Assessing the Effects of Ownership Change on Careers: New Evidence from Matched Employer-Employee Data

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Abstract

Changes in corporate ownership, via merger, acquisition, or divestiture, could influence career paths for workers. Although there have been several papers on the employment and wage effects of mergers and acquisitions, the unit of analysis in such studies is typically the plant or firm. In contrast, the unit of observation in our study is the individual worker, which allows us to provide direct, systematic empirical evidence on the effects of ownership change on compensation and career development. Specifically, we analyze linked employer-employee data for the entire population of Swedish workers and over 19,000 manufacturing plants for the period 1985-1998. For each worker employed in these establishments (as well as the entire population of workers), we have data on gender, age, national origin, level of education, type of education, location, industrial sector, annual earnings, as well as each employee's complete work history during the period. We also have data on numerous plant and firm-level characteristics, which allows us to control for additional factors that might result in changes in worker compensation. We also are able to determine whether workers change employers, lose their jobs, or become self-employed in the aftermath of an ownership change, and their subsequent compensation. Our preliminary findings suggest that workers were typically compensated about 1-2% above the norm immediately before ownership change, and that the compensation of male workers who did not enter unemployment fell to about the normal level following ownership change, while the compensation of female workers who did not enter unemployment remained above the norm for several years.

Keywords: Mergers and Acquisitions, Human Capital, Earnings
JEL Codes: G34, J23, J31, C81

I. INTRODUCTION

The recent resurgence in mergers and acquisitions has focused greater attention on assessing the impact of these transactions on organizations and workers. Changes in corporate ownership could influence career paths for workers. Although there have been several papers on the employment and wage effects of mergers and acquisitions, the unit of analysis in such studies is typically the plant or firm. In contrast, the unit of observation in our study is the individual worker, which allows us to provide direct, systematic empirical evidence on the effects of ownership change on compensation and career development.

Some scholars have asserted that corporate takeovers have deleterious effects on workers. For example, Shleifer and Summers (1987) conjecture that the new owners of a firm in the aftermath of a hostile takeover are more likely to abrogate implicit contracts with employees, with respect to wages, benefits, and pension contributions. More specifically, they assert that shareholder wealth creation arising from corporate takeovers need not reflect improvements in economic welfare or efficiency. Instead, the increase in economic performance may reflect a transfer of wealth from employees and other non-financial stakeholders to shareholders. Others have alleged that ownership change leads to substantial downsizing or even mass layoffs, usually basing their conclusions on small samples of "event studies" of large corporations. Such layoffs have been alleged to have a traumatic, lasting negative impact on workers who are fired and also on "survivors," or those who remain with the firm in the aftermath of the layoff (Brockner et al. (1987), Brockner (1988)).

On the other hand, some theories predict that ownership change could constitute a mechanism for enhancing careers, by stimulating additional investment in human capital and promoting "skill upgrading" of the workforce, particularly if these transactions result in the

implementation of new technologies. For example, Jovanovic and Rousseau (2002, 2004) conjecture that high quality managers and high quality projects are complements. Moreover, they assert that ownership change results in the diffusion of new technologies and the reallocation of capital to more efficient uses and to better managers. An empirical implication of their model is that technological change and ownership change are complements, which implies that these transactions should lead to some job reduction (e.g., labor-saving technological innovations), but also "skill upgrading" and wage increases for employees that remain with the firm.[1] Bresnahan, Brynjolfsson and Hitt (2002) present evidence on the connection among technological change, organizational change, and organizational performance.

Although there have been numerous empirical studies of the employment and wage effects of mergers and acquisitions at the plant and firm-levels (Brown and Medoff (1988), Lichtenberg and Siegel (1987, 1990a, 1990b), McGuckin et al. (1998), McGuckin and Nguyen (2001), Conyon et al. (2002a, 2002b, 2004), and Gugler and Yurtoglu (2004)), there is little direct, systematic empirical evidence on the effects of ownership change on individual workers and their career development. This can be attributed to a lack of information on employees involved in these transactions. That is unfortunate, since an ability to track workers who are involved in an ownership change might allow us to discriminate between the alternative theories mentioned earlier.

It might also be interesting to assess the impacts of these transactions on the careers of different types of workers: male vs. female, Swedish-born vs. immigrant, young vs. old, and highly-educated vs. non-highly-educated workers. There is a vast theoretical and empirical literature on discriminatory bias in labor force decisions. To the best of our knowledge, these

[1] A review of the literature on skill-biased technological change" in Siegel (1999) reveals that technological change is associated with downsizing and skill-upgrading of the workforce.

studies have not directly considered the relationship between ownership change and the extent of discriminatory employment practices.

While there has been some attention in the management and finance literatures devoted to assessing the consequences of ownership change on the careers of top-level managers (e.g., CEOs), there has been little analysis of the effects of such events on other types of employees. It is also important to note that much of the empirical work on this topic has been based on non-representative samples of corporate control changes and companies, typically, full-firm mergers and acquisitions of publicly-traded companies. That is unfortunate, since it is well known that most ownership changes involve privately-held companies and occur below the firm level (e.g., the sale of an individual plant or division of a company).

In this paper, we analyze a unique file that links economic and demographic census data and allows us to address these gaps in the literature on the labor market consequences of ownership change. The remainder of the paper is organized as follows. In the following section, we briefly review empirical studies of the employment and wage effects of ownership change. Section III considers some econometric issues. Section IV describes the data and the construction of key variables. Empirical results are presented in Section V, followed by some (very) preliminary conclusions.

II. BRIEF REVIEW OF PLANT AND FIRM-LEVEL STUDIES OF THE EMPLOYMENT AND WAGE EFFECTS OF OWNERSHIP CHANGE

Table 1 summarizes selected plant and firm-level studies of the impact of mergers and acquisitions on employment and compensation (typically, average wages). Much of the plant-level evidence seems to indicate that ownership change does not result in statistically significant declines in the employment and wages of production workers at production establishments. The

most comprehensive evidence, presented in McGuckin and Nguyen (2001), suggests that wages and employment increase after ownership change. On the other hand, Lichtenberg and Siegel (1990a) find that employment and wage growth are lower in central office or "auxiliary" establishments in the aftermath of an ownership change, suggesting that white-collar workers suffer more than blue-collar employees when such transactions occur.

Table 1 also reveals that these effects vary by type of ownership change. For instance, Baldwin (1998) reported that mergers in Canada had a negative impact on employment and compensation of non-production workers. Conyon, Girma, Thompson, and Wright (2002a) report similar results, based on firm-level data from the U.K. The authors also find greater declines in employment associated with related mergers, relative to those associated with unrelated mergers. In a subsequent paper (Conyon, Girma, Thompson, and Wright (2004)), they report that wage increases tend to follow mergers, especially related mergers. Gugler and Yurtoglu (2004) compare the employment effects of U.S. and European mergers. The authors find that there is a 10% decline in labor demand in the aftermath of mergers involving European firms. Bhagat, Shleifer, and Vishny (1990) report that 45% of the firms involved in hostile takeovers laid off workers, affecting about 6% of the workforce.

It appears that similar patterns emerge in the aftermath of leveraged and management buyouts in the U.S. and U.K. Based on data from U.S. evidence, Lichtenberg and Siegel (1990b) report declines in levels of employment and wages of non-production workers at manufacturing plants that experience a buyout. These patterns do not emerge for production workers, however. Harris, Siegel, and Wright (2005) analyze British data and conclude that management buyouts result in a reduction in the labor intensity of production.

Bliss and Rosen (2001) analyze the effect of bank mergers on CEO compensation. They report that these ownership changes have a positive effect on CEO remuneration. In more than 75% of the transactions they observed, the post-merger increase in CEO compensation exceeded 10% of the CEO's pre-merger remuneration.

Others have directly analyzed the effects of takeovers on the compensation of non-executive employees. Mitchell and Mulherin (1989) report that only a small percentage of corporate takeovers result in pension fund terminations. Similarly, Pontiff, Shleifer, and Weisbach (1990) find that only 15% of hostile takeover bids and 8% of friendly takeover bids lead to pension fund terminations. Rosett (1990) examines whether takeovers result in labor contract settlements that favor management, as opposed to workers. He reports that takeover activity is unrelated to wage growth. More importantly, Rosett concludes that, contrary to the Shleifer and Summers (1988) hypothesis, the gains to shareholders arising from corporate takeovers do not appear to be the result of losses to employees.

In the following section, we outline our econometric model and discuss other empirical issues.

III. ECONOMETRIC ANALYSIS

To analyze how ownership change relates to employment and earnings, we estimate equations for earnings (conditional on employment), the probability of employment, and the probability of different types of employment (self-employment versus organizational employment). In the current version of the paper, econometric results are presented for the earnings equations only.

The benchmark model that we estimate is:

$$(1) \qquad EARN_{ieft} = \alpha + \sum_{\ell=-13}^{12} \gamma_\ell OC_{it-\ell} + \boldsymbol{\delta}'\mathbf{X}_{it} + \boldsymbol{\phi}'\mathbf{Y}_{et} + \boldsymbol{\psi}'\mathbf{Z}_{ft} + \lambda_t + \varepsilon_{it}$$

where $EARN_{ieft}$ denotes the natural logarithm of annual earnings of individual i working in establishment e of firm f in year t, α is an intercept term,[2] $\sum_{\ell=-13}^{12} \gamma_\ell OC_{it-\ell}$ parameterizes the relationship to ownership change using coefficients γ_ℓ as discussed below, $\mathbf{X}_{it}$ is a vector of *individual-specific* characteristics, $\mathbf{Y}_{et}$ is a vector of *establishment-specific* characteristics, $\mathbf{Z}_{ft}$ is a vector of *firm-specific* characteristics, $\boldsymbol{\delta}$, $\boldsymbol{\phi}$, and $\boldsymbol{\psi}$ are vectors of coefficients, λ_t is a year-specific fixed effect, and ε_{it} is the remaining classical disturbance term.

The vector of individual-specific factors $\mathbf{X}_{it}$ includes dummy variables for gender, national origin, employee age, categories of educational attainment, field of education, and location, along with a continuous measure of employee experience.[3] We also include dummies for self-employment (and self-employment simultaneously with organizational employment), in order to assess the extent to which changes in compensation can be attributed to shifts between organizational and self employment. The establishment-specific variables, $\mathbf{Y}_{et}$, are (plant) age, size (as measured by the logarithm of employment), average wage, relative labor productivity (to other plants in the same industry), relative total factor productivity, and five-digit SIC industry dummies. Firm-level characteristics, $\mathbf{Z}_{ft}$, include total employment, R&D intensity (commonly thought to reflect technological change), average wage, number of plants, and a dummy variable denoting whether the firm operates in diverse industries.

[2] In the current version of the paper, the intercept term is constrained to be the same for all individuals. In a revised version, we will present fixed effects estimates.

[3] Age is treated as a dummy variable because very young and very old workers in Sweden are often subject to mandatory restrictions on compensation.

The treatment of ownership change in the econometric analysis requires careful consideration. In equation (1), ℓ denotes the year relative to the year of ownership change, so that negative values of ℓ signify years preceding ownership change, $\ell = 0$ denotes the year during which an employee's plant changed owners, and positive values of ℓ pertain to years following ownership change. The matching from employee to plant is done in the year preceding ownership change. That is, the analyses carried out here address the performance of workers who were in establishment e in November of the year preceding the year during which the establishment changed owners. $OC_{it-\ell}$ is a dummy variable that equals 1 if individual i was in a plant, in November of the preceding year, that changed owner (with certainty) ℓ years preceding the current year t for $\ell \geq 0$, or $|\ell|$ years following the current year for $\ell < 0$, or 0 otherwise. Note that our sample allows us to identify each manufacturing plant's owner for the years 1985 through 1998, so a new owner can be identified in each year for 1986 through 1998. For an individual observed in 1985, we wish to know up to 13 years in the future whether they will have just been (in the preceding November) in a plant that is experiencing an ownership change event, while for an individual observed in 1998, we wish to know whether they experienced such a plant ownership change event up to 12 years in the past. This consideration of past and future ownership changes yields a possible range of leads and lags from –13 to +12.

The relation of earnings and employment status to past and future ownership change events can then be assessed, at each value of ℓ, by including in the model the terms $\sum_{\ell=-13}^{12} \gamma_\ell OC_{it-\ell}$, where γ_ℓ parameterizes the relation to ownership change at lead/lag ℓ. To avoid model specification bias, each γ_ℓ is unconstrained, and parameters are estimated over the full

range of ℓ from –13 to +12. The fitted terms of γ_ℓ provide estimates of the relationship of ownership change to earnings and employment status.

If just the ownership change dummies were included as regressors, the estimates would be subject to sample selection and measurement error biases. Sample selection bias would result because for large positive or negative values of ℓ, the ownership change variable $OC_{it-\ell}$ equals one only if the individual remained an employee in the sample over a large number of years (at least $-\ell+1$ years for $\ell<0$ or at least $\ell+2$ years for $\ell\geq 0$). Any characteristics of employees that remain in the sample, such as above-average earnings, would thus be partially attributed to ownership change.

Measurement error bias would also result, given that ownership changes are unmeasured when they occur for non-manufacturing plants or outside the sample time frame. Consider first simply the issue of sample time frame. As an example, for $\ell=-13$, $OC_{it-\ell}$ can equal one only if $t=1985$ (so $t-\ell=1998$); for other values of t information about ownership changes is unavailable (since $t-\ell>1998$, the last year of data), causing, by definition, $OC_{it-(-13)}=0$. Similarly; for $\ell=-12$, $OC_{it-\ell}$ can equal 1 only if $t\leq 1986$; …; for $\ell=-1$, $OC_{it-\ell}$ can equal 1 only if $t\leq 1997$; for $\ell=0$, $OC_{it-\ell}$ can equal 1 only if $t\geq 1986$; …; for $\ell=12$, $OC_{it-\ell}$ can equal 1 only if $t=1998$. If observations are evenly dispersed across years and the probability of ownership change remains constant at p over time, the expected value of $OC_{it-\ell}$ would equal 1/14 p for $\ell=-13$ (as it is artificially 0 in 13 of 14 years of data), 2/14 p for $\ell=-12$, …, 13/14 p for $\ell=-1$ or $\ell=0$, …, 1/14 p for $\ell=12$. Thus, values of $OC_{it-\ell}$ would constitute error-ridden indicators of ownership change, with the error greatest for the largest (absolute) values of ℓ. If these ownership change measures are uncorrelated with each other and with all other

regressors, the resulting coefficient estimates would be biased toward zero, with the greatest bias for estimates at large (absolute) values of ℓ. If the true coefficients all equaled the same constant number c, the expected values of the estimates would follow a U-shape (if $c < 0$) or inverted-U-shape (if $c > 0$). This bias would occur even if ownership change could be observed among all plants; the lack of information for non-manufacturing plants would further increase the bias. Hence both sample selection and measurement biases could confound our analysis of the relationship between ownership change, earnings, and employment status.

Such biases can be especially severe when researchers use a balanced panel (e.g., Lichtenberg and Siegel (1987)), restrict the range of ℓ (McGuckin and Nguyen (1995)), or analyze pre- versus post-acquisition periods using a single coefficient for each. For example, the use of a balanced panel imparts a strong selection bias, because the analysis is based only on those plants that survived throughout the sample period. Restrictions on the range of ℓ effectively constrain γ_ℓ to equal zero outside of the range, yielding possible specification error. An instinctive approach in the analysis of employee outcomes would be to include ownership change measures only for $\ell \geq 0$, but this presupposes that the distributions of earnings and employment outcomes for employees who will experience ownership change in future is necessarily the same as the distributions for employees who will not experience ownership change. Similarly, simply using pre- versus post-ownership change periods would effectively constrain γ_ℓ to be identical across values of ℓ and hence constitute an additional source of specification error. Moreover none of these approaches entirely gets rid of the sample selection and measurement biases pointed out above, unless all data points are dropped from analysis if they are within L+1 years of the start and L years of the end of the sample and the range of ℓ is constrained to $-\mathrm{L} \leq \ell \leq \mathrm{L}$.

We hypothesize that there is a way to address this problem without excluding any observations. The intended comparison is between individuals who were just in plants that experienced ownership change in year $t-\ell$ and those who were just in plants that could have experienced but did not experience measured ownership change in year $t-\ell$ (not between individuals who were just in plants that did experience, versus those just in plants that *might* have experienced or did not experience ownership change). For each ℓ, we divide the observations into three categories: (i) observations (person-years) whose plants in the preceding November definitely did experience ownership change in year $t-\ell$, (ii) observations whose plants in the preceding November are known not to have experienced ownership change in year $t-\ell$, and (iii) observations whose plants in the preceding November are unknown or for which it is unknown whether they experienced ownership change in year $t-\ell$. To ensure that the coefficients γ_ℓ describe the difference between categories (i) and (ii), it is sufficient to introduce into the model a dummy variable $ND_{it-\ell}$ that equals 1 for any observations meeting condition (iii) in year $t-\ell$ and 0 for all other observations. This gives rise to one additional variable for each ℓ, yielding the sum $\sum_{\ell=-13}^{12} \delta_{1\ell} ND_{it-\ell}$, comparable to the ownership change term in the models. $ND_{it-\ell}=1$ implies either no data about whether the individual's plant in the preceding November experienced ownership change in year $t-\ell$, nonexistence of the employee in the sample in the preceding year, or nonexistence of the plant in year $t-\ell$. Hence these controls potentially remove a substantial source of bias in the estimates. Indeed, in simulations for similar analyses at the plant level, we have found substantial bias without these controls but no bias once they are introduced.

IV. DATA

Our empirical analysis is based on linked, longitudinal employer-employee data on Swedish workers and plants that employ them. The file covers every employee in Sweden in every year from 1985 to 1998. The full database contains 36,398,617 records across the 14 years of data, for an average of 2.6 million workers per year, consistent with the Swedish population of close to 9 million. Establishment level data are available for the majority of employees if and when they were employed in the manufacturing sector, so that 9,251,962 records have matching information available about the employee's plant (and usually firm) workplace.

The database facilitates our investigation of employment status and earnings. Employment is recorded each year in November, and given that the database covers all employees, we infer that a worker whose record is missing in a given year was not employed in Sweden during that year. Annual earnings are recorded from employees' official tax filings, and are divided into earnings paid by an organization versus self-employment and other earnings.[4] Self-employment income serves as a proxy for whether the employee was self-employed, and we use the two sources of income to divide each working employee into one of three categories in each year: organizationally employed, self-employed, or both.

For individual employees, the data include the person's gender, national origin, age, geographic location, year of last educational exam, categorical variables for educational attainment and field of education, and 5-digit SIC industry classification of employment. In a previous paper (Siegel, Simons, and Lindstrom (2005)), we used parts of this information to construct plant-level measures of workforce characteristics, such as the percentage of workers who are female, the percentage who were born in Sweden versus immigrated, the mean age of employees, mean experience as proxied by years elapsed since last year of education, and the

[4] The data do not include hours worked or hourly wages, only annual total income, for specific employees.

percentage of employees with at least some college-level education. Here we use the employee-specific data in each year as controls and to check for possible differences in effects of ownership change across different types of people.

Each record contains data on gender and national origin. The national origin is based on their birthplace, divided between Sweden, other Nordic countries, the remainder of Europe, and five other world regions (Asia, Africa, North America, South America, and other nations). Employees' geographic locations, available for 99.6% of records, correspond to 338 local governments. Educational attainment and broad field of educational are likewise recorded categorically, and are available for 97% of records. Attainment is categorized as 0-8 years, 9-10 years (obligatory in Sweden), 11-12 years, 13-14 years (equivalent to a normal high school education similar to U.S. grade twelve), college or university education for one to two years (including extended high school engineering programs), college or university education for three or more years but not PhD education, or PhD education. Field of education is categorized as basic (general) education; esthetics, language, and religion; pedagogy; trade, office, economic, social, and behavioral degrees; industry-relevant education including handcrafts, engineering, mathematics, physics, chemistry, and biology; transportation and communication; caring including nursing, child care, and geriatric care; farming, gardening, forestry, and fishing; general service skills including private guards and military service; or other areas of education.

The data record the year of an employee's last educational examination in 45% of records, and a proxy for employee work experience is constructed in these cases as the logarithm of the number of years (including the last educational year) since finishing education. This proxy for experience is likely to be an adequate control despite the paucity of information on educational examination year, because examination year information is mainly lacking among

older employees, for whom age dummies (also included as control variables) provide a good proxy for experience. (The proxy for years of work experience may be better for male employees than female employees because males are more likely to work throughout the period following the last exam year, and accordingly we plan in future to introduce an interaction between years of experience and gender.) The employee's current industry classification of activity, available in 97.6% of records, divides employees into one of 1,092 categories based on either 1969 Standard Industrial Classification (SIC) codes, used where available, or 1992 SIC codes, used in later years. Given that 1969 and 1992 industrial classifications cannot be matched precisely, separate categories are used for 1969 versus 1992 industry codes.[5] Categorical variables (gender, national origin, geographic locations, educational attainment, field of education, and industry) are represented in our analyses using 0-1 dummy variables.

Although employee, plant, or firm data are missing for some observations, we do not exclude any records from the sample on the basis of missing data, to avoid any potential sample selection bias. Instead, we set the values of missing variables equal to the population mean or zero, and add dummy variables that equal one when the relevant type of data is unavailable or zero otherwise.[6] Hence all these variables are used as controls to the full extent possible, while records with missing observations are allowed a constant shift parameter in case they differ on average from records with available information.

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[5] This makes the industry categories perfectly multi-collinear with the year-specific dummy variables, requiring that an appropriately chosen dummy variable be dropped from the model, with the ramification that estimated coefficients of year and industry dummy variables cannot be construed to have their obvious meanings and hence are not reported but simply used as controls.

[6] Setting values of missing variables equal to the population mean is preferable in that non-zero coefficients for the corresponding no-data dummies would then reflect differences between records with and without data, facilitating comparison. However, this was not done systematically for two reasons. First, since the variables are included as controls, there is little need to study the differences between populations with and without available data. Second, computation of the estimates is a lengthy procedure that can be sped up enormously by maintaining 0-1 variables.

In the earnings equations, we include control variables to address potentially lower average earnings in years of job transition. If an employee has a short non-employment spell between jobs, his annual earnings are likely to be reduced in proportion to the duration of the non-employment spell. If a worker begins or stops working, commencement or cessation of employment is likely to occur mid-year, with a consequent reduction in annual earnings. Accordingly, each employee-year record was classified according to whether the employee was not employed in the previous year, maybe employed in the previous year (unknown because the year of the record is 1985), employed in a different plant in the previous year, or employed in the same plant in the previous year. An analogous classification was used for the forthcoming year, and 0-1 dummy variables were created based on the *interaction* of these two classifications.[7,8]

The data on individual manufacturing employees were linked to data at the plant level. Although plant-level data are available only for manufacturing plants, they provide a means to control for potentially important effects of plant-related characteristics on earnings. Moreover, the measures of ownership change used here depend upon the plant-level data. Consistent estimation of the relationship of earnings to ownership change, given that data are missing when employees cannot be matched to the plant sample, has been addressed in the preceding section. Plant-level data are available for 26.9% of employee-year records, and 50.4% of the records

[7] These controls facilitate consistent estimation when the dependent variable is earnings per calendar year, while the true intended measure is earnings per year of work: if on average employees starting a job work on average only half of a year, then the coefficient on the dummy variable for employees starting jobs will reflect the resulting half-reduced earnings. The omitted group is employees working in the same plant in both preceding and following years.

[8] One factor that we do not control for is the duration of short-term unemployment spells, since no direct measure is available for durations less than a year. As a result, in the earnings equations our estimates for the relation of ownership change to earnings implicitly assume that unemployment spells related to ownership change have similar duration to unemployment spells in general. If ownership change tends to trigger job loss immediately following the ownership change, and in fact the duration of unemployment for these employees is lower (or higher) than the average among employees who change or lose jobs for other reasons, this will bias the estimates for a few years immediately after the time of ownership change.

correspond to employees who were employed in a plant with available data in at least one year of the sample.

Following conventional international standards, the plant or establishment is defined as a physically independent unit within a firm. Each plant is assumed to focus on one industry. Firms that are involved in multiple activities at the same physical address report separate figures for each activity, which are then assigned to a separate facility. In most cases, however, firms focus on a single activity, implying that the local units are seldom split into several plants. Plants that were considered to be "non-active" and "help plants," such as sales offices (or what would be considered "auxiliary" establishments in the U.S.), were also excluded from the data.

Next, we address the issue the question of whether our sample of manufacturing plants is representative of the population of establishments. According to Swedish law, each business is required to report information to Statistics Sweden on an annual basis. In 1946, the certainty criterion for inclusion in the annual survey of manufacturing plants was established at a minimum of 5 employees and 10,000 Swedish kroner (about 1,300 US dollars) in production value. In 1990, this certainty threshold was raised to a minimum of 10 employees, while a stratified sampling procedure is applied to smaller plants.[9]

Table 2 compares the size distribution of our sample (top panel) with corresponding values for the population of Swedish manufacturing plants (bottom panel) at three points in time: 1986, 1990, and 1995. These figures reveal that our sample contains an overwhelming majority of plants with more than 10 employees. Sample coverage of plants with more than 20 employees

[9] In 1997, the certainty threshold officially was raised to a minimum of 20 employees, but given evolving sampling procedures for smaller plants this change appears to have had little effect. The plant-level dataset actually includes mining as well as manufacturing plants; mining plants make up about 3% of the total. The certainty threshold increases in 1990 and 1997 affected only manufacturing plants.

increased from 86% in 1986 to 95% in 1995. The corresponding figures for plants with more than 10 employees increased from 80% in 1986 to 88% in 1995.

Table 3 presents some statistics on the incidence of ownership change. Over the entire sample period (1985-1998), 5.1 % of plants experienced at least one ownership change. These rates of plant turnover appear to be slightly higher when they are weighted by value-added and employment (columns 2 and 3). An analysis of the annual figures reveals that the incidence of ownership change appears to have risen during the late 1980s, reaching a peak in the early 1990s.

We include measures of plant productivity as control variables in our regressions, since employees may capture a share of the gains from more efficient production. Labor productivity and total factor productivity (TFP) of the plant are computed as residuals from Cobb-Douglas production function regressions of the following form:

$$\ln Q_{it} = f_{it} + \omega_{it} \tag{4}$$

where Q_{it} denotes the logarithm of plant i's output in year t, f_{it} is the logarithm of plant i's production function, and ω_{it} is the efficiency residual. The production function includes labor, capital and materials for the TFP regression, as well as fixed effects for each year and 4-digit SIC industry. In addition, the production function coefficients of labor, capital, and materials are allowed to differ by industry, by including interaction terms equal to industry-specific dummies times each production function parameter.

A critical issue in the calculation of TFP is construction of a capital measure. Some researchers avoid analyzing TFP, and instead compute labor productivity (LP), which is easier to measure. We use both measures of productivity as separate control variables, with the TFP measure available only for the years 1989-1998 when capital stock measures could be constructed. We calculated estimates of capital stock as follows: initial values of capital were

estimated in 1989, based on the assumption of a constant capital-to-sales ratio across all plants in each 3-digit SIC industry. Using these initial estimates, capital is constructed using the usual perpetual inventory algorithm, $K^c_{it} = (1-\delta^c)K^c_{it-1} + \rho_t I^c_{it}$, where i denotes a plant, t denotes a year, c is either machinery or buildings & land, K denotes capital, I denotes investment, δ denotes the depreciation rate, and ρ denotes an investment deflator.[10] The capital estimates for machinery plus buildings and land were summed to create a single combined capital stock measure, K_{it}.

VI. EMPIRICAL RESULTS

Table 4 contains descriptive statistics for key variables used in the econometric analysis. We also distinguish among the three levels of analysis: individual, plant, and firm. … Results and Discussion to follow.

As a first cut, in Table 5, we present OLS estimates of earning equations, based on the specification outlined in equation (1). In Column (1), we constrain the effects of ownership change to be the same for all workers. Column (2) relaxes that restriction, allowing the effects of ownership change to differ for females and non-Swedes.

We focus our attention on the coefficients on the ownership change dummy variables. These estimates indicate that workers whose plants are destined to experience an ownership change earned 1-2% more than observationally equivalent employees in years shortly before the date of ownership change, with wages falling back to comparable amounts over the next few years. Figure 1 plots this relationship in the estimates, showing the estimated coefficients (multiplied by 100 to convert to percentages) in the seven years preceding and following an ownership change. The findings in column (2), which allow for differential effects, imply that

[10] The depreciation rate is 0.123 for machinery and 0.036 for buildings and land.

female employees benefit more from ownership change than comparable male workers. In fact, female employees who remain employed seem appear to have maintained relatively high average wages, possibly because they often moved to higher-paying jobs. On the other hand, Swedish-born workers appear to do better than their non-Swedish counterparts in that they have higher salaries in job positions where ownership change is soon to occur, and slightly higher salaries in the immediate aftermath of ownership change. Figures 2-4 illustrate these patterns graphically.[11]

In Table 6, we present averages of the coefficients on the ownership change dummies in the earnings equations for 5 years before and 5 years after the transaction (we exclude year 0, which is the year of the acquisition). In the third row of each panel, we formally test whether the post vs. pre ownership change effects are statistically significant. Our findings are also presented separately in Table 6 for three categories of people addressed by the estimates of the second statistical model: Swedish-born males, Swedish-born females, and non-Swedish-born males.

… Additional results and discussion to follow

[11] The vertical axis of Figure 3 plots 100 times the coefficients associated with ownership change, plus those associated with the interaction of ownership change times female, for each ℓ. The vertical axis of Figure 4 plots similar estimates, but substituting the non-Swedish-born dummy in place of the female dummy variable.

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Table 1
Selected Studies of the Effects of Mergers and Acquisitions on Employment and Compensation

Authors	Nature of Transactions	Findings
Lichtenberg and Siegel (1987)	Changes in Ownership of Manufacturing Plants	Labor Input Growth Rates Were Lower For Plants Changing Owners Than Comparable Plants Before the Transaction; Slightly Higher After the Transaction
Brown and Medoff (1988)	3 Types of Ownership Change Involving Firms : Simple Sales, Assets-Only, Sale, Merger	Simple Sales: 9% Increase in Employment, 5% Decline in Wages; Assets-Only Sale: 5% Decline in Employment, 5% Increase in Wages; Mergers: 2% Increase in Employment, 4% Decline in Wages
Mitchell and Mulherin (1989)	Corporate Takeovers	A Small Percentage of Takeovers Result in Pension Fund Terminations
Bhagat, Shleifer, and Vishny (1990)	Hostile Takeovers of Firms	45% of the Firms Involved in Hostile Takeovers Laid Off Workers (Approximately 6% of the Workforce)
Pontiff, Shleifer, and Weisbach (1990)	Tender Offers (Corporate Takeovers)	15% of Hostile Takeover Bids and 8% of Friendly Takeover Bids Result in Pension Fund Terminations
Lichtenberg and Siegel (1990a)	Plant-Level Analysis of Leveraged Buyouts (LBOs) and Management Buyouts (MBOs) of Divisions and Firms	Employment and Wages of Non-production Workers at Plants (But Not Production Workers) Declines After an LBO or MBO
Lichtenberg and Siegel (1990b)	Changes in Ownership of Manufacturing Plants and Auxiliary Establishments	Employment and Wage Growth is Significantly Lower in Auxiliary Establishments Changing Owners Than in Those Not Changing Owners, But Not for R&D Employees; Much Smaller Effects at Production Establishments
Rosett (1990)	Corporate Takeovers	Gains to Shareholders Arising From Corporate Takeovers Do Not Appear to be the Result of Losses to Employees
Baldwin (1998)	Related and Unrelated Mergers; Spin-offs	Mergers and Spin-offs Had Very Little Impact on Labor Costs; Related Mergers Had a Positive Impact on Wages; Mergers Had A Negative Impact on Employment and Compensation of Non-Production Workers

Table 1 (cont.)
Selected Studies of the Effects of Mergers and Acquisitions on Employment and Compensation

Authors	Nature of Transactions	Findings
McGuckin, and Nguyen (2001)	Changes in Ownership of Manufacturing Plants	For Representative Plants, Wages and Employment Increase After Ownership Change; Effects Worse For Workers in Large Plants
Bliss and Rosen (2001)	Bank Mergers	Mergers Have a Positive Effect on CEO Compensation; More Than 75% of The Mergers Led To An Increase in CEO Compensation Exceeding 10%
Conyon, Girma, Thompson, Wright (2002a)	Related and Unrelated Mergers	19% Decline in Employment for Related Mergers; 8% Decline in Employment for Unrelated Mergers
Conyon, Girma, Thompson, Wright (2004)	Related and Unrelated Mergers	Increases in Wages For All Mergers, But Especially for Related Mergers
Gugler and Yurtoglu (2004)	Mergers	Mergers Do Not Reduce Labor Demand in the U.S.; There is a 10% Decline in Labor Demand in Europe in the Aftermath of Mergers
Harris, Siegel, and Wright (2005)	Management Buyouts (MBOs)	Plants Involved in an MBO Experience a Substantial Reduction in Employment

Table 2

Sample of Manufacturing Plants (N=19010) Compared to Population of Manufacturing Plants

Variable	1986	1990	1995
% of Plants With More Than 20 Employees Included in Our Sample	85.6%	91.2%	94.5%
% of Total Employment in Plants With More Than 20 Employees Included in Our Sample	92.0%	95.7%	98.6%
% of Plants With More Than 10 Employees Included in Our Sample	79.8%	84.3%	87.5%
% of Total Employment in Plants With More Than 10 Employees Included in Our Sample	89.7%	92.4%	94.7%
% of Plants With More Than 5 Employees Included in Our Sample	63.6%	62.9%	62.4%
% of Total Employment in Plants With More Than 5 Employees Included in Our Sample	84.9%	87.0%	90.7%

Table 3
Percentage of Manufacturing Plants Changing Owners during 1986-1998 (n=19,010)

Year	% of Plants Involved in an Ownership Change	% of Value-Added Involved in an Ownership Change	% of Employment Involved in an Ownership Change
1986	3.2%	3.1%	3.3%
1987	4.3%	5.2%	5.7%
1988	5.5%	8.3%	7.5%
1989	5.0%	5.1%	5.6%
1990	4.8%	7.7%	8.2%
1991	4.8%	7.8%	7.4%
1992	5.6%	5.0%	5.7%
1993	6.0%	4.7%	5.2%
1994	4.6%	7.3%	6.7%
1995	3.9%	6.0%	5.3%
1996	3.9%	2.1%	3.1%
1997	3.7%	4.7%	3.8%
1998	3.2%	2.3%	3.0%
Entire Period	5.1%	5.4%	5.6%

Table 4
Means and Standard Deviations (in parentheses) of Worker, Plant, and Firm Characteristics

Variable	Unit of Analysis	Mean	Standard Deviation	Minimum	Maximum
Log Earnings	Worker	11.71	0.81	0	17.92
Log Experience	Worker	0.99	1.20	0	4.11
Age	Worker				
Percentage of Female Employees	Plant	29.4	45.6	0	100
Percentage of Non-Swedish Employees	Plant	10.8		0	100
Percentage of Employees With At Least A College Education	Plant	6.06		0	100
Log Employment	Plant	2.84	3.00	0	9.20
Log R&D Intensity	Firm	1.27	1.45	-3.67	11.69
# of Manufacturing Plants	Firm	1.97	4.33	0	50

Note: Preliminary estimates based on the subsample of all employees who participated in at least one sample year in a manufacturing plant for which plant data are available (about 50% of overall sample).

Table 5
Parameter Estimates from OLS Regressions of Earnings Equations
(Robust Standard Errors in Parentheses)
Dependent Variable: Log Earnings

Coefficient on:	Ownership Change Effects Same for All Individuals	Ownership Change Effects Allowed to Differ For Females and Non-Swedes
Experience	.095*** (.002)	.096*** (.002)
Female	-.377*** (.001)	-.323*** (.052)
National Origin Dummies	Yes	Yes
Level of Education Dummies	Yes	Yes
Type of Education Dummies	Yes	Yes
"No-Data" Dummies	Yes	Yes
Industry Dummies	No	No
Year Dummies	Yes	Yes
Location Dummies	Yes	Yes
Plant Age Dummies	No	No
OC_{t-13}	.039*** (.006)	.047*** (.011)
OC_{t-12}	.006** (.003)	.009 (.006)
OC_{t-11}	.005** (.002)	.008*** (.003)
OC_{t-10}	.008*** (.002)	.005* (.003)
OC_{t-9}	-.009*** (.002)	-.009*** (.002)
OC_{t-8}	-.004*** (.001)	-.005*** (.002)
OC_{t-7}	-.001 (.001)	.001 (.001)
OC_{t-6}	.005*** (.001)	.007*** (.001)
OC_{t-5}	.004*** (.001)	.005*** (.001)
OC_{t-4}	.004*** (.001)	.002* (.001)
OC_{t-3}	.006*** (.001)	.005*** .001)
OC_{t-2}	.010*** (.001)	.009*** (.001)
OC_{t-1}	.014*** (.001)	.012*** (.001)
OC_{t+0}	.020***	.018***

	(.001)	(.001)
OC_{t+1}	.004*** (.001)	.017*** (.001)
OC_{t+2}	.005*** (.001)	.001 (.001)
OC_{t+3}	.000 (.001)	-.002* (.001)
OC_{t+4}	.000 (.001)	-.002* (.001)
OC_{t+5}	.004*** (.001)	.001 (.011)
OC_{t+6}	.007*** (.001)	.005*** (.001)
OC_{t+7}	.003*** (.001)	-.002 (.002)
OC_{t+8}	.004*** (.001)	-.003 (.002)
OC_{t+9}	-.002 (.002)	-.006*** (.002)
OC_{t+10}	-.006*** (.002)	-.008*** (.003)
OC_{t+11}	-.006** (.003)	-.006 (.004)
OC_{t+12}	.008** (.004)	.007 (.007)
$FEMALEOC_{t-13}$	-------	-.011 (.023)
$FEMALEOC_{t-12}$	-------	-.003 (.013)
$FEMALEOC_{t-11}$	-------	-.001 (.010)
$FEMALEOC_{t-10}$	-------	.004 (.009)
$FEMALEOC_{t-9}$	-------	.000 (.006)
$FEMALEOC_{t-8}$	-------	.006 (.005)
$FEMALEOC_{t-7}$	-------	.001 (.005)
$FEMALEOC_{t-6}$	-------	-.007† (.004)
$FEMALEOC_{t-5}$	-------	-.001 (.003)
$FEMALEOC_{t-4}$	-------	.004 (.003)
$FEMALEOC_{t-3}$	-------	.000

		(.003)
$FEMALEOC_{t-2}$	-------	.001 (.002)
$FEMALEOC_{t-1}$	-------	.007*** (.002)
$FEMALEOC_{t+0}$	-------	.007*** (.002)
$FEMALEOC_{t+1}$	-------	.013*** (.002)
$FEMALEOC_{t+2}$	-------	.016*** (.003)
$FEMALEOC_{t+3}$	-------	.006† (.003)
$FEMALEOC_{t+4}$	-------	.008** (.004)
$FEMALEOC_{t+5}$	-------	.004 (.004)
$FEMALEOC_{t+6}$	-------	.002 (.005)
$FEMALEOC_{t+7}$	-------	.013** (.006)
$FEMALEOC_{t+8}$	-------	.019*** (.008)
$FEMALEOC_{t+9}$	-------	.007 (.008)
$FEMALEOC_{t+10}$	-------	.002 (.012)
$FEMALEOC_{t+11}$	-------	-.014 (.017)
$FEMALEOC_{t+12}$	-------	-.003 (.029)
$NONSWEDEOC_{t-13}$	-------	-.041 (.037)
$NONSWEDEOC_{t-12}$	-------	-.013 (.021)
$NONSWEDEOC_{t-11}$	-------	-.033*** (.010)
$NONSWEDEOC_{t-10}$	-------	-.002 (.008)
$NONSWEDEOC_{t-9}$	-------	-.018*** (.007)
$NONSWEDEOC_{t-8}$	-------	-.022*** (.005)
$NONSWEDEOC_{t-7}$	-------	-.019*** (.005)
$NONSWEDEOC_{t-6}$		-.007†

	-------	(.004)
$NONSWEDEOC_{t-5}$	-------	-.003 (.004)
$NONSWEDEOC_{t-4}$	-------	.000 (.003)
$NONSWEDEOC_{t-3}$	-------	.004 (.003)
$NONSWEDEOC_{t-2}$	-------	-.001 (.003)
$NONSWEDEOC_{t-1}$	-------	-.002 (.002)
$NONSWEDEOC_{t+0}$	-------	-.008*** (.003)
$NONSWEDEOC_{t+1}$	-------	.001 (.003)
$NONSWEDEOC_{t+2}$	-------	-.003 (.004)
$NONSWEDEOC_{t+3}$	-------	.004 (.004)
$NONSWEDEOC_{t+4}$	-------	.007† (.004)
$NONSWEDEOC_{t+5}$	-------	.010** (.004)
$NONSWEDEOC_{t+6}$	-------	.015*** (.005)
$NONSWEDEOC_{t+7}$	-------	.016*** (.005)
$NONSWEDEOC_{t+8}$	-------	.012† (.006)
$NONSWEDEOC_{t+9}$	-------	.016** (.007)
$NONSWEDEOC_{t+10}$	-------	.011 (.010)
$NONSWEDEOC_{t+11}$	-------	.023† (.013)
$NONSWEDEOC_{t+12}$	-------	.037† (.022)
R^2	0.477	0.478
Number of Plants	15946	15946
Number of Workers	2096580	2096580
Number of Observations	18337355	18337355

Notes: 18337355 observations on 2096580 people. Preliminary estimates based on the subsample of all employees who participated in at least one sample year in a manufacturing plant for which plant data are available (about 50% of overall sample).
† p<.10, * p<.05, ** p<.01, *** p<.001. These are two-tailed significance levels using robust standard errors.

Table 6
Averages of Coefficients of Ownership Change Five Years Before and After the Event
(Robust Standard Errors in Parentheses)

Coefficient on:	Ownership Change Effects Same for All Individuals	Ownership Change Effects Allowed to Differ For Females and Non-Swedes
All employees:		
MEAN OF OC_{t-5} TO OC_{t-1}	.0075*** (.0004)	
MEAN OF OC_{t+1} TO OC_{t+5}	.0025*** (.0005)	
INCREASE PRE TO POST	-.0050*** (.0006)	
Swedish-born males:		
MEAN OF OC_{t-5} TO OC_{t-1}		.0066*** (.0004)
MEAN OF OC_{t+1} TO OC_{t+5}		-.0002 (.0008)
INCREASE PRE TO POST		-.0068*** (.0007)
Swedish-born females:		
MEAN OF OC_{t-5} TO OC_{t-1}		.0089*** (.0013)
MEAN OF OC_{t+1} TO OC_{t+5}		.0086 (.0026)
INCREASE PRE TO POST		-.0003 (.0021)
Non-Swedish-born males:		
MEAN OF OC_{t-5} TO OC_{t-1}		.0062*** (.0014)
MEAN OF OC_{t+1} TO OC_{t+5}		.0037 (.0025)
INCREASE PRE TO POST		-.0025 (.0023)

† p<.10, * p<.05, ** p<.01, *** p<.001. These are two-tailed significance levels using robust standard errors.

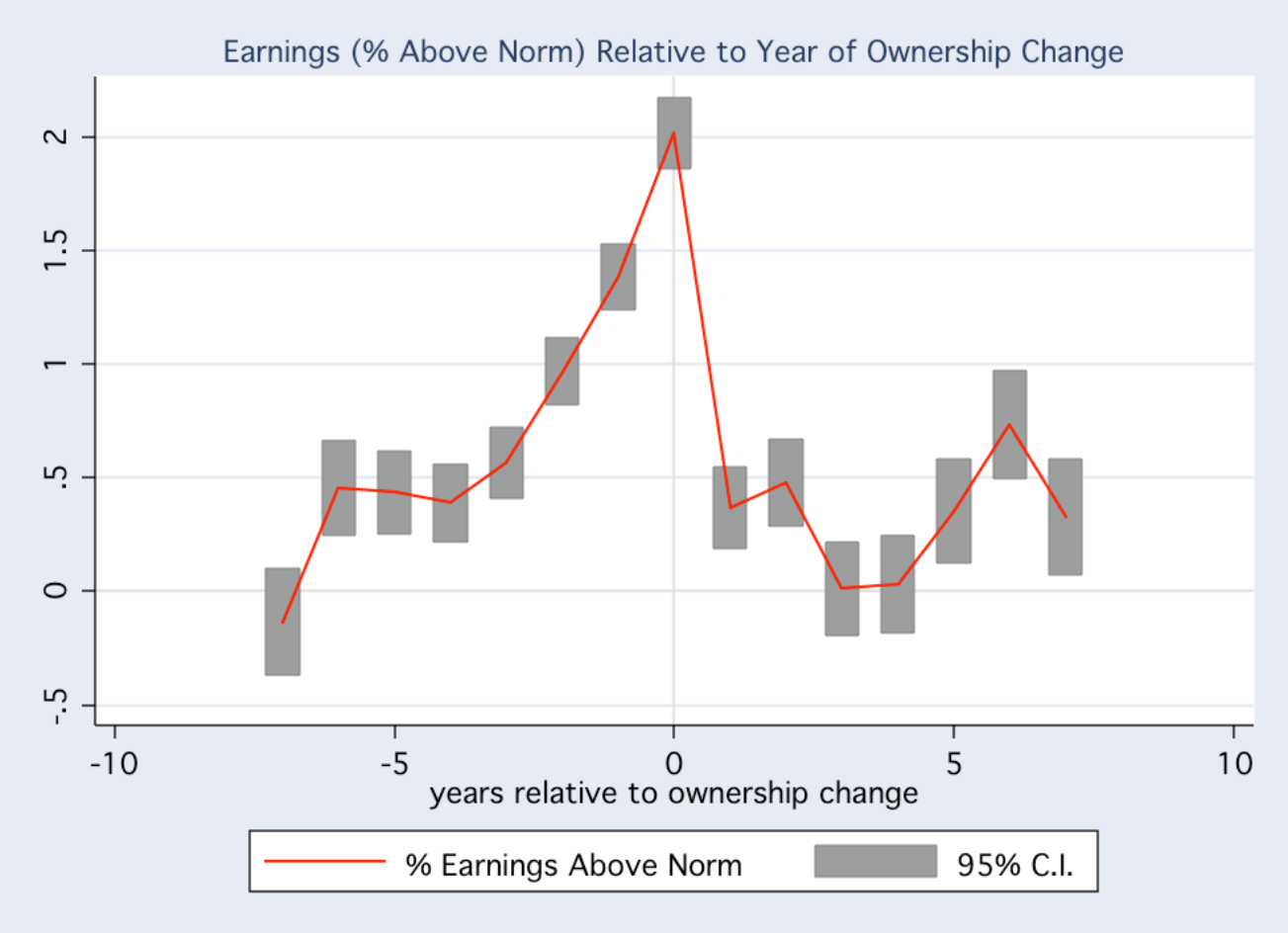

Figure 1. Percentage mean earnings above norm, relative to year of ownership change.

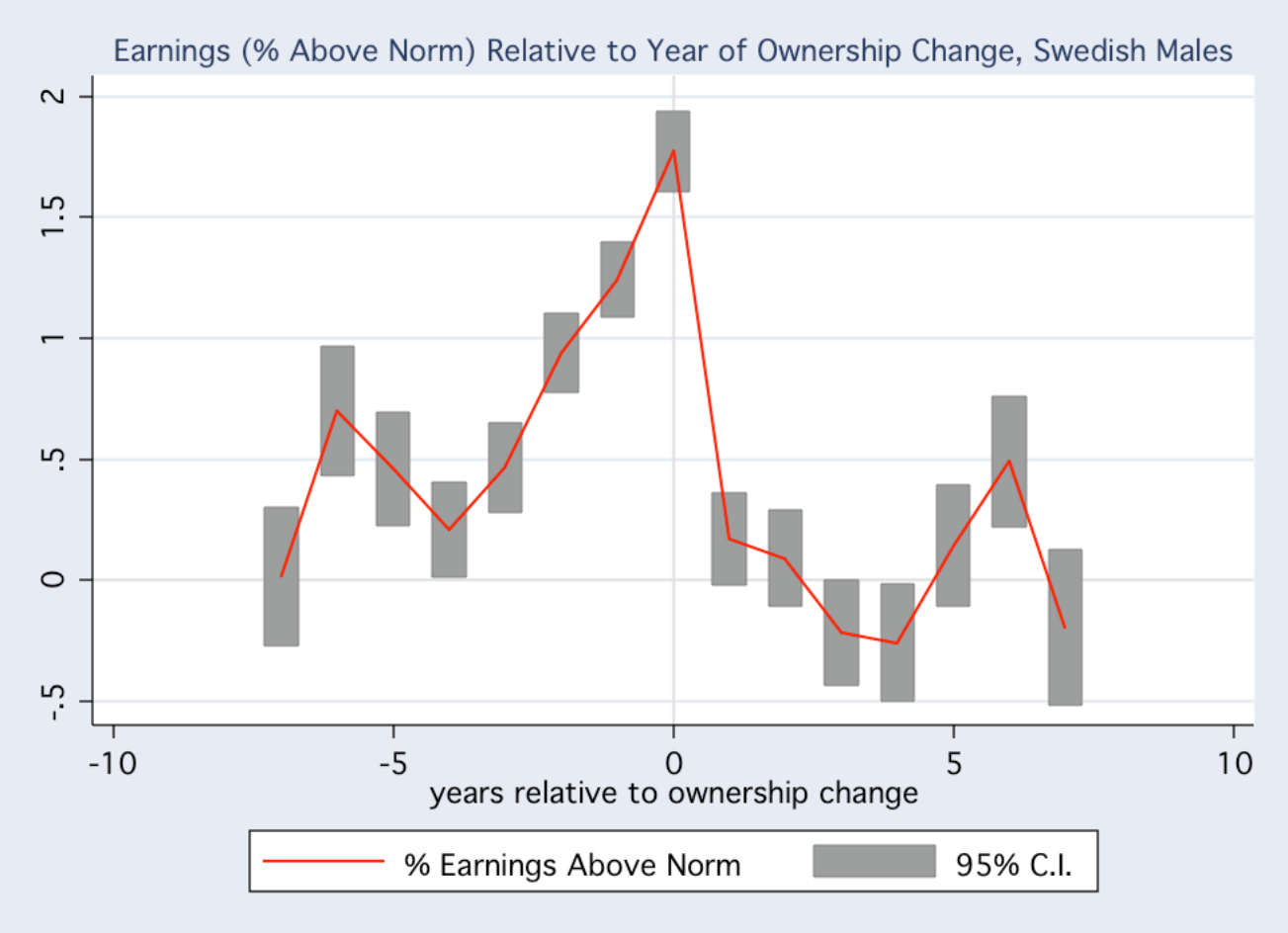

Figure 2. Percentage mean earnings above norm, relative to year of ownership change, for Swedish-born males.

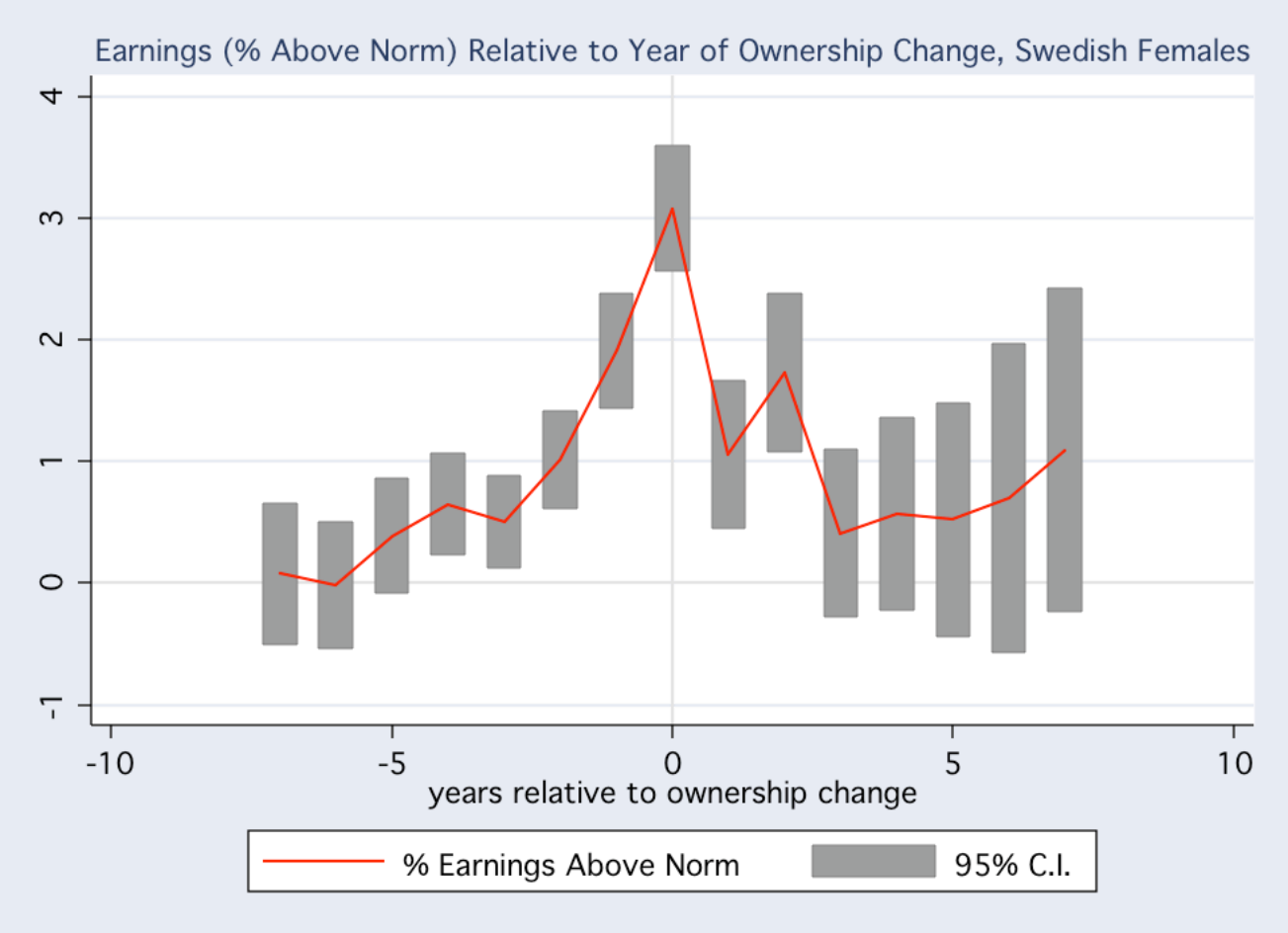

Figure 3. Percentage mean earnings above norm, relative to year of ownership change, for Swedish-born females.

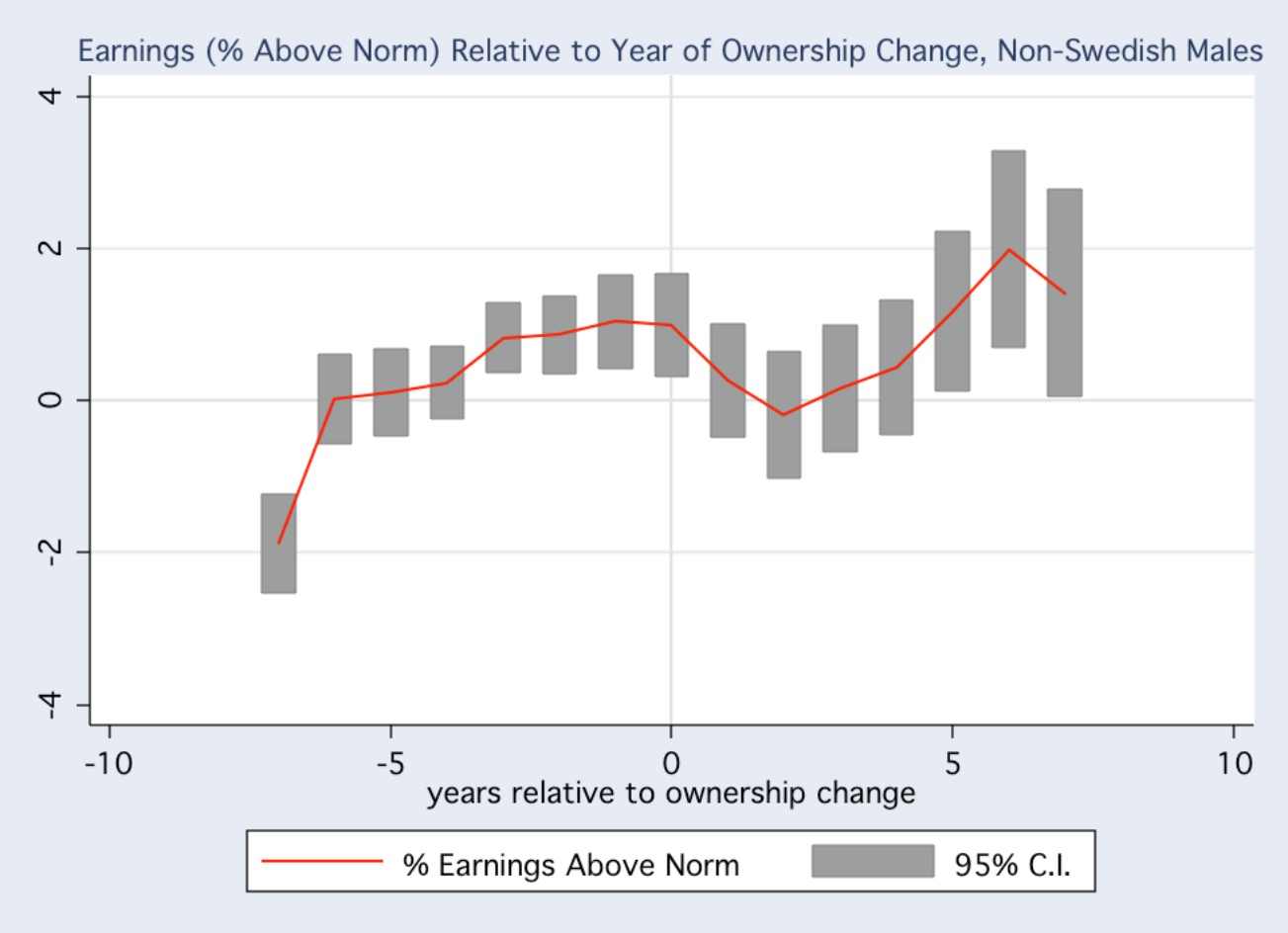

Figure 4. Percentage mean earnings above norm, relative to year of ownership change, for non-Swedish-born males.