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REDUCE EXTRAMARGINAL
WAGE PAYMENTS?

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ABSTRACT

Hostile takeovers may reduce the prevalence of long-term employment contracts if they facilitate the opportunistic expropriation of extramarginal wage payments. Our tests of two versions of the expropriation hypothesis improve on existing research by using firm- and establishment-level data from an employer salary survey, and by performing both ex ante and ex post tests.

First, we study the relationship between proxies for extramarginal wage payments and subsequent hostile takeover activity, and find little evidence of an expropriation motive. Then, since we observe wage and employment structures both before and after takeovers, we investigate whether proxies for extramarginal wages drop after hostile takeovers. The ex post experiments provide evidence consistent with one version of the expropriation hypothesis. In particular, such takeovers appear to reduce extramarginal wage payments to more-tenured workers, mostly through flattening wage-seniority profiles in firms with relatively senior work forces.

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I. Introduction

Hostile takeovers may deter the formation of long-term employment contracts by abrogating existing contracts in target firms. If so, opening the market for corporate control could cause social welfare losses that would detract from the welfare gained by disciplining management. This paper examines whether hostile takeovers reduce two possible manifestations of implicit employment contracts: employer-specific wage differentials and steeper-than-average seniority-wage profiles.

Shleifer and Summers (1988) argue that hostile takeovers may facilitate opportunistic behavior at the expense of workers by making possible the removal of "entrenched" managers who would otherwise uphold implicit contracts with employees. To explain why managers insist on upholding implicit contracts, the authors invoke the hypothesis of management loyalty instead of reputation effects. They argue that since loyalty is an ingredient necessary to make implicit contracts in the first place, a long-time manager tends to avoid breaking implicit contracts with employees even when doing so would benefit shareholders.

Neumark and Sharpe (1992) contend that hostile takeovers may facilitate such opportunistic behavior even if there are reputation effects. The new management taking command after an unfriendly takeover breaks the implicit contracts of the previous managers, and therefore may not suffer a damaged reputation, particularly if the target firm is rapidly resold.¹ The authors' study of the relationship between ex ante extramarginal wage payments and

¹Bhagat, Shleifer, and Vishny (1990) argue that because corporate raiders typically resell acquired assets to firms in similar industries, hostile takeovers are largely an "industrial organization problem," resolving issues similar to those that trigger friendly mergers, and were perhaps spurred by lenient antitrust enforcement in the 1980s that released pent-up demand for acquisitions in related industries. The argument in Neumark and Sharpe, which emphasizes reputation effects, suggests another reason why other firms are interested in acquiring hostile takeover targets from raiders.

subsequent hostile takeovers provides evidence consistent with the view that hostile takeovers are more likely for firms with high extramarginal wages, although the effect diminishes in the presence of controls for financial characteristics.

This paper improves on that empirical analysis in two important ways. First, this paper uses firm- and establishment-level data from an employer salary survey (similar to that used in Groshen [1991c])--in contrast to Neumark and Sharpe's use of industry-level characteristics--to proxy for firm-level extramarginal wage payments. These data should yield better proxies for extramarginal wages at the firm or establishment level, given evidence of firm-specific wage differentials within industries (Groshen [1991a, 1991b]). Second, these data permit more than an ex ante experiment. Because we can observe characteristics of the establishment's wage and employment structures both before and after the occurrence of a hostile takeover, we can see whether the data are consistent with firms in fact expropriating extramarginal wages following hostile takeovers, by examining changes in extramarginal wage payments. This analysis can potentially reveal effects of hostile takeovers that are obscured by heterogeneity bias in the ex ante analysis. Furthermore, these data cover the same reporting unit (one or a number of establishments) before and after the takeover, so there is no problem of attributing changes in wages or employment to the acquired entity, rather than to the new parent company.

We emphasize that our empirical exercises constitute joint tests of two components of the expropriation hypotheses: that the characteristics captured in these proxies represent extramarginal wage payments; and that hostile takeovers transfer extramarginal wage "wealth" away from workers. The absence of evidence linking a proxy to the occurrence of hostile takeovers may imply that only one component of the associated hypothesis is invalid. However,

evidence that a proxy declines following hostile takeovers (such as we find in the ex post analysis) lends support to both components of the hypothesis.

II. Extramarginal Wages

A considerable body of labor economics research documents evidence that is consistent with extramarginal wages, or attempts to provide related theoretical models. The hypothesized wealth transfers that occur after hostile takeovers could involve two types of extramarginal wages: shared rents and Lazear quasi-rents to older workers.

The shared rents hypothesis arises from research on industry- and firm-specific wage premia. In wage regressions that control for variables that economic theory suggests ought to determine wages, substantial industry wage differentials persist (Krueger and Summers [1988]). Some explanations that have been considered and rejected are compensating differentials (Murphy and Topel [1987]), unobserved ability (Gibbons and Katz [1992], Blackburn and Neumark [1992]), demand or supply shifts (Helwege [1992]), and union threat effects (Neumark and Wachter [1992]).

This evidence of industry-level wage differentials has been supplemented with evidence of persistent, unexplained wage differences at the firm and establishment level (Groschen [1991a]). Employer wage differentials within industry are about the same size as differences between industries, are linked to observable characteristics of the establishments (such as size, technology, product, and unionization), and appear to be long-lived (Groschen [1991b]).

Some researchers, such as Dickens and Katz (1987a, 1987b), have concluded that these industry-, firm-, and establishment-level wage differentials reflect rents. Such rents can arise under unionism or its threat (where employers pay more to avert strikes or unions), or in the gift-exchange model of Akerlof (1982), in which workers receive above-market-

clearing wages (in some firms) in return for exerting above-minimal effort. They can also arise if firms base wages partly on ability to pay, perhaps out of equity concerns (see the review in Levine [1991]).

A second labor market model that leads to the payment of extramarginal wages, although only to more-tenured workers, is the incentive contract model of Lazear (1979). In this model, firms can only imperfectly monitor workers, who therefore have some incentive to shirk on the job. To eliminate or reduce shirking, firms pay workers less than their marginal product when their tenure is low, and more than their marginal product when their tenure is high. In this deferred compensation scheme, employees essentially post a "bond" that is forfeited if they are caught shirking. They are willing to accept the deferred-compensation contract ex ante because it results in greater output, and hence a higher present value of earnings, relative to the spot market outcome.² The extramarginal wages received by senior workers in this model are quasi-rents, rather than pure rents, because they compensate for the bonds posted earlier in workers' careers.

Empirical research on Lazear contracts seeks to distinguish the deferred compensation explanation of rising wages from the general human capital investment explanation. In particular, this research asks whether wages rise

²An alternative hypothesis under which workers receive less than their marginal product when young, and more when old, is that workers prefer rising wage profiles as a forced-saving mechanism (Frank and Hutchens [1992], Loewenstein and Sicherman [1991]). In this case, however, workers sacrifice their present value of earnings in order to receive deferred compensation. Nonetheless, older workers are still paid extramarginal wages. Neumark (1992) provides some evidence consistent with the forced-saving hypothesis. Neumark and Taubman (1992) exploit the different implications of alternative explanations of wage growth for present values of earnings streams to distinguish among the explanations.

There is also a version of the human capital model (Garmichael [1983]) with the same empirical implications. In this paper, we do not distinguish between the Lazear model and these models; any evidence regarding the validity of one bears equally on the others. Thus, our research sheds light on the existence of extramarginal wage payments to older, more-tenured workers, but does not address the source of these payments.

faster than marginal product, consistent with Lazear's model, or rise in concert with marginal product. Numerous researchers have found evidence consistent with wages rising faster than marginal product (Medoff and Abraham [1981], Lazear and Moore [1984], Kotlikoff and Wise [1985], Kotlikoff and Gokhale [1991]), although others have argued to the contrary (Brown [1989]).

Based on these two avenues of research, we construct two measures of possible payments of extramarginal wages for our sample of firms. First, to study extramarginal wage payments captured in wage levels (i.e., shared rents), we estimate the unexplained fixed establishment component of wages. Second, to examine extramarginal payments to more-tenured workers (i.e., Lazear quasi-rents), we construct estimates of the steepness of the wage profile at the firm level, as well as the relative employment of senior-level workers. The particular combination of steep profiles plus high employment of more-tenured workers should be most consonant with this latter type of extramarginal wage payment.

III. Existing Research on Hostile Takeovers

A number of recent papers look at the consequences of alternative types of corporate restructurings for wages, pensions, and employment (for example, Lichtenberg and Siegel [1989], Brown and Medoff [1988]); a subset of these focus on hostile takeovers. One obstacle to studying hostile takeovers explicitly is that it is difficult to attribute changes that occur for the acquiring firm to the acquired entity per se. Thus, studies of the consequences of these takeovers have used unusual, specially constructed samples. Bhagat, Shleifer, and Vishny (1990) and Bhide (1989) study small samples of firms that were hostile takeover targets, finding that substantial layoffs are frequently reported at target firms following such events, although they conclude that layoffs explain only a fraction (11 to 26 percent)

of the takeover premium for their firms. Unfortunately, these studies consider only hostile takeover targets, in order to focus on changes in the acquired entity. Standard data sources (such as Compustat) immediately subsume the target into acquirer, making it difficult to attribute observed changes to the acquired entity.³ In addition, there is no "control group" of firms that fail to experience takeover bids. On the other hand, firms that did not experience such bids may have taken actions similar to those that ensued at takeover targets to forestall the bids, in which case comparisons of targets to nontargets could understate the effects of hostile takeovers.

In contrast, for a sample of union contracts matched to firms, Rosett (1990) finds that hostile takeovers (identified as those accompanied by CEO changes) do not result in lower real wage growth than friendly takeovers (in specifications excluding year and industry effects) and have no discernible impact on real wage growth (in specifications including these effects). One problem with this sample, however, is that the union contracts apply to only a fraction of the firms' work forces, so it is difficult to generalize to real wage changes for all employees.⁴

However, none of these papers explicitly asks whether extramarginal wages are expropriated, or (in the language of Shleifer and Summers [1988]) implicit contracts are breached, after a hostile takeover. That is, none attempts to identify the extramarginal components of wage levels, and none

³Bhagat, Shleifer, and Vishny do present some partial evidence, based on Wall Street Journal reports of industry and firm layoffs, that layoffs were higher in hostile target firms than in similar nontarget firms and that the higher layoffs followed the takeover (or attempt).

⁴Fallick and Hassett (1992) use the proportional unionization of firms as a proxy for extramarginal wages, and find that mergers are more likely between firms with similar rates of unionization, although this study does not distinguish between hostile and friendly takeovers. They argue that this runs contrary to the wealth-transfer hypothesis, on the presumption that the hypothesis predicts that nonunion firms should acquire unionized firms.

focuses on the steepness of the wage profile or on the relative employment of senior workers. Instead, the studies reviewed so far focus on wage or employment cuts per se, which may not affect extramarginal wage payments, and therefore may have nothing to do with the gains from hostile takeovers.⁵ These limitations of existing studies are imposed by the data, since none of the standard data sources (such as Compustat or the Census Bureau's Longitudinal Research Database) or the nonstandard sources used to date (as in Bhagat, Shleifer, and Vishny [1990]) contains information on the shape of the wage profile, on the relative employment of older workers, or on skill measures of workers.

In contrast, the empirical analysis in Neumark and Sharpe (1992), like the analysis in this paper, constructs a variety of measures of extramarginal wage payments and studies their relationship to hostile takeover bids. The study also differs from others because it is ex ante in nature, studying whether extramarginal wage proxies are associated with subsequent hostile takeover bids. Thus, the authors can use standard data sources (such as Compustat) to compare hostile targets and other firms, but avoid the attribution problem discussed above.

Neumark and Sharpe construct two types of proxies for extramarginal wages: differences in wage levels unattributable to standard wage equation controls in the industries in which firms conduct business, and differences both in the steepness of age-earnings profiles and in the relative employment of older workers across the industries in which firms conduct business.

These proxies are positively related to hostile takeover bids in logit estimates for the probability of hostile takeovers, although the effects are

⁵This point is recognized explicitly in Bhagat, Shleifer, and Vishny (1990), but the authors nonetheless estimate savings from layoffs as the wage bill previously paid to laid-off workers.

often statistically insignificant after the inclusion of controls for financial and other characteristics of firms. A high value for the interaction between profile steepness and the employment of older workers appears to be the most consistent predictor of hostile takeovers. Given the likely imprecision of industry-level proxies for extramarginal wages, it is perhaps surprising that any relationship is detected, even if the expropriation hypothesis is correct. Finally, the authors find that the extramarginal wage proxies predict hostile takeovers most strongly in comparison to other corporate restructurings. This result, they argue, provides compelling evidence in favor of the expropriation hypothesis, since this test focuses on the unique aspect of hostile takeovers--the change in management.

An alternative avenue, which focuses more explicitly on expropriation of extramarginal compensation or breaches of implicit contracts, investigates defined-benefit pension plan terminations and reversions of excess assets from overfunded plans.⁶ Employees can lose out from a termination because the sponsor's explicit obligation to jobholders upon termination is based only on current wages rather than on wages at retirement. Firms may be tempted to terminate and revert because of the overfunding that results from the requirement that they fund plans based on projected salaries at retirement.⁷ This research finds some evidence (although not one-sided) consistent with hostile takeovers leading to terminations and reversions as breaches of implicit contracts (Pontiff, Shleifer, and Weisbach [1990], Mittelstaedt

⁶Petersen (1992) provides empirical evidence supporting the notion that reversions are breaches of implicit contracts of a Lazear nature.

⁷When a pension plan is terminated, the firm has the option of buying annuities with a value equal to the explicit pension obligations, based on current salaries, or replacing the plan with one of at least that value. In cases where the plan is overfunded, the firm can retain the assets left over.

[1991], Ippolito and James [1992], Mitchell and Mulherin [1989]).⁸

To summarize, most existing evidence on the effects of hostile takeovers on wages and employment does not speak directly to whether such takeovers are a means of expropriating extramarginal wages or of breaching implicit contracts. Evidence on pension plan terminations and reversions is more relevant to the expropriation hypothesis and provides some (although not one-sided) support. This paper extends the Neumark and Sharpe (1992) approach of linking hostile takeovers to proxies for extramarginal wage payments or implicit contracts. This paper is unique because we use firm- or establishment-level estimates of wage and employment structures, and because we conduct both *ex ante* and *ex post* analyses.

IV. The Data

A. The Community Salary Survey

We use data from 1980 through 1991, constructed from the annual Community Salary Survey (CSS) conducted by the Federal Reserve Bank of Cleveland (FRBC) personnel department. The survey, which covers employers in Cleveland, Cincinnati, and Pittsburgh, assists in annual salary budgeting at the Bank.⁹ In return for their participation, surveyed companies are issued result books for their own use. Salary surveys such as the CSS currently offer the only source of longitudinal wage data accompanied by both detailed

⁸Mitchell and Mulherin (1989) provide the strongest empirical evidence against interpreting pension terminations as breaches of implicit contracts, disputing some of the evidence in Pontiff, et al. (1990) that hostile takeovers are more likely to lead to terminations. However, they do not dispute that announcement of pension plan terminations lead to increases in employers' stock prices.

⁹In general, Cleveland, Cincinnati, and Pittsburgh are more urban, have more cyclically sensitive employment, and have undergone more industrial restructuring than the nation as a whole. Prior to the 1980s, wages in these three cities were higher than the national average. Now, they are approximately average for the country.

occupation and information on employers.¹⁰

The FRBC chooses participants in each city to be representative of large employers in the area.¹¹ Each one judges which establishments to include in the survey, according to their internal organization. Some include all branches in the metropolitan area, while others report wages for only a single facility. We use the purposely vague term "employer" to mean the employing firm, establishment, division, or collection of local establishments for which the participant reports wages.¹²

The industries included vary widely, although the emphasis is on obtaining employers with many "matches," or employees in the occupations surveyed. The number of companies participating on an ongoing basis in these years averages about 93. Overall, 133 employers participated in the survey at one time or another over these 12 years and had enough data for use in the analysis.¹³

¹⁰See Hotchkiss (1990) for a summary of data sets with information on employers. For example, the microdata collected in Industry Wage Surveys and Area Wage Surveys by the Bureau of Labor Statistics have occupational detail, but lack any way to identify changes in ownership, are not easily linked over time, and are not preserved for long periods. Unemployment Insurance ES-202 data, when available, report average employee earnings by employer, not individual wages, and lack occupational detail. The Longitudinal Research Database, maintained by the Center for Economic Studies, goes back to 1972, but covers only manufacturers and provides only mean establishment earnings for production and nonproduction workers, with no occupational detail.

¹¹To check whether the wages paid by members of the CSS sample were unrepresentative of the areas' rates, wages in the survey were compared to Bureau of Labor Statistics' Area Wage Surveys (AWS) in the same cities for the late 1970s and early 1980s. The AWS also oversamples large employers. Movements of mean wages for similar occupations were found to be highly correlated across the two surveys, and levels were usually within 5 percent of each other.

¹²Since a participant's choice of the entities to include presumably reflects those for which wage and personnel policies are actually administered jointly, the ambiguity here is not particularly troublesome.

¹³Twelve companies had no employees in any of the job families used to estimate extramarginal wages. Thus, they had to be excluded from the analysis, although they were used to estimate overall establishment

The CSS covers 75 occupations each year; each employer reports wages for an average of 28 of these. The surveyed occupations are almost exclusively nonproduction jobs, since these positions are found in all industries. Included are office, maintenance, technical, supervisory, and professional personnel.

Many jobs are further divided into a number of grade levels, depending on required responsibilities and experience. Job descriptions for each are at least two paragraphs long. In consultation with the FRBC personnel department, we group 50 (two-thirds) of the surveyed occupations into 17 job "families." Each family comprises at least two, and up to as many as five, levels. Appendix A presents a list of the job families and levels (with their associated job titles) used in the analysis.

Each observation in the original data set gives the salary of an individual employed in a surveyed occupation by a surveyed employer. Cash bonuses are included as salary, but fringe benefits are not.

B. Identification of Takeovers

Takeovers, whether hostile or not, were identified by a combination of four methods. In 1989 and 1990, participants were asked if their ownership had changed during the past five years and, if so, by whom they had been acquired. We also looked up the CUSIP number of all publicly-held firms and took any change in number as an indication of a possible takeover.¹⁴

When ownership had changed, we traced the history of changes of company ownership during the sample period. We then consulted W.J. Grimms and Co.'s Mergerstat Review, as well as the Wall Street Journal Index, to determine whether a hostile takeover had been attempted during any of the ownership differentials.

¹⁴Note that firms that acquired others were not classified as having undergone an ownership change of any sort.

spells.¹⁵ Mergerstat Review lists hostile tender offers, whether or not the bids were ultimately successful. We classify companies as having undergone a hostile takeover if a change in ownership followed a hostile bid, even if the eventual acquiring company was considered to be a "white knight" or the company was eventually taken private.¹⁶ When an employer was not referenced in Mergerstat Review, we searched the Wall Street Journal Index for all references to the company. We classify tender offers as hostile if the article referred to the offer as "hostile," "unsolicited," or "resisted by management," etc., and if it was followed by a change in management. We also used the Index to confirm the Grimms' classification, using the same criteria for classifying tender offers. In a few cases, the company contact for the CSS was also consulted to make a final determination.

C. Ex Ante Observations vs. Ex Post Observations

We organize the data into ex ante and ex post observations. Ex ante observations are taken for the first year the employer enters the sample and are described in table 1A. In most cases, the year of entry is 1980, but 55 employers join the sample at some later date. In the first column, we see that employers represent all industry groups, but are most heavily concentrated in durable goods manufacturing and in finance, insurance, and real estate (FIRE). The other columns divide the sample into four categories: 34 employers that were not candidates for takeover (governmental agencies, public or regulated utilities, or nonprofit organizations), 67 potential candidates that had no

¹⁵Thus, suppose Company A was acquired by Company B in a friendly takeover and, later, Company B was acquired by Company C in a hostile takeover. Then, the Company A establishment in our data set would be classified as having experienced a hostile takeover.

¹⁶We use this classification scheme for two reasons. First, we have a small sample of hostile takeover bids. Second, it seems likely that the new owners, whether or not they were the initial bidders, have to act on some or all of the proposals that motivated the hostile bid in the first place.

change of ownership, 25 entities that were acquired in a (nonhostile) merger or acquisition, and seven employers that underwent hostile takeovers.¹⁷

Although seven hostile takeovers is certainly not a large number, we know of no reason to conclude that they are not representative of the universe. First of all, hostile takeovers occur at low frequency, even during booms such as the late 1980s. According to a tally in the Mergers and Acquisitions data base, approximately 290 contested tender offers were made from 1981 to 1990. Assuming that one half of these led to a change in ownership (which is typical), the CSS sample represents about 5 percent of all hostile takeovers (or 2.5 percent of all contested offers) in the US during this period. Second, we do not think the restricted geographic area covered by the CSS is particularly problematic, because many of the hostile takeovers observed were national in scope. That is, the entire takeover target often included other units beyond those in the CSS. Furthermore, we are aware of no reason to suspect that the motivation for, or impacts of, takeovers vary regionally.

Finally, we note that firms are free to refuse to participate in the survey. If this generates any selection bias, it seems to us most plausible that hostile takeover targets that experience particularly severe changes related to expropriation of extramarginal wages (such as large-scale firing of older workers) may be most likely to discontinue participation. This would bias the results against finding evidence consistent with the expropriation hypothesis.

Table 1B reports sample characteristics for the 97 observations with complete data on unionization and employer size. Most of the observations lost are unmerged candidates or employers that underwent friendly mergers.

¹⁷For reasons of confidentiality, we cannot divulge the names of the companies from which the data were collected.

Noncandidates have strikingly higher rates of unionization (UNION, defined as at least 25 percent unionized) and average size (SIZE) than do candidates. Among candidates, employers that did not undergo friendly mergers or hostile takeovers are larger but less unionized than those that did.

Ex post observations are defined from the first year observed (usually 1980) until the last year observed (usually 1991). Table 2 reports sample characteristics for this sample. It is smaller (121 observations) than the ex ante data set because employers that participated only once in the CSS (mostly in Pittsburgh), or for which an observation after a merger or takeover occurred was unavailable, are excluded.¹⁸ The industrial distribution remains about the same. We divide the sample into three subgroups: 101 employers that were not merged (noncandidates plus intact candidates), 14 mergers and acquisitions, and 6 hostile takeovers. On average, our observations on the wage and employment structure occur 3.7 years after hostile takeovers, and 2.5 years after mergers and acquisitions.

The ex post analysis looks at changes in the variables describing the wage and employment structures. Using the data in difference form controls for all static differences among these employers. The more direct forms of controlling for differences among employers, such as merging in data from other sources (Compustat, for example), cannot be used for the ex post analysis (the unique contribution of the paper) because financial data for units within firms are not available.

D. Measures of Extramarginal Wages

We use three conceptually distinct, employer-specific measures of the

¹⁸There were two observations with mergers/acquisitions for which the date of the merger occurred immediately before the first year the company was observed. These are included in the ex ante data set as nonmergers, but are excluded from the ex post data set because some effects of the earlier mergers may take a number of years to occur.

presence of extramarginal wages: overall wage differentials, wage profile slope differentials, and employment concentration in senior levels. We also interact the slope and concentration measures as a proxy for the size of the seniority-related extramarginal wage bill.

Employer wage differentials (i.e., unexplained deviations from mean wages) are estimated independently for each city and year, from an OLS regression of log median wages for each occupation in each employer, controlling for detailed occupation (following Groshen [1991a]). Estimated coefficients on employer dummies (after standardizing the mean to zero for each city-year) are denoted LEVMED, and represent the average log wage differential across occupations paid by that employer in that year.¹⁹

Similarly, employer slope differentials capture the extent to which the slope of an employer's wage profile deviates from its annual city mean. We base our estimates on the 17 job families that were found in the occupations surveyed in the CSS (see appendix A). Employer-specific steepness of age-earnings profiles is estimated in the same nonparametric way as employer wage differentials. In every case where two occupations in a family are observed at a single employer, the difference in the mean of log wages between the steps is used as a dependent variable for the regression of wage gaps on a set of fully interacted family-step dummies and a set of employer dummies. The estimated employer coefficients are labeled WCDIF and measure the average seniority-wage slope difference (relative to the mean in the city and year) paid by the employer in a particular year. A positive number for a company reflects above-average wage differences between steps in these 17 job-family ladders among their employees.

Third, to measure the relative seniority of the work force, we estimate

¹⁹Log-point wage differentials can be interpreted as approximate percentage-point differences from the mean.

what we call the "employment concentration" for each CSS employer. The log employment difference between steps in a job-family ladder is regressed on the same set of dummy variables (for all possible family-step combinations and for employers) used in the slope estimates. The estimated employer coefficients are retained; a positive coefficient, labeled EMPDIF, reflects an above-average concentration (within their city in that year) of senior employees in these job ladders.

But wage or employment differences alone need not reflect high overall rent payouts to employees. Firms with high concentrations of upper-level jobs but with small wage differences across job levels or, conversely, those with high wage slopes but with relatively low employment in upper-level jobs, may not be attractive hostile-takeover targets because both of these cases are unlikely to be associated with substantial extramarginal wage payments to more-tenured workers. However, the joint incidence of relatively high employment in senior job levels and high inter-job-level wage slopes may represent high overall extramarginal payments that could be susceptible to expropriation. Thus, the interaction variable WGDEM, computed as the product of WGDIF and EMPDIF, provides the strongest test of the expropriation hypothesis with respect to extramarginal wages paid to more-tenured workers.

Of course, the wage-slope and wage-level differentials across firms may reflect skill differences rather than extramarginal wages. In this case, though, there is no reason to expect these differences to shrink after a hostile takeover. On the other hand, under the wealth-transfer hypothesis, evidence of such reductions would strongly suggest that high wage slopes or wage levels reflect extramarginal wages. This is the sense in which this paper provides a joint test of the wealth-transfer and extramarginal wage hypotheses. Only if hostile takeovers seek to transfer wealth from workers and these wage-slope or wage-level differences reflect extramarginal wages

will these differences fall after a hostile takeover.

V. Results

A. Ex Ante Tests: The Effects of Wage and Employment Structures on the Probability of Hostile Takeovers

Descriptive statistics for the wage and employment structure variables for the ex ante analysis are reported in the last rows of tables 1A and 1B. As a group, the seven companies that underwent hostile takeovers had the lower average wages (LEVMEDE), relatively flat seniority profiles (WGDIF), and a higher concentration of workers in senior job classifications (EMPDIF). However, these differences between groups are small relative to the variation within groups.²⁰

We now turn to a more formal investigation of whether establishment-level employment distributions and wage structures are related to the probability of a hostile takeover. For each firm, the estimates of the employer-specific log median wage differential (LEVMEDE), log inter-job-level wage difference (WGDIF), and log inter-job-level employment difference (EMPDIF) are used to predict the occurrence of a hostile takeover. We expect positive coefficients on all three measures.

Table 3 reports these probit estimates for various specifications. The sample is restricted to firms that were candidates for a hostile takeover. The first nine columns show the regressions on a subsample with nonmissing data on union status, UN, and the log of firm size, LNSIZE. This subsample contains 7 firms that experienced a hostile takeover during the period spanned by the data, and 64 that did not. Table 4 reports results when the

²⁰The means for the whole sample are different from zero because the observations come from different years; only within a year is the average firm-specific differential equal to zero.

noncandidates are included. The signs of the estimated coefficients of LEVMED and WGDIF are negative, reflecting the differentials in tables 1A and 1B. The estimated coefficients of WGXEM and EMPDIF are almost always positive. These signs are robust to the inclusion of UN and LNSIZE in the regressions (columns 5 through 9), to the inclusion in the sample of firms with missing data on UN and LNSIZE (columns 10 through 13), and to the inclusion of nontakeover candidates in the regressions (table 4).

Results for all of these samples suggest that a more-senior work force (EMPDIF) is positively related to probability of hostile takeovers, except in regressions that control for LEVMED, WGDIF, and WGXEM (column 9 in tables 3 and 4). The probability of a hostile takeover based on a one-standard-deviation increase in each independent variable (holding the others at their means) is shown in brackets below each coefficient estimate; these can be compared to the probability at the means, in the last row of the table.²¹ For example, in column 7 of table 3, increasing the interaction between seniority and profile steepness (WGXEM) by one standard deviation raises the probability of a hostile takeover from 0.10 to 0.12.

While none of the estimated coefficients of the wage and employment structure variables is statistically significant, the signs of the estimated coefficients point to several conclusions. First, higher wage levels per se are not positively associated with hostile takeover probabilities. Second, high wage slopes between job levels do not attract predators. Third, the positive signs on the interaction variable, WGXEM, suggest that firms with higher wage payouts to senior workers in conjunction with high employment concentrations in senior-level jobs are more likely to experience a hostile takeover. These results and their lack of statistical significance are

²¹For the probit model, in contrast to the logit model, the estimated probability at the means need not exactly equal the unconditional probability.

consistent with Neumark and Sharpe's (1992) findings. Presumably the inclusion of financial controls would, as found by Neumark and Sharpe, further weaken these results. Fourth, support is weak at best for the hypothesis that a higher employment concentration in upper-level jobs, by itself, leads to a higher probability of a hostile takeover.

B. Ex Post Tests: The Effects of Hostile Takeovers on Wage and Employment Structures

As shown in the variable means of the ex post observations (the bottom rows of table 2), employer-specific wage differentials rise rapidly for employers that experienced a hostile takeover (ΔLEVME). However, the concentration of employees in senior positions drops dramatically following hostile takeovers (ΔEMPDIF), while it rises after a merger. Wage profiles seem to be relatively unaffected (ΔWGDIF).

The effects of hostile takeovers on firm wage and employment structures are analyzed using data on the last year available for each firm. OLS regressions are estimated for differences in LEVME , WGDIF , WGXEM , and EMPDIF (denoted with Δ 's) between the last and first years data were available, ensuring that for firms that were taken over, the change occurred in an intervening year. The differences are regressed on a dummy explanatory variable, H , that indicates the occurrence of a hostile takeover.

Table 5 reports results from regressions of the four change variables on H alone (in column [1] for each dependent variable) and H with YRAFTH , the span between the year of takeover and the year to which the changes are computed (in column [2]). The coefficients on H are negative in the ΔWGXEM and ΔEMPDIF regressions, but only the coefficient on ΔWGXEM is significant (at the 10 percent level). The ΔWGDIF regression produces a positive estimated coefficient that is not significant, suggesting that inter-job wage differences remain substantially unchanged after hostile takeovers.

The inclusion of YRAFTH in the regression preserves the negative signs on both $\Delta WGXEM$ and $\Delta EMPDIF$ and renders the coefficient for $\Delta WGXEM$ more strongly significant. These coefficient estimates indicate that hostile takeovers are followed by relatively large reductions in employment concentration in senior positions and in employment-weighted wage slopes. The positive coefficients on YRAFTH in the $\Delta WGXEM$ and $\Delta EMPDIF$ regressions suggest that the initial reductions in these variables after a hostile takeover are partially reversed over time.

This apparent reversal could stem from at least two sources. In line with the hypothesis advanced here, suppose hostile predators extract extramarginal rents from more-senior workers at the time of takeover, but do not change the Lazear-type bonding offered to continuing or new employees. If Lazear contracts are efficient and the new parent's offer is credible (because the company has been resold or the predator is expected to honor its own implicit contracts), then wage profiles would be unchanged after the takeover, and, for example, the drop in the seniority concentration of workers would dissipate over time.

The second possibility arises because changes in the wage and employment structures for the earliest takeovers in the sample were observed over the longest period; it is possible that the nature of the hostile takeovers differed between the early and late 1980s. Under this interpretation, the positive coefficients on YRAFTH in the $\Delta WGXEM$ and $\Delta EMPDIF$ regressions, and the stronger effects of hostile takeovers on these variables once YRAFTH is included, suggest that the reductions in extramarginal wage payments were larger in the later takeovers. This is consistent with evidence in Kaplan and Stein (1991), showing that going-private transactions had higher prices relative to cash flow in the latter half of the 1980s, which suggests that in order to service the debt incurred in these later transactions, reductions in

the claims of other stakeholders (such as more-tenured workers) were more likely.

In order to reconcile the interaction results with the results for ΔEMPDIF and ΔWGDIF , we estimate the specifications shown in columns (3) and (4) for the two variables. To probe whether hostile takeovers had more effect on the wage slopes of employers with senior workers, we add the initial value of EMPDIF and its interaction with H to the change in profile equation, and perform the analogous exercise for the change in employment seniority equation. The results suggest that most of the reduction in extramarginal payments occurs through reductions in inter-job wage differentials by employers with high initial employment concentrations in upper-level jobs.

Contrary to expectations, the coefficient on H for ΔLEVME is positive and significant, and remains so when YRAFTH is added to the regression. Higher wages in the aftermath of a hostile takeover may reflect "house cleaning," as the new owners/managers fire some less-efficient (lower-wage) workers. Or, it may reflect an attempt to retain more-efficient workers who might otherwise quit when they see abrogation of implicit wage contracts with senior workers, or whose level of job stress is now higher.²² Alternatively, such an increase might occur if Lazear contracts were eliminated and many senior workers left. It may also reflect the incorporation of the target into a new, larger entity, since wage levels (for reasons poorly understood) are higher in larger firms. As some of these explanations suggest, this result also implies that overall employer wage differentials may not consistently

²²It could also reflect a failed attempt by the previous management to avoid the hostile takeover by raising wages enough to scare off the predator. However, this argument is a bit tenuous since wages at this level of the corporate hierarchy are generally not contractual obligations of the firm.

reflect extramarginal wages.²³ Finally, it is also possible that new efficiencies instituted after hostile takeovers raise profits sufficiently that workers' shares of profits rise afterward.

Table 6 shows the same set of regressions with additional controls for nonhostile mergers and acquisitions, M and YRAFTM. Including these variables does not substantially change the signs and standard errors of the coefficients on H and YRAFTM. Especially interesting is the fact that none of the estimated coefficients of the dummy variable for nonhostile mergers is significant and, in most cases, the signs are opposite those on H. Thus, the reductions in employment concentrations in senior positions and in employment-weighted inter-job wage differences, and the increases in median wage levels following hostile takeovers, do not occur following mergers and acquisitions.

The preservation of the negative and significant coefficient on H in the $\Delta WGXEM$ regression and on $H*EMPDI$ in the $\Delta WGDIF$ equation are consistent with the hypothesis that post-hostile-takeover restructuring results in an expropriation of extramarginal payments to the more-tenured workers of firms, particularly through reductions in returns to seniority by employers with high initial concentrations of senior workers.

It is interesting that we find much stronger evidence in support of the wealth-transfer hypothesis in the ex post analysis than we do in the ex ante results. We offer two explanations for the apparent discrepancy. First, we never observe the entire takeover target (because it includes units and occupations outside the CSS), so the conditions in the rest of the target that attracted the predator may not be highly correlated with what we do observe.

²³Alternatively, it is possible that these firm-wide extramarginal wage payments are not reduced following hostile takeovers, while those paid to more-tenured workers are decreased. Since the two types of extramarginal wage payments may have different sources, it is conceivable that only the expropriation of the latter type is profitable from the perspective of an acquiring firm.

Second, Lazear quasi-rents may be particularly difficult for outsiders to observe. Thus, other more-visible factors may motivate hostile takeovers. These explanations do not invalidate our ex post findings. Rather, they suggest that the impacts we detect may not have been the primary attractions behind the initial offers, but after the takeover, new owners extract Lazear quasi-rents wherever they are found.

VI. Interviews With Hostile Takeover Targets

When we had obtained a complete set of results, we contacted each hostile takeover target in our sample, in order to add qualitative evidence to our findings. We were able to speak with current personnel officers at five of the seven targets.

After confirming the history of the takeover, we asked open-ended questions about the adjustment process and ensuing changes in personnel policy. In all cases, the respondents reported substantial employment restructuring, continuing for as long as three years after the takeover. In each case, most of the effort was directed at thinning out middle management. Often, employees were encouraged to leave through early retirement plans or "voluntary separation agreements" (including severance payments based on seniority). In three cases, a substantial number of workers were laid off or fired, after an evaluation of each person and his/her position.

Despite the indications in the previous section, the respondents reported that wage levels and differentials were not usually explicitly redesigned after the takeover. However, some benefit packages and incentive programs underwent substantial change to bring them in line with parent company policies. A number of respondents reported that pension plans proved hardest to redesign or replace, so the plans were usually left intact. Finally, we note that four of the seven respondents have had another change of

ownership since the hostile takeover. Such resales are expected if implicit contracts are efficient, and violating them damages credibility in the future.

VII. Conclusion

Using firm- and establishment-level data, this paper conducts ex ante and ex post tests of two wealth-transfer hypotheses for the motivation behind hostile takeovers: that acquirers extract shared rents from workers, and that acquirers extract quasi-rents from senior workers with Lazear-type employment contracts.

The ex ante results do not provide evidence that extramarginal wages paid to all workers in a firm, or to more-tenured workers, are associated with subsequent hostile takeovers. Furthermore, we find ex post evidence that employer wage differentials rise after a hostile takeover, in contradiction of the hypothesis that hostile takeovers extract rents from workers, or of the supposition that firm differentials represent shared rents.

However, our ex post results are consistent with the hypothesis that hostile takeovers (but not other types of mergers) reduce extramarginal wage payments to senior workers, mostly through cutbacks in the steepness of wage-seniority profiles in firms with high concentrations of employment in senior positions. Moreover, the signs of the estimated coefficients from the ex ante analysis are consistent with the targeting of such payments in hostile takeovers (i.e., the interaction between seniority of the work force and the slope of the wage profile is positively associated, ex ante, with hostile takeovers), even if the results are not statistically significant.

These ex post results are consistent with findings that pension plan terminations and reversions of excess assets--entailing breaches of implicit Lazear-type contracts--are more likely to follow hostile takeovers. In addition, since our analysis also tests the Lazear contract hypothesis itself,

these results also suggest that steep wage-seniority profiles reflect extramarginal wages.

One implication of these findings is that an active market for corporate control may weaken Lazear-type bonding solutions to the effort-extraction problem in the labor market. If the prices at which hostile takeovers are transacted reflect the costs and benefits of this bonding, then these results do not necessarily imply any inefficiency generated by the market for corporate control. But if hostile takeover premia indicate short-term gains from expropriating extramarginal wages, but not longer-term costs of the reduction of this bonding activity, then hostile takeovers may ultimately prove destructive, if the interests of senior workers cannot be credibly protected. Since employees in some companies may come to discount the value of their long-term implicit contracts as other companies abrogate theirs, the social costs of hostile takeovers may not be reflected in the prices at which firms trade.

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Appendix A: Occupation Families in the GSS

Family	Level	
1. Economists	1	Economist 1
	3	Economic Advisor
2. Administrators	1	Admin. Assistant 1
	2	Admin. Assistant 2
	3	Admin. Assistant 3
3. Secretaries	1	Stenographer
	2	Secretary 1
	3	Secretary 2
	4	Secretary 3
4. Security	5	Executive Secretary
	1	Security Guard 1
	2	Guard Supervisor
	4	Protection Manager
5. EDP Audit	1	EDP Audit Analyst 1
	2	EDP Audit Analyst 2
	3	Internal Audit Manager
6. Audit Analysts	1	Audit Analyst 1
	2	Audit Analyst 2
	3	Audit Analyst 3
7. Programmer/ Analysts	1	Programmer/Analyst 1
	2	Programmer/Analyst 2
	3	Programmer/Analyst 3
	4	Systems Analyst 1
8. Computer Operators	5	Consulting Analyst
	1	Computer Operator 1
	2	Computer Operator 2
	3	Lead Operator
	3	Programmer 1
9. Programmers	2	Programmer 2
	2	Maintenance Mechanic 1
10. Mechanics	2	Maintenance Mechanic 2
	3	Chief Maintenance Mechanic
	1	Building Engineer 1
11. Building Engineers	2	Building Engineer 2
	3	Chief Building Engineer
	1	Payroll Clerk 1
12. Payroll Clerks	2	Payroll Clerk 2
	3	Payroll Supervisor
	1	Statistical Clerk 1
13. Statistical Clerks	2	Statistical Clerk 2
	1	Check Processing Clerk 1
14. Check Processors	2	Check Processing Clerk 2
	3	Check Processing Clerk 3
	1	Accounting Supervisor
15. Accounting	2	Accounting Manager
	1	Employee Benefits Counselor
16. Benefits	2	Benefits Administrator
	1	Data Processing Supervisor
17. Data Processing	2	Data Processing Manager

Table 1A: Ex Ante Observations, Complete Sample

	<u>Whole Sample</u>	<u>Noncandidates</u>	<u>Unmerged Candidates</u>	<u>Mergers/Acquisitions</u>	<u>Hostile Takeovers</u>
Number of observations	133	34	71	21	7
City					
Cleveland	46	11	23	8	4
Cincinnati	37	7	21	7	2
Pittsburgh	50	16	27	6	1
Year entered survey					
1980	78	21	39	12	6
1981	6	1	3	2	-
1982	5	3	1	1	-
1983	7	3	3	1	-
1984	5	3	1	1	-
1985	7	1	4	1	1
1986	7	1	5	1	-
1987	4	1	3	-	-
1988	4	-	3	1	-
1989	1	-	1	-	-
1990	5	-	4	-	-
1991	4	-	4	-	-
One-digit industry					
Mining and construction	1	-	1	-	-
Non-durable manufacturing	17	-	12	2	3
Durable manufacturing	35	-	23	10	2
Transportation, communication, and utilities	14	9	5	-	-
Trade	4	-	1	2	1
Finance, insurance, and real estate	38	3	28	6	1
Miscellaneous services	1	-	1	-	-
Professional services	11	10	-	1	-
Government	12	12	-	-	-
Employer wage and employment differentials					
LEVMEI (wage level)	-0.026 (0.167)	0.009 (0.142)	-0.027 (0.173)	-0.067 (0.192)	-0.065 (0.116)
WGDIF (wage profile steepness)	0.009 (0.104)	-0.022 (0.086)	0.018 (0.113)	0.037 (0.089)	-0.020 (0.113)
EMPDIF (employment seniority)	-0.041 (0.596)	-0.087 (0.601)	-0.027 (0.620)	-0.042 (0.591)	0.035 (0.374)

Cell counts are reported, except for employer wage and employment differentials, where means are reported, with standard deviations in parentheses.

Source: Authors' calculations.

Table 18: Ex Ante Observations, Sample with Unionization and Employer Size Data

	<u>Whole Sample</u>	<u>Noncandidates</u>	<u>Unmerged Candidates</u>	<u>Mergers/Acquisitions</u>	<u>Hostile Takeovers</u>
Number of observations	97	26	56	8	7
City					
Cleveland	35	8	21	2	4
Cincinnati	24	5	15	2	2
Pittsburgh	38	13	20	4	1
Year entered survey					
1980	56	17	28	5	6
1981	2	-	2	-	-
1982	3	2	1	-	-
1983	5	2	2	1	-
1984	4	3	1	-	-
1985	5	1	2	1	1
1986	5	-	5	-	-
1987	4	1	3	-	-
1988	3	-	3	-	-
1989	1	-	1	-	-
1990	5	-	4	1	-
1991	4	-	4	-	-
One-digit industry					
Mining and construction	1	-	1	-	-
Non-durable manufacturing	11	-	8	1	3
Durable manufacturing	20	-	15	3	2
Transportation, communication, and utilities	12	7	5	-	-
Trade	3	-	1	1	1
Finance, insurance, and real estate	32	3	25	3	1
Miscellaneous services	1	-	1	-	-
Professional services	8	8	-	-	-
Government	8	8	-	-	-
Employer wage and employment differentials					
LEVWED (wage level)	-0.038 (0.151)	0.000 (0.145)	-0.043 (0.161)	-0.107 (0.101)	-0.065 (0.116)
WGDIF (wage profile steepness)	0.013 (0.109)	-0.024 (0.086)	0.022 (0.116)	0.090 (0.080)	-0.020 (0.113)
EMPDIF (employment seniority)	-0.054 (0.588)	-0.112 (0.670)	-0.052 (0.607)	0.042 (0.314)	0.035 (0.374)
UNION (unionized)	0.37	0.73	0.23	0.25	0.29
SIZE (firm size)	2758 (8826)	5900 (16363)	1797 (2744)	1009 (1158)	773 (389)

Cell counts are reported, except for employer wage and employment differentials, where means are reported, with standard deviations in parentheses.

Source: Authors calculations.

Table 2: Ex Post Observations

	Whole Sample	Nonmergers	Mergers/Acquisitions	Hostile Takeovers
Number of observations	121	101	14	6
City				
Cleveland	46	38	4	4
Cincinnati	35	29	5	1
Pittsburgh	40	34	5	1
Year of takeover or merger/acquisition				
1981	2	-	2	-
1982	1	-	-	1
1983	1	-	-	1
1984	2	-	2	-
1985	4	-	3	1
1986	2	-	2	-
1987	2	-	-	2
1988	2	-	2	-
1989	2	-	2	-
1990	1	-	1	-
1991	1	-	-	1
One-digit industry				
Mining and construction	-	-	-	-
Nonurable manufacturing	15	11	2	2
Durable manufacturing	34	25	7	2
Transportation, communication, and utilities	11	11	-	-
Trade	4	1	2	1
Finance, insurance, and real estate	36	32	3	1
Miscellaneous services	1	1	-	-
Professional services	10	10	-	-
Government	10	10	-	-
Changes in employer wage and employment differentials				
Δ LEVWED (wage level)	0.033 (0.097)	0.030 (0.090)	-0.001 (0.128)	0.151 (0.038)
Δ WGDI (wage profile steepness)	-0.022 (0.117)	-0.020 (0.110)	-0.043 (0.125)	0.004 (0.204)
Δ EMPDIF (employment seniority)	0.004 (0.743)	0.030 (0.716)	-0.005 (0.870)	-0.422 (0.913)
YRAFTH (mean no. of years observed after hostile takeover)	-	-	-	3.7
YRAFTH (mean no. of years observed after merger/acquisition)	-	-	2.5	-

Cell counts are reported, except for changes in employer wage and employment differentials, where means are reported, with standard deviations in parentheses.

Source: Authors' calculations

Table 3: Probit Estimates of the Effects of Ex Ante Wage and Employment Structure on Hostile Takeovers

	Excludes Noncandidates and Firms With Missing Observations On Union Status and Firm Size									Excludes Noncandidates Only			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
LEVVED (wage level)	-0.35 (1.43) [0.09]	-	-	-	-0.54 (1.50) [0.08]	-	-	-	-0.61 (1.62) [0.08]	-0.54 (1.20) [0.06]	-	-	-
WGDIF (wage slope)	-	-2.19 (1.94) [0.06]	-	-	-	-2.21 (1.98) [0.06]	-	-	-2.08 (2.06) [0.06]	-	-1.83 (1.85) [0.05]	-	-
WGXEM (wage bill)	-	-	0.92 (1.29) [0.13]	-	-	-	0.92 (1.30) [0.12]	-	0.93 (2.65) [0.12]	-	-	0.83 (1.25) [0.09]	-
EMPDIF (empt. seniority)	-	-	-	0.14 (0.40) [0.11]	-	-	-	0.16 (0.41) [0.11]	-0.08 (0.78) [0.08]	-	-	-	0.10 (0.35) [0.08]
UN (union)	-	-	-	-	0.29 (0.51) [0.12]	0.08 (0.51) [0.10]	0.26 (0.49) [0.12]	0.26 (0.49) [0.12]	0.18 (0.54) [0.11]	-	-	-	-
LNSIZE (size)	-	-	-	-	-0.14 (0.22) [0.07]	-0.16 (0.22) [0.07]	-0.12 (0.23) [0.07]	-0.13 (0.22) [0.07]	-0.14 (0.23) [0.07]	-	-	-	-
Log like- lihood	-22.83	-22.19	-22.59	-22.80	-22.55	-21.95	-22.36	-22.54	-21.72	-25.19	-24.78	-25.06	-25.25
Prob. at means	0.10	0.10	0.10	0.10	0.09	0.09	0.10	0.10	0.09	0.07	0.07	0.07	0.07

LEVVED = employer-specific level of median wage; WGDIF = employer-specific inter-job-level wage difference; WGXEM = interaction between WGDIF and EMPDIF; EMPDIF = employer-specific inter-job-level employment difference; UN = union status (=1 if 25% unionized); LNSIZE = log of firm size. Standard errors are reported in parentheses. The intercepts are not reported. In columns (1)-(9), there are 7 hostile takeovers and 64 other observations. In columns (10)-(13), there are 7 hostile takeovers and 92 other observations. The last row reports the estimated probability of a hostile takeover at the sample means; the numbers in square brackets report the estimated probability following a one-standard-deviation increase in the variable in each row, holding the other variables at the sample means.

Source: Authors' calculations.

Table 4: Probit Estimates of the Effects of Ex Ante Wage and Employment Structure on Hostile Takeovers

	Excludes Firms With Missing Observations on Union Status and Firm Size									All Observations			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
LEVWED (wage level)	-0.69 (1.36) [0.06]	-	-	-	-0.55 (1.44) [0.06]	-	-	-	-0.59 (1.52) [0.05]	-0.79 (1.19) [0.04]	-	-	-
WGDIF (wage slope)	-	-1.52 (1.86) [0.05]	-	-	-	-2.03 (1.93) [0.04]	-	-	-1.92 (1.95) [0.04]	-	-1.32 (1.79) [0.04]	-	-
WGDEM (wage bill)	-	-	0.91 (1.25) [0.09]	-	-	-	0.84 (1.27) [0.09]	-	1.03 (2.18) [0.09]	-	-	0.82 (1.23) [0.07]	-
EMPDIF (empt. seniority)	-	-	-	0.16 (0.36) [0.09]	-	-	-	0.12 (0.38) [0.08]	-0.14 (0.62) [0.05]	-	-	-	0.12 (0.33) [0.06]
UN (union)	-	-	-	-	-0.05 (0.46) [0.06]	-0.26 (0.46) [0.05]	-0.08 (0.43) [0.06]	-0.09 (0.43) [0.06]	-0.17 (0.49) [0.05]	-	-	-	-
LNSIZE (size)	-	-	-	-	-0.15 (0.19) [0.05]	-0.16 (0.19) [0.04]	-0.15 (0.19) [0.05]	-0.15 (0.19) [0.05]	-0.17 (0.20) [0.04]	-	-	-	-
Log like- lihood	-25.01	-24.80	-24.87	-25.04	-24.62	-24.11	-24.46	-24.64	-23.85	-27.20	-27.14	-27.20	-27.35
Prob. at means	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.06	0.05	0.05	0.05	0.05

See footnotes to table 3 for details. In columns (1)-(9), there are 7 hostile takeovers and 90 other observations. In columns (10)-(13), there are 7 hostile takeovers and 126 other observations.

Source: Authors' calculations.

Table 5: Effects of Hostile Takeovers on Firm Wage and Employment Structure

	Dependent Variables:											
	Wage Level Change		Wage Bill Change		Wage Profile Steepness Change		Employment Seniority Change					
	(1)	(2)	(1)	(2)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Intercept	0.025 (0.009)	0.013 (0.022)	0.013 (0.022)	0.013 (0.022)	-0.021 (0.011)	-0.021 (0.011)	-0.021 (0.011)	-0.021 (0.011)	0.015 (0.069)	0.015 (0.069)	0.009 (0.069)	0.009 (0.069)
H (hostile takeover)	0.126 (0.039)	0.148 (0.059)	-0.179 (0.099)	-0.317 (0.148)	0.025 (0.049)	0.073 (0.059)	0.126 (0.073)	0.127 (0.073)	-0.423 (0.310)	-0.574 (0.467)	-0.474 (0.314)	-0.691 (0.482)
YRAFTH	-0.006 (0.012)	-0.038 (0.030)	-	-0.013 (0.015)	-	-	-0.001 (0.015)	-0.001 (0.015)	-	0.038 (0.095)	-	0.057 (0.097)
EMPOIF*	-	-	-	-	-	0.009 (0.018)	0.009 (0.019)	0.009 (0.019)	-	-	-	-
WGDIF*	-	-	-	-	-	-	-	-	-	-	0.748 (0.666)	0.748 (0.668)
HAEMPOIF*	-	-	-	-	-	-0.675 (0.222)	-0.672 (0.233)	-0.672 (0.233)	-	-	-	-
HAWGDIF*	-	-	-	-	-	-	-	-	-	-	-3.176 (2.768)	-3.510 (2.832)
R ²	0.079	0.081	0.026	0.039	0.002	0.008	0.074	0.074	0.016	0.017	0.033	0.036

H = dummy variable indicating hostile takeover; YRAFTH = years elapsed after hostile takeover (=0 for other firms). See footnotes to table 3 for other variable definitions. Standard errors are reported in parentheses. There are 123 observations.

*Initial value of variable indicated.

Source: Authors' calculations.

Table 6: Differential Effects of Hostile Takeovers and Mergers and Acquisitions on Firm Wage and Employment Structure

	Dependent Variable:										
	Wage Level Change Δ LEVME		Wage Bill Change Δ WKBEM		Wage Profile Steepness Change Δ WGDIF		Employment Seniority Change Δ EMPDIF				
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(3)	(4)	
Intercept	0.030 (0.009)	0.030 (0.009)	0.017 (0.023)	0.013 (0.022)	-0.021 (0.011)	-0.020 (0.011)	-0.020 (0.011)	0.030 (0.074)	0.030 (0.074)	0.025 (0.074)	0.026 (0.075)
H (hostile takeover)	0.121 (0.039)	0.143 (0.058)	-0.183 (0.099)	-0.317 (0.148)	0.073 (0.074)	0.125 (0.059)	0.127 (0.075)	-0.452 (0.312)	-0.589 (0.471)	-0.491 (0.317)	-0.708 (0.489)
M (nonhostile takeover)	-0.031 (0.028)	-0.066 (0.034)	0.021 (0.070)	0.020 (0.087)	0.008 (0.054)	-0.022 (0.042)	0.008 (0.035)	0.035 (0.219)	0.009 (0.276)	-0.080 (0.235)	-0.025 (0.284)
YRAFTH	-	-0.006 (0.012)	-	0.038 (0.030)	-	-0.013 (0.015)	-	-	0.038 (0.096)	-	0.057 (0.098)
YRAFTM	-	0.015 (0.009)	-	0.009 (0.023)	-	-0.013 (0.011)	-	-	-0.019 (0.072)	-	-0.025 (0.073)
EMPDIF*	-	-	-	-	-	0.008 (0.020)	0.008 (0.020)	-	-	-	-
WGDIF*	-	-	-	-	-	-	-	-	-	0.591 (0.727)	0.591 (0.732)
HxEMPDIF*	-	-	-	-	-	-0.673 (0.222)	-0.671 (0.233)	-	-	-	-
MxEMPDIF*	-	-	-	-	-	0.003 (0.054)	0.007 (0.054)	-	-	-	-
HxWGDIF*	-	-	-	-	-	-	-	-	-	-3.018 (2.803)	-3.352 (2.879)
MxWGDIF*	-	-	-	-	-	-	-	-	-	0.628 (2.186)	0.726 (2.218)
R ²	0.088	0.112	0.028	0.042	0.006	0.024	0.080	0.091	0.017	0.019	0.033

H = dummy variable indicating hostile takeover; YRAFTH = years elapsed after hostile takeover (=0 for other firms);
 YRAFTM = years elapsed after nonhostile takeover (=0 for other firms). See footnotes to table 3 for other variable definitions.
 Standard errors are reported in parentheses. There are 121 observations.

*Initial value of variable indicated.

Source: Authors' calculations.