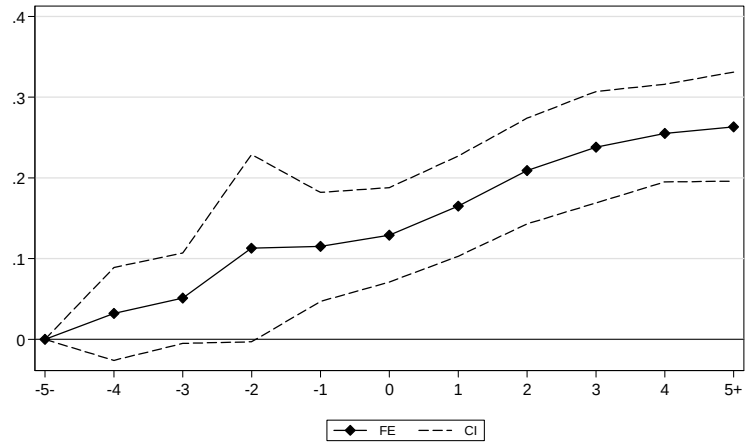
Figure 1: Evolution of Foreign Ownership and Average Real Earnings



Notes: Number of observations for the ownership variables: 29,176. Percent foreign firms = percent of firms majority foreign owned. Percent foreign employment = percentage of employees employed by foreign owned firms. The evolution of average real earnings is presented as estimated year effects from a regression including firm fixed effects to control for sample changes (dependent variable = log real earnings, normalized at 100 in 1986).

Figure 2: Dynamics of the Foreign Acquisition Effect
Full Sample Results





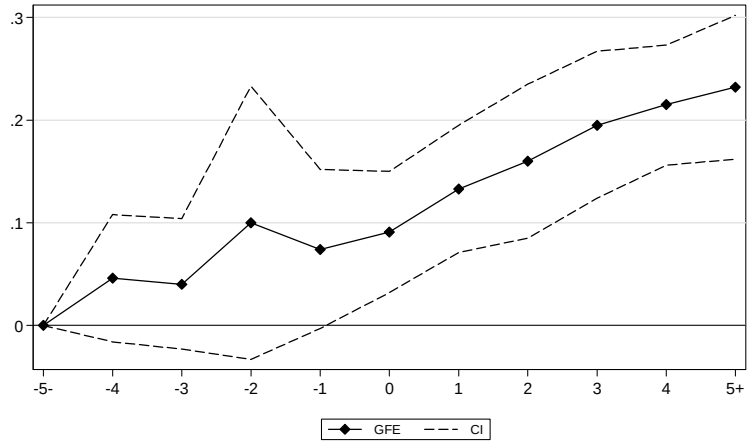
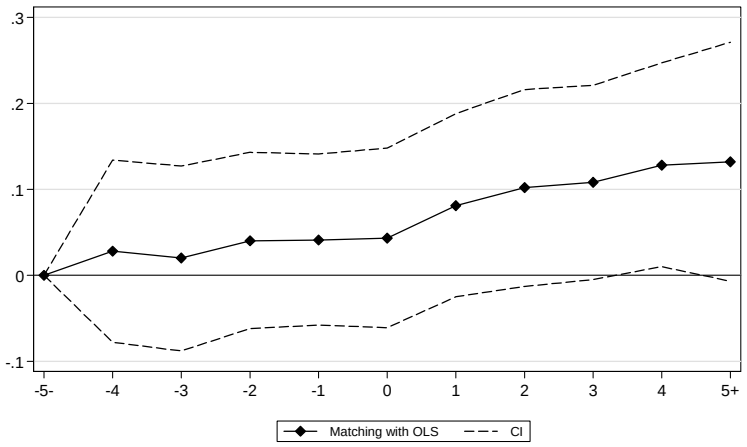
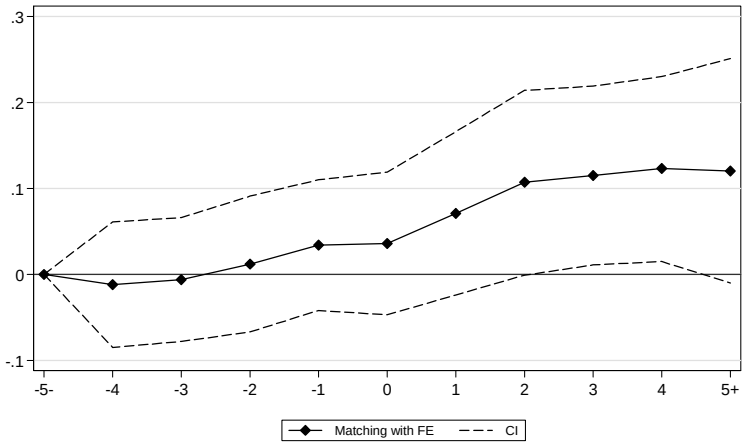


Figure 3: Dynamics of the Foreign Acquisition Effect
Matched Sample Results





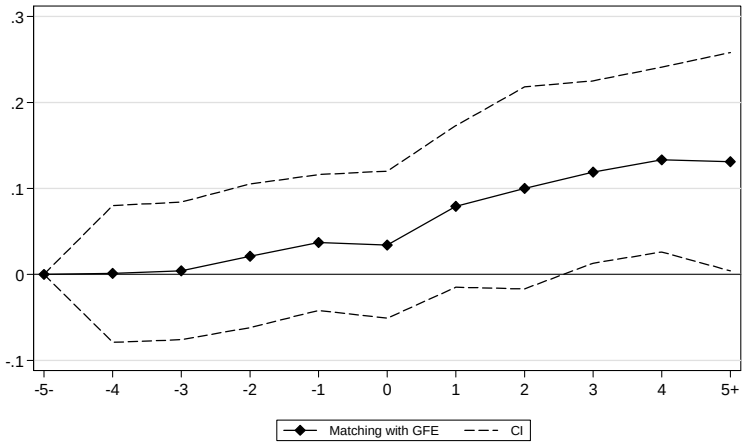


Table 1: Sample Size by Year

	Unweighted		Weighted
Year	Workers Observed	Firms Observed	Total Employment
1986	527.6	3,596	3,580.9
1989	355.6	3,956	3,530.0
1992	76.6	5,107	1,912.8
1993	80.8	5,808	1,732.8
1994	98.2	7,568	1,697.0
1995	100.4	7,720	1,635.3
1996	97.2	7,309	1,625.0
1997	96.7	7,318	1,633.8
1998	97.7	7,239	1,673.9
1999	99.2	7,802	1,671.0
2000	116.8	9,984	1,820.3
2001	118.3	10,641	1,835.9
2002	116.8	7,446	1,856.0
2003	115.0	7,155	1,951.5
2004	125.5	7,923	1,961.4
2005	109.3	6,103	1,983.0

Note: Workers observed = thousands of workers in the sample with information on earnings, education, experience, and gender. Firms observed = number of firms with information on ownership and location, and with at least one worker in the given year with information on earnings, education, experience, and gender. Total employment = total employment in thousands, as represented by the sum of weights in the sample (approximating total employment in the corporate sector).

Table 2: Number of Observations in Regressions by Ownership type

	Number of Firms	Number of Firm-Years			
		Total		Average	
Always Foreign	2,830	11,713		4.14	
Always Domestic	25,534	94,349		3.70	
		Total Before	Total After	Average Before	Average After
Foreign Acquisition	592	1,920	3,384	3.25	5.72
Foreign Divestment	311	501	1,555	1.61	5.00

Table 3: Individual Characteristics by Ownership Type

	Domestic	Foreign
Earnings	115.9	177.5
	(101.3)	(188.7)
Female	38.5	45.0
Education		
Elementary	29.1	19.4
Vocational	32.8	31.9
High school	29.4	33.0
University	8.7	15.7
Experience	22.6	19.8
	(11.0)	(10.9)
New Hire	10.8	15.2
Manager	10.2	8.5
N	2,019,957	311,649

Notes: Weighted results. Earnings measured in thousands of 2005 HUF, deflated by CPI. Female, education, new hire and manager measured as percentages of total workforce. Standard deviations in parentheses.

Table 4: Firm Characteristics by Ownership Type

	Domestic	Foreign
Tangible Assets	284 (7,564.0)	1,152 (11,262.0)
Employment	54 (574)	111 (443)
Labor Productivity	20.2 (213.8)	63.4 (274.2)
Exporter	0.12	0.48
Export Share	0.40 (0.30)	0.60 (0.34)
Industry		
Agriculture	8.4	1.9
Industry	25.7	43.4
Construction	10.6	2.2
Trade	28.7	27.0
FIRE	4.2	8.1
Business Services	9.2	8.8
Other Services	13.0	8.5
N	97,147	15,524

Notes: Tangible assets and labor productivity measured in millions of 2005 HUF. Standard deviations in parentheses. Export share defined as the ratio of export sales to total sales. Industrial distribution measured as percentages within ownership type. A firm is considered to be an exporter if its export share is larger than 5% in a given year.

Table 5: Tests of Covariate Balance

	Normalized Difference Treated-Controls	Probability of Rejecting Inequality of Means
One Year Before Acquisition		
Average Earnings	0.042	0.473
Sales	0.058	0.317
Employment	0.068	0.247
Capital	0.016	0.787
Export Share	0.003	0.954
Two Years Before Acquisition		
Average Earnings	0.036	0.520
Sales	0.053	0.362
Employment	0.058	0.312
Capital	-0.060	0.291

Table 6: The Effect of Foreign Ownership on Wages
OLS Estimation

	(1)	(2)	(3)	(4)	(5)
Foreign	0.371**	0.371**	0.373**	0.385**	0.354**
	(0.023)	(0.017)	(0.017)	(0.016)	(0.011)
Individual Characteristics					
Female		-0.218**	-0.218**	-0.202**	-0.171**
		(0.007)	(0.007)	(0.007)	(0.004)
Vocational		0.118**	0.103**	0.092**	0.104**
		(0.004)	(0.005)	(0.005)	(0.004)
High school		0.378**	0.372**	0.312**	0.263**
		(0.009)	(0.009)	(0.009)	(0.006)
University		0.951**	0.944**	0.792**	0.731**
		(0.015)	(0.014)	(0.015)	(0.009)
Experience		0.025**	0.025**	0.021**	0.019**
		(0.001)	(0.001)	(0.001)	(0.001)
$Experience^2 * 100$		-0.037**	-0.036**	-0.031**	-0.027**
		(0.001)	(0.001)	(0.001)	(0.001)
Job Characteristics					
New Hire				-0.114**	-0.093**
				(0.007)	(0.004)
Manager				0.353**	0.399**
				(0.008)	(0.008)
Interactions between gender, education and experience	No	No	Yes	Yes	Yes
Industry effects	No	No	No	No	Yes
R^2	0.124	0.369	0.372	0.401	0.460

Notes: N = 2,331,606 for specifications (1)-(4) and N = 2,331,605 for specification (5). Dependent variable = ln(real gross earnings). *Foreign* = 1 if the firm is majority foreign owned in *t-1*. All equations include year and region effects. Standard errors (corrected for firm clustering) are shown in parentheses. ** = significant at 0.01; * = significant at 0.05.

Table 7: The Effect of Foreign Ownership on Wages
Estimations with Correction for Selection Bias

		(1)	(2)	(3)	(4)
Firm FE	Foreign	0.172**	0.147**	0.147**	0.153**
		(0.018)	(0.016)	(0.016)	(0.015)
	R^2	0.066	0.331	0.334	0.401
Group Effects	Foreign	0.124**			0.130**
		(0.018)			(0.017)
	R^2	0.071			0.157
Matching and OLS	Foreign	0.085	0.103**	0.104**	0.102**
		(0.045)	(0.031)	(0.031)	(0.032)
	R^2	0.087	0.416	0.420	0.468
Matching and FE	Foreign	0.106**	0.098**	0.098**	0.096**
		(0.021)	(0.018)	(0.018)	(0.019)
	R^2	0.083	0.400	0.406	0.481
Matching and GE	Foreign	0.093**			0.092**
		(0.018)			(0.019)
	R^2	0.068			0.181
Individual Characteristics		No	Yes	Yes	Yes
Job Characteristics		No	No	No	Yes

Notes: See Table 6. The structure of covariates is the same as in Table 6, except for group effects, where specifications (2) and (3) are missing. For the matched sample, N=245,382 for specifications (1)-(4). Group effects control for unobserved heterogeneity by grouping workers within the same employer along the lines of gender, education, experience and county. Within R^2 reported for firm and group effects.

Table 8: The Effect of Foreign Acquisition and Divestment on Wages

		(1)	(2)	(3)	(4)
OLS	Acquisition	0.360**	0.337**	0.337**	0.343**
		(0.033)	(0.020)	(0.020)	(0.021)
	Divestment	-0.022	-0.013	-0.013	-0.012
		(0.043)	(0.028)	(0.028)	(0.028)
	R^2	0.102	0.345	0.347	0.374
Firm FE	Acquisition	0.172**	0.146**	0.147**	0.154**
		(0.021)	(0.018)	(0.018)	(0.017)
	Divestment	0.025	0.024	0.023	0.019
		(0.028)	(0.028)	(0.028)	(0.029)
	R^2	0.066	0.331	0.334	0.401
Group Effects	Acquisition	0.113**			0.120**
		(0.020)			(0.019)
	Divestment	0.055*			0.052
		(0.028)			(0.029)
	R^2	0.071			0.157
Matching with OLS	Acquisition	0.096	0.113**	0.113**	0.109**
		(0.049)	(0.034)	(0.034)	(0.035)
	Divestment	-0.050	-0.041	-0.042	-0.033
		(0.057)	(0.035)	(0.035)	(0.035)
	R^2	0.087	0.417	0.421	0.468
Matching with FE	Acquisition	0.103**	0.098**	0.099**	0.095**
		(0.024)	(0.020)	(0.020)	(0.021)
	Divestment	0.010	-0.001	-0.000	0.004
		(0.025)	(0.025)	(0.025)	(0.025)
	R^2	0.083	0.401	0.406	0.481
Matching with GE	Acquisition	0.084**			0.081**
		(0.020)			(0.022)
	Divestment	0.035			0.038
		(0.024)			(0.025)
	R^2	0.069			0.181
Ind. Characteristics		No	Yes	Yes	Yes
Job Characteristics		No	No	No	Yes

Notes: See Table 6. Foreign acquisition: the firm was either majority state or majority domestic private in *t-2* and majority foreign in *t-1*. Divestment: the firm was majority foreign in *t-2* and majority domestic private in *t-1*.

Table 9: Foreign Wage Effect for Incumbent Workers

		(1)	(4)
Firm-Worker Effects	Acquisition	0.049**	0.049**
		(0.017)	(0.017)
	Divestment	0.017	0.017
		(0.024)	(0.025)
	R^2	0.072	0.072
Matching with Firm-Worker Effects	Acquisition	0.051*	0.051*
		(0.024)	(0.024)
	Divestment	0.020	0.020
		(0.044)	(0.044)
	R^2	0.088	0.089
Individual Characteristics		No	Yes
Job Characteristics		No	Yes

Table 10: Effects of Foreign Acquisition on the Wage Structure

	Fixed Effects	Matching (FE)	Group Effects	Matching (GFE)
Female	0.009	0.008	0.003	-0.005
	(0.009)	(0.014)	(0.018)	(0.016)
Vocational	0.014	0.010	-0.016	0.005
	(0.011)	(0.016)	(0.014)	(0.016)
High school	0.039**	0.063**	0.014	0.054**
	(0.012)	(0.015)	(0.016)	(0.017)
University	0.166**	0.107**	0.095**	0.113**
	(0.026)	(0.025)	(0.025)	(0.025)
Experience	-0.001	-0.006**	-0.001	-0.001
	(0.001)	(0.002)	(0.002)	(0.002)
Experience^2 * 100	-0.001	0.010**	-0.003	-0.002
	(0.002)	(0.003)	(0.004)	(0.004)
New Hire	-0.009	0.030*	0.014	0.036*
	(0.008)	(0.013)	(0.011)	(0.014)
Manager	0.079**	-0.001	0.088*	-0.000
	(0.020)	(0.031)	(0.037)	(0.037)

Note: The table shows coefficients and standard errors from adding interactions of the foreign acquisition dummy with the individual characteristics shown. The other variables in the regressions correspond to those in specification (4) in Table 6.

Table 11: Wage Underreporting and Ownership

	GFE	Matching with GFE
Size Interactions		
Foreign	0.098**	-0.057
	(0.034)	(0.048)
Foreign * Above 50	0.021	0.137**
	(0.033)	(0.051)
Interactions with Cheating Industry		
Foreign	0.066*	0.048
	(0.026)	(0.038)
Foreign * Non-Cheating Industry	0.057*	0.035
	(0.027)	(0.035)
Proportion of Workers at Minimum Wage		
Foreign	-0.032**	-0.023
	(0.011)	(0.015)

Table 12: Productivity and Wage Effects—Joint SUR Estimation I

		(1)	(2)
Average Compensation	Foreign	0.096**	
		(0.004)	
	Acquisition		0.099**
			(0.005)
	Divestment		0.031**
			(0.006)
Labor Productivity	Foreign	0.118**	
		(0.007)	
	Acquisition		0.166**
			(0.009)
	Divestment		-0.015
			(0.010)
P ($\beta_{comp} = \beta_{lp}$)		0.000	0.000
Corr (u_{comp}, u_{lp})		0.472	0.472
Breusch-Pagan test		0.000	0.000

Table 13: Productivity and Wage Effects—Joint SUR Estimation II

Output	Foreign	0.166**
		(0.038)
	Low Skill	-0.139**
		(0.028)
	High Skill	0.022**
		(0.005)
	Foreign * Low Skill	-0.010
		(0.044)
	Foreign * High Skill	0.040**
		(0.014)
Wage	Foreign	0.127**
		(0.031)
	Low Skill	0.024
		(0.023)
	High Skill	0.024**
		(0.004)
	Foreign * Low Skill	-0.062
		(0.036)
	Foreign * High Skill	0.028**
		(0.011)
Corr (u_{out}, u_{wbill})		0.386
Chi^2 (Breusch-Pagan) Test of independence		3836.672
		(0.000)
Chi^2 (Output ratio=Wage ratio) in domestic firms		1.74
		(0.189)
Chi^2 (Output ratio=Wage ratio) in foreign firms		0.59
		(0.442)
Chi^2 (Output ratio=Wage ratio) in foreign relative to domestic firms		0.04
		(0.843)

Note: The table shows the results of SUR estimation of production and wage functions controlling for capital, materials, and industry and year dummies. "Output ratio" and "wage ratio" refer to the ratio of coefficients of high versus low skill employment in the output and wage equations, respectively.