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DO UNION WEALTH CONCESSIONS EXPLAIN TAKEOVER PREMIUMS? THE EVIDENCE ON CONTRACT WAGES

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ABSTRACT

I estimate changes in levels of union real wage growth associated with corporate takeovers and accompanying chief executive officer changes. The effects are statistically insignificant. The results are used to construct union wealth changes associated with corporate control events. Target firm shareholder wealth premiums are estimated using a simple market model. The union and shareholder wealth changes are compared, and I conclude that transfers of wealth from unions to shareholders are not an economically significant explanation of shareholder wealth premiums.

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Empirical studies show that target firm shareholder wealth increases when a takeover occurs.[1] The sources of the wealth increase are not well understood. Most explanations of takeover premiums fall into one of two categories: efficiency or transfer. The efficiency hypothesis states that newly expected reallocation of poorly performing resources into more highly valued uses causes upward revision of the expected return to owning those resources. The share price is bid up, and target firm shareholders receive the premium. The alternative, or transfer, hypothesis attributes target firm shareholder wealth increases to transfers from other parties with an interest in the firm. The transfers are expected to occur as a result of the takeover, and this expectation raises share prices. Both explanations may play a role in the observed target firm shareholder wealth gains. Other hypotheses attribute takeover premiums to increased product market power[2] or irrationality in stock pricing.

This study uses both stock market and contract wage data to determine the extent to which target firm shareholder wealth increases are accounted for by transfers from unions. The changes in union wage growth associated with takeovers and accompanying chief executive officer (CEO) changes[3] are estimated and the implied union wealth changes associated with takeovers are calculated. These wealth changes are then compared to the corresponding wealth changes experienced by target firm shareholders in association with takeovers.

For companies traded on the New York or American Stock Exchanges and for which the Bureau of National Affairs reported at least one union contract settlement between 1976 and 1987, the data indicate the following regarding the effect of takeovers and CEO changes on the level of the rate of growth of real wages measured in percentage terms: the lower bound for a takeover

accompanied by a CEO change is about -1% per year. Hence if real wage growth for the firm was .5% per year prior to the takeover/CEO change, it would be -.5% per year afterwards. The lower bound for a takeover alone is about -.6% per year. The upper bounds are, roughly, +.3% following a takeover alone, and no effect for a takeover with a CEO change.

Lower bound estimates come from regressions that do not allow sector-specific takeover effects, so they are essentially sample mean effects. The lower bounds are statistically insignificant. Upper bounds come from specifications that allow sectoral disaggregation of effects. Upper bound estimates vary both between sectors and within sector across specifications, but within the largest sectors (durables and non-durables manufacturing) estimates are statistically significant and generally more stable than for the lower bounds.

The wage effects are economically insignificant in the sense that the implied union wealth gains or losses are small relative to shareholder wealth gains at the time of a takeover announcement. Under assumptions designed to give the transfer hypothesis the greatest chance of success and using statistically insignificant parameter estimates from specifications not favored by the data, the median value of the shareholder wealth gain that can be explained by union concessions is 12%. When the same assumptions are used in conjunction with results least favorable to the transfer hypothesis, union wealth actually increases following a takeover, by up to 4% of the value of the shareholder wealth premium. Given these extremes and the assumptions that generate them, I conclude that transfers of union wealth do not explain target firm shareholder gains.

The paper has six sections. Section I summarizes related literature and indicates the contribution of this paper. Section II describes the data. Section III addresses the representativeness of the sample. Section IV presents the empirical findings regarding contract wage growth rates and takeovers. Section V compares union wealth changes associated with takeovers to target firm shareholder wealth changes. Section VI draws conclusions.

Section I.

Theories regarding the market for corporate control support both the transfer and efficiency hypotheses for takeover premiums. From the shareholder's point of view, management performs poorly in two ways: either resources are inefficiently used, or the portion of the wealth generated by the ongoing operations of the firm distributed to shareholders is too small. Monitoring costs can cause either sort of poor performance to be tolerated by shareholders to some degree.[4] If performance is sufficiently bad that a third party expects the gain from taking over the firm and improving performance to exceed the cost of the takeover, the market for corporate control provides external disciplining incentives.[5] Competition in the market for corporate ownership ensures that target firm share prices are bid up to reflect the expected increase in the value of owning those shares in the post-takeover environment (less the costs of the takeover).[6]

Event studies of takeovers do not generally address the reasons for the takeover directly, but they do strongly support the role of the market for corporate control in transferring to current shareholders the present value of some expected increase in income from owning an asset. Some explanations, such as that of Manne (1965), attribute the large premiums to efficiency.

However, there are relatively few empirical studies that try to address efficiency. Lichtenberg and Siegel (1987, 1989) find evidence for increased productivity following ownership changes. They examine measures such as total factor productivity using large samples of establishment level Census Bureau data. Ravenscraft and Scherer (1988) reach the opposite conclusion in an examination of pre- and post-merger performance based on accounting measures of profitability. Healy, Palepu, and Ruback (1989) argue that cash-flow is a better accounting measure of post-merger performance. They find post-merger improvements in asset productivity.

Other authors have emphasized the role of transfers in explaining shareholder wealth increases. The potential sources of wealth for such transfers include the following: unions (which will be examined here) and other employees;[7] bidding firm shareholders;[8] the government (in the form of tax benefits which could not be realized without the takeover);[9] management;[10] over-funded pension funds that may be raided by new management;[11] bondholders, if the risk of default on pre-existing bonds is increased by the takeover (as when a highly leveraged takeover is based on junk-bond financing); and suppliers of inputs other than labor. Most careful empirical studies of potential sources of transfers have not found systematic explanations across many takeovers of more than a small fraction of the takeover premium.

The additional piece of the puzzle examined here, the role of union wealth concessions, has been emphasized by Shleifer and Summers (1988). They consider the welfare and efficiency implications of hostile takeovers which impair the ability of managers and employees to form implicit contracts. They make two arguments supporting the notion that hostile takeovers are inefficient. First,

> To take advantage of implicit contracts, shareholders must be trusted by potential stakeholders. Otherwise, stakeholders would expect breach whenever it raises the firm's value and would never enter into implicit contracts. (p.38)

A hostile takeover is one in which new managers replace old managers and breach implicit contracts. The fact that such an event may occur leads to ex ante inefficiency as the ability to contract implicitly is compromised. This proposition is difficult to test as all firms face the same environment in the market for corporate control and observable differences pre- and post-takeover are not relevant.

Second, they argue that breaches can cause ex post inefficiency. They examine two aspects of this story: first, problems such as informational asymmetries between the new owner and employees in an environment of lack of trust lead to inefficient contracting. Second, transfers may account for a large fraction of the shareholder wealth premium. When both of these conditions hold, there is ex post inefficiency.

The weakness in the Shleifer and Summers argument is that transfers may not explain the premium. If factors such as increases in productive efficiency in dimensions other than labor explain the premium, then the net effect of the takeover could be a substantial efficiency increase. Hence establishing the magnitude of transfers is important to the ex post inefficiency case.

Shleifer and Summers do not restrict their analysis to union concessions. However, if takovers occur in order to appropriate rents, then the breach/transfer model they posit should leave a trace in observed union wages.

This paper contributes to the takeover literature by providing an

additional piece of evidence regarding the sources of observed target firm stock value premiums.

Section II.

This section describes the data sources and the construction of the data set used here, and it provides tables of descriptive statistics. The principal sources of data used in this study are the Bureau of National Affairs' (BNA) *Collective Bargaining Negotiations and Contracts (CBNC)*[12] file, which records the details of labor contract settlements, and the University of Chicago's Center for Research in Security Prices (CRSP) files, which cover stock prices, quantities, and related information. These sources jointly determined the primary sample analyzed in the next section and supplied the basic information necessary for calculating employee and shareholder wealth. The unit of observation on the BNA file is a contract settlement between a company (or employer organization) and a union. The principal data items used from the BNA file were company and union identifiers, an industry code (SIC), the settlement, effective, and expected expiration dates of the contract (expected length is calculated as expected expiration date minus effective date), the wage at the end of the previous (expiring) contract (W_0), immediate and scheduled deferred wage increases and the dates on which they became effective, information regarding cost of living adjustment (COLA) clauses, and the number of employees covered. CRSP company identification numbers (CUSIPs)[13] were matched to the BNA records by hand using as much relevant identifying information as possible from both sources. The resulting sample of 5,353 contracts signed by 1,009 companies over the period January 1, 1976 to June 30, 1987 includes all companies for which at least one contract

settlement was reported by the BNA between January 1976 and June 1987, and which could be matched to the CRSP tapes covering the New York Stock Exchange (NYSE) and the American Stock Exchange (AMEX) through December, 1986. Hence both wage and stock information are available for all observations in the sample.

Wage profiles, rates of wage growth, and contract costs over the lives of the contracts were produced using the BNA data and auxiliary information. In general, I assume that expectations formed at the time of the contract settlement follow the simple rule that growth rates will persist at the level observed over the 12 months prior to the settlement. The nominal wage profile starts with W_0, the wage at the end of the previous contract (as recorded by the BNA at the time of the settlement), and information regarding the contract settlement is used to project the wage level in each month of the contract. For example, a 36 month contract has $W_1,\ldots,W_{36}$ in addition to W_0. The projected wage profile reflects all immediate and scheduled deferred wage increases as of the dates specified in the contract, and the COLA if it is present and active. The COLA is evaluated using the 1967-based CPI and, as described above, the assumption that, at the time of the contract settlement, inflation is expected to persist at the rate observed over the previous 12 months.[14] The real wage profile, w_t, is constructed from the nominal profile using the CPI and the same assumption concerning the expected rate of inflation. The annual percentage growth rate of the real wage over the contract was calculated as

$$\dot{w} = 100 \times \left[\left(\frac{w_{36}}{w_0} \right)^{(12/36)} - 1 \right]$$

for a contract with and expected length of 36 months, and similarly for other lengths.

The expected contract cost per employee for each month, t, of the contract, was calculated using the projected wages and auxiliary information:

$$C_t = \frac{W_t \times (H+1.5xOT) \times (1+F) \times 4.3452}{\left[1 + \left(\frac{r}{100} \right) \right]^{(t+M)}}$$

where W_t is the nominal wage in month t of the contract, H is hours worked, OT is overtime hours,[15] F is the value of fringe benefits[16] as a fraction of payroll, 4.3452 is weeks per month, r is Moody's Baa corporate bond yield in the month of the settlement (converted to a monthly rate), and M is a correction for the time (in months) from the settlement to the effective date of the contract (H, OT, and F are all merged to the contract observations by two-digit SIC in the year of the settlement). Hence C_t is the expected current dollar present value of the contract cost per employee, as of the settlement date, for month t of the contract. The monthly costs can then be summed to produce the present value of the total contract cost or the cost for any sub-period within the contract. Real dollar figures use the CPI for deflation.

Table 1 summarizes the constant (1967) dollar wage, wage growth, and contract cost figures. The number of contracts observed by year and sector

can also be seen in this table. Employment weighted means and standard deviations are shown for all 5,353 observations as well as for each year (grouped by the settlement date of the contract), whether the contract was in manufacturing (SIC's 20-39) or not, and a further disaggregation into seven sectors. The wage at the end of the previous contract (W_0) and percentage annual wage growth figures jointly contain much of wage information from the BNA in compact form. Both are used in the regression specifications of Section IV. The wage growth figure serves as the dependent variable, and W_0 is used in conditioning variables. In addition, Table 1 shows the present value of the contract cost per employee-year in thousands of 1967 dollars. The union wealth changes calculated in Section V are based, in part, on these numbers.

Figures 1 and 2 give a visual presentation of the wage level and wage growth information in Table 1 for all companies, and manufacturing and non-manufacturing firms separately. Figure 1 shows the yearly (employment weighted) average real wage level at the end of the previous contract, and Figure 2 shows the corresponding real wage growth over the new contract. The variation in wage levels from year to year reflects both real factors, which are evident in the decline in wage growth seen in Figure 2, and the sectoral composition of new wage settlements, which varies substantially from year to year. The last seven columns of Table 4 show the sectoral variation over time in terms of employees covered.

Figures 3 and 4 present the same information at a finer degree of sectoral disaggregation. The main reason for presenting these figures is to show that most sectors behave similarly over this time-period, but also that airlines are evidently quite different from the other sectors. Part of the

difference in wage levels can be explained by the occasional appearance of a pilots' union contract, with wages an order of magnitude higher than other wages in the data set. However, this explanation is not so satisfactory for rates of real wage growth, which, as Figure 4 shows, persist at levels much greater than all other sectors for most of the sample period. As will be elaborated below, this data set is deficient with regard to post-takeover contract settlements in the airline industry, but the patterns presented in Figures 3 and 4 suggest that, even if results could be obtained for airlines, they would not be broadly representative of industry overall.

Table 2 provides a rough check of the wage profile predictions. It compares the realized levels of wages at contract expirations (W_0) in a given year with the predicted levels from contracts settled in previous years and due to expire in the given year. This rough check of the data indicates that the predicted wage profiles end up close to observed values just prior to the start of subsequent contracts. Apart from the first predicted figure, which is based on just 34 observations, the largest differences occur for contracts signed before and expiring during or just after the recession of the early 1980's. This is not surprising as expected and realized inflation (and other factors) differ over these years.

Takeover and related event information was developed as follows: using the CRSP tapes, a list was produced of all companies in the sample for which trading was suspended at any time after January 1973. This list served as the basis for collecting auxiliary files regarding takeovers and related events. Takeover event dates and information regarding hostility,[17] CEO[18] and other top management changes in conjunction with the takeover, acquirer, and form of acquisition were recorded from the *Wall Street Journal Index* (*WSJI*)

(1973–1987). Completion of takeover date and acquirer were obtained from the Commerce Clearing House, *Capital Changes Reporter* (1988). Announcement date, completion date, acquirer, and various comments regarding the bid were recorded from W.T. Grimm & Company, *Mergerstats Review* (1981–1987). All data sources were compared for corroboration of announcement and completion of takeover dates. In cases of inconsistency between sources, the source with the most explicit date and event-type reference was used for analysis. This rule dictated the use, in general, of announcement dates from the *WSJI* and completion dates from *Capital Changes Reporter*. *WSJI* completion dates or Grimm's announcement or completion dates were used on occasion, if they appeared to be more reliable in the given case.

The auxiliary files were used to merge the following corporate control events and dates to the contract information: takeovers, hostility in the takeover process, and CEO changes associated with the takeover. Table 3 summarizes the event indicators. Of the 1,009 companies represented in the data set, 288 were taken over after January 1973. In 43 cases, the *WSJI* indicated that the takeover was 'hostile' and that in 68 cases, a CEO change accompanied the takeover. The table also shows the distributions of contracts (as a single company can generate more than one contract at a time) with events indicated. The event information is set up to indicate not just that the event occurred, but whether it occurred before or after the current contract settlement date. Only the first two contracts following the takeover are considered. Very few contracts are observed beyond that point, and it is reasonable to expect that if takeovers occur because of the possibility of wage reductions, then a significant effect (economically as well as statistically) should be evident within the first six to nine years following

the takeover. The indicators for CEO and other management changes associated with a takeover take on the value 1 only if the takeover indicator also takes on the value 1, so these indicators capture the marginal effect of, for example, a CEO change, given that a takeover occurred.[19]

Section III.

This section discusses the representativeness of the sample. The sample covers a large proportion of both the total unionized work force in the U.S. and of all firms traded on the NYSE or AMEX. Furthermore, the frequency of takeovers does not appear to differ greatly between the sample and the CRSP population. Unfortunately, the number of observed contracts declines following a takeover. This may bias the results presented in the following sections, but there is some evidence that the potential bias is not severe.

The sample covers a substantial portion of the total unionized work force in the U.S. Table 4 shows the number of employees covered by contracts signed in each year of the sample (column 3) and the number of union members in the U.S. in the same year (column 2). All figures in the table are in thousands of union members. As most contracts last three years,[20] the number of employees covered in the BNA data in a given year can be estimated by summing the total coverage figures over three year windows (column 4). Column 5 shows the number of union members covered in the BNA data as a percentage of total U.S. union membership in the same year. More than 15% of the unionized work-force is covered in this study.

The 1,009 companies in the sample cover only about one fifth of the companies listed on the CRSP tapes covering the NYSE and the AMEX (the CRSP tapes covered 5,019 through 1986, but the number with valid information during

the sample period is somewhat smaller as companies no longer traded are not deleted from the tapes). Under the hypothesis that takeovers occur at least partly in response to rent-seeking opportunities, takeovers might be more prevalent among more highly unionized firms. The takeover information used for the empirical analysis presented in the next section is not exactly comparable to the CRSP information regarding trading stoppages, but Table 5 compares the frequencies of CRSP trading status codes for the three main categories (still trading, mergers, and exchanges) between companies in the sample and not in the sample. There is no apparent difference in the frequencies as the Chi-square test for homogeneity between the groups is only significant at the .30 level.

A potentially serious drawback in this data set is that fewer contracts are observed following takeovers than before. Identifying target company contracts in the post-takeover environment was done both by matching the names of units which retained their pre-takeover names and by checking contracts signed with acquiring companies to see if any could be re-assigned to the original company. The former procedure was most likely possible for divisions of target firms which were taken over and then continued to operate as a division of the acquiring firm while retaining their original names. The latter procedure resulted in only a few additional post-takeover contracts. I feel that most of the contracts that could be positively identified as being between the former target company and one of its unions were discovered by these methods.

Despite these efforts, the number of contracts observed per year at risk of reporting declines following a takeover. This measure has the advantage of controlling for the length of time over which reporting occurs. For a company

not taken over, the measure of contract observations per year at risk is $O_1=N_1/11.5$, where N_1 is the number of contracts observed for the given company, and 11.5 is the number of years from 1/1/76 to 6/30/87. For a company with a takeover, two additional statics are calculated: $O_2=N_2/T_2$, where N_2=contracts prior to the completion of the takeover, and T_2=the number of years from 1/1/76 to the completion of the takeover; and $O_3=N_3/T_3$, where N_3=contracts following the takeover, and T_3=the number of years from the completion of the takeover to 6/30/87.

Table 6 shows the distributions of these three statistics by giving the values of each at each quartile and extreme values, and the mean and standard deviation for each. The mean number of contracts observed per year declines by about half following a takeover. This would be an indication of a serious sample selection bias if the decline were due to dramatic reporting declines for some firms and no decline for others. In this case, wage growth rates could be highly correlated with reporting (if, for example, union-busting takeovers occur), causing a sample selection bias. However, if average observed contracts per year decline for all firms equally following the takeover, then such a bias would only be present if there was great variation in the effect of the takeover on wage growth within each firm, and the contracts with lower wage growth were the ones not reported. As the next section will show, there is little evidence that wage growth reductions are associated with the contracts actually observed following takeovers. Given this, the presence of other contracts from the same firms that feature both much larger wage reductions and non-reporting seems unlikely. A more plausible explanation for the case of proportional declines in observed

contracts is that matches are simply more difficult to make as the identifying information changes.

The former case, which would indicate a serious potential for sample selection bias, would cause the values of contracts per year at the lower percentiles to be lower for post- than for pre-takeovers, while the values at the upper percentiles would be unchanged. This would also increase the standard deviation in the post-takeover period. The implication of the latter case is that contracts per year would decrease proportionately at each percentile, and the standard deviation would be reduced. Table 6 supports the latter case, which reduces the worry of sample selection bias. The apparent relative decrease in observations per year in the lower percentiles of the post-takeover column reflects the fact that for many companies only one contract is observed, so zero contracts observed per year is reached at roughly twice the percentile rank as for the pre-takeover distribution. Finally, even if sample selection biases are present, they would have to be extremely severe to change the economic significance of the findings in Sections IV and V.

Despite the evidence that, overall, the reduction in observed contracts following takeovers may not be a problem, there are particular instances in which it is. Most notably, no post-takeover contracts could be found for the airline industry, which is one of the major examples in the Shleifer and Summers analysis. Hence no direct response can be made to their stylized example involving TWA. However, Figures 3 and 4 made it clear that airlines were a special case during the sample period.

Section IV.

This section provides empirical evidence on the pattern of union wage growth relative to takeovers and CEO changes. Employee wealth changes presented in Section V are based on the results shown in this section.

The primary goal in running the regressions presented here is to determine the extent to which wage growth is associated with takeovers and CEO changes, other things equal. The dependent variable is the compound annual percentage growth rate of the wage level over the contract, as described in Section II and summarized in Table 1. The relation between wage growth and corporate control events is explored using indicators for takeovers and CEO changes both independently and interacted with other regressors. These indicators are constructed as follows.

There are three mutually exclusive classes representing a takeover: no takeover for the given company either before or (up to two contracts) after the current contract settlement date; a takeover (up to two contracts) following the current settlement date (FTO); or a takeover prior to that date (PTO). The no-takeover category is omitted as the base case. If the takeover is associated in time with a change in regime regarding wages, then the difference between the parameter estimates associated with the two included indicators should capture it, and a test of their equality (PTO = FTO) is appropriate for determining such a difference. If the two parameters are not different from each other but are both different from zero, then there is some systematic difference between companies involved in takeovers and other companies, but the manifestation of this difference observable in wages does not change when the takeover occurs.

Additional indicators are included for a CEO change accompanying the takeover,[21] one for pre- (PTC) and one for post-takeover (FTC) contract wage settlements. These can only have a value of 1 if the corresponding pre- or post-settlement takeover indicator is equal to 1. The interpretation of the parameter estimates is the marginal effect of the CEO change, given that the takeover occurs. The total effect of the takeover and CEO change is the sum of the parameter estimates of takeover and CEO change. As in the takeover with no CEO change case, a test of the marginal association of the CEO change with wage growth is that the indicators for pre- and post-takeover joint with a CEO change are equal (PTC = FTC). Similarly, the total effect of a takeover joint with a CEO change can be judged by a test of PTO + PTC = FTO + FTC.

Previous work with these data (Rosett (1989)) has shown that, although there is some variation of the wage growth response to the takeover as time (contracts) passes, grouping the data by the number of contracts or years since the takeover produces poorly estimated and, when statistically significant, contradictory results. The data do not appear to be able to support any detailed tracing of wage effects over time past the takeover. The number of contracts observed at points more than two contracts following the takeover is small and wage effects for those contracts are generally insignificant. Also, differences between the wage coefficients when the takeover effect is limited to one contract or two are small, so I have chosen to estimate effects by pooling up to two contracts following the takeover. This choice is arbitrary, but unimportant with respect to parameter estimates.

Several regressors are included to control for economy-wide and sectoral conditions. Macroeconomic variables include (the rates of growth over the 12 months prior to the settlement of) the unemployment rate, the CPI, and real

(1982=100) GNP. In addition, the difference between the wage at the end of the expiring contract and the (two-digit SIC) industry average wage in the month of the settlement (expressed as a percent of the industry wage), and the log of the wage at the end of the expiring contract are included as well. All regressors are entered linearly and as squares. F-statistics for the joint significance of the squared terms indicate their inclusion at the .001 level.[22]

The summary statistics and graphs in the previous section indicate that real wage growth from 1976 to 1981 was changing dramatically, first rapidly falling and then rising. From 1982-1987, the pattern was much more stable and was characterized by slight erosion of real wages. The apparent change of regime beginning with the long expansion of the 1980's indicates that some sort of time effects may be useful as proxies for omitted regressors if the available macro- and industry-level information is not sufficient to explain much of the variance in wage growth. Similarly, sector indicators capture differences across sectors explained by fundamentals such as the regulatory climate in the given industry, the state of supply of raw materials or demand for the product of that industry (which may not be highly correlated across industries or sectors).

Two methods were tried for both time and industry effects. For time effects, indicators for the year of settlement of the contract (excluding 1976 as the base year) and a period effect for the 1980's expansion (effectively allowing a structural shift in 1982) were both tried. For industry differences, both including as many two-digit SIC indicators as possible and dividing the sample into seven larger sectors[23] (excluding services as the base) were tried.

Table 7 shows four specifications using the full sets of year and SIC indicators in addition to the macro- and industry-level quantitative regressors. Using the set of settlement year indicators is strongly supported by an F-test for their joint significance, which rejects the hypothesis that they are jointly zero at the .0001 level. The same is true for the full set of SIC indicators. In all four specifications, the point estimates on the pre- and post-takeover indicators support the hypothesis that wages rise more quickly than average prior to and less quickly than average following a takeover. However, the effects are uniformly small and statistically insignificant. For takeovers without CEO changes, the estimated level of wage growth declines from before to after a takeover by -0.36% per year (= -0.03 - 0.33) in the first specification, and is highest in the third specification (-.6% per year). The marginal effects of a CEO change with the takeover are as expected in the third and fourth equations, indicating even higher increases before and lower after the takeover. However, they are reversed in the first two specifications, and again all estimates are insignificant. If CEO change effects are included, the largest estimated decline in the level of wage growth is -1.04% per year (= -0.24 -0.03 - 0.31 - 0.46) in the fourth specification. Hence if real wage growth was 1.04% per year prior to a takeover (which, in the fourth specification, indicates that wages were growing .27% per year faster than they otherwise would have been), then the largest estimated effect would lower real wage growth to zero. In the first specification, the level of wage growth increases .12% per year following a takeover and CEO change.

In addition to being individually insignificant according to the t-statistics, the event indicator estimates are generally insignificant in

other tests. The F-statistic for the joint significance of the four indicators does not reject the hypothesis that they are all equal to zero in the first two specifications. For the first specification, that statistic is $F(4,5290) = .66$, with the probability of a greater F value of .62. The statistic is similar for the second specification. However, the four indicators are found to be jointly significantly different from zero in the last two specifications, with probability of a greater F of .02 and .04, respectively.

Tests of equality of the pre- and post-takeover parameter estimates, as discussed earlier in this section, also show mixed results. None of the three tests reject the null for the first two specifications. The highest level of significance among the six tests is for the test that the pre-takeover and post-takeover indicators are the same in the second equation, which has a probability of a greater F value of .10. In the last two equations, equality of the pre- and post-takeover indicators is rejected in both cases (at the .02 and .04 levels, respectively), while equality of the pre- and post-CEO change estimates are not significant even at the .10 level. The last test, for the equality of the total effects of takeover and CEO-change pre- and post-takeover, is significant at the .02 level in the third and fourth specifications.

Overall, these findings do not support a statistically significant relation between takeovers (with or without CEO changes) and wage growth. The first specification is most strongly favored by tests for the inclusion of year and SIC indicators. This specification shows a decline in wage growth following a takeover alone, and an increase following a takeover and CEO change. All significance tests indicate no statistical relationship in this

specification. All point estimates are insignificant in all specifications, and a few joint tests are significant in unfavored specifications.

One objection to these results is that no account is taken of sector specific variation in takeover effects. Such effects can not be examined using the full sets of year and industry indicators, as degrees of freedom are quickly used up in year/SIC cross-effects. Instead, the specifications shown in Table 8 are based on period and sector groupings, and they appear to capture most of the variance explained by the year and SIC indicators. These specifications have the advantage of allowing cross-effects to be estimated and tested.[24] These cross-effects may be important if, for example, the regulatory climate changes within an industry over time (as it did in airlines and trucking).

The first specification in Table 8 has the interpretation of allowing a structural break in 1982 (for all aspects of wage determinants other than takeovers) as the period indicator is interacted with the other regressors as well. An F-test for the joint significance of the period indicator and its interactions with the other regressors has the interpretation of a Chow test[25] for the structural shift. In a specification excluding all takeover and CEO change indicators, this test is significant at the .0001 level.

The sector indicators exclude the FIRE (Finance, Insurance, and Real Estate) and other services sector as the base. Hence if interactions of the six included sectors and the four takeover-related indicators are made, there are 24 possible indicators which capture the marginal effects of each event in each sector, in addition to the main effects. As the four uninteracted event indicators and the six uninteracted sector indicators are included as well,

the base case is a service sector contract for a firm that does not experience a takeover between 1973 and 1987.

In Table 8, the change in wage growth within a sector from before to after a takeover (with no CEO change) is the difference between the uninteracted takeover estimates plus the difference between the interacted takeover estimates within the given sector. For example, in the first specification, the effect of a takeover (with no CEO change) in durables manufacturing is (-1.99) - (+.45) + (+2.77) - (0) = .33. The overall effect of a takeover in durables is actually to increase wage growth slightly.

Table 9 summarizes the wage growth changes from before to after takeovers (and CEO changes) by industry for industries for which all necessary coefficients could be estimated. The increased wage growth result found for the first specification in durables manufacturing is found in two other specifications for durables, and in all four cases for both non-durables and transportation/utilities. When CEO change effects are included as well, the signs reverse in all sectors for which estimates of all parameters could be obtained, but the effects are modest.

Only mining shows a relatively large wage effect in the expected direction (for the transfer hypothesis), and then only for takeovers without a CEO change. Table 3 shows that mining accounts for only 2.5% of the contracts in the sample. The point estimates for mining are not significantly different from zero.

The results shown in tables 8 and 9 again do not support wage growth reductions following takeovers. Rather, the opposite now appears to be true in many cases. Further, the point estimates are more stable across specifications and in many cases statistically significant at the 5% level in

Table 8. However, the magnitudes of the effects are again quite small. There is evidence that a CEO change with the takeover reduces wage growth in manufacturing, but this result is reversed in mining.

The results of this section do not support the hypothesis that union wages are strongly affected by takeovers. Estimates of the change in the level of real wage growth from before to after a takeover (and CEO change) range from a reduction of about 1% to and increase of about .3%. The estimates of reductions are statistically insignificant, and the results are not robust to the specification. Results also differ across sectors. The most reasonable conclusion is that there is no clear pattern of wage growth changes associated with takeovers and CEO changes.

Section V.

This section compares union wealth changes calculated from the results of Section IV to target firm shareholder wealth changes associated with takeovers. Estimates of union wealth changes are made under assumptions designed to show the largest possible concessions or gains given the results of the previous section. Target firm shareholder wealth premiums are estimated using a simple market model, and the magnitudes of the premiums are well within the normal range. This procedure gives the transfer hypothesis its best chance to explain takeover premiums. It also allows some perspective on the economic significance of the findings in the previous section.

Employee wealth changes associated with takeovers are calculated as follows: Real contract costs per employee-year were calculated for each contract (see Table 1). These are used to find the average real cost per employee-year in the pre-takeover time period, C_t, for each firm taken over.

The average rate of real wage growth, $\dot{w}_t$, prior to the takeover is found for each firm. Estimates of changes in level of the rate of wage growth from before to after takeovers and CEO changes, d, were taken from the regression results in the previous section. Assuming contract costs are linear in wages, the information above and the interest rate at the time of the takeover, r_t (Moody's Baa corporate rate), are sufficient to calculate the present value of the change in contract costs per union member over the first two contracts (assumed to be 3 years each) following the takeover as

$$\Delta W_6 = C_t \times \sum_{n=1}^{6} \left[\frac{(1+\dot{w}_t)^n}{(1+r_t)^n} - \frac{(1+\dot{w}_t-d)^n}{(1+r_t)^n} \right]$$

This calculation allows the effect of the change in the level of wage growth to compound over the six year period. Hence the full estimated effect in the previous section is first calculated.

It is not reasonable to assume that the decrease in wage growth persists indefinitely, as the mean real wage growth in the sample is just 0.17% per year. A reduction by 0.6% per year implies real wages declining asymptotically to zero. A more realistic assumption is that wage levels are lowered at first (relative to what they would have been in the absence of the takeover), but that the growth rate reverts to the industry- or economy-wide rate over time. The movement to a new wage path parallel to the old one implies constant wealth concessions per year after some point.

The infinite horizon present value of such a yearly concession is the measure used here for the wealth concession per employee. It is calculated as

$$\Delta W = \frac{r_t \times \Delta W_6}{1 - \left[\frac{1}{(1+r_t)^6}\right]}$$

This formula finds the infinite horizon present value of the perpetuity with the yearly value V, where V solves

$$\sum_{n=1}^{6} \frac{V}{(1+r_t)^n} = \Delta W_6$$

That is, it extends the six equal yearly concessions implied by ΔW_6 to a permanent basis.

Finally, rather than multiply the wealth change per union member figure by the number of employees covered by each contract, total union employment figures for each firm are estimated by obtaining total employment figures for each firm at the time of the takeover[26] and multiplying them by the percent unionized in the sector.[27] These figures are generally larger than estimates from the sample as contracts are missing. The implicit assumption that firm production worker employment does not change much following takeovers is empirically supported by Lichtenberg and Siegel (1989) and Brown and Medoff (1988). The calculations described should provide a good chance for wage growth reductions to explain takeover premiums.

The estimates of target firm shareholder wealth changes are calculated from a simple market model of the form

$$R_{it} = \alpha_i + \beta_i R_{Mt} + \epsilon_t$$

where R_{it} is the monthly return for firm i in month t, R_{Mt} is the value-weighted market return (including dividends) in month t, and ϵ is a classical disturbance term. The excess return was calculated over the month containing the first mention of the takeover. This procedure is standard and the details are omitted.[28] The median of the premium distribution is 25%, which is well within the typical range for event studies of takeover wealth effects. To make the dollar figures comparable to the union wealth change figures, they have been deflated by the 1967-based CPI.

Table 10 summarizes the wealth change information. Each column from the second to the seventh shows a distribution with values at the percentile ranks shown in the first column. The second column shows the distribution of (constant) dollar amounts of the shareholder wealth change, while the third shows the distribution of premium percentages. The fourth to the seventh show the distributions of the ratio of union wealth concessions to shareholder wealth gains using the wage growth change coefficients from the indicated table and specification. Union concessions are positive numbers, and union gains are negative numbers. Shareholder gains are positive and losses are negative. Hence negative ratio values indicate that shareholder and union wealth moved in the same direction. The distribution in each column is ordered independently of the other columns, so figures at the same percentile rank in two different columns do not necessarily correspond to the same company. All of the information necessary to calculate both union and target firm shareholder wealth could be found for 212 companies,[29] but coefficients

were not available for all sectors in the specifications from Table 8, so the last two distributions have fewer observations.

The last four columns show the evidence on the transfer hypothesis. The distributions shown are for all takeovers (with and without CEO changes) combined. The fifth column is constructed by combining the coefficients most favorable to that hypothesis from the last two specifications on Table 7. Even with the liberal assumptions for constructing wealth concessions described above and mixing the results of two estimations to magnify the effect, at the median only 12% of the shareholder gain is explained by union concessions. The fourth column is constructed from the first specification on Table 7, which was the most favored one in terms of included regressors. In this specification, only 3% of the premium is explained at the median value. The final two columns were constructed from the sector-specific information from Table 9. In some sectors, the effect of the takeover alone was to increase wage growth. This is reflected in the negative values at the medians in both columns. In the sixth column, the union gains 4% of the shareholder wealth increase. The corresponding figure in the last column is 2%.

A slightly stronger case for the transfer hypothesis can be made by looking only at takeovers that include a CEO change. The final footnote on Table 10 shows the median values for the four distributions in this case. Using the fourth specification from Table 7, the median value is 21%. I present this number only in order to make the following points: It is possible to explain one fifth of the takeover premium by union concessions. To accept this result, however, one must focus only on the results of the least favored specification (both year and industry indicators are excluded), ignore statistical insignificance, and allow the liberal assumptions used in

constructing the union wealth changes. When year and industry effects are included (the first specification of Table 7), the result is reversed in sign. In this case, unions gain 2% of the value of the shareholder premium. Both specifications based on Table 9 show modest union concessions (5% and 6%) as a percentage of shareholder premiums. Although it is possible to show larger concessions by focusing only on takeovers with CEO changes, I believe that the most reasonable conclusion is again that the effect is small. These results are not sufficiently different from the results for all takeovers to warrant emphasis.

The minimum and maximum values in the last four columns of Table 10 deserve comment. They are all generated in cases where the shareholder wealth change is very small compared to the median value, as documented at the bottom of the table. Hence small variations in union wealth changes can cause large movements in the ratios. Apart from the two extreme values at each end of each distribution, the ranges are fairly narrow. These characteristics of the distributions make it clearer that that the median values are reasonable statistics to represent the overall rate of transfer.

The evidence from this table rejects the proposition that wealth transfers from unions explain target firm shareholder wealth premiums. Rather, there is just as much evidence that unions gain when a takeover occurs. In either case, the relative magnitudes make it clear that union wealth changes associated with takeovers and CEO changes are economically insignificant compared to target firm shareholder wealth changes.

Section VI.

This paper provides empirical evidence that transfers of wealth from unions do not account for target firm shareholder wealth premiums. Regression analysis reveals that reductions in real wage growth associated with takeovers and CEO changes are statistically insignificant, and sectoral decompositions show increased real wage growth in major manufacturing sectors. In the extreme specifications, levels of real wage growth may be reduced by up to 1% annually or increased by up to .3%. In more favored specifications, the effects are much smaller.

Union wealth concessions associated with wage growth reductions were calculated under assumptions favorable the transfer hypothesis, and one month target firm shareholder wealth premiums were calculated using a simple market model. These figures were used to calculate the ratio of union concessions to shareholder premiums. At most, 12% of the shareholder premium can be explained. At the opposite extreme, union wealth increases by 4% of the value of the shareholder premium. Again, the effects are smaller in more reasonable specifications. I conclude that the effects of takeovers and CEO changes on union wages and wealth are both statistically insignificant and economically insignificant in the sense that they do not explain target firm takeover premiums.

Footnotes

1. Most studies find that target firm share prices rise 15% to 50% around the time of the initiation of the takeover, with variations in the magnitude partly explained by the type of takeover. See Jensen and Ruback (1983), and Jarrell, Brickley, and Netter (1988) for summaries of this literature.

2. Eckbo (1985), Eckbo and Wier (1985), and McGuckin, Warren-Boulton, and Waldstein (1988) provide evidence against the market concentration hypothesis using stock market data.

3. CEO changes are included as an indicator of 'hostility.' Previous work with these data shows that traditional hostility indicators, such as resistance by the target firm board, perform poorly relative to CEO changes (Rosett, 1989). I argue that CEO changes accompany real shifts in corporate strategy, while protestations by board members at the time of a takeover bid contain less information.

4. Jensen and Meckling (1976) and Fama and Jensen (1983a, 1983b) discuss monitoring, agency costs, and the separation of ownership and control.

5. Manne (1965) provides an insightful discussion of the role of the market for corporate control as a disciplining mechanism.

6. Ruback (1983) provides evidence that "...on average, the successful offer price exhausts the potential gains for unsuccessful bidders" (1983, p.152). He concludes that the market for corporate control is competitive.

7. Brown and Medoff (1988) used a sample of Michigan firms to estimate the effects of takeovers on wages and employment. Their results for wages vary somewhat by acquisition type and the time period examined, ranging from 5% higher than would be expected in the absence of the acquisition for "assets

only" acquisitions (the bidder purchases the target's assets without absorbing its work force), to 4% to 5% lower for acquisitions involving changes of ownership with or without the integration of the target into an existing firm (pp.19-20). Their analysis is not explicitly aimed at determining whether employee wealth is transferred to shareholders through takeovers, but the results do not appear to support that hypothesis.

8. Roll (1986) points out that both target and bidding firm wealth effects must be accounted for in determining the extent of wealth creation through takeovers. If acquiring firms are larger than target firms, then even small negative returns to bidding firm shareholders may imply zero or negative net wealth creation. Roll defends the possibility of such a result on the basis of irrational behavior by managers: "If there actually are no aggregate gains in takeover, the phenomenon depends on the overbearing presumption by bidders that their valuations are correct" (Roll, 1986, p. 200). Bradley, Desai, and Kim (1988) respond to Roll's point using a sample of 236 tender offer contests using matched bidder/target pairs. All companies were listed on the NYSE or AMEX, and all acquisitions occurred after 1963. They found company value weighted combined returns of 7.43%, based on .97% returns for bidders and 31.77% for targets. In millions of 1984 dollars, they found mean wealth increases of $17.3, $107.08, and $117.11 respectively for bidders, targets, and both combined.

9. Auerbach and Reishus (1988) provide evidence that tax advantages do not drive takeover activity. They show that the probability of tax advantages through mergers and acquisitions is similar between company pairs actually involved in takeovers and pairings made randomly. However, Kaplan (1989) finds significant tax effects in the case of management buyouts.

10. Lichtenberg and Siegel (1989) find significant cuts in central office personnel, but not for production workers, following takeovers.

11. Pontiff, Shleifer, and Weisbach (1989) find that pension reversions follow roughly 10% of takeovers (slightly higher for hostile, lower for friendly), and that the reversion explains roughly 10% of the takeover premium. Hence, roughly 1% of the value in takeovers might be explained through reversions. However, their evidence that the reversions would not have occurred in the absence of the takeover is weak.

12. Published every other week in the BNA *Daily Labor Report*. Contracts from 1/1/76 to 6/30/87 are used here.

13. The CRSP CUSIP is described on p.20 of the March, 1986 edition of the *CRSP Stock File User's Guide*.

14. COLAs for contracts settled after 1/1/83 were evaluated using the specifics of the COLA clause available in the BNA data. COLAs prior to that date were separately coded by Abowd, who assigned the expected value of COLA payments based on Hendricks and Kahn (1985). See Abowd (1989).

15. Average weekly hours and overtime hours were coded by year at the 2-digit SIC level from the Bureau of Labor Statistics *Employment, Hours, and Earnings, United States, 1909-84*, Volumes 1 and 2 (March 1985), and the BLS *Supplement to Employment, Hours, and Earnings* (July 1987).

16. The average value by year and 2-digit SIC of fringe benefits as a percent of wages was obtained by adding lines 1 (legally required payments, employer's share only), 2 (pensions, insurance, and other agreed upon payments, employer's share only), and 5 (other items) of Table 6 in the U.S.Chamber of Commerce publication Employee Benefits (1975, 1977-1986).

17. Hostility is recorded if the target firm board or management oppose the bid, legal action is taken against the bidder, or the *Wall Street Journal Index* characterizes the bid as hostile.

18. Further information regarding CEO changes not necessarily associated with a takeover was recorded from the *Forbes* annual executive compensation summary (May or June, 1979–1988) for as many companies as possible in the full sample (not just companies with trading suspensions). In previous versions of this study these were included in the regression analysis to account for the independent effect of CEO changes. Excluding this information greatly simplifies the interpretation of the results without substantially altering them. See Rosett (1989).

19. Also see Chapter 2 and the Data Appendix in Rosett (1989) and the Data Appendix in Abowd (1989).

20. 36 months is both the median and the modal contract length, as 3,379 of 5,353 contracts have that length. 1,137 contracts last 24 months. The mean and standard deviation are 32.5 and 7.1, respectively.

21. In previous work with these data, a traditional hostile takeover indicator was used in addition to the CEO change indicator. The distribution of this indicator is shown in Table 3. The wage growth effects were weaker and often of the wrong sign using the hostile indicator. See Rosett (1989).

22. Alternative specifications including interactions of takeover indicators with the quantitative regressors were explored but not presented as they complicate the interpretation without providing evidence for stronger wage effects.

23. Mining, durable manufacturing, non-durable manufacturing, transportation

and utilities (excluding air transportation), airlines, wholesale and retail trade, and FIRE (Finance, Insurance, and Real Estate) and other services.

24. Additional specifications in which takeover and CEO change indicators were interacted with other regressors were explored. In particular, changes in bargaining power or breaches associated with takeovers can be interpreted as altering the responsiveness of contracted wage growth to current differences between firm and industry wages. Interacting the takeover indicators with the firm-to-industry relative wage variable should capture such a change. The results were not substantially different from the specifications presented, but are difficult to interpret and are omitted.

25. See Kennedy (1985), page 186.

26. COMPUSTAT data item 29 reports the average or year-end employment, including all part-time and seasonal employees, and employees of both domestic and foreign subsidiaries. See Section 8, p. 46 of the April 15, 1986 Industrial COMPUSTAT guide. In most cases, the employment figure is for the year prior to the completion of the takeover.

For 49 companies, COMPUSTAT did not provide information. Figures for 45 of these companies were obtained from Moody's, again for the year prior to the takeover in most cases.

27. The sectors are as elsewhere in this paper. Figures on unionization by sector are from the Bureau of Labor Statistics Employment and Earnings, January, 1987, Table 60. Figures for 1985 are used.

28. Fama (1976) provides an extensive review of the methodology and early event study literature.

29. Firm level employment figures could not be obtained for a few companies,

and companies for which contracts are only observed following a takeover are not included.

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Figure 1

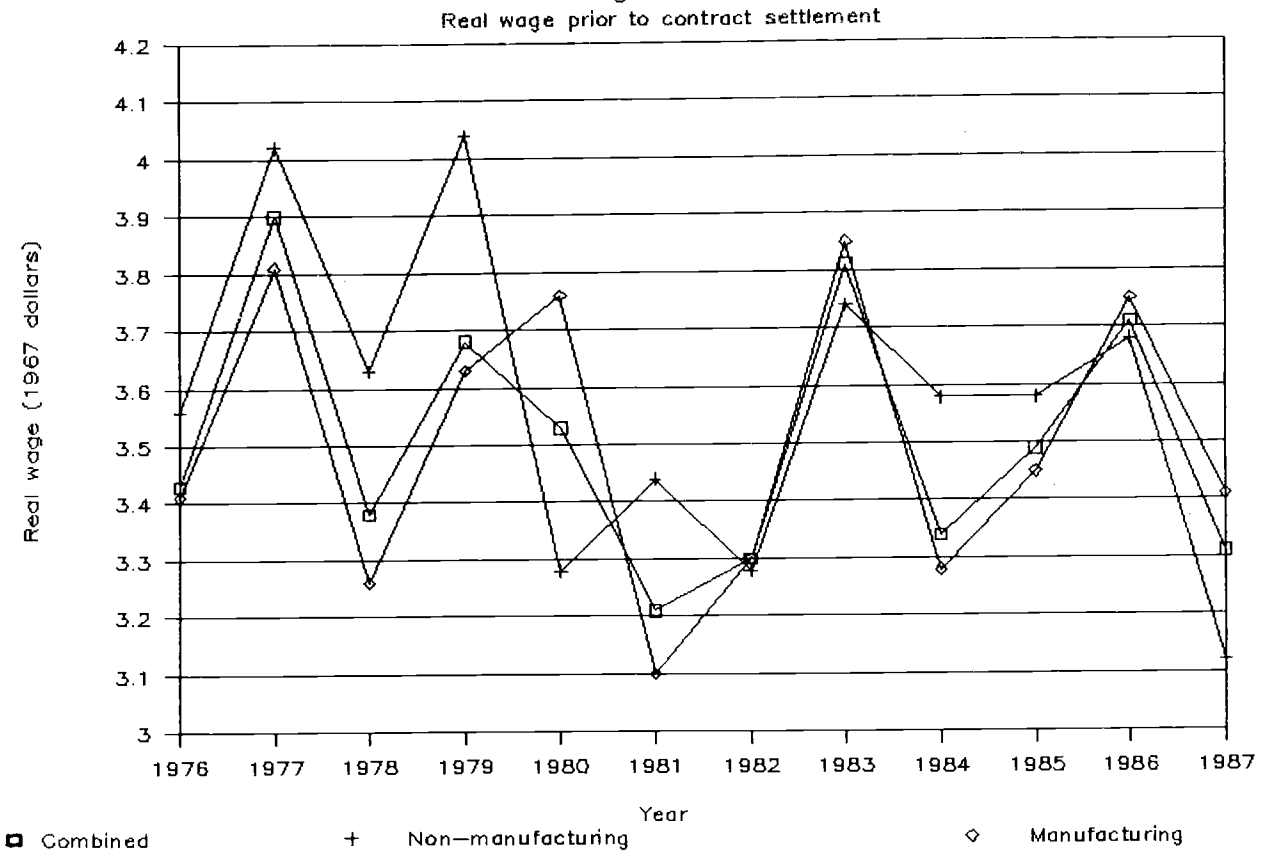
Real wage prior to contract settlement
Real wage (1967 dollars)
4.2
4.1
4
3.9
3.8
3.7
3.6
3.5
3.4
3.3
3.2
3.1
3
1976
1977
1978
1979
1980
1981
1982
1983
1984
1985
1986
1987
Year
Combined
Non—manufacturing
Manufacturing

Figure 2

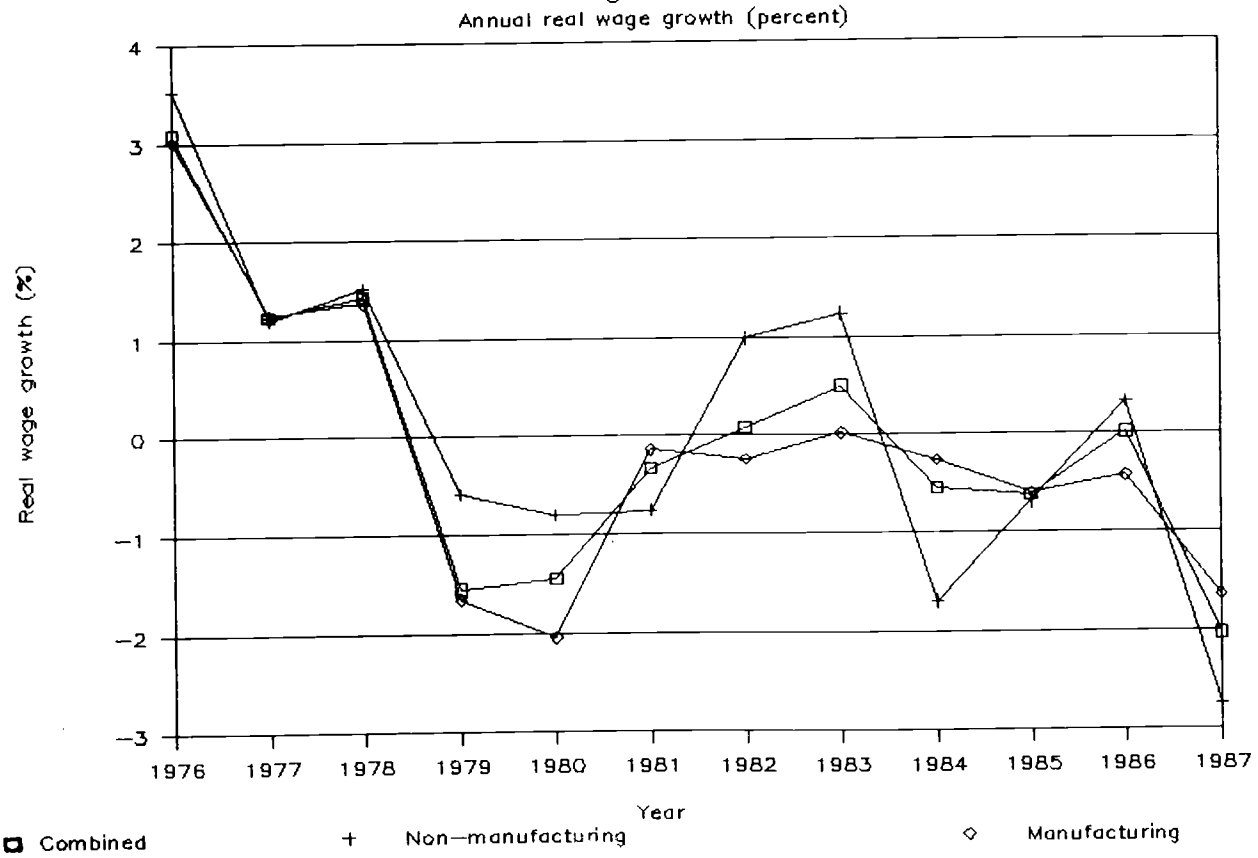
Annual real wage growth (percent)
Real wage growth (%)
4
3
2
1
0
−1
−2
−3
1976
1977
1978
1979
1980
1981
1982
1983
1984
1985
1986
1987
Year
Combined
Non—manufacturing
Manufacturing

Figure 3

Real wage prior to contract settlement

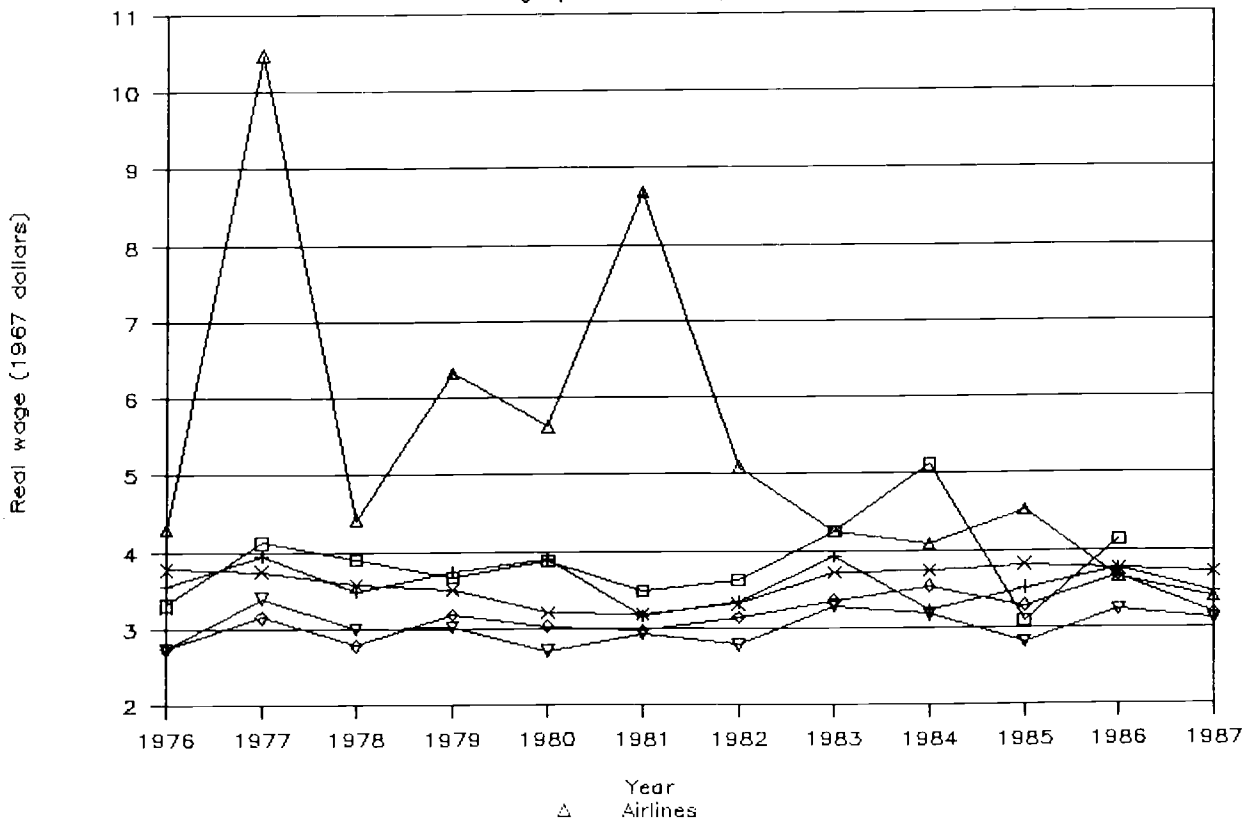

Figure 4

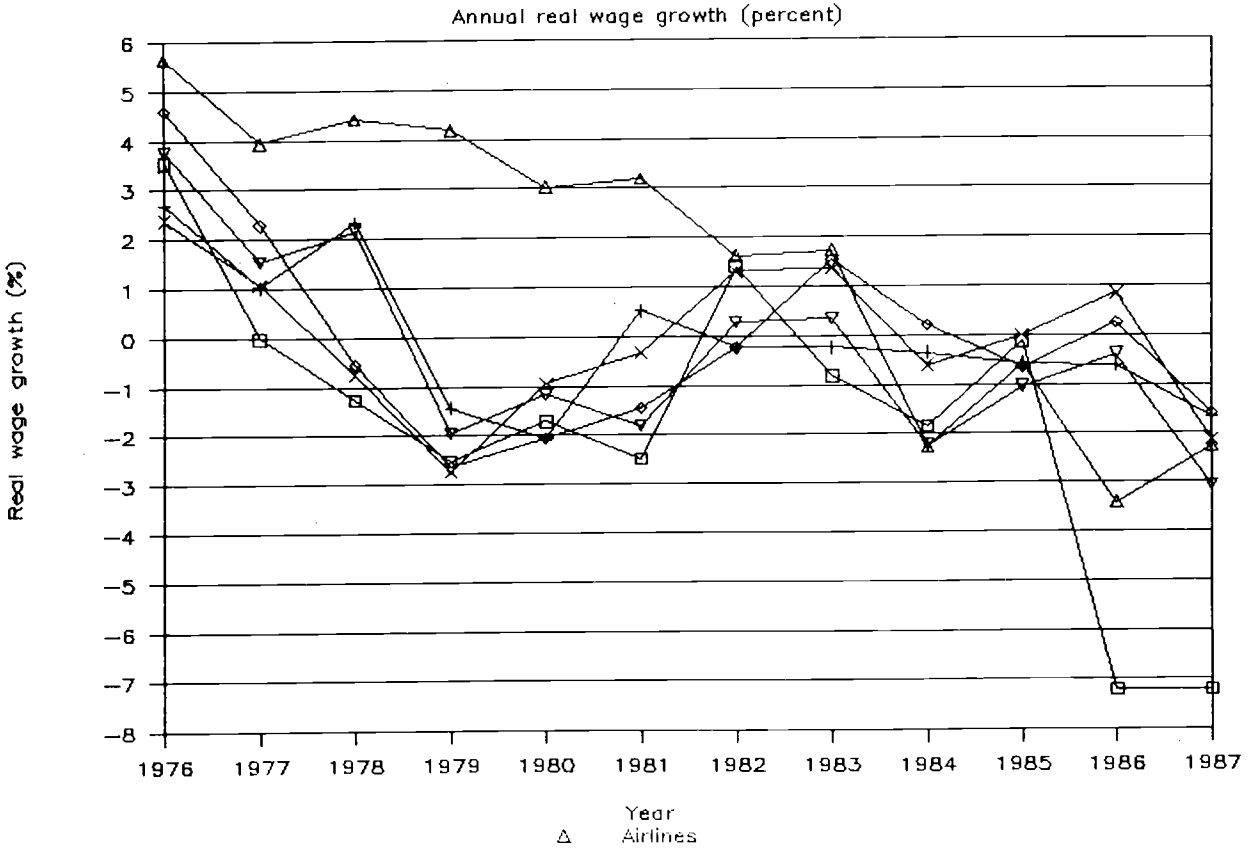
Annual real wage growth (percent)
Real wage growth (%)
6
5
4
3
2
1
0
−1
−2
−3
−4
−5
−6
−7
−8
1976
1977
1978
1979
1980
1981
1982
1983
1984
1985
1986
1987
Year
Airlines

Table 1
Summary of real wage levels, growth rates, and real contract costs, employment weighted means and standard deviations

Sector	Year	Obs	Real Wage at End of Previous Contract (1967 $s)	Expected Annual Rate of Growth (as a Percent) of Real Wage over New Contract	Real Cost of Contract per Employee-Year (Thousands of 1967 $s)
Combined	Pooled	5353	3.57 (1.55)	0.17 (4.81)	9.99 (4.83)
Combined	1976	502	3.43 (1.12)	3.07 (3.74)	10.48 (3.82)
Combined	1977	769	3.90 (2.61)	1.23 (2.76)	11.44 (6.96)
Combined	1978	434	3.38 (0.92)	1.42 (3.48)	10.20 (3.30)
Combined	1979	398	3.68 (2.21)	-1.55 (4.41)	11.23 (6.92)
Combined	1980	545	3.53 (1.33)	-1.45 (4.73)	10.01 (4.31)
Combined	1981	508	3.21 (1.27)	-0.33 (3.62)	8.62 (3.36)
Combined	1982	476	3.30 (1.04)	0.08 (6.91)	8.34 (3.63)
Combined	1983	423	3.81 (1.12)	0.50 (5.56)	9.65 (3.29)
Combined	1984	411	3.34 (0.96)	-0.55 (3.86)	9.17 (3.24)
Combined	1985	356	3.49 (1.14)	-0.62 (2.77)	9.10 (3.78)
Combined	1986	389	3.71 (0.96)	0.02 (3.61)	9.48 (3.88)
Combined	1987	142	3.31 (0.89)	-2.03 (4.09)	8.21 (3.84)
Manufacturing	Pooled	3835	3.54 (1.00)	0.10 (4.88)	10.53 (3.47)
Non-manufacturing	Pooled	1518	3.62 (2.43)	0.33 (4.62)	8.84 (6.83)
Mining	Pooled	134	3.99 (0.36)	-1.63 (2.65)	9.54 (1.03)
Durables Manufacturing	Pooled	1911	3.64 (1.17)	0.08 (6.04)	10.82 (4.14)
Non-durables Manufacturing	Pooled	1924	3.13 (0.69)	0.16 (3.34)	9.36 (2.33)
Airlines	Pooled	154	5.11 (6.58)	1.84 (8.03)	13.09 (17.38)
Transportation* & Utilities	Pooled	559	3.58 (0.87)	0.39 (3.98)	9.23 (2.42)
Retail & Wholesale Trade	Pooled	578	3.02 (0.87)	-0.48 (4.23)	5.55 (1.81)
F.I.R.E.** & Other Services	Pooled	93	2.43 (0.72)	0.23 (2.35)	4.81 (1.76)

Sources: BNA "Collective Bargaining Negotiations and Contracts," 1976 to 1987. BLA "Employment, Hours, and Earnings," 1985 (Supplement, 1987). U.S. Chamber of Commerce "Employee Benefits," 1975 to 1987. Author's calculations.
Notes: All figures employment weighted.
*Not including airlines.
**Finance, Insurance, and Real Estate.

Table 2

Comparison of realized and predicted end-of-contract wages

Year of Settlement	Real Wage Obs	Real Wage at End of Contract	Predicted Wage Obs	Predicted Real Wage at End of Contract
1976	502	3.43		.
		(1.12)		.
1977	769	3.90	34	3.42
		(2.61)		(1.12)
1978	434	3.38	167	3.33
		(0.92)		(0.94)
1979	398	3.68	512	3.88
		(2.21)		(3.16)
1980	545	3.53	718	3.91
		(1.33)		(1.15)
1981	508	3.21	430	3.70
		(1.27)		(2.19)
1982	476	3.30	436	3.49
		(1.04)		(1.50)
1983	423	3.81	496	3.37
		(1.11)		(1.54)
1984	411	3.34	528	3.33
		(0.96)		(0.93)
1985	356	3.49	406	3.24
		(1.14)		(1.50)
1986	389	3.71	456	3.82
		(0.96)		(0.87)
1987	142	3.31	374	3.25
		(0.89)		(1.13)

Source: "Collective Bargaining Negotiations and Contracts," 1976 to 1987, and author's calculations.

Table 3

Companies, contracts, and events by sector

Sector	Total Companies in Sector	Companies with a Takeover in 1973-1987	Companies with Contract Following a Takeover (within 2 Contracts)	Companies with Joint Takeover and CEO Change in 1973-1987	Companies with Contracts Following a Joint Takeover and CEO Change	Companies with a Hostile Takeover in 1973-1987	Companies with Contracts following a Hostile Takeover
Mining	12	4	3	1	0	0	0
Manufacturing - Durables	344	114	58	26	13	16	7
Manufacturing - Nondurables	373	96	53	25	14	18	8
Airlines	25	9	0	5	0	2	1
Transport and Utilities	122	15	8	3	2	2	1
Retail/Wholesale Trade	86	35	18	5	2	3	1
F.I.R.E & Other Services	47	15	7	3	2	2	1
Total	1009	288	147	68	33	43	18

Sector	Total Companies in Sector	Contracts from Companies with a Takeover in 1973-1987	Contract Following a Takeover (within 2 Contracts)	Contracts from Companies with Joint Takeover and CEO Change in 1973-1987	Contracts Following a Joint Takeover and CEO Change	Contracts from Companies with a Hostile Takeover in 1973-1987	Contracts following a Hostile Takeover
Mining	134	58	17	19	10	9	1
Manufacturing - Durables	1911	522	130	173	27	115	19
Manufacturing - Nondurables	1924	449	93	144	33	78	17
Airlines	154	66	0	36	0	5	1
Transport and Utilities	559	58	12	9	1	4	1
Retail/Wholesale Trade	578	228	63	17	12	12	4
F.I.R.E & Other Services	93	23	7	5	2	2	1
Total	5353	1404	322	403	85	225	38

Sources: BNA "Collective Bargaining Negotiations and Contracts," 1976 to 1987. Commerce Clearing House "Capital Changes Reporter," 1988. "Wall Street Journal Index," 1973 to 1987. W.T. Grimm & Co. "Mergerstat Review," 1981 to 1987.

Table 4

Number of union members in the U.S. and in the Bureau of National Affairs sample

		B.N.A. Data									
					Sectors						
Year of Settlement	U.S. Union Membership*	All Sectors	All Sectors 3 Year Totals	3 Year Totals as % of U.S. Total	Mining	Durables Manufacturing	Non-Durables Manufacturing	Airlines	Transportation and Utilities	Retail and Wholesale Trade	F.I.R.E and Other Services
1976	22,153	1,591			2	1,146	233	47	97	61	5
1977	21,632	1,725			22	824	174	31	618	54	2
1978	21,757	532	3,847	17.7%	0	235	118	49	75	49	5
1979	22,025	1,493	3,750	17.0%	2	1,086	255	40	46	65	0
1980	20,968	1,527	3,552	16.9%	23	653	139	32	615	53	12
1981	20,647	595	3,615	17.5%	7	275	138	11	94	67	4
1982	19,571	932	3,055	15.6%	5	560	128	26	104	94	14
1983	18,634	1,215	2,743	14.7%	17	633	102	83	311	54	16
1984	18,306	1,072	3,220	17.6%	1	719	136	54	70	86	6
1985	16,996	624	2,911	17.1%	1	298	129	56	59	72	8
1986	16,975	1,055	2,751	16.2%	12	341	88	27	500	82	5
1987	16,913	285			0	147	37	2	13	81	5
U.S. Employment Total in 1986*		96,903,000			822	12,171	8,124		5,175	19,839	27,461
U.S. Union Total in 1986*		16,975,000			144	3,104	1,765		2,023	1,425	1,497

Sources: BNA "Collective Bargaining Negotiations and Contracts," 1976 to 1987. BLS "Employment and Earnings."
Note: All figures in thousands.
*Figures from BLS "Employment and Earnings."

Table 5

Comparison of trading stoppage reasons between companies in the Bureau of National Affairs' sample and the remainder of the CRSP NYSE/AMEX data

CRSP Code for Trading Status of Stock* (Variable Name ISTPCD)	Not in Sample	In Sample	Total
Company Is Still Trading	1773 (76.8%)*	535 (23.2%)	2308 (100%)
Merge into Another Company	1273 (78.3%)	352 (21.7%)	1625 (100%)
Exchanged	115 (73.7%)	41 (26.3%)	156 (100%)
Total	3161 (77.3%)	928 (22.7%)	4089 (100%)
Chi-square with 2 Degrees of Freedom:		2.443	
Probability of Greater Chi-square:		0.295	

Sources: BNA "Collective Bargaining Negotiations and Contracts," 1976 to 1987. CRSP tapes with data through 12/31/86.

*Row percentages in parentheses.

Note: Categories for Unknown at this time, Liquidated, Delisted by Exchange, Suspended by Exchange, and Suspended by SEC are omitted. Hence the Total in Sample in this table is smaller than the total number of companies used in this study.

Table 6

Observations per year of reporting risk by takeover status

Percentile Rank	Companies Not Taken Over	Companies Taken Over: Prior to Takeover	Companies Taken Over: Following Takeover
Maximum	11.31	13.53	7.14
99%	3.74	3.87	3.03
95%	1.65	2.31	0.87
90%	1.13	1.29	0.63
75%	0.52	0.65	0.29
50%	0.17	0.28	0.10
25%	0.09	0.10	0
10%	0.09	0	0
5%	0.09	0	0
1%	0.09	0	0
Minimum	0.09	0	0
Observations	721	288	288
Mean	0.48	0.57	0.25
Standard Dev	0.76	1.11	0.62

Sources: BNA "Collective Bargaining Negotiations and Contracts," 1976 to 1987.

Table 7

Regression coefficients and test statistics for levels of real wage growth (percent) prior to and following takeovers and CEO changes

Variable*	Coefficient Estimates (Standard Errors)			
Annual Growth (Percent) of Real Wage	Dependent Variable			
Event Indicators:				
PTO: Contract Settlement Date prior to Takeover Date	0.03	0.11	0.19	0.24
	(0.14)	(0.15)	(0.15)	(0.16)
PTC: Contract Settlement Date prior to Takeover Date with CEO Change	−0.01	−0.01	0.15	0.03
	(0.23)	(0.25)	(0.23)	(0.25)
FTO: Contract Settlement Date following Takeover Date*	−0.33	−0.32	−0.41	−0.31
	(0.22)	(0.22)	(0.22)	(0.23)
FTC: Contract Settlement Date following Takeover Date* with CEO Change	0.47	0.42	−0.19	−0.46
	(0.42)	(0.44)	(0.43)	(0.46)
Intercept	10.14	1.18	5.74	−0.09
	(1.34)	(1.17)	(0.95)	(0.68)
Industry effects**	Yes	Yes	No	No
Year effects**	Yes	No	Yes	No
Standard error of the equation	3.91	4.17	4.11	4.39
Adjusted R-squared	.34	.25	.27	.17
F-statistic for PTO=PTC=FTO=FTC=0	0.66	0.74	2.85	2.52
Degrees of Freedom	(4,5290)	(4,5301)	(4,5327)	(4,5338)
Probability of greater F	.62	.56	.02	.04
F-statistic for PTO=FTO	2.06	2.65	5.36	4.23
Degrees of Freedom	(1,5290)	(1,5301)	(1,5327)	(1,5338)
Probability of greater F	.15	.10	.02	.04
F-statistic for PTC=FTC	1.03	0.73	0.50	.86
Degrees of Freedom	(1,5290)	(1,5301)	(1,5327)	(1,5338)
Probability of greater F	.31	.39	.48	.36
F-statistic for PTO+PTC=FTO+FTC	0.09	0.00	5.18	5.45
Degrees of Freedom	(1,5290)	(1,5301)	(1,5327)	(1,5338)
Probability of greater F	.76	.99	.02	.02

Note: OLS weighted by employment.

*All equations also include the following conditioning variables: The CPI, constant (1982) dollar GNP, and the civilian unemployment rate, all entered as rates of change over the year prior to the settlement date; the log of the real wage at the end of the previous contract, and the percent difference between the wage at the end of the previous contract and the wage in the (2-digit SIC) industry in the month prior to the settlement month. All variables are entered both linearly and as squares. The F-statistic for the joint significance of the squared terms is significant at the .001 level in all specifications.

**Year and Industry indicators are both jointly significant at the .0001 level in an F-test of joint significance.

Table 8

Regression coefficients for levels of real wage growth (percent) prior to and following takeovers

Variable	Coefficient Estimates (Standard Errors)			
Annual Growth (Percent) of Real Wage	Dependent Variable			
Event Indicators:				
PTO: Contract Settlement Date prior to Takeover Date	0.45	0.54	0.69	0.50
	(0.38)	(0.38)	(0.40)	(0.36)
PTC: Contract Settlement Date prior to Takeover Date with CEO Change	3.82	2.85	3.67	1.94
	(1.58)	(1.57)	(1.68)	(1.51)
FTO: Contract Settlement Date following Takeover Date*	-1.99	-1.94	-2.20	-1.93
	(0.49)	(0.49)	(0.53)	(0.49)
FTC: Contract Settlement Date following Takeover Date* with CEO Change	3.68	3.60	3.61	3.44
	(1.72)	(1.72)	(1.84)	(1.74)
Interaction of Mining and PTO Indicators	-0.44	0.19	-0.12	-1.37
	(1.56)	(1.54)	(1.64)	(1.46)
Interaction of Mining and PTC Indicators	-4.20	-3.33	-4.17	-2.36
	(3.19)	(3.19)	(3.41)	(3.19)
Interaction of Mining and FTO Indicators	0.83	-0.16	0.19	-1.75
	(1.40)	(1.19)	(1.27)	(1.07)
Interaction of mining and PTC Indicators	-2.72	-1.03	-1.45	-0.86
	(3.31)	(3.16)	(3.38)	(3.19)
Interaction of Durables Manufacturing and PTO Indicators	-0.00	0.11	0.06	0.09
	(0.47)	(0.47)	(0.50)	(0.46)
Interaction of Durables Manufacturing and PTC Indicators	-4.62	-3.66	-4.62	-2.80
	(1.62)	(1.61)	(1.72)	(1.56)
Interaction of Durables Manufacturing and FTO Indicators	2.77	2.74	2.82	2.70
	(0.63)	(0.63)	(0.68)	(0.63)
Interaction of Durables Manufacturing and FTC Indicators	-5.17	-5.01	-5.09	-4.97
	(1.82)	(1.81)	(1.94)	(1.83)
Interaction of Non-durables Manufacturing and PTO Indicators	-0.57	-0.67	-0.63	-0.78
	(0.48)	(0.48)	(0.51)	(0.45)
Interaction of Non-durables Manufacturing and PTC Indicators	-3.85	-3.12	-3.71	-2.17
	(1.66)	(1.65)	(1.77)	(1.60)
Interaction of Non-durables Manufacturing and FTO Indicators	2.27	2.18	2.34	2.07
	(0.72)	(0.72)	(0.77)	(0.71)
Interaction of Non-durables Manufacturing and FTC Indicators	-4.31	-4.26	-3.66	-4.14
	(2.00)	(2.00)	(2.14)	(2.02)
Interaction of Airlines and PTO Indicators	-1.85	-2.28	-2.90	0.11
	(0.76)	(0.75)	(0.80)	(0.71)
Interaction of Airlines and PTC Indicators	-1.80	-1.32	-2.04	-0.34
	(1.73)	(1.72)	(1.84)	(1.67)
Interaction of Transportation/Utilities and PTO Indicators	-0.35	-0.69	-0.91	-0.50
	(0.47)	(0.46)	(0.49)	(0.45)
Interaction of Transportation/Utilities and PTC Indicators	-3.35	-3.02	-3.27	-2.21
	(2.05)	(2.04)	(2.18)	(2.00)
Interaction of Transportation/Utilities and FTO Indicators	1.68	1.47	2.41	1.62
	(0.63)	(0.63)	(0.66)	(0.63)

Table 8 (continued)

Regression coefficients for levels of real wage growth (percent) prior to and following takeovers

Variable	Coefficient Estimates (Standard Errors)			
Annual Growth (Percent) of Real Wage	Dependent Variable			
Interaction of Retail/Wholesale Trade and PTC Indicators	-5.71	-5.31	-5.10	-4.30
	(2.47)	(2.45)	(2.62)	(2.42)
Intercept	6.03	4.77	-1.64	6.45
	(1.28)	(1.02)	(0.79)	(0.96)
X: Conditioning Variables as in Table 7	Yes	Yes	Yes	Yes
Interactions of X and Period ('82-'87) Indicators	Yes	No	No	No
Sector Indicators	Yes	Yes	Yes	No
Interactions of Sector and Period Indicator	Yes	No	No	No
Standard Error of the Equation	4.04	4.04	4.33	4.08
Adjusted R-squared	.29	.30	.19	.28

Note: OLS weighted by employment.

Table 9

Summary of changes in levels of wage growth (percent) by sector from before to after takeovers and CEO changes based on Table 8

Change in Annual Percentage Wage Growth from Before to After Event Below:	Columns Below Correspond to Columns on Table 8			
	Mining			
Takeover	-1.17	-2.83	-2.58	-2.81
Takeover with CEO change	0.17	0.22	0.08	0.19
	Durables Manufacturing			
Takeover	0.33	0.15	-0.13	0.18
Takeover with CEO change	-0.36	-0.45	-0.66	-0.49
	Non-durables manufacturing			
Takeover	0.40	0.37	0.08	0.42
Takeover with CEO change	-0.20	-0.02	0.07	-0.05
	Transportation* and Utilities			
Takeover	-0.41	-0.32	0.43	-0.31

Source: Table 8 and author's calculations.
*Excluding airlines.

Table 10

Distributions of target firm shareholder wealth changes and premiums, and ratios of union wealth changes to target firm shareholder wealth changes

Percentile Rank	Target Firm Shareholder Wealth Gain (Millions)	Target Firm Shareholder Wealth Premium (%)***	Ratio of Union Wealth Concession to Target Firm Shareholder Wealth Gain for All Takeovers.****			
			From Table 7, Specification 1	From Table 7, Specification 3*	From Table 9, Specification 1	From Table 9, Specification 4
Maximum	1,462	131%	4.59**	22.05**	9.81**	10.48**
99%	711	89%	1.27	3.32	7.71	5.34
95%	296	78%	0.34	1.06	0.46	0.36
90%	120	59%	0.25	0.51	0.15	0.20
75%	44	42%	0.10	0.29	0.04	0.02
50%	16	25%	0.03	0.12	-0.04	-0.02
25%	5	12%	-0.02	0.05	-0.12	-0.09
10%	0	2%	-0.07	0.01	-0.27	-0.21
5%	-2	-3%	-0.34	-0.42	-0.52	-0.43
1%	-40	-18%	-2.59	-2.35	-4.32	-5.42
Minimum	-481	-32%	-10.59**	-17.58**	-5.16**	-10.12**
Observations	212	212	212	212	173	173

Sources:
BRA "Collective Bargaining Negotiations and Contracts," 1976 to 1987.
BLS "Employment, Hours, and Earnings," 1985 (Supplement, 1987).
U.S. Chamber of Commerce "Employee Benefits," 1975 to 1987.
Results from tables 7 and 9.
CRSP tapes with information through 12/31/86.
Author's calculations.

Note: Values are ordered within each column. Hence shareholder wealth gains at each percentile rank are not necessarily the same across columns.

*Specification 4 is used for the takeover with CEO change cases. Hence this combines the estimates most favorable to the wealth transfer hypothesis.

**Constant dollar (1967 based) figures, in millions, for the minimum and maximum values of the ratio colums are as follows (Union concession/Shareholder gain):

	From Table 7, Specification 1	From Table 7, Specification 3*	From Table 9, Specification 1	From Table 9, Specification 4
Maximum	11.5/2.5	11.1/0.50	-0.15/-0.015	5.3/0.50
Minimum	0.16/-0.015	0.27/-0.015	-12.9/2.5	5.4/-0.53

***Target firm shareholder wealth gain is the increase in the constant (1967) dollar value of outstanding of common stock over the month containing the first mention of the takeover. The premium is the gain divided by the value at the end of the previous month.

****For takeovers with CEO changes only, the corresponding median values are:
Table 7, Specification 1: 52 observations, ratio of -0.02 at the median.
Table 7, Specification 4: 52 observations, ratio of 0.21 at the median.
Table 9, Specification 1: 39 observations, ratio of 0.05 at the median.
Table 9, Specification 4: 39 observations, ratio of 0.06 at the median.