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THE IMPACT OF INCENTIVE PLANS ON PRODUCTIVITY, WORKER QUALITY,
AND THE EXTENT OF SUPERVISION

By

Daniel G. Hansen

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ABSTRACT

THE IMPACT OF INCENTIVE PLANS ON PRODUCTIVITY, WORKER QUALITY, AND THE EXTENT OF SUPERVISION

By

Daniel G. Hansen

Incentive plans such as profit sharing plans or gain sharing plans have become an important part of the economy. This dissertation explores three distinct aspects of them.

The first chapter uses monthly, individual-level data collected from two units within a large U.S. financial corporation to estimate the individual response to the introduction of a group incentive plan. The findings indicate a large and statistically significant response to the plan. A closer look at the data reveals that the incentive plan caused performance to converge to a standard. The initially least productive workers improved greatly, while the workers who were initially performing well did not change their behavior. However, the incentive plan was successful in increasing the average level of productivity.

The second chapter examines whether higher-quality workers are attracted to (or retained by) profit sharing firms. Studies of the effect of profit sharing on productivity do not account for changes in the composition of the work force. Therefore, if turnover leads to a more productive labor pool for a firm, one cannot necessarily make the claim that the current studies demonstrate that profit sharing plans increase the performance of *individual* workers. Some findings from this study, indicating that workers entering profit sharing firms are “better” than those entering non-profit sharing firms, are

consistent with this bias. However, attrition out of profit sharing firms is no different than for non-profit sharing firms.

The final chapter explores whether compensation policies -- efficiency wages, profit sharing plans or performance-based bonuses -- act as substitutes for supervision. The results show that the shirking model of efficiency wages is not relevant for the majority of workers. However, evidence consistent with this theory is found for a sample of supervisors and for a sample composed of piece rate and commission workers. In addition, workers are not more likely to be offered incentive plans as the extent of supervision is reduced. However, *supervisors* are more likely to be offered performance-based bonuses as the number of workers supervised increases.

For my parents.

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Chapter 1

INDIVIDUAL RESPONSES TO A GROUP INCENTIVE

Introduction

It is well known that firms have difficulty in getting the desired level of productivity out of their workers. Piece rate wages and individual incentive plans can lead to higher individual effort, but present problems of their own. First, cooperation is not encouraged in these systems. For example, a bonus that pays only to the worker with the highest sales volume puts the members of the sales force in competition with one another, providing an incentive to sabotage a co-worker's efforts. Second, it may not be possible to implement an individual incentive because the output of individual workers is often difficult to measure. For example, a team of workers may be responsible for developing a software application. In the end, the application is produced, but it would be difficult to determine what was contributed by any one worker.

An alternative is to offer incentives based on group productivity in the form of either profit sharing or gain sharing plans. A profit sharing plan simply shares a fraction of the profits the company earns with the workers, usually on an annual basis. Gain sharing plans are based on more specific goals, with workers sharing in the financial gain that comes from achieving these goals. Several variants of gain sharing have been used, some with very specific compensation formulas.¹

Ideally, gain sharing plans are linked to quantifiable goals (such as fewer programming errors in the example above), but the workers share equally in the gain.

¹ For example, in the Rucker Plan, the worker's share is calculated by finding value added, computing from this the *allowed* labor costs (by multiplying value added by an historical percentage), and subtracting from this the actual labor cost. The remainder is shared between the workers and the managers.

Unlike with individual incentives, the workers are not pitted against one another, so any potential gains from cooperation should be realized. However, group incentives are plagued by the free rider problem. Under individual incentives, if there are N workers in the firm, and one employee improved her productivity by \$100, this would (could) lead to a \$100 reward. But if the gains are shared with the work group, this \$100 gain in productivity only leads to a $\$100/N$ gain to the worker. As the size of the group grows, the individual incentive to expend more effort decreases. Furthermore, if it is assumed that the marginal cost of effort increases with effort and workers have identical cost-of-effort functions, the hardest workers are the least likely to respond to the incentive plan. That is, the workers who are expending the highest levels of effort prior to the implementation of the incentive plan need a larger reward in order to elicit increases in their effort.

The majority of the previous studies on the effects of these incentive plans have used firm-level data, typically using value added as the dependent variable. Weitzman and Kruse (1990) provide a thorough survey of the literature.² They conclude that the "mean estimated effect of profit sharing on productivity for 'average' amounts of profit sharing ... is 7.4 percent, with a median estimate of 4.4 percent."³

In contrast, this paper examines *individual* responses to the introduction of a gain sharing plan. The data come from two units within American Express Financial Advisors, a subsidiary of the American Express Corporation. The only comparable study appears to be Weiss (1987).⁴ He studies three plants within a large U.S. electronics manufacturer. At these plants, newly hired workers face an individual incentive, but once they have reached a performance threshold--usually occurring after three months--

² Weitzman and Kruse consider profit sharing and gain sharing plans to be fundamentally the same, and therefore examine them together.

³ Weitzman and Kruse (1990), p. 138.

⁴ A recent paper by Shearer (1996) uses individual-level data from a firm using a group incentive plan. However, the goal of that paper is to estimate a tenure-productivity profile *controlling for* the incentive effects, and not to directly estimate the impact of the group incentive.

the workers' pay is proportional to the output of their work group.⁵ Weiss then estimates the change in individual output when the workers change incentive schemes. He finds that "Almost all of the workers whose first month performance was above the median decreased their output between months 1 and 4, ...[but] only half of the low performing workers decreased their output."⁶ In addition, "Among workers whose first month output was more than 10% below the median, 79% increased their output. Out of the 208 workers whose first month output was more than 10% above the median only one worker increased his output."⁷ In other words, the group incentive failed to motivate the majority of the workers, and workers' performance converged to a standard. Weiss believes that the most able workers are held back both by "trivial financial incentives" and pressure from other workers.⁸ The conclusion generally reached from his study is that group incentives can fail where work groups are large.⁹

The findings in this study indicate that, as in Weiss (1987), the workers who are initially the most productive are not motivated, and that performance converges to a standard. However, the incentive plan was effective at improving the mean level of productivity and therefore should be considered a success.¹⁰

The Data and Setting

The two units to be examined comprise the service representatives for the discount brokerage area of American Express Financial Advisors. The units perform identical tasks, and are physically adjacent. One unit is responsible for clients in the Western half of the United States, while the other covers the remainder of the country.

⁵ The work groups studied in Weiss (1987) average 126 members in size. As shown in Table 1, the groups studied in this paper are substantially smaller, averaging about 10 workers per group.

⁶ Weiss (1987), p. 140.

⁷ Weiss (1987), p. 140.

⁸ Weiss (1987), p. 143.

⁹ Brown (1990), p. 174-S.

¹⁰ It cannot be determined if the incentive plan was, strictly speaking, a financial success for the firm. While the payouts to workers can be calculated, it is not possible to place a monetary value on the change in worker behavior that occurred.

Each unit has its own supervisor, who reports to still another manager who oversees both units. The units are rated jointly to determine the size of their bonus. Although they could operate in isolation, in practice there is a great deal of cooperation between the units, so a joint rating is not inappropriate.

The workers are primarily responsible for data entry, bookkeeping and receiving calls from clients.¹¹ There are two distinct phone lines to which a worker may be assigned: the service line and the trading line. New transactions (the actual buying and selling of securities) take place on the trading line. As this is a discount brokerage firm, it is not the worker's job to give advice or promote sales. Because of this, conversations on the trading line follow a standard process of information gathering (finding out how many shares to trade, etc.). The service line handles a wider variety of calls, ranging from simple questions about the status of a trade to a complex solution to a trade error. All of the workers are qualified to work on the service line, but only some of them have obtained the licensing required to work on the trading line.

This study examines only performance on the service line. This is limiting in that it cannot be determined whether any measured response in this area comes at the expense of the other tasks.¹² As is noted below, the specific goals of the incentive plan focus on improving performance on this line, and in fact constitute the only quantifiable goal. Therefore, an analysis of the impact of the plan using telephone performance data should be sufficient for determining whether or not the incentive plan succeeded.

The telephone performance data do not depend on individual reporting. In order to take calls off of the queue, workers log onto a system, after which the number of calls answered, the amount of time spent on calls, the amount of time available to take calls (i.e. logged onto the system), and the number of calls transferred are all recorded. The

¹¹ The information regarding details of the incentive plan, work conditions and responsibilities of the workers was obtained while I was employed with this firm. I was a temporary employee, however, and therefore not involved in the incentive program.

¹² One advantage Weiss (1987) may have relative to this study is that the performance measure he uses may be a better measure of a worker's total output on the job.

numbers are totaled for each worker on a weekly basis and sent to the supervisor. Only monthly data were available for use here. The average minutes spent per call in a month (abbreviated "minutes per call" hereafter) is used as a proxy for productivity. Workers who take fewer minutes per call can handle a higher volume of calls, and are therefore considered to be more productive. There are some potential problems with this interpretation, however, which will become apparent below.

The distribution of incoming calls across workers is random, so in principle the variety among types of calls should not affect the average minutes per call across workers. However, a worker can choose to transfer a problem to someone who is considered more able. Although any call that a worker receives from the queue is included in the data, a call received via transfer from another worker is not recorded. Since transferred calls are likely to be short, there may be a bias in minutes per call, associating less able workers with lower minutes per call. In the empirical analysis, this is taken into account by adding the percentage of calls that a worker transfers to other workers as a control variable.

Workers differ substantially in their methods. Some prefer to call back clients instead of leaving them on hold, some are more thorough (and therefore lengthy) in their descriptions, etc. These differences make it difficult for supervisors to judge relative performance in this area. If a worker is taking more time per call it could mean that better service is being given, fewer problem calls are being transferred to other workers, or that he or she simply likes to chat with clients.

Given these problems with the data, one must be careful in interpreting what the variable minutes per call can say about worker performance. One cannot necessarily compare two workers in one month, observe that one's minutes per call is higher, and conclude that he or she is less productive. It is true that fewer calls can be answered, but if more complex problems are dealt with, it could be that this worker is more valuable.

Although the level of minutes per call may not be as informative as one would like, the change in minutes per call for a given worker when the incentive plan is implemented does indicate whether effort was exerted to fulfill the established goals. That is, the primary goal of the plan was to reduce the amount of time that a client must hold before speaking to a representative. From an individual standpoint, the only way to achieve this is to spend less time on each call, thereby getting to the calls in the queue faster. Therefore, if workers take less time per call after the implementation of the incentive, they have responded positively to the incentive.

The details of the incentive plan are as follows. It was conceived in periodic meetings between managers, supervisors and workers in late 1991, and the performance was to be evaluated during the following year. The meetings were used to discuss the most pressing needs (or shortcomings) of the department, and the best means of addressing them. Following this, the specific goals were agreed upon, put in print, and distributed to all of the concerned parties.

The amount of the bonus was based on a unit rating given by upper management, and all members of the unit, including supervisors, were eligible for the bonus. Workers hired during 1992 were eligible for a pro-rated bonus. The awards were to be paid in early 1993, with the highest rating paying each worker \$1,150, the second highest \$800 and the third highest \$600.¹³ There were two lower ratings that did not merit bonus pay. In addition, all of the workers were had to meet a minimum individual performance rating (given annually by the supervisors) in order to be eligible for the awards. The requirements for meeting this minimum were not likely to have been stringent enough to preclude the possibility of free riding, however, as the lowest ratings were the equivalent

¹³ Since the reward schedule is specified in advance, the nature of the free rider problem in this firm is somewhat different than that described in the introduction. That is, any gains in individual productivity are not directly shared with the other workers, but contribute toward the team goal. Free riding is still an issue, however, as a worker who contributed little to an otherwise successful plan would receive the same reward as the other workers.

of being placed on probation and were rarely given. All of the workers in the sample were given performance ratings the exceeded the minimum.

As stated above, the plan focused on reducing the amount of time callers had to wait to receive service. This is measured at the unit level by the "ASA", or average speed of answer.¹⁴ In June 1991, the ASA was about 79, meaning that on average callers waited 79 seconds before speaking to anyone. The targets for the plan were as follows: a reduction to 35 for the highest rating, 55 for the second and 65 for the third.¹⁵

The Model and Empirical Results

The dependent variable in all of the following specifications is the monthly average minutes spent per call for each worker. The independent variables control for changes in the workplace conditions such as supervisory policy and demand conditions. Data are not available to determine if a change in supervisors occurred in the sample period, but information on supervisory policies do exist. That is, there are two actions that a supervisor may take to reduce a client's time on hold: hire more workers, or schedule more telephone time for the existing workers. In fact, as Table 1 shows, the number of workers did increase over the sample period, from about ten workers per unit in 1991 to thirteen workers per unit in 1992.¹⁶

A change in the allocation of a worker's time is indirectly measured by the percentage of a worker's time spent logged on the phone (i.e. available to take a call), but not speaking to a client (abbreviated PERCTIME). Telephone time was scheduled for each day, and supervisors may have increased the amount of time scheduled in order to

¹⁴ Because the ASA is only available at the unit level, but the analysis is conducted at the individual level, minutes per call is used as the proxy for productivity.

¹⁵ Other, more qualitative goals were mentioned. As an example, "Actively develop a pro-active vs. re-active listening approach." Given the difficulty in measuring any progress towards such a goal, it is easy to see why managers would focus on changes in the ASA as the definition of success or failure of the plan.

¹⁶ This count includes all of the workers in the units, including those not in the sample.

help meet the goal of reducing the ASA. PERCTIME increased from 1991 to 1992, a finding one would expect to observe if more time was allocated to the telephones.

The monthly volume of calls received by each unit controls for changes in behavior due solely to fluctuations in the amount of business received. Table 1 shows that the volume of business was, on average, increasing over the sample period, presenting the possibility that workers may have hurried through calls simply because more calls were coming into the queue.

An examination of the workers' characteristics in Table 1 reveals that there were slightly more women than men, the vast majority were college educated and workers averaged about seven years of pre-company experience.¹⁷ The average nominal wage was between \$12 and \$13 per hour.

The data are monthly, from June 1991 through November 1992. Although a total of forty-two people were employed in the two units during this time period, only twenty-one of them are in the sample both before and after the implementation of the incentive plan.¹⁸ The average number of workers per unit in Table 1 refers to the *actual* number of workers per unit. This variable, and not the number of workers in the sample, is used in the analysis because it is the actual number of workers that could affect the behavior of the workers. The average number of workers *in the sample* is also shown in Table 1, revealing that on average there are fewer workers in the sample in 1992 than in 1991.

The basic form of the model that is estimated is:

$$\ln(\text{minutes per call})_{it} = \sum_i \alpha_i I_i + \beta_1(\text{incentive plan})_t + \beta_2(\text{workplace controls})_{it} + \varepsilon_{it}$$

¹⁷ Pre-company experience was estimated by age-tenure-education-6, where a high school degree was equivalent to twelve years, an associate's fourteen and a bachelor's sixteen.

¹⁸ Complete data on the entire work force were not made available, preventing a study including all of the workers.

where i indexes workers, t indexes months and I_i are dummy variables for individuals. The model is run both with and without the workplace controls. The "incentive plan" variable is a dummy variable equaling zero for months in 1991 and one for months in 1992. This specification of the incentive plan may be considered a shortcoming of this study because other changes affecting the productivity of workers may have occurred during this time. Under these circumstances, the dummy for the incentive plan may be picking up these other effects.

Several measures have been taken in order to minimize the possibility that the results are contaminated in this way. First, the data do not begin until June of 1991 because of a reorganization. It was at this time that the workers split into two units handling accounts from different regions of the country. No other reorganization occurred during the sample period. Second, the analysis controls for the monthly volume of calls to account for any demand fluctuations. Third, variables controlling for supervisory behavior are included. Specifically, variables are included to reflect the fact that more workers were hired and that the time within the work day was reallocated.¹⁹

The first set of regression results is reported in Table 2. Fixed effects estimation is used throughout. An F-test of the joint significance of the individual dummies produces an F-statistic of 11.22, rejecting the null hypothesis that the pooled model is efficient.

Column 1 shows the effect of the incentive plan with no control variables included. On average, minutes spent per call went down by about thirteen percent in 1992. Column 2 adds the workplace controls, increasing the estimated effect of the

¹⁹ The supervisors could also increase the level of their monitoring, but this cannot be measured. A possible problem with the analysis is that the supervisory policies that can be included (hiring and reallocating time) may lead to higher minutes per call for existing workers, while the excluded action (increased monitoring) should reduce minutes per call. This may bias the results toward finding a large independent response from workers to the incentive plan. The entire analysis has been done excluding all supervisory behavior (dropping PERCTIME and number of workers in the unit from the regressions). The results from these regressions show a stronger estimated response in most specifications, with decreases in the estimates (in magnitude and significance) occurring only when a time trend is included.

incentive plan. The estimates of the workplace controls are generally highly significant, and consistent across models.

A potential problem arises from the fact that the panel of workers is unbalanced, raising the possibility that attrition from the panel may be affecting the results. That is, one may falsely observe that the incentive plan was successful if less productive workers are in the sample prior to the beginning of the plan, but leave soon after implementation. To test for this, the sample is divided into two groups: those who performed better than and worse than the median level in 1991.²⁰ A worker's presence in the panel is represented by two variables, the number of months present in the data in 1991 ("LENGTH91") and the number of months present in the data in 1992 ("LENGTH92").²¹ A test for the equivalence of means between the two groups is done for these variables. In both cases, the test does not reject the hypothesis that the means for the two groups are equal.²² Since neither group is greatly over-represented either before or after the incentive plan is introduced, one cannot make the claim that an improvement may be observed only because inferior workers were weeded out of the sample.

A second test for sample attrition bias is conducted by observing how the results change as the sample period is shortened. The results (using the specification in column 2 of Table 2) are contained in Table 3. Notice that, regardless of when the sample is cut off, the qualitative finding of a strong response to the incentive plan remains. In addition, the way the estimated effect of the incentive plan evolves in relation to the sample size shows that sample attrition bias is not driving the results. In February 1992, the sample size is at its largest level, containing all twenty-one workers. If the data are

²⁰ This is done based on the fact that the estimated response is driven primarily by the workers who performed worse than the median in 1991.

²¹ There are 18 months of data, beginning in June of 1991. So, for example, a worker who was in the data from November of 1991 through March of 1992 would have LENGTH91 = 2, LENGTH92 = 3.

²² The means are as follows (workers better than median, workers worse than median): LENGTH91 (5.9,6.3) and LENGTH92 (7.5,7.7).

cut off at this point, the estimated effect of the incentive plan is about an eleven percent improvement. Five workers leave the sample from February to March, the largest drop between any two months. Yet, the estimated effect of the incentive plan using data through March is almost identical to the February estimate. Furthermore, no additional workers are lost through May of 1992, yet the estimated effect of the incentive plan increases to about fifteen percent. Given these facts, it does not appear that the estimated improvements in minutes per call are due to attrition from the sample.

Columns 4 and 5 of Table 2 refine the previous estimates by correcting for serial correlation and including a time trend. Quasi first-differencing is performed in order to correct for serial correlation using a separate value of ρ for each worker.²³ As shown in column 3, the finding that the incentive plan was a success is robust to the correction for serial correlation. Furthermore, while the addition of a linear time trend reduces both the magnitude and significance of the estimated effect of the incentive plan, the estimate is still sizable and significant at the ten percent level.

It seems that the finding of a significant response to the incentive plan is fairly robust. Given that the variable representing the incentive plan is merely an indicator equaling one for months in 1992, this result shows that the average minutes per call dropped significantly across years, but gives no indication about the immediacy of the impact. Figure 1 graphs the pooled average minutes per call across months. This shows clearly that it took several months for a large effect to occur. There is no excluded variable known to the author that would cause a large change from March through May.²⁴ The volume of business was increasing, as was the number of employees, but

²³ The quasi first-differencing is performed as follows: ρ is estimated as the coefficient on the residuals from month $t-1$ regressed on the residuals from month t (separately for each worker). The data are then transformed by: $x_t - \rho(x_{t-1})$. The first observation for each individual and any observations following gaps in the data series within individuals are dropped in this process.

²⁴ Changing the specification of the incentive plan dummy variable to begin one to two months before and after the actual implementation produces the following coefficients (using the specification from column 2 of Table 2): November 1991: -.135, December 1991: -.139, January 1992 (actual start date): -.184, February 1992: -.158, March 1992: -.160. Note that the largest estimated effect of the incentive

this has been controlled for in the regressions. One possible explanation is that free riding occurred in the time shortly after the implementation of the incentive plan. It may be rational for the workers to attempt to free ride early on, as an *immediate* response is not required for the incentive plan to be considered a success. This could be taken as evidence that the free rider problem may be overcome in a repeated game setting, as was theorized in Weitzman and Kruse (1990).

The next issue to explore is whether the workers who were initially more productive responded as strongly to the incentive plan. To do this, the average minutes per call is calculated for each worker and for the total unit for 1991. The sample is then divided into those who started better and worse than the median level of minutes per call.²⁵ Table 4 shows the estimates for these two groups. The first two columns can be compared to column 2 of Table 2, while the second two columns employ the specification used in column 4 of Table 2. These estimates show that the initially slow workers responded strongly to the incentive plan, but the initially fast workers showed no response at all.

Figure 2 shows the change in the productivity of individual workers graphically. The horizontal axis measures the average minutes per call for each worker in 1991, while the vertical axis measures the change in minutes per call from 1991 to 1992, calculated so that a positive number indicates an improvement (reduction in minutes per call). The vertical line is drawn at the 1991 median level of minutes per call. Of the ten workers who were better than the median in 1991 (to the left of the vertical line), six showed a decline in performance while only three improved.²⁶ However, all of the ten workers who performed worse than the median in 1991 improved their performance.

plan occurs at the actual start date, as one would expect if the estimates are picking up the effect of the incentive plan, and not some other event.

²⁵ The worker at the median is dropped from the sample.

²⁶ Only one of these differences is significantly different from 0, however.

There are several possible explanations for the fact that the initially faster workers did not improve in 1992. First, this finding could support Weiss' (1987) result that group incentives do not motivate the "best" employees. Second, it could be that there is a minimum amount of time that is required to provide service, so that those who were already performing well had very little room to improve.

Third, given that the calls are initially randomly distributed, it is possible that this effect merely reflects a regression to the mean. That is, the workers who were slow in the early months may have been as productive as other workers, but were "unlucky" in that they happened to draw long calls of the queue. The following test is done in order to see if this may be affecting the results. A dummy variable is created for those who were slower than the median worker from July through September of 1991. A regression is then run using the previous specification, but including the newly created dummy, for the months of October through December of 1991. The use of only 1991 data ensures that the incentive plan is not affecting the test. This regression produces a coefficient of .529 (with a standard error of .093) on the dummy variable for the initially slow workers, showing that workers who began the sample by taking more time on calls continued to be slow in the following months. Given this, the results do not seem to be affected by regression to the mean.

Finally, an Akerlof-type gift exchange model might explain why the better workers seemed to ignore the incentive plan. If they felt that the slower workers were made to look bad by the disparity in performance, they may have attempted to lessen the difference by waiting for the slower workers to improve before attempting such an improvement themselves (Akerlof, 1986). According to Akerlof, this represents a gift exchange between workers motivated by the fact that, "in working together, workers acquire sentiment for each other."²⁷ The situation here is somewhat different than Akerlof describes, however. The workers that he describes are performing in excess of

²⁷ Akerlof (1986), p. 74.

the established standard as a gift to the firm. In exchange, the firm must not raise the standard, ensuring that the marginal workers meet the required performance standard.

In contrast, the "better" workers studied here are theorized to be refraining from raising the standard, or helping the less efficient workers by handling the more difficult calls (via transfer, which would not be recorded in the data). In exchange, the "worse" workers are responsible for increasing their level of performance enough to meet the goals of the incentive plan. The firm receives the gift of faster service, and in exchange must pay all of the workers for any improvements. Frequent feedback regarding the progress made toward lowering the ASA (recall that telephone performance data were available each week) makes this exchange among workers possible. If, after a few months, the goal appeared to be unattainable under the arrangement, the faster workers could simply speed up and achieve the goal. Therefore, the gift that the faster workers are attempting to give may not jeopardize their chances for the bonus pay.

The final issue to be addressed here is whether the observed change in minutes per call was purely cosmetic, or a real improvement on the part of the workers. In principle, minutes per call could easily be manipulated by answering the phone, taking a message, and then returning the call. Only the short initial call would be recorded as data, while the total time spent with the client may have increased. However, it is unlikely that this occurred, since the managers are not directly concerned with a change in minutes per call, but in the average speed of answer (ASA), which is not as easy for a worker to manipulate without a real improvement.

There is one way that ASA can be manipulated, but managers were not only aware of this possibility, but actually tried to use it to their advantage the previous year. As one might expect, this job faces a peak-load problem. During peaks, wait times for callers may increase dramatically. In the Spring of 1991, the "release valve" was created in an attempt to solve this problem. One person would log onto the telephone queue and only take brief messages for the calls answered. The messages were then distributed to

the workers so that the calls could be returned when business slowed. Note that the use of this method may result in a reduction of the ASA, even though the clients do not receive service any sooner (nor, possibly, any more efficiently). The release valve was considered to have benefits in spite of this because it alleviated the frustration clients felt from remaining on hold for an extended period of time.

Although the theory behind the release valve was sound, it ultimately failed because business rarely slowed enough to allow for time to return the calls. Because of this, the method was abandoned within months of its inception. Its existence, however, signals that the managers knew how the ASA could be manipulated. The fact that, with this knowledge, they chose to award bonuses suggests that the incentive plan caused a real improvement in service.

Comparison to Other Studies

Given that this paper finds that a group incentive can increase the average level of productivity but Weiss (1987) does not, it is instructive to compare the two environments that are studied. First, the relative size of the work groups studied are quite different. A total of 42 employees worked in the two units studied here over the sample period, versus an average work group of 126 in Weiss' paper. Given that the free rider problem should worsen as work groups increase in size, this may be an important reason for the difference in findings.

There is, however, some evidence suggesting that size may not be negatively correlated with the success of an incentive plan. For example, Schuster (1984) uses longitudinal plant-level data to examine the effect of a Scanlon Plan. This variant of gain sharing uses committees of workers to evaluate suggestions from other employees, and a bonus formula to share gains in productivity among the workers. In spite of the fact that there were 890 non-supervisory production employees in a plant,²⁸ Schuster finds that the

²⁸ The size of the work groups is not reported.

plan had a significant impact on productivity, and gave much of the credit for its success to employee participation in decision making. In addition, in his survey of the literature Schuster concludes that "firm size correlated positively with rated Plan success and Plan retention".²⁹ This may be evidence that the specific form of the incentive plan, and not the size of the work groups, is what led to the difference in the measured success between this study and Weiss (1987).

Second, the form of the incentive plan examined here differs from the one in Weiss (1987) in several ways. In the plan studied here, specific goals were set, so that the success or failure of the incentive plan could be easily determined. In Weiss (1987), the group incentive is not a goal-based incentive, but an everyday compensation formula, in which an individual is placed in a group, and each worker receives pay in proportion to the output of his or her group. In addition, the firm studied in this paper used committees consisting of workers and managers to set the goals. These could be important differences, and form a common theme in the industrial relations literature on group incentives.

Strauss (1990) lists some of the reasons that goal-setting and employee participation may raise productivity. They include: "Participation may result in better decisions..."; "Participation may improve communication and cooperation..."; and "Participation frequently results in the setting of goals. There is considerable evidence that goal setting is an effective motivational technique."³⁰ Schuster (1984) provides an example of the empirical support for these assertions, in that the use of these methods resulted in an increase in productivity.

²⁹ Schuster (1984), p. 25.

³⁰ Strauss (1990), p. 5.

Summary and Conclusion

This paper uses individual level data from the discount brokerage area of a large firm to examine the impact of a group incentive plan. It shows that there was a significant unit level response, about a seventeen percent improvement from the pre-plan average. Given the imperfection of the proxy for productivity and the fact that only one dimension of performance is examined, there is some question as to whether this can strictly be thought of as an increase in productivity, but there can be little doubt that at least a change in behavior occurred.

Although the group incentive did increase the average performance for the units, those workers who were initially the best may not have been motivated by the plan. Some of these workers did not merely show less improvement than those who were initially worse, but actually declined in performance after the introduction of the incentive. Here too, the limitation of the dependent variable makes the conclusion tenuous. That is, if those who were initially taking very little time per call were subsequently asked to handle more time consuming problems or provide better service, they may have improved in a way that the data cannot measure. Whatever this researcher's judgment is regarding the success or failure of the incentive, the managers who evaluated the plan deemed it successful enough to merit the second highest rating, giving each worker \$800.³¹

The finding that performance converges to a standard was also observed in Weiss (1987). Given the many differences between the environments, this is a surprising similarity, and may indicate a pattern in workers' responses to group incentives.

However, Weiss (1987) can also be interpreted as showing that group incentives may be ineffective when work groups are large, whereas this study found an

³¹ You may recall that, in order to be given this rating, the incentive plan called for a reduction in the ASA from 79 seconds to 55, a 41 percent reduction. It may seem that the 17 percent reduction observed in individual minutes per call could not account for this, but the two statistics are not comparable in this way. Remember that ASA is a unit-level measure, and would also be affected by such things as hiring more workers and the allocation of more of each employee's day to the telephones.

improvement in mean performance. Evidence from other studies has been presented suggesting that the design and implementation of the incentive plan, and not the difference in the size of the work groups, led to this disparity.

This paper therefore presents the possibility that a well designed group incentive can increase the mean level of productivity in a variety of settings. An important caveat to this is that the initially most productive workers may be largely unaffected (and possibly adversely affected) by the incentive.

Table 1
Means of the data by year

	1991	1992
Minutes per call	4.86	4.22
Percent of worker's time logged on the phone, but not on a call	.22	.24
Percentage of the unit's volume taken by the worker	.09	.05
Percentage of calls transferred by the worker	.26	.21
Average number of workers in the unit	9.4	13
Average number of workers in the sample (two units combined)	18.7	15.8
Average monthly volume of calls received by the unit	5,411	6,277
Male	.43	.43
High School	.10	.10
Associate's	.10	.10
Bachelor's	.80	.80
Tenure (months)	11.9	18.9
Pre-company experience (years)	7.2	7.2
Nominal wage (hourly)	12.41	12.92

Table 2
Fixed Effects Estimates of the Effect of the Incentive Plan

	(1)	(2)	(3)	(4)
Incentive plan	-.134** (.031)	-.184** (.043)	-.174** (.043)	-.100* (.061)
Serial Correlation Correction	No	No	Yes	Yes
Trend	---	---	---	-.019** (.009)
Percent of worker's time logged on the phone, but not on a call	---	-.620** (.113)	-.547** (.117)	-.467** (.120)
Percentage of the unit's volume taken by the worker	---	-.979** (.361)	-.925** (.366)	-1.085** (.370)
Percentage of calls transferred by the worker	---	-.613** (.290)	-.726** (.300)	-.781** (.304)
Number of workers in the unit	---	-.007 (.010)	-.0004 (.012)	.015 (.014)
Log(Volume of calls received by the unit)	---	.115 (.083)	.114 (.077)	.038 (.085)
N	290	290	266	266

Notes: Standard errors are in parentheses. Log(minutes per call) is the dependent variable. Therefore, lower numbers mean higher productivity. All models include a constant. The incentive plan variable is a dummy variable equaling one for months in 1992. Observations are lost in the correction for serial correlation because of breaks in the data series for three of the workers. ** denotes significance at the five percent level. * denotes significance at the ten percent level.

Table 3
Fixed Effects Estimates Testing for Sample Attrition Bias

	<u>Sample Cut-off Date</u>				
	<u>February 1992</u>	<u>March 1992</u>	<u>May 1992</u>	<u>August 1992</u>	<u>November 1992</u>
Incentive plan	-.109* (.059)	-.106* (.057)	-.149** (.054)	-.151** (.048)	-.184** (.043)
Number of workers in the sample at the cut-off date	21	16	16	14	11
N	169	185	216	260	290

Notes: Standard errors are in parentheses. Log(minutes per call) is the dependent variable. Therefore, lower numbers mean higher productivity. All models include a constant. The incentive plan variable is a dummy variable equaling one for months in 1992. The specification used is the same as the one used in column 2 of Table 2. ** denotes significance at the five percent level. * denotes significance at the ten percent level.

Table 4
Fixed Effects Estimates of the Separate Samples

	No Serial Correlation Correction		Corrected for Serial Correlation	
	<u>Initially better workers</u>	<u>Initially worse workers</u>	<u>Initially better workers</u>	<u>Initially worse workers</u>
Incentive plan	.007 (.051)	-.339** (.065)	-.014 (.067)	-.173* (.095)
Trend	---	---	.001 (.011)	-.033** (.014)
Percent of worker's time logged on the phone, but not on a call	-.358** (.121)	-.889** (.187)	-.309** (.113)	-.818** (.224)
Percentage of the unit's volume taken by the worker	-.332 (.390)	-2.060** (.625)	-.090 (.385)	-2.103** (.633)
Percentage of calls transferred by the worker	-.645* (.339)	-.419 (.431)	-.884** (.345)	-.315 (.488)
Number of workers in the unit	-.009 (.012)	-.019 (.016)	.002 (.016)	.015 (.023)
Log(Volume of calls received by the unit)	.048 (.100)	.221* (.125)	.031 (.107)	.140 (.135)
N	134	140	122	129

Notes: Standard errors are in parentheses. Log(minutes per call) is the dependent variable. Therefore, lower numbers mean higher productivity. All models include a constant. The incentive plan variable is a dummy variable equaling one for months in 1992. Observations are lost in the correction for serial correlation because of breaks in the data series for three of the workers. ** denotes significance at the five percent level. * denotes significance at the ten percent level.

Figure 1
Average Minutes Per Call by Month

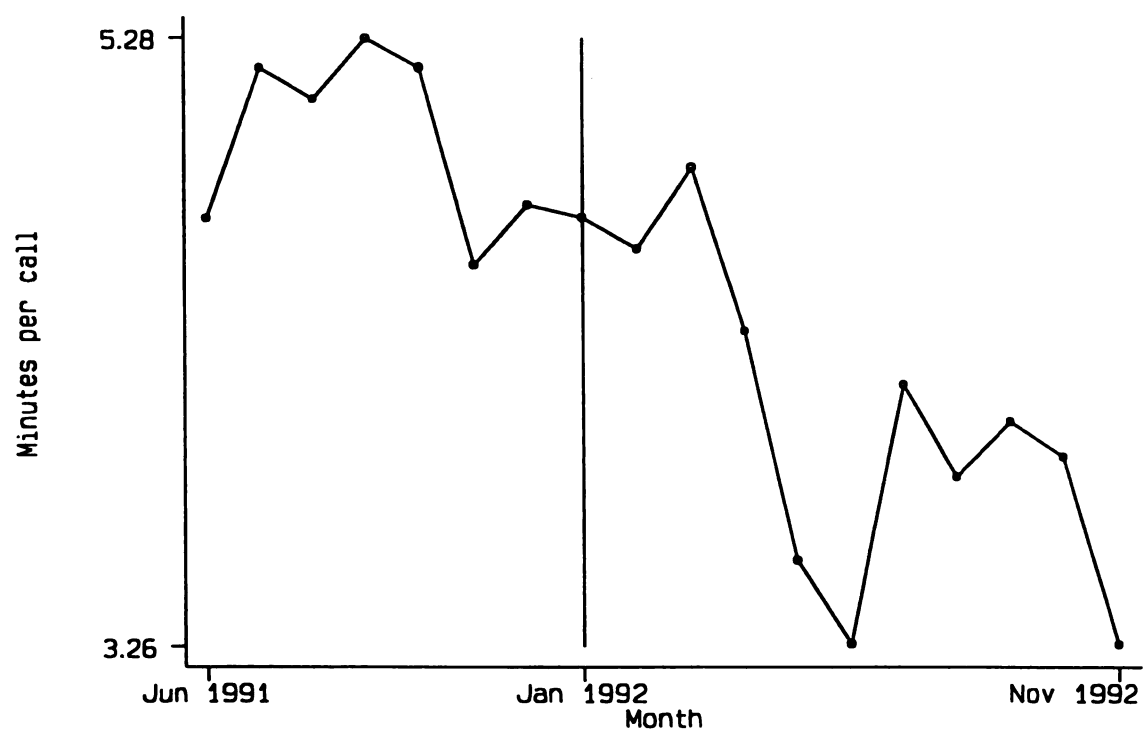
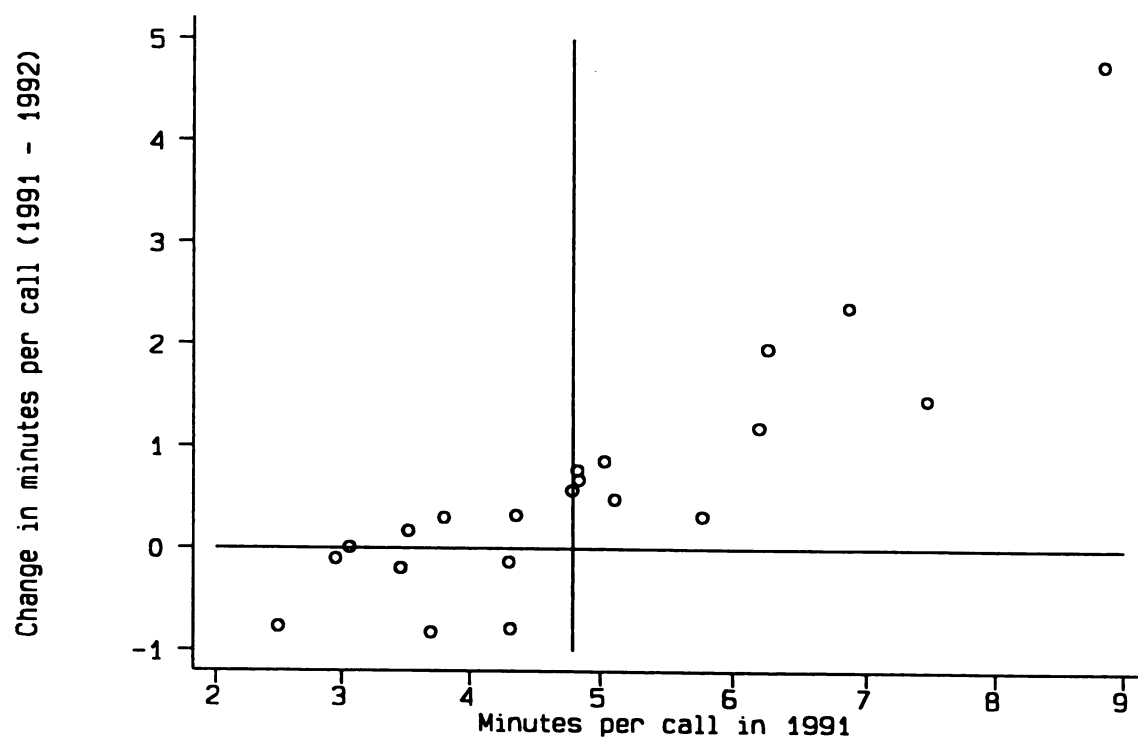


Figure 2
The Change in Individual's Minutes Per Call



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Chapter 2

WORKER QUALITY AND PROFIT SHARING: DOES UNOBSERVED WORKER QUALITY BIAS FIRM-LEVEL ESTIMATES OF THE PRODUCTIVITY EFFECTS OF PROFIT SHARING?

Introduction

It has been suggested that profit sharing plans may be used to increase the productivity of workers. While theoretical work on this issue is inconclusive (largely because of the free-rider problem), the empirical evidence has generally been favorable towards profit sharing plans. Kruse (1993) reviews 265 estimates of the effect of profit sharing and finds that, "8.7 percent take on negative values, and nearly all of these are within the range of sampling error [relative to zero], while 57.4 percent take on positive values where random sampling error can be ruled out as an explanation."¹ Weitzman and Kruse (1990) conduct a meta-analysis of the (then existing) firm-level studies, and conclude that the "median productivity difference associated with profit sharing was 4.4 percent."² Supporters of profit sharing plans interpret this figure as reflecting the incentive effects profit sharing has on worker behavior.

However, it is possible that some of the estimated productivity difference between profit sharing and non-profit sharing firms is not due to individual workers becoming more efficient, but instead is observed because the composition of the firms'

¹ Kruse (1993), p.55.

² Weitzman and Kruse (1990), p.138-9.

labor has changed. This possibility is most easily considered in the context of cross-sectional studies of firms, in which it is difficult to determine if observed differences between firms are due to the effects of profit sharing plans, or worker selection into profit sharing firms. For example, if firms with more productive workers tend to adopt profit sharing plans, or if higher-quality workers sort into firms with profit sharing plans, the estimated productivity premium for profit sharing firms in a cross-section includes this difference, and does not entirely reflect an improvement in the performance of workers.

Longitudinal studies of firms are less prone to this form of bias, but are not immune. Because data are available both before and after the adoption of a profit sharing plan, any characteristics of the firms that remain constant through the regime change can be made irrelevant by the use of fixed effects estimation. So, for example, if firms with more productive workers adopt profit sharing, and the same workers are present in the firm both before and after the adoption of profit sharing, the within-firm change in productivity captures the effect that the profit sharing plan has on the productivity of the workers.

However, this result only occurs if the composition of the work force is *identical* before and after the adoption of a profit sharing plan. This assumption is almost certainly not true, but its violation is only relevant if the incoming workers change the average quality of the firm's work force (and, as in previous studies, no information is available to explicitly account for this change). Therefore, this study does not explore the average differences between workers in profit sharing and non-profit sharing firms,

but instead examines the effect that profit sharing plans have on the quality of workers entering and leaving firms.

Worker selection into profit sharing firms could have policy implications, for if the previous estimates of productivity changes are partially due to worker selection, then mass adoption of profit sharing plans would not lead to as large of a gain for society as the current estimates suggest.³ For example, in the extreme case in which worker selection accounted for the entire estimated productivity difference, the adoption of profit sharing plans causes high quality workers to change location (though the magnitude of the sorting will dissipate as more firms adopt the plans), but the nationwide output per worker remains constant.

A longitudinal firm-level study by Kruse (1993) provides some indirect evidence on worker selection into profit sharing firms. If worker selection into profit sharing biases the estimated changes in productivity, one would expect that firm-level productivity would continue to rise following adoption, as the pre-adoption workers are gradually replaced by more productive workers through turnover. However, Kruse (1993) finds no significant trend effect for profit sharing firms.⁴ Even this may not solve the problem entirely, as a linear time trend may not reflect the turnover pattern following adoption. That is, if profit sharing brought about a large amount of turnover shortly after

³ That is, the productivity effect would be overstated. If, as Weitzman (1983) asserts, the national level of employment is stabilized, there may be some benefit. This paper will not speak to this issue, but Kruse (1995) uses the same data to address it.

⁴ Shepard (1994) also estimates the effect of profit sharing using longitudinal firm-level data. He includes a time trend, but does not interact it with the profit sharing indicator. The trend that is estimated is not significantly different from zero.

adoption and very little thereafter, a trend effect may not be observed, but worker selection could be affecting the results.⁵

This paper directly examines the issue of worker selection into profit sharing firms using recent data from the National Longitudinal Survey of Youth (NLSY). Workers are compared using three characteristics: years of education, scores on the Armed Forces Qualifications Test (AFQT) and wages. Both selection into and attrition out of profit sharing firms are examined. The findings indicate that workers *entering* profit sharing firms are “better” than those entering non-profit sharing firms, but attrition out of profit sharing firms is no different than that of non-profit sharing firms. The estimates indicate (under assumptions specified below) a .6 to .85 percent annual improvement in firm-level productivity due to the fact that higher-quality workers enter profit sharing firms. Given to the 4 percent increase in productivity found in Kruse (1993), the majority of the estimated effect of profit sharing plans represents a pool of workers becoming more efficient, rather than a shift in the composition of the pool toward more productive workers.

Theory

Although theory provides no clear answer regarding worker selection into and out of profit sharing plans, there are some reasons to believe that higher-quality workers may

⁵ Wilson and Peel (1991) find that quits are significantly lower in profit sharing firms than in non-profit sharing firms. However, this does not preclude worker selection from affecting the productivity estimates for these firms, as the level of quits is still positive (around 5 percent per year), and the period just following adoption is not observed (because none of the firms in the panel adopt profit sharing during the sample period).

prefer this form of compensation. For example, they may be attracted to profit sharing firms if they believe that profit sharing more accurately rewards them for their productivity. Alternatively, lower-quality workers may be attracted to profit sharing firms if they believe that they can benefit from the skill and effort of their co-workers through free riding.

Risk is another issue that the workers must face. Because compensation is tied to firm performance, profit sharing may increase the variability of a worker's income. If workers are assumed to have concave and identical utility functions, those with higher income should be more attracted (or less averse) to profit sharing plans. If it is further assumed that high-quality workers are paid more than low-quality workers, profit sharing may tend to attract (or retain) higher-quality workers.

Alternatively, workers may be attracted to a characteristic of firms that is correlated with the presence of a profit sharing plan. This has the same impact on firm-level productivity studies as the case in which workers are attracted to the compensation system itself, but may not require very strong assumptions about the agents. As an example, Kruse (1993) finds that a strong predictor of profit sharing adoption is an increase in the profit margin in the previous year. If successful firms tend to adopt profit sharing plans, these firms may be able to select the "best" workers from a queue. Workers are attracted to the firm because of the possibility of sharing in the firm's rent, allowing them to earn wages in excess of their marginal product.

The theories presented above are meant only to present the *possibility* that better workers may choose to enter profit sharing firms. This paper does not address which of

the above theories (or the many conceivable theories that are omitted) dominates a worker's decision about where to work. Rather, its task is to determine whether patterns in the characteristics of workers moving into and out of profit sharing firms are consistent with selection on the basis of worker quality.

Data and Methodology

This study uses data from the National Longitudinal Survey of Youth (NLSY) from 1988 through 1993.⁶ The survey dates back to 1979, but beginning in 1988 respondents were asked if profit sharing was made available by their current or most recent employer. Using this measure, three methods are employed to examine the issue of worker selection and profit sharing. First, workers changing jobs and entering a profit sharing firm are compared to those who change jobs and enter a non-profit sharing firm (both groups having left jobs in non-profit sharing firms). This is an attempt to explore whether workers who choose to enter (or simply tend to enter) profit sharing firms are different from those who do not.⁷

Job changers are of interest because they enter the firm following the adoption of a profit sharing plan. The alternative of including *all* of the workers who change profit

⁶ The percentage of U.S. firms making profit sharing payments was fairly constant over this period at about 22 percent (Kruse 1993, p. 8).

⁷ Kruse (1995) uses the same data with the primary intent of examining the association of profit sharing "with the disposition of a job over a five-year period, focusing on the risk of layoff." A portion of that work bears a surface similarity to this study: Kruse tracks job changers "to examine the change in pay associated with joining or dropping out of profit sharing (p. 16-7)." This study is concerned with a different measure: comparing the wages (and other characteristics) of workers when they report the *same* profit sharing status (but later differ), thus providing a proxy for productivity by not including any difference in wages due to profit sharing.

sharing status is not correct if the results are to have any bearing on a longitudinal firm-level study because workers who remain in a firm as it adopts a profit sharing plan do not affect fixed-effects estimates of the change in firm-level productivity. A second reason for interest in job changers is that they are more likely to be revealing some preference for the profit sharing environment. A worker who remains in a firm that subsequently offers a profit sharing plan may not be particularly fond of their situation, but may face mobility costs that exceed any disutility suffered due to the change in the compensation system.

The second part of the analysis examines workers who leave firms that adopt a profit sharing plan. To do this, the sample is reduced to include only those who held a non-profit sharing job, then stayed in that job as the firm adopted profit sharing. The workers who leave the firm in the following year are then compared to those who stay. This approach has the advantage of controlling for firm selection into profit sharing. Remember that for worker selection to have a bearing on a longitudinal firm-level study, turnover in the firm must change the average quality of the workers. Therefore, workers leaving profit sharing firms must be compared to the workers who stay in these same firms. Because the sample is reduced to *only* those workers in firms that adopted profit sharing, the workers leaving profit sharing firms are compared to the appropriate group.

The final method is similar to the second, but relaxes the sample restrictions. In this section, workers are categorized using two years of observations (rather than three). In the first year, only those workers who report that profit sharing is made available are included in the sample. Their status in the second year forms the categories (by second

year profit sharing status and whether or not the worker remained with the same firm). The advantage of this method is that it may provide evidence of the effect of turnover further after the adoption of a profit sharing plan. Because of the construction of the sample, the second method examines turnover immediately following adoption. In contrast, the third method potentially includes turnover at many points following profit sharing adoption. Therefore, while this study cannot speak to the quantity of turnover occurring at different points following adoption, it may be able to show a temporal pattern in the quality of the workers leaving firms with profit sharing plans.

Several proxies for worker quality are used. Years of education and the score on the Armed Forces Qualifications Test (AFQT) are used to approximate the respondent's skill level. While these measures should provide a good indication of the skill level of the workers, they may not be as effective in capturing less tangible qualities such as effort and the ability to work well in groups. The third proxy, wages, may be superior to the others because it conveys information on all productivity-related traits (as long as the correlation of wages with these traits has the expected sign).

Those reporting their industry as public administration in the current or previous year are removed from the sample. There should be no instances of profit sharing in public administration, as government work does not generate profits.⁸ In addition, those who report working less than 35 hours per week, less than 6 years of education, an hourly wage in excess of \$100 per hour or below one half of the minimum wage, are

⁸ In spite of this, 7 percent of those workers reporting themselves as working in public administration also report that profit sharing was made available (compared to 30 percent of the workers in the private sector).

excluded. Lastly, if a worker meets the requirements for entry into a sample more than once, one observation is randomly selected and used for the analysis. This is done so that workers with an increased propensity to change jobs do not receive greater weight.

The profit sharing variable is an indicator equaling one if profit sharing is reported as being made available by the employer in that year. This is not an ideal variable in that there are many types of profit sharing plans, and a worker's reaction may differ across types. For example, a deferred plan (which pays into a retirement fund) should provide a smaller incentive than a cash plan (which pays in the same year).⁹ This concern is lessened by the fact that the prediction for all of the plan types is in the same direction; only the strength of the incentive differs.

The fact that the profit sharing indicator depends on self-reporting raises the potential for measurement error. Kruse (1995) employs the same data, and notes that this response may be a bad indication of profit sharing status if workers report profit sharing as "not being made available" in years that a firm earns no profits, and therefore the value of the profit share per worker is zero.¹⁰ On the other hand, the self-reporting of profit sharing status may allow for a better measure of a worker's response if it captures the *perception* that the employer provides profit sharing. After all, in order for there to be an incentive effect, a worker must believe that she is, in fact, eligible to receive a share of the profits, regardless of the firm's classification of her status.

⁹ There is some evidence that the NLSY workers frequently report a profit sharing plan that contains at least some deferred component, in that 41% of those reporting that they have a retirement plan (other than Social Security) also report the presence of a profit sharing plan, while only 10% of those who do not report having a retirement plan report having a profit sharing plan.

¹⁰ Kruse (1995), p. 7.

Results for Job Changers Leaving Non-Profit Sharing Jobs

Table 1 shows the mean years of education, AFQT scores and hourly wages for the workers who changed jobs, classified by the profit sharing status of their new job.¹¹ The AFQT score reported in this table (and used in the regressions that follow) has been modified in two ways. First, each respondent's score has been rescaled to standard deviations from the mean. Second, the score is adjusted to reflect the fact that the respondents were administered the exam at different ages. All of the respondents were asked to take the AFQT in 1980, at which time their ages ranged from 15 to 23. Potentially, therefore, differences exist that reflect the fact that less schooling had been acquired at the time the test was taken. To correct for this, the AFQT scores are regressed on age dummies, and the residuals from this regression are used in the analysis that follows.

The means in Table 1 indicate that, relative to workers entering non-profit sharing firms, workers entering profit sharing firms score .124 standard deviations (4.9%)¹² higher on the AFQT, have .41 (3.2%) more years of education and are paid 27 cents (3.2%) more per hour prior to changing jobs.¹³ This wage measure, as opposed to the wage obtained after changing jobs, is used to avoid including any differences in wages

¹¹ Wages are expressed in 1983 dollars.

¹² The base used in the calculations is the weighted average for both groups. The percent difference for the AFQT scores is calculated using the raw scores on the exam (63.31 for those entering profit sharing versus 60.29 for those not entering profit sharing).

¹³ Job changers are defined as those reporting less weeks on the current job than weeks between interviews.

due solely to the presence of a profit sharing plan (because the sample is limited so that all of the workers are in non-profit sharing firms before changing jobs).

Table 2 presents OLS regression results with each of the three characteristics as dependent variables. The first row of columns 1 and 2 simply regresses the years of education and AFQT scores (respectively) on a constant and a dummy for workers entering a profit sharing firm. Columns 3 and 4, which present estimates using wages as the dependent variable, proceeds differently. An attempt is made in these regressions to net out any differences in wages due to discrimination or region, while retaining variation due to differences in “productive” characteristics such as tenure. For example, if profit sharing workers tend to have more tenure or experience than non-profit sharing workers, one does not want to remove these differences by including these variables in a wage regression as one typically would. However, it may also be inappropriate to exclude these variables entirely. If, for example, men have a higher tendency than women to enter profit sharing firms, but also tend to have more experience, the estimate of the wage difference for those entering profit sharing firms is biased downward if a dummy variable for men is included but experience is not.

In order to account for this, the wage regressions include two variables that are constructed as the residuals from regressions of male and white dummies on tenure and its square, experience and its square, education and marital status.¹⁴ This technique

¹⁴ The inclusion of marital status is based on the Korenman and Neumark (1991) finding that married workers are more productive, and therefore may deserve the premium they are observed to earn.

should remove the influence of differences in “productive” characteristics from the sex and race indicators.

In addition, in column 3 the residuals from a regression of wages on AFQT scores and years of education are used as the dependent variable in attempt to derive a result that is distinct from those obtained in columns 1 and 2. The estimates from the first wage regression (row 1 of column 4), show that the wages prior to changing jobs were almost seven percent higher for those entering profit sharing firms versus those entering non-profit sharing firms.¹⁵

Examining simple differences (as above) is sufficient if profit sharing is distributed equally across occupations within firms. Unfortunately, this does not seem to be the case. While Kruse (1993) finds that the occupational mix within profit sharing firms is not significantly different from other firms, the coverage within profit sharing firms does vary by occupation. Specifically, he finds that “[w]ithin profit-sharing firms, production and service workers are somewhat less likely to be covered (75.8 percent) than are clerical / technical (86.5 percent) and professional / administrative employees (87.6 percent).”¹⁶ This is important, for if one finds (as is the case here) that workers who report having a profit sharing plan are more educated on average, this could simply reflect that, within a firm more white collar workers are offered profit sharing, and not that profit sharing firms attract higher-skilled workers. Given this, it may be more

¹⁵ Region (northeast, south and west, with north central excluded) and year dummies are also included in the wage specifications.

¹⁶ Kruse (1993), p. 19.

appropriate to look at the differences in characteristics controlling for occupation. The second row Table 2 presents these results.

Row 2 of Table 2 shows that the inclusion of occupation dummies¹⁷ reduces the estimated differences between workers in each of the specifications. Still, the finding that profit sharing firms attract better workers persists in two of the four columns. Also, note that the inclusion of occupation dummies may be over-controlling if firms change their occupational mix following the adoption of profit sharing. Firms may have the incentive to substitute away from low-skilled workers and into capital in order to avoid sharing profits. If this results in an increase in the hiring of high-skilled workers, the average skill level for the firm increases, but not necessarily *within* occupations at the firm. This, combined with the fact that profit sharing is not distributed equally across occupations within firms, suggests that rows 1 and 2 bound the correct estimate of the difference between workers entering profit sharing and non-profit sharing firms.

There is another issue to be addressed before one can conclude that these results have any bearing on a longitudinal firm-level study. That is, even if the workers entering profit sharing firms are “better” on average than those who do not, the workers entering the firm must be better than the average worker *already in* the profit sharing firm. In an

¹⁷ There are eleven categories: Professional, technical and kindred; Managers, officials and proprietors; Sales workers; Clerical and kindred; Craftsmen, foremen and kindred; Operatives and kindred; Laborers, except farm; Farmers and farm managers; Farm laborers and foreman; Service workers, except private household; and private household. Those reporting private household as their occupation are excluded from the sample.

effort to account for this, various measures of firm size are used as proxies for the labor quality at the firm the worker enters.¹⁸

Rows 3 and 4 of Table 2 show the results including the proxies for firm size (of the firm into which the worker switched). The proxies are the log of the number of workers at the respondent's site, a dummy indicating a firm with multiple sites, and an interaction between the multiple-site dummy and an indicator for more than 1,000 employees at the other sites combined. These proxies are included with and without occupation dummies, so that row 1 can be compared to row 3, and row 2 to row 4. Although several coefficients remain significant following the introduction of these proxies, the fact the magnitude of the estimate is reduced seven of the eight specifications may indicate that the inclusion of better information about existing labor quality would reduce the estimates further.

This section has provided somewhat mixed evidence about workers entering profit sharing firms. Although it seems that the differences in AFQT scores and wages net of education and AFQT scores are negligible, more significant differences between workers exist for years of education and prior wages (not net of the other characteristics). To provide some perspective, the estimates show that if the *entire* work force turned over in the year of adoption, one would expect to see about a 3.5 to 5 percent increase in firm level productivity.¹⁹ The next section shifts the focus to workers *leaving* profit sharing firms.

¹⁸ This is based on the Brown and Medoff (1989) finding that larger firms pay higher wages in part because they have higher labor quality.

¹⁹ This number is based on the estimated wage differences in rows 3 and 4 of column 4 of Table 2. The alternative method of adding the first three columns of these rows (and assuming a return of .051 to

Results for Workers in Firms that Adopt Profit Sharing

This section presents a different attempt to control for the existing level of worker quality by studying workers in firms that adopt a profit sharing plan. While the previous analysis focused on workers who tend to *enter* profit sharing firms, this section examines those who *leave* firms following the adoption of a profit sharing plan. No attempt is made to differentiate between voluntary and involuntary separations, as either could contribute to a change in the composition of the firm's labor.

Three years of observations are required to categorize the respondents. For three of the groups, the respondents are required to have been in a non-profit sharing firm in year 1, then be observed in the same job in year 2, but reporting that a profit sharing plan is offered. Workers are placed in the groups based on their response in year 3: the respondent remains in the job and still reports having a profit sharing plan (PS-PS), remains in the job and now does not report having a profit sharing plan (PS-NPS), or the respondent leaves the firm (PS-LV). The remaining two groups are composed of workers who never report the presence of a profit sharing plan (from 1988-1993). NV-LV workers are observed to leave their job after holding it at least one year (to match the selection criterion for the profit sharing workers), and NV-ST workers remain in their jobs for at least two years.

standardized AFQT scores taken from a “kitchen sink” wage equation, and a 7 percent return for a year of education) yields the same estimate.

The observations meeting the requirements (from data series beginning in 1988, 1989, 1990 and 1991) are pooled to form the data set. In the rare case that an individual meets these qualifications more than once over the sample period, one observation is selected randomly. Years of education, standardized AFQT scores and wages (from year 1) are used to compare the workers.

The use of wages in this section is somewhat problematic. That is, if the wage from year 3 is used, a comparison is made between workers with a different profit sharing status, which is misleading if institutional differences exist in the setting of wages between the profit sharing and non-profit sharing sectors. The wage from year 2 may also be a bad proxy because of potential correlation between the selection categories and the error term. That is, a comparison is made between workers who are about to leave the firm with those who will be staying. It is difficult to distinguish whether observing a low wage draw for those leaving the firm reflects a lower level of productivity, or the worker leaving because of the perception that she is underpaid. Because of these problems, the analysis instead uses the wage from year 1. Due to the high correlation between the year 1 and year 2 wages (.64), this may not solve the endogeneity problem entirely, but should be less problematic than the alternatives.

Table 3 reports the means of the data for four categories of workers: PS-LV, PS-PS, NV-LV and NV-ST. PS-NPS workers are dropped from the sample because of the possibility that one of the two observations on profit sharing status is incorrectly reported. That is, in order to be classified as PS-NPS, a worker must report that her firm

adopted and then abandoned a profit sharing plan in the span of one year.²⁰ Therefore, the group containing workers remaining in profit sharing plans (in row 2) includes only workers classified as PS-PS.

A comparison of the first two rows of Table 3 shows that the workers remaining in profit sharing firms have significantly higher wages, slightly higher (though not significantly) AFQT scores, and essentially the same number of years of education (although the mean is slightly higher for workers *leaving* profit sharing firms). Although this comparison of means indicates that profit sharing firms retain workers of slightly higher quality, a simple comparison of the means may be misleading. That is, turnover in *any* firm may leave it with a higher quality work force, regardless of the profit sharing status of the firm. Because of this, a differences-in-differences estimator is introduced. The model that is used is:

$$\text{Trait}_i = \alpha_1 + \beta_1(\text{PS-LV})_i + \beta_2(\text{NV-LV})_i + \beta_3(\text{NV-ST})_i + \gamma' X_i + \varepsilon_i$$

where the trait is either the standardized AFQT score, years of education or the log of the hourly wage,²¹ X is included in the wage specifications and is a vector containing year dummies, region dummies and the transformed dummies for male and white (see previous section). PS-PS is the excluded group.

Table 4 reports the OLS estimates: row 1 reports β_1 , the difference between leavers and stayers for profit sharing firms, and row 2 reports the differences-in-differences estimator: $\beta_1 - (\beta_2 - \beta_3)$. Focusing first on the columns that do not include

²⁰ It should be noted that the relative size of this group (400) is disturbing if one believes that its size is an indication of the extent of the measurement error in the profit sharing indicator.

²¹ As in the previous section, two wage variables are used: one is the residual from a regression on education and AFQT scores while the other is simply the reported wage.

occupational controls, there is no significant difference between PS-LV and PS-PS workers in years of education or AFQT scores, but the wages for workers leaving profit sharing firms are significantly lower. However, once the “leaver” effect is removed using the differences-in-differences estimator (shown in row 2), this difference vanishes. Therefore, the differences between future leavers and stayers in profit sharing firms are no different than they are in non-profit sharing firms.

It should be emphasized that the labor quality proxies (the firm-size variables) used in the previous section are not necessary here because the PS-PS and PS-LV workers were *all* in firms that adopted a profit sharing plan. Therefore, if a below average worker is observed to leave their firm, it is now certain that the average quality of the remaining pool of profit sharing workers is higher.

The occupational controls are also not vital here, but may have some use. That is, because all of the workers in this sample report that their firm adopted a profit sharing plan, there is no longer the problem of comparing groups composed of different percentages of white collar workers. However, the use of occupational controls may be useful in determining whether firms change their occupational mix following adoption. To illustrate this, consider the case in which a firm shifts its occupational mix toward high-skilled workers. A larger fraction of low-skilled workers will then leave the firm (if the low-skilled workers are forced out), making the estimated difference between the stayers and leavers appear to be large simply because workers in low-wage occupations with lower educational requirements are leaving more frequently. If one controls for occupation, however, the differences between leavers and stayers should disappear if the

firm changes the ratio of high-skilled to low-skilled workers without changing the average level of quality within each category. For example, one would expect to see such a change in the estimated differences if secretaries that leave are no different from the secretaries that stay, but more secretaries than executives leave. Table 4 shows that this is not the case, however. Comparing the wage regressions in row 1 (containing the only the statistically significant estimates) shows that the estimated differences are unaffected by the inclusion of occupation dummies.

Of course, whether a change in productivity is due to lower quality workers leaving the firm (across all occupations) or a shift away from low-skilled labor is not important to the central issue of this paper. Either response leads to a change in the estimated firm-level productivity without an increase in the performance of individual workers. Because of this, the most informative estimates are those from the regressions without occupational controls.

Results Relaxing the Sample Restrictions

This section uses the same estimation method as the previous section, but relaxes the requirements for inclusion in the sample. While the previous section required three years of observations in order to categorize respondents, this section uses only two. For three of the groups, the first year includes only workers reporting the presence of a profit sharing plan. The observation in the second year is used to create the same categories as in the previous section: PS-PS, PS-NPS, and PS-LV. As before, the PS-NPS workers are dropped from the sample. In addition, the NV-LV and NV-ST groups are formed, but in

this case, “leavers” are workers who left the firm in year 2 (in the previous section, both groups remained in the same job from year 1 to year 2).

This method has two benefits. First, the sample size of profit sharing workers is greatly increased, allowing for more precise estimates of the differences. Second, it may allow an examination of turnover further distant from the adoption of a profit sharing plan. By construction, the second methodology examined workers immediately following the adoption of a profit sharing plan (which may be considered an advantage of the second method). In contrast, this section may be studying workers at any point beyond adoption, creating the possibility of observing qualitative differences in turnover across time.

Table 5 presents the means for this sample. A comparison of this table with Table 3 shows that the sample sizes for the profit sharing groups are three to four times larger, and that there are larger differences between the PS-PS and PS-LV groups in the new sample. Table 6 presents the results using the same specifications as in the previous section. Row 1 of Table 6 shows that there are consistent differences between future leavers and stayers from profit sharing firms. However, the differences-in-differences estimates, shown in row 2, show that these differences are the same for workers staying in or leaving non-profit sharing firms. Furthermore, a comparison across the columns shows that the occupational controls move the estimates in the expected direction, although frequently the difference is not substantial.

The previous two sections have attempted to observe whether attrition out of a profit sharing firm improves the average quality of its workers. The findings indicate

that, while some differences are observed between leavers and stayers, these differences are the same in non-profit sharing firms. Therefore, the adoption of a profit sharing plan does not appear to affect attrition out of a firm.

Conclusions

Previous studies of the effects of the adoption of profit sharing plans on worker productivity employ firm-level panel data, and may not adequately control for changes in the quality of labor due to turnover. If the quality of the work force improves following the adoption of a profit sharing plan, one may not be able to interpret results from these studies as fully representing incentive effects due to profit sharing plans. This paper uses individual-level data in an attempt to find if the quality of the work force in a profit sharing firm improves because higher-quality workers enter the firm and lower-quality workers leave.

The results show that higher quality workers tend to enter profit sharing firms versus non-profit sharing firms, which increases the average quality of the workers in profit sharing firms. However, profit sharing plans have no effect on attrition out of the firm, as the estimated differences between leavers and stayers is the same for profit sharing and non-profit sharing firms.

The magnitude of the differences between workers entering profit sharing firms can be interpreted as follows: assume that the number of employees at the firm is constant (so that everyone who leaves the firm is replaced by one worker), and that the

effect of higher quality workers entering the firm creates a 3.5 to 5 percent improvement per worker (see footnote 19). These assumptions, in conjunction with the (approximately) 19 percent of profit sharing workers changing jobs per year, produce a .67 to .95 percent annual improvement in firm-level productivity due a change in worker quality.²² Given that Kruse (1993) estimates a 4 percent improvement in the year following the adoption of a profit sharing plan, the exercise shown above suggests that less than one-fourth of the increase in firm-level productivity is due to a change in the quality of the firm's labor.

It is possible, however, that this fraction is larger. First, given that the profit sharing status is self reported, there may be a significant amount of measurement error in the selection categories, which biases the estimates toward zero,²³ reinforcing one's confidence that the observed differences exist.

The 19 percent annual turnover figure may also understate the actual conditions because it is possible that turnover is higher in the first year of the new compensation system. That is, if workers select jobs based on the form of compensation that is used, one might expect that turnover would increase when the method of compensation changed.

While the size of the impact on the firm-level productivity studies may be in doubt, it seems more certain that higher quality workers are more likely to enter profit

²² The estimate of annual turnover (19 percent) is from the NLSY sample of full-time workers. Although this estimate may be high because this is a sample of young workers, it is modest in magnitude relative to the estimates contained in Anderson and Meyer (1994), which finds a 17 percent *quarterly* permanent separation rate.

²³ Freeman (1984).

sharing firms. Further research may better indicate why these workers prefer the profit sharing environment.

Table 1
Means of the Data for Job Changers¹

	Standardized AFQT Score	Years of Education	Wage in the Previous Year	N
Entered a profit sharing firm	.096 (.037)	13.15 (.09)	8.53 (.20)	728
Entered a non- profit sharing firm	-.028 (.020)	12.74 (.05)	8.26 (.11)	2,477

¹ All workers in the sample were in a non-profit sharing job and then changed jobs. They are classified by "year 2" status. Standard errors are in parentheses. Wages are expressed in 1983 dollars.

Table 2
The Difference Between Workers Entering Profit Sharing Firms and Non-Profit Sharing Firms by Worker Characteristic

	Dependent Variable			
Specification	Education	AFQT Scores	Log (Wage) ¹ Net of Other Traits	Log (Wage) ¹
No Controls	.422** (.103)	.119** (.042)	.035* (.020)	.067** (.021)
Including Occupation Controls	.178** (.087)	.047 (.039)	.029 (.019)	.042** (.020)
Including Firm Size Variables	.350** (.105)	.089** (.043)	.024 (.020)	.050** (.021)
Including Firm Size and Occupation Controls	.180** (.089)	.033 (.040)	.021 (.020)	.033 (.020)

N=3,205 in rows 1 and 2 of columns 1 and 2. N=3,077 in rows 1 and 2 of columns 3 and 4. N=2,999 in rows 3 and 4 of columns 1 and 2. N=2,880 in rows 3 and 4 of columns 3 and 4. ¹ The wage variable is first regressed on AFQT scores and years of education, with the residuals from this regression used in the specifications of this table. Standard errors are in parentheses. All specifications include geographic region dummies. Wages are in 1983 dollars. ** denotes significance at the five percent level. * denotes significance at the ten percent level.

Table 3
Means of the Data for Workers Already in Profit Sharing Firms¹

	Standardized AFQT Score	Years of Education	Wage in Year 1	N
Left a firm with a profit sharing plan (PS-LV)	.012 (.065)	13.21 (.18)	7.43 (.27)	173
Stayed in a firm with a profit sharing plan (PS-PS)	.058 (.041)	13.15 (.09)	8.17 (.15)	560
Left a non-profit sharing firm for another non-profit sharing firm (NV-LV)	-.097 (.045)	12.98 (.11)	7.00 (.21)	542
Stayed in a non-profit sharing firm (NV-ST)	.034 (.021)	13.04 (.05)	7.43 (.09)	2,115

¹ The workers in rows 1 and 2 report profit sharing jobs in "year 1." They are classified by their status the following year. Workers in rows 3 and 4 never report having a profit sharing plan. Standard errors are in parentheses. Wages are expressed in 1983 dollars.

Table 4
Estimates of the Differences in Characteristics for
Workers Already in Profit Sharing Firms

	Years of Education		Standardized AFQT Scores		Log (Wage) Net of Other Traits ¹		Log (Wage) ¹	
Occupational Controls	No	Yes	No	Yes	No	Yes	No	Yes
Leave PS - Stay in PS	.062 (.208)	-.069 (.172)	-.046 (.086)	-.087 (.079)	-.112** (.036)	-.101** (.035)	-.114** (.039)	-.112** (.036)
(L - S) _{PS} - (L - S) _{No PS}	.122 (.238)	-.130 (.196)	.085 (.098)	.008 (.090)	-.030 (.041)	-.025 (.040)	-.019 (.045)	-.029 (.041)

N = 3,390. ¹ The wage used is from "year 2," when all of the workers are in a profit sharing firm. Two variables are included in the wage regressions in order to remove the effects of discrimination. These variables are constructed by forming the residuals from regressions of male and white on tenure, tenure-squared, experience, experience-squared, education and marital status. This is done so that any variation in wages due to differences in tenure, et al. by group are retained in the estimates. Standard errors are in parentheses. ** denotes significance at the ten percent level. * denotes significance at the five percent level.

Table 5
Means of the Data for Workers in Firms Adopting Profit Sharing²

	Standardized AFQT Score	Years of Education	Wage prior to PS adoption	N
Left a firm with a profit sharing plan (PS-LV)	-.009 (.033)	12.97 (.08)	9.11 (.19)	723
Stayed in a firm with a profit sharing plan (PS-PS)	.128 (.024)	13.28 (.05)	10.90 (.13)	1,635
Left a non-profit sharing firm for another non-profit sharing firm (NV-LV)	-.188 (.031)	12.49 (.08)	7.84 (.18)	1,021
Stayed in a non-profit sharing firm (NV-ST)	.0003 (.020)	13.02 (.05)	9.49 (.11)	2,422

² The workers in rows 1 and 2 first report a non-profit sharing job, remained in the job the following year, but reported that profit sharing is offered. They are classified by their status the following year. Workers in rows 3 and 4 never report having a profit sharing plan. Standard errors are in parentheses. Wages are expressed in 1983 dollars.

Table 6
Estimates of the Differences in Characteristics for
Workers in Firms Adopting Profit Sharing

	Years of Education		Standardized AFQT Scores		Log (Wage) Net of Other Traits ¹		Log (Wage) ¹	
	No	Yes	No	Yes	No	Yes	No	Yes
Occupational Controls								
Leave PS - Stay in PS	-.302** (.104)	-.159* (.089)	-.137** (.044)	-.098** (.041)	-.159** (.019)	-.145** (.018)	-.184** (.020)	-.161** (.019)
(L - S)_{PS} - (L - S)_{No PS}	.228* (.136)	.035 (.116)	.051 (.057)	-.006 (.053)	-.007 (.024)	-.015 (.024)	.011 (.026)	-.013 (.024)

N = 5,801. ¹ The wage used is from "year 1," when all of the workers are in a non-profit sharing firm. Two variables are included in the wage regressions in order to remove the effects of discrimination. These variables are constructed by forming the residuals from regressions of male and white on tenure, tenure-squared, experience, experience-squared, education and marital status. This is done so that any variation in wages due to differences in tenure, et al. by group are retained in the estimates. Standard errors are in parentheses. ** denotes significance at the ten percent level. * denotes significance at the five percent level.

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Chapter 3

SUPERVISION, EFFICIENCY WAGES AND INCENTIVE PLANS: HOW ARE MONITORING PROBLEMS SOLVED?

Introduction

Many models of the employment relationship assume that monitoring worker behavior is costly. Examples include the shirking model of efficiency wages in Shapiro and Stiglitz (1984) and the bonding model in Lazear (1981). A common characteristic of these models is that firms employ a compensation mechanism in order to extract effort from workers. This paper examines the use of three such mechanisms: efficiency wages,¹ performance-based bonuses and profit sharing plans.

In theory, any one of the three schemes could be observed where monitoring is difficult (costly). In the shirking model of efficiency wages, for example, as monitoring becomes more costly, the firm benefits by substituting away from supervision and instead pays its workers a higher wage.² A similar motivation may lead to the use of both performance-based bonuses and profit sharing plans.³ Because these plans allow the workers to share in productivity gains, they may no longer find it optimal to shirk, as shirking now has an opportunity cost associated with it. Because all three compensation

¹ Throughout the paper, the term “efficiency wages” refers to the Shapiro and Stiglitz (1984) variant of the efficiency wage model. Other versions, such as Akerlof (1986), do not necessarily imply the same tradeoff between supervision and wages that is contained in the shirking model of Shapiro and Stiglitz.

² See Shapiro and Stiglitz (1984) or the final section of this paper for the mechanism through which this affects worker effort.

³ The difference in the definition of these plans can be seen on p. 62.

mechanisms have the potential to perform the same task, a common prediction is that, *ceteris paribus*, each one is more likely to be observed as monitoring becomes more difficult.

It should be noted that this study explores the *prevalence* of these plans vis-à-vis the level of supervision, but cannot directly determine the *effectiveness* of these three pay schemes at reducing shirking. However, assuming that firms would not knowingly adopt ineffective methods, prevalence and effectiveness should be closely related.

The majority of the previous work in this area has focused on the relationship between wages and supervision as tests of the shirking model of efficiency wages. Table 1 summarizes these studies, which tend to follow one of two methods. The first type of study isolates a source of previously unexplained wage dispersion (e.g., the firm-size wage effect in Brown and Medoff, 1987, or the inter-industry wage differential in Neal, 1993), and then introduces a proxy for monitoring in an attempt to explain the wage dispersion. Other studies, such as Kruse (1992) or Groshen and Krueger (1990), take a more direct approach by regressing wages on a measure of supervision (typically the supervisor-to-staff ratio or the frequency of supervision) in order to test whether wages increase as the extent of supervision decreases.

The results of these studies have provided mixed evidence on the shirking model. Studies using the supervisor-to-staff ratio as a proxy for monitoring difficulty (Leonard, 1987; Fitzroy and Kraft, 1988; Groshen and Krueger, 1991) find almost no support for the shirking model. However, many of the studies using alternative proxies, including Krueger (1991), Kruse (1992), Arai (1994), Osterman (1994) and Rebitzer (1995) find

evidence of a tradeoff between wages and the extent of supervision. The support among such studies is not universal, though, as Brown and Medoff (1989) and Neal (1993) do not observe this tradeoff.

Groshen and Krueger (1990) provide a possible explanation for the difference in the results depending on the form of the monitoring proxy that is employed. If supervisors and workers are substitutes in production, as the wages of workers increase, one expects a shift in the composition of the work force toward supervisors (now the relatively cheaper input). This creates a positive relationship between the worker's wage and the supervisor-to-staff ratio, biasing studies using this proxy against finding the prediction of the efficiency wage model. This "substitution bias" is potentially important in this paper because the data contain a proxy for monitoring that is functionally identical to the supervisor-to-staff ratio -- the number of employees that the respondent's supervisor supervises (the inverse of this variable is used in the analysis so that the interpretation of the coefficients is consistent with studies utilizing the supervisor-to-staff ratio).

A second bias, based on unobserved worker ability, may also affect these studies. As a way of illustrating this bias, assume that supervisors monitor high-ability workers less closely. If the data cannot identify these workers, one would expect a negative correlation between the extent of supervision and the wages of these workers, biasing the estimated effect of supervision on wages downward. Therefore, this bias makes it more likely to falsely observe the prediction of the efficiency wage model. Alternatively, the bias works in the opposite direction if high-ability workers want to ensure that they are

rewarded for their productivity, and therefore seek out jobs in which they are closely supervised. This study attempts to mitigate the effect of unobserved ability bias by including standardized test scores in otherwise standard specifications from the literature. Although test scores may be a controversial proxy for ability (and are certainly imperfect), the results indicate that unobserved ability does not play a role in estimating the relationship between wages and supervision.

Omitted firm characteristics may also bias the results. Rebitzer (1995) provides an example of the effect of omitted firm characteristics by focusing on the screening of employees. As is shown below, the effect that the omitted variables have on the estimates is determined by whether the variable is a substitute or a complement for supervision. Because in general it is not possible to determine the degree of complementarity, the sign of this bias is difficult to determine.

The study then goes beyond the usual wage-supervision analysis by focusing on the extent to which performance-based bonuses or profit sharing plans are used as substitutes for monitoring. Although the proxy for the level of supervision is the same as above, the substitution bias that may affect the study of a wage-supervision tradeoff should not be present. There is no compelling reason to believe that a firm would substitute away from workers and toward supervisors following the adoption of an incentive plan. Provided that the incentive is linked to productivity, the cost of the input (workers) should not increase without a concomitant increase in its productivity, eliminating the need for substitution away from the input. In addition, the potential for

omitted ability bias may be lessened, as the relationship between ability and incentive plans is less certain than its relationship with wages.⁴

An additional innovation in this study is its ability to separately examine the relationship between monitoring and compensation schemes for *supervisors*, as well as those supervised. Proxies are available for both the monitoring difficulty that the supervisors face (the number of workers reporting to them) and the level of monitoring to which supervisors are subjected (the number of other people their manager oversees). The distinction between workers and supervisors proves to be important, as it appears that only supervisors are offered incentives as work groups grow larger.

The findings may be summarized as follows: wages are related to the extent of supervision for one of three samples of supervisors and for piece rate and commission workers, but not for the majority of workers. In addition, neither performance-based bonuses nor profit sharing plans are more likely to be given to workers as it becomes more difficult to monitor them. However, the results do show a strong relationship between supervisory bonuses (but *not* profit sharing plans) and the number of employees that a supervisor monitors.

Data

The data are from the National Longitudinal Survey of Youth (NLSY). Although this survey has been taken since 1979, the length of the panel used here is substantially reduced because some of the questions of importance are only included in the survey in a

⁴ This theory is borne out in the data, as AFQT scores are not a significant determinant of either form of incentive plan studied here, but have a statistically significant impact on wages.

limited number of years. Supervisors and workers are examined separately, as defined by responses to questions about supervisory duties and the size of work groups.

Appendix A displays the text of the questions used to identify the samples.

Three definitions are used to form the samples of supervisors. The broadest of these includes anyone who reports that they “supervise the work of others, or tell them what work to do, on a day-to-day basis.” A follow-up question on the respondent’s power to set pay or determine promotions is used to narrow the sample to ensure that it is composed of supervisors with real authority (so that supervisory monitoring, and not inter-worker monitoring, may be examined). The final sample of supervisors is formed using three-digit 1980 Census of Population occupation codes. The criterion used for inclusion is that the word “supervisor” is in the description of the occupation (the specific occupations selected are shown in Appendix B). This method should exclude most managers (those higher-up in the ladder of authority) and include only the “front-line” supervisors. Therefore, this method may provide a cleaner test of the traditional interpretation of the shirking model: the supervision of workers involved in the production of a good or service (as opposed to the supervision of other supervisors). Regardless of which definition is best, looking at three definitions of supervisors should provide a sense of the robustness of the results.

The sample of workers consists of those responding to the question, “[b]esides yourself, for how many other people does/did your supervisor serve as the immediate supervisor on a day-to-day basis?” This question is only included in the survey in 1990.

It should be noted that, because many supervisors have their own supervisor, this sample includes some respondents who are also in the supervisory samples.⁵

The profit sharing indicator is taken from the question “Does/Did your employer make available to you...profit sharing.” The performance-based bonus indicator comes from the question:

The earnings on some jobs are based all or in part on how a person performs on the job. On this card are some examples of earnings that are based on job performance. Please tell me if any of the earnings on your job are/were based on any of these types of compensation.

1. Piece rate
2. Commissions
3. Bonuses (based on job performance)
4. Stock options
5. Tips

The indicator equals one for those responding that earnings are based on bonuses (part 3), and zero otherwise. Parts 1 and 2 of this question are used to further define the sample. For the primary analysis, those workers and supervisors reporting pay based on a piece rate or commission are excluded because these forms of compensation may themselves be substitutes for supervision. The use of these methods of pay may signal either a greater need for monitoring on these jobs, or that any monitoring problem that may have existed is solved by these pay schemes (without the use of efficiency wages or other incentive plans). Because of this, it may be useful to examine these workers separately.

The ability proxy comes from standardized test scores on the Armed Services Vocational Aptitude Battery (ASVAB), administered to the sample in 1980. Four of the

⁵ The qualitative results do not change when supervisors are excluded from this sample.

ten test scores from the ASVAB are used to form the Armed Forces Qualifications Test (AFQT) score.⁶ All of the respondents were asked to take the AFQT in 1980, at which time their ages ranged from 15 to 23. Because scores may differ because less schooling had been acquired at the time the test was taken, the AFQT scores are regressed on age dummies, and the residuals from this regression used in the analysis that follows.

Finally, those reporting their industry as public administration,⁷ working less than 35 hours in the survey week, in a union, reporting a wage less than one half of the minimum wage, or greater than \$100 per hour are excluded from the samples.⁸ Table 2 shows the means for the samples to be examined.

Re-Examining Evidence on the Shirking Model of Efficiency Wages for Non-Supervisory Workers

The methodology used to test for the shirking model of efficiency wages follows previous attempts in the literature. A wage regression of the following form is used:

$$\ln(\text{wage}) = X\beta + E\delta + S\gamma + A\lambda + \varepsilon$$

where X is composed of the standard controls,⁹ E is the log of the establishment size (from a respondent's estimate), S is the supervisor-to-staff ratio, and A is the

⁶ The AFQT score is calculated by adding the scores from arithmetic reasoning, word knowledge, paragraph composition and one-half of the numerical operations exams.

⁷ Those reporting public administration as their industry are removed from the analysis because of the frequent difficulty in determining either the level of productivity or the monetary value of increases in productivity in this industry. Support for this restriction comes in Arai (1994). He separately examines public and private sector workers, and finds that the efficiency wage model is consistent with the results for the private sector, but not public sector workers.

⁸ All monetary figures are converted to 1983 dollars.

⁹ The dummies that are included are: male, white, married, northeast, north central, south, a dummy for multiple-site firms, an interaction of this with a dummy for multiple-site firms with more than 1,000

standardized AFQT score. First, this model is run with the restriction that $\gamma = 0$ and $\lambda = 0$. The restrictions are then removed in stages.

When examining the samples of workers, three hypotheses may be tested. First, $\gamma < 0$ -- showing that workers are paid more as they are supervised less closely -- is consistent with the shirking model. Alternatively, finding $\gamma > 0$ suggests that workers are paid a compensating differential for having to endure closer supervision, or that the worker substitution bias has overwhelmed the true estimate. Second, it has been theorized that large firms (or establishments) pay higher wages in part because monitoring worker behavior is more difficult in that setting.¹⁰ If this is true, the inclusion of a proxy for monitoring difficulty moves the estimate of δ toward zero. Third, researchers have hypothesized that the wage variation across industries may be due to systematic monitoring differences (Krueger and Summers, 1988; and Neal, 1992). An examination of the industry indicators before and after the addition of the supervision proxy provides a test of this theory.

Table 3 shows the estimates from OLS wage regressions for workers who are not paid by piece rate or commission. The magnitude of the establishment size-wage effect in column 1 (.034) is consistent with the findings from individual-level data in Brown and Medoff (1989), which range from .013 to .038. The supervisor-to-staff ratio, included in column 2, has a coefficient that suggests a negative relationship between

employees, two-digit industry (see Appendix C) and one-digit occupation. The other variables included are: years of education, experience (actual) and its square and tenure and its square.

¹⁰ See Brown and Medoff (1989) for a discussion of this. These data are consistent with the theory that monitoring is more difficult in large firms, as the correlation between the establishment size and the supervisor-to-staff ratio is negative in all of the samples examined.

wages and supervision, but the magnitude is small. A one standard deviation increase in the supervisor-to-staff ratio decreases a worker's wage by less than one percent.

Furthermore, the inclusion of the monitoring proxy does not affect the estimate of the establishment size-wage effect. According to these findings, supervision is not strongly related to wages for these workers.

An attempt is made to eliminate the omitted ability bias by including standardized AFQT test scores.¹¹ If it is true that higher-ability workers are paid more and do not need to be supervised as closely, the inclusion of a proxy for ability should increase the estimate of γ . However, column 3 shows that, while the respondent's score on the AFQT is a significant determinant of wages, the coefficient on the supervisor-to-staff ratio is essentially unchanged.¹²

The focus shifts to employees paid a piece rate or by commission in Table 4.¹³ Working under these compensation systems presumes that at least some dimension of a worker's productivity is easily determined (i.e. one has to be able to count the pieces in order to pay a piece rate). Therefore, it may be that the number of workers per supervisor does not restrict the level of monitoring. For example, if all one has to do to monitor perfectly is add up the daily production of widgets, it may be as easy to monitor

¹¹ Firm size, establishment size (see Brown and Medoff, 1989) and education may also act as proxies for ability.

¹² An attempt was made to correct for *substitution* bias by instrumenting with the average supervisor-to-staff ratio for the other three regions in the same two-digit industry and for the other regions in the same one-digit industry and one-digit occupation. The use of these instruments actually *increased* the estimates of γ , however, suggesting that either the substitution bias is in the opposite direction than others have theorized, or that the instruments are not valid.

¹³ The hourly wage used for the majority of these workers may actually represent hourly earnings. That is, only 19 percent of the piece rate and commission workers report their wage on a per hour basis, but over 40 percent of the non-piece rate and commission workers report an hourly wage.

one hundred workers as it is to monitor one worker. The results do not bear out this prediction, however, as the estimate of γ is negative and statistically significant (in column 2), and the estimate of δ (the establishment size-wage effect) is reduced by the inclusion of the supervisor-to-staff ratio.

There are two possibilities for reconciling this result with existing theories. First, recall that $\gamma < 0$ is consistent with an alternative theory, that wages increase as supervision falls because of unobserved worker ability. Support for this theory applying to piece rate workers comes from Lazear (1996), who finds that the variance in the ability of a firm's work force increases with the use of piece-rate pay relative to hourly pay. Therefore, concerns about the quality of the good or service produced (for example) may lead firms to supervise inferior workers more closely when they are paid per unit produced. If this is the case, and ability is not accounted for in the regression, the supervision variable will not provide an estimate of the relationship between supervision and wages for workers of *equal* ability. This line of reasoning does not hold up when AFQT scores are added to the regression (in column 7), as the coefficient on the supervisor-to-staff ratio is unaffected. However, it should be noted that the coefficient on the AFQT scores is not significantly different from zero, pointing to the possibility that ability, as it applies to these jobs, is not captured by AFQT scores.

The second explanation for finding $\gamma < 0$ for piece rate and commission workers is that these may be workers who are particularly in need of monitoring, and more than one method of alleviating this problem is used simultaneously. That is, paying these workers a commission or piece rate may be used in combination with an efficiency wage or a

higher level of supervision in order to prevent them from shirking. An example for each type of worker may illustrate the point: piece rate workers may have the incentive to shirk on quality in order to increase the quantity produced, while workers paid by commission may benefit in the short-term by using tactics such as high-pressure sales, which may reduce the profitability of the firm in the long run. In either case, closer supervision is required to elicit the desired behavior from the worker.

These results, as in other studies that use the supervisor-to-staff ratio as a proxy for monitoring, show little or no relationship between supervision and wages for workers. The exception to this may be that piece rate and commission workers are paid an efficiency wage.

Evidence on the Shirking Model of Efficiency Wages for Supervisory Workers

The analysis now turns to an examination of the relationship between wages and monitoring for supervisors. Tables 5 through 7 present the similar specifications to those estimated in Tables 3 and 4, but instead using the three samples of supervisors defined above. Columns 1 and 2 take advantage of the fact that panel data are available for this sample (except when the manager-to-supervisor ratio is included) by including individual fixed effects in the estimation, which should eliminate bias due to fixed unobserved ability.¹⁴ A surprising finding comes from the inclusion of fixed individual effects, in that it completely eliminates the establishment size-wage effect across all three samples,

¹⁴ Hausman tests strongly support the use of fixed effects, as reflected in the p-values reported in Table 5 through 7.

while Brown and Medoff (1989) show at most a 45 percent reduction.¹⁵ A negative and significant coefficient on the supervisor-to-staff ratio in Table 5 is less surprising, as the interpretation of this coefficient differs from the regressions examining the samples of workers.

For supervisors, $\gamma < 0$ is consistent with two theories. First, it may reflect a compensating differential paid to supervisors because of a distaste for having to oversee more employees. Second, it may be due to the fact that supervisors and workers are substitutes in production. As the wages of the supervisors are increased, more workers are hired simply because they have become a relatively cheaper input.

The inclusion of the supervisor-to-staff ratio does not provide a test of the shirking model, however. This is tested in columns 4 and 5, which explore the relevance of the shirking model for supervisors by including the manager-to-supervisor ratio, for which a negative coefficient is consistent with the shirking model. This variable represents the level of monitoring to which the *supervisor* is exposed (fewer supervisors per manager is interpreted as each supervisor being monitored more closely).¹⁶ Unfortunately, information about the manager-to-supervisor ratio is only available in 1990, so that fixed effects estimation can no longer be used to eliminate omitted ability bias. Column 3 re-estimates the specification in column 1 without individual fixed effects and using only 1990 data. The supervisor-to-staff ratio and manager-to-

¹⁵ It should be noted that this result comes from data on supervisors, who are not examined in Brown and Medoff (1989). If the establishment size-wage effect is estimated using *all* NLSY workers from 1988 to 1993, the OLS coefficient on $\ln(\text{establishment size})$ is .028, and the fixed-effects estimate is .011. Both of these estimates are consistent with the findings of Brown and Medoff (1989).

¹⁶ This variable is taken from the same survey question as the supervisor-to-staff ratio for the worker sample. See Appendix A for details.

supervisor ratio are included in column 4 of Tables 5 through 7, producing mixed results. The findings for supervisors defined by occupation codes (Table 7) are consistent with the shirking model, but the other two samples show no relationship between wages and supervision. Column 5 adds AFQT scores in attempt to eliminate omitted ability bias, but, as in the case with the sample of workers, this does not affect the other coefficients.

The difference in the results by sample may make sense. That is, the supervisors defined by occupation codes should be those who directly monitor the workers who produce goods or services, as opposed to managers overseeing supervisors. Therefore, shirking on the part of these supervisors could lead to an increase in shirking among non-supervisory workers, who have the most direct effect on the product of the firm (less intensive monitoring leads to a lower probability of detection, lowering the expected cost of shirking). It may therefore be wise for the firms to focus the use of efficiency wages on this subgroup.

Recall that the regressions performed in Tables 3 through 7 may be used to test another hypothesis regarding efficiency wages. Previous studies (Neal, 1993, for example) have explored whether the inter-industry wage differential can be explained by monitoring differences across the industries.

Figures 1 through 3 display the coefficients on the industry dummies from the samples examined in Tables 3 through 7. Figure 1 shows the coefficients from model estimated in column 2 of Table 3, with and without the supervisor-to-staff ratio.^{17,18} So,

¹⁷ See Appendix C to match industries with the codes used in the figures.

¹⁸ The coefficients are expressed as deviations from the sample mean (there is no omitted industry), following the procedure described in Suits (1982).

for example, wages in the construction industry (coded “CON”) are about 15 percent higher than average wages, even after introducing a control for the level of supervision. The coefficients on the industry dummies for the piece rate and commission workers are shown in Figure 2 (based on the regression from column 2 of Table 4). Finally, the results for the sample of supervisors are shown in Figure 3 (from the regression in column 4 of Table 7). The three figures tell a similar story. Clearly, the variation in wages across industries cannot be explained by monitoring differences. This conclusion is reinforced in Neal (1993), who uses a different proxy for supervision, but similarly finds that “controls for frequency of supervision do not affect estimates of industry wage premiums.”¹⁹

Taken together, the results from the previous two sections do not provide support for the widespread applicability of the shirking model of efficiency wages. At most, piece rate and commission workers and front-line supervisors are offered this form of incentive to reduce shirking. The next section explores alternative methods that may be used to solve monitoring problems.

Evidence on the Use of Incentives

A lack of evidence pointing toward the widespread applicability of the efficiency wage model may not mean that extracting effort is costless, however. It could be that other pay schemes are used to accomplish this task. Incentive plans, whether they are based on individual or group goals, may be able to prevent workers from shirking by

¹⁹ Neal (1993), p. 416.

giving them a share of the value of increases in their productivity. For example, profit sharing plans linking a worker's compensation to the profitability of the firm have been proposed as a solution. While research on profit sharing plans indicates that its adoption increases firm-level productivity,²⁰ there is a reason to be skeptical of the ability of the plans to provide a financial incentive to individuals. The problem is that, as the number of workers in a firm increases, the incentive for any one worker to enhance profits is reduced because any gain that a worker creates must be shared with the rest of the workers. Because of this, profit sharing plans may not be as effective as other incentives are at providing a substitute for supervision.

There are a variety of other incentive plans that may act as a substitute for supervision. These include group incentive plans such as gain sharing plans,²¹ in which the incentive pay is based on well-defined goals instead of profits, and individual incentives, which can range from sales goals to more subjective performance ratings on which to base pay.

There are two incentive-plan variables available for use here: a self-report of whether profit sharing is "made available by the employer," and the presence of bonuses based on job performance. The profit sharing indicator has two potential flaws. First, workers may report that profit sharing is not "made available" for cases in which a profit sharing plan is in place, but there are no profits to be distributed.²² Second, many pay

²⁰ Kruse (1993) finds a 4 percent increase in firm-level productivity due to the adoption of a profit sharing plan using longitudinal data. This finding is consistent with studies using other (usually cross-sectional) data sets.

²¹ Case-study evidence on the success of a gain sharing plan can be found in Hansen (1997).

²² This notion is introduced in Kruse (1995).

schemes that are referred to as “profit sharing plans” may have very little to do with profits, but instead add a fixed percentage of a worker’s contributions to a 401(K) plan (for example). In addition, a distinction exists between cash and deferred profit sharing plans for which this study cannot account. Since cash plans involve more immediate rewards, they are predicted to have a larger effect on behavior. While it is unfortunate that the data cannot determine which form of profit sharing plan is being reported, the predicted direction of the impact on behavior is the same across plan types, with only the magnitude of the effect differing.

The performance-based bonus indicator could represent a number of specific incentives. However, the question from which this indicator comes (see page 62 for the text of the question) separately lists piece rate pay, commissions, bonuses (based on job performance), stock options and tips, so it may be expected this indicator does not represent the other forms of pay listed in the survey question because it is defined to refer to bonuses only.

The dependent variable in the following specifications is an indicator for the presence of the incentive plan, with separate regressions run for performance-based bonuses and profit sharing plans. Because the dependent variable is binary, the logit model is used for estimation:

$$\text{Prob}[\text{incentive plan} = 1] = \frac{e^{X\beta + S\gamma}}{1 + e^{X\beta + S\gamma}}$$

where X includes the same controls as in the wage equations, adding the log of the hourly wage, and removing the region dummies.²³ S represents the supervisor-to-staff ratio.

The explanatory variables are intended to control for variation in the provision of incentive plans based on the position of the employee in the firm (occupation indicators, tenure, experience, and wages) or in the technology involved in the job (through industry and firm size indicators). In addition, some demographic characteristics (male, white and married) are included to control for the possibility that some groups are given preference in the provision of incentives.

The key hypothesis is this: if incentive pay plays a role in reducing monitoring problems, the presence of profit sharing plans or performance-based bonuses should be associated with a lower supervisor-to-staff ratio, or $\gamma < 0$. Once again, separate analyses are performed for the supervisor and worker samples.

Table 8 displays the results of this model estimated using both profit sharing plans and performance-based bonuses as the dependent variable, and for each of the five samples that have been formed.²⁴ Only γ is reported in Table 8, and each cell represents a different regression. As the first two rows show, there is very little relationship

²³ The inclusion of the region dummies does not affect the results.

²⁴ For the samples of supervisors, where panel data are available, fixed effects logit models are estimated. In all cases, a Hausman test cannot reject the hypothesis of no heterogeneity bias. For the broadly defined supervisors and supervisors responsible for pay or promotion, the coefficients from the fixed effects logit model are quite similar to the coefficients from the logit model. For example, the fixed effects logit coefficient corresponding to row 3 of column 1 is -.340 with a standard error of .116. However, because of the small sample size, the fixed effects logit coefficients for the supervisors defined by occupation codes are very imprecisely estimated (the fixed effects logit coefficient corresponding to row 5 of column 1 is -1.791 with a standard error of 1.428).

between incentives and supervision for the worker samples. The only statistically significant result is found for workers (not piece rate or commission) and the presence of profit sharing plans. However, the magnitude of this result is small. A movement from one standard deviation below the mean of the supervisor-to-staff ratio to one standard deviation above the mean only decreases the probability of reporting a profit sharing plan from 32.8 percent to 29.5 percent.

These findings are consistent with Osterman (1994), who uses a dummy variable for little or no supervision as a proxy for the extent of monitoring, and finds that non-supervisory workers are not more likely to be offered profit sharing plans or group incentives in the absence of supervision.

This study goes beyond Osterman (1994) by looking at the relationship between incentives and monitoring for *supervisors*. Although there is no relationship between monitoring and profit sharing plans, rows 3 through 5 of the first column show that performance-based bonuses are strongly related to supervision across all three samples.²⁵ Recall that these specifications may be less biased than the wage specifications presented in the previous sections. That is, the provision of a bonus should not lead to substitution between the factors of production (supervisors and workers) in the same way that wage changes do. This may lead one to be more confident of the results from Table 8 relative to the wage specifications in earlier tables.

The results in the performance-based bonus specifications for supervisors are not only statistically significant, but the magnitude is also large enough to represent a

²⁵ The manager-to-supervisor ratio (not shown in Table 8) has no significant relationship with report of profit sharing plans of performance-based bonuses.

meaningful effect. The weakest effect is for the broad definition of supervisors (in row 3), for which an increase from one standard deviation below the mean of the supervisor-to-staff ratio to one standard deviation above the mean reduces the probability of reporting a performance-based bonus from 21.4 percent to 14.1 percent. The strongest relationship is seen for the supervisors defined by occupation codes (row 5), for whom the aforementioned change in the supervisor-to-staff ratio decreases the probability of reporting a bonus from 29.3 percent to 12.6 percent.²⁶

Taken together, the results from the previous three sections indicate that supervisors, and not the workers themselves, are given the incentive to reduce the level of shirking as monitoring becomes more difficult. This is seen in the fact that front-line supervisors show evidence of earning efficiency wages, and that all of the samples of supervisors are more likely to be offered a performance-based bonus as they monitor more workers.

Rebitzer (1995), however, provides a potentially important criticism of studies such as this. That is, in estimating the relationship between supervision and wages (or the provision of incentive plans), one typically cannot account for other human resource practices -- or other firm characteristics -- that may also affect productivity.²⁷ An example of this is employee screening. As more resources are devoted to weeding out

²⁶ Omitted ability bias does not appear to be an issue in this section, as the addition of the AFQT scores does not significantly impact the results of Table 8. This is also reflected in the fact that Hausman tests reject the need for individual fixed effects where panel data are available.

²⁷ There is some evidence that firm heterogeneity may not strongly affect the results. Wage regressions are run without firm size or industry indicators. Then firm size variables are added, followed by one-digit industry and then two-digit industry indicators. There is little change in the coefficient on the supervisor-to-staff (or manager-to-supervisor) ratio across the samples. For example, for supervisors identified by occupation codes, the coefficient evolves as follows: -.027, -.025, -.032, -.043.

shirkers, less effort is required to monitor the behavior of workers. Rebitzer states that this omitted variable problem “will lead to *underestimates* of the trade off between wages and supervision.”²⁸ However, this is only true if the omitted human resource practices are associated with lower wages (they are substitutes for efficiency wages) and, more importantly, the human resource practices are substitutes for supervision. To illustrate the point, consider the following model.²⁹

$$\ln(\text{wage}) = X\beta + HR\delta + S\gamma + \epsilon$$

where X is composed of the standard regressors, HR represents resources devoted to screening, etc. and S is the supervisor-to-staff ratio. Recall that the efficiency wage model predicts that $\gamma < 0$. If it is assumed that screening acts as a substitute for supervision and it is omitted from the regression, γ is biased upward. In this case, as Rebitzer suggests, it is more difficult to observe a wage-supervision trade off.

However, it seems equally plausible that human resource practices and supervision act as complements. For example, if a firm is very concerned about quality (e.g. it manufactures computer processors), it may screen employees heavily during the hiring process *and* supervise work closely. In this case, the omission of the screening variable biases γ downward, in favor of finding the prediction of the efficiency wage model.

The results from this paper can therefore be interpreted given two scenarios: HR and S are substitutes, or HR and S are complements. If Rebitzer is correct, and HR and S

²⁸ Rebitzer (1995), p. 111.

²⁹ The discussion in Rebitzer (1995) focuses on the relationship between supervision and wages. However, the same arguments may be made about the relationship between supervision and the provision of incentive plans.

are substitutes, then the findings from the wage regressions may be called into question. That is, little evidence of a relationship between wages and supervision is found, but this may be because the true relationship is masked by omitted variable bias. However, the direction of this bias further supports the most interesting finding from the incentive plan regressions, showing a strong relationship between the size of the supervisor's work group and the provision of a performance-based bonus.

Alternatively, one can examine the case in which employee screening and supervision are complements. Under this assumption, the findings from the wage equations are reinforced. That is, there is little evidence in favor of the efficiency model even when the statistical model is biased toward that finding. However, the results for the provision of incentives may be called into question, although the magnitude of the coefficient on the supervisor-to-staff ratio and its statistical significance are reassuring.

Conclusions and Discussion

The goal of this study is to determine if compensation policies -- efficiency wages, profit sharing plans or performance-based bonuses -- act as substitutes for supervision. According to the results, the shirking model of efficiency wages is only relevant for piece rate and commission workers and front-line supervisors. An attempt is made to remove bias due to omitted worker ability, but it does not produce evidence that is more favorable towards the shirking model for the large sample of workers.

In addition, it does not appear that workers are offered incentive plans as a substitute for supervision. However, the evidence does support the idea that *supervisors*

are more likely to have a performance-based bonus as a part of their compensation package when they supervise a larger group of workers. This may be an economically sound approach by firms, as offering only supervisors a bonus for increases in productivity may involve a smaller total cost to the firm than offering an incentive (or higher wage, in the case of the efficiency wage model) to *every* employee. This assumes, of course, that supervisors are equal to the task, and are able to monitor effectively enough to prevent their subordinates from shirking.

There may be another reason for the fact that the strongest results are found for the supervisory samples; it may be that supervisors are more difficult to monitor than workers. That is, workers frequently produce output that is observable, whereas supervisors, who exert effort in the form of increased monitoring, have less observable output. If this makes it more costly to monitor supervisors, then it is reasonable for managers to substitute toward other methods in order to prevent them from shirking.

To get a sense of the macroeconomic implications of these results, one can incorporate supervisory bonuses into the shirking model. The bonuses for supervisors can be imposed on the model exogenously by affecting the probability that *workers* are caught shirking. This is seen in the no-shirking condition as modeled in Shapiro and Stiglitz (1984):

$$w \geq w' + e + e(a + b + r)/q$$

where w = wage, w' = unemployment benefit, e = worker effort, a = probability of re-employment, b = exogenous separation rate, r = interest rate, and q = probability of being caught shirking.

In this model, a single firm has the incentive to increase wages above the market-clearing level in order to create a cost of losing one's job, preventing workers from shirking. Because the model contains identical firms, if one firm has the incentive to increase wages, all of the others do as well. The fact that economy-wide wages exceed the market-clearing level creates involuntary unemployment (an appealing aspect of the model to some). In the end, workers are motivated by a fear of suffering through a period of unemployment.

Providing supervisors with the incentive to monitor more closely (i.e. firm performance is increased by increased monitoring, so the supervisor monitors more closely in order to receive a bonus) increases q for workers. Notice that in the equation above, an increase in q reduces the size of the wage premium that is paid to workers. The reasoning for this is as follows: the higher probability of job loss can be offset by an increase in aggregate demand, which reduces involuntary unemployment in the economy.

In other words, because the wage premium is smaller, aggregate demand is reduced less than it is in the absence of bonuses paid to supervisors. Therefore, involuntary unemployment is reduced by the use of the bonuses. In fact, it can be shown that as q approaches one, involuntary unemployment goes to zero.³⁰

³⁰ It could therefore be true that a tradeoff exists between wages and supervision for workers, but this study does not observe it because it cannot control for the presence of supervisory bonuses (i.e., there are fluctuations in q controlling for the supervisor-to-staff ratio). One attempt is made to approximate this, however. The percentage of supervisors reporting a performance-based bonus is calculated for each two-digit industry. This is then compared to the industry wage differential from a standard wage equation using the worker sample. The correlation of these two variables is .06, showing that high-wage industries offer more supervisory bonuses. This contradicts the idea that supervisory bonuses act as substitutes for worker's wages.

It is also interesting to note the lack of a relationship between supervision and the presence of profit sharing plans. This is in accordance with much of the theoretical discussion on profit sharing plans, in that the free rider problem inherent in these schemes may limit the financial incentive to monitor oneself. This suggests that profit sharing plans may be adopted (or maintained) for reasons other than alleviating monitoring difficulties (e.g., the desire to reduce turnover).

Table 1
Summary of Previous Studies on the Shirking Model of Efficiency Wages

Author(s) and Year	Monitoring Proxy	Data	Findings
Leonard (1987)	Supervisor-to-staff ratio	1982 survey of 200 plants in the high-technology sector of one state	Inclusion of supervision variable does not explain the dispersion of wages; does not support shirking model.
Fitzroy and Kraft (1988)	Supervisor-to-staff ratio	65 West German metalworking firms	Supervision is not related to wages.
Brown and Medoff (1989)	Examines piece rate workers, who should require less monitoring.	BLS Industry Wage Survey	Establishment size-wage effect exists for piece rate workers, and therefore cannot be explained by monitoring difficulty.
Groshen and Krueger (1990)	Supervisor-to-staff ratio	BLS 1985 Hospital Industry Wage Survey of 300 hospitals	Wages fall as supervision increases for 1 of the 4 occupations studied.
Krueger (1991)	Franchised vs. company-owned outlets	1982 National Institute for Work and Learning	Supports shirking model; pay is higher in company-owned outlets, where monitoring is more difficult.
Kruse (1992)	Frequency of supervision. Uses continuous and categorical variables.	1980 Survey of Job Characteristics from the Center for Survey Research	Supports shirking model; wages increase as supervision decreases.
Neal (1993)	A categorical response to: "How often does your supervisor check up on your work?"	1977 Supplement to the PSID	Industry wage premiums are not affected by inclusion of the supervision variables.
Arai (1994)	Measure of "autonomy"; a combination of the possibility of varying work effort, the use of a punch card and flex time.	Swedish Level of Living Survey 1981	Supports shirking model for private sector workers, but not for public sector workers.
Osterman (1994)	Dummy variables for little or no supervision and whether or not the worker has discretion over method.	1992 survey of 875 private-sector establishments with 50 or more employees	Both proxies support the shirking model.
Rebitzer (1995)	Indicator for contractors having to report safety problems to the host plant management.	1990 survey of contract personnel employed in the petrochemical industry.	Supports shirking model.

Table 2
Descriptive Statistics

	Workers (Not Piece Rate or Commission)	Piece Rate and Commission Workers	Supervisors	Supervisors Responsible for Pay or Promotion	Supervisors Defined by Occupation Codes
Supervisor-to- Staff Ratio	.189 (.233)	.173 (.226)	.354 (.307)	.269 (.279)	.194 (.211)
Manager-to- Supervisor Ratio	---	---	.173 (.214)	.191 (.244)	.141 (.183)
Bonus	.144 (.351)	.295 (.457)	.191 (.393)	.257 (.437)	.241 (.428)
Profit Sharing	.337 (.473)	.370 (.484)	.343 (.475)	.388 (.487)	.417 (.493)
Hourly Wage	7.49 (4.02)	8.34 (4.85)	8.02 (4.51)	9.23 (5.21)	7.88 (4.06)
Education (Years)	13.00 (2.58)	13.03 (2.31)	13.26 (2.61)	13.75 (2.56)	12.89 (2.27)
Tenure (Years)	3.37 (3.30)	3.00 (3.07)	3.55 (3.10)	3.97 (3.20)	4.23 (3.44)
Male	.564 (.496)	.690 (.463)	.585 (.493)	.610 (.488)	.677 (.468)
White	.730 (.444)	.795 (.404)	.763 (.425)	.813 (.390)	.777 (.417)
N in 1988	---	---	1,938	834	207
N in 1989	---	---	1,892	845	277
N in 1990	3,329	332	1,770	624	238

Note: Standard deviations are in parentheses.

Table 3
OLS Estimates of the Effect of Supervision and Firm Size on
Wages for Workers (Not Piece Rate or Commission)

	(1)	(2)	(3)
Supervisor-to-Staff Ratio	---	-.006 (.005)	-.007 (.005)
Standardized AFQT Score	---	---	.042** (.008)
Establishment Size	.034** (.004)	.033** (.004)	.032** (.004)
N	3,329	3,329	3,329

Note: All variables are in log form. Standard errors are in parentheses. * denotes significance at the ten-percent level. ** denotes significance at the five-percent level. Other independent variables are: tenure, tenure-squared, experience (actual), experience-squared, and dummies for male, white, married, northeast, north central, south, two-digit industry and one-digit occupation.

Table 6
Estimates of the Effect of Supervision and Firm Size on Wages
for Supervisors Responsible for Pay or Promotion

Estimation Method	Fixed Effects		OLS		
	(1)	(2)	(3)	(4)	(5)
Supervisor-to-Staff Ratio	---	-.011 (.011)	---	.007 (.015)	.008 (.015)
Manager-to-Supervisor Ratio	---	---	---	-.005 (.012)	-.006 (.012)
Standardized AFQT Score	---	---	---	---	.005 (.020)
Establishment Size	-.006 (.008)	-.009 (.008)	.037** (.010)	.038** (.010)	.038** (.010)
p-value from Hausman Test	.00	.00	---	---	---
N	2,303	2,303	588	588	588

Note: All variables are in log form. Standard errors are in parentheses. * denotes significance at the ten-percent level. ** denotes significance at the five-percent level. Other independent variables are: tenure, tenure-squared, experience (actual), experience-squared, and dummies for male, white, married, northeast, north central, south, two-digit industry and one-digit occupation.

Table 8
Logit Estimates of the Relationship Between the Supervisor-to-Staff Ratio
and the Presence of Performance Based Bonuses and Profit Sharing Plans

Dependent Variable:	Performance-Based Bonus	Profit Sharing Plan
<u>Sample</u>	Coefficient on the Supervisor-to-Staff Ratio	
Workers (Not Piece Rate or Commission)	-.061 (.046) [-.007] N=3,289	-.066* (.036) [-.014] N=3,329
Piece Rate Workers and Workers Paid on Commission	-.115 (.145) [-.022] N=309	.016 (.158) [.004] N=280
Supervisors	-.215** (.034) [-.031] N=5,600	-.030 (.030) [-.006] N=5,595
Supervisors Responsible for Pay or Promotion	-.281** (.048) [-.049] N=2,293	.007 (.044) [.002] N=2,300
Supervisors Defined by Occupation Codes	-.480** (.108) [-.076] N=685	.061 (.092) [.014] N=717

Note: All variables are in log form. Standard errors are in parentheses. Marginal changes in probability are reported in brackets. * denotes significance at the ten-percent level. ** denotes significance at the five-percent level. Other independent variables are: ln(wage), tenure, tenure-squared, experience, experience-squared, years of education, establishment size, and dummies for male, white, married, multiple-site firm, an interaction between multiple-site firm and more than 1,000 total employees, two-digit industry and one-digit occupation.

Figure 1
Inter-Industry Wage Differences and Supervision
for the Sample of Workers

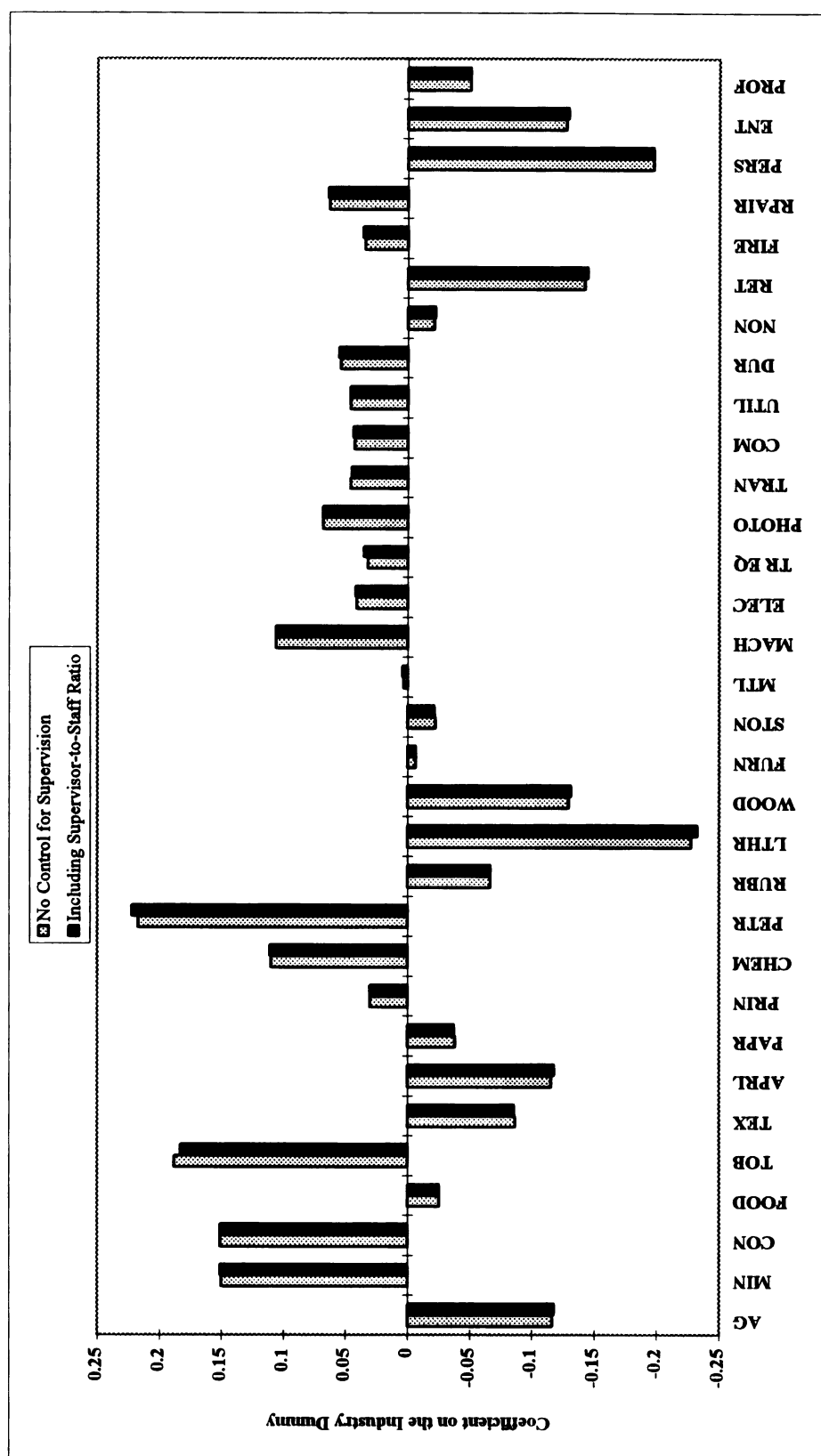


Figure 2
Inter-Industry Wage Differences and Supervision
for the Piece Rate and Commission Workers

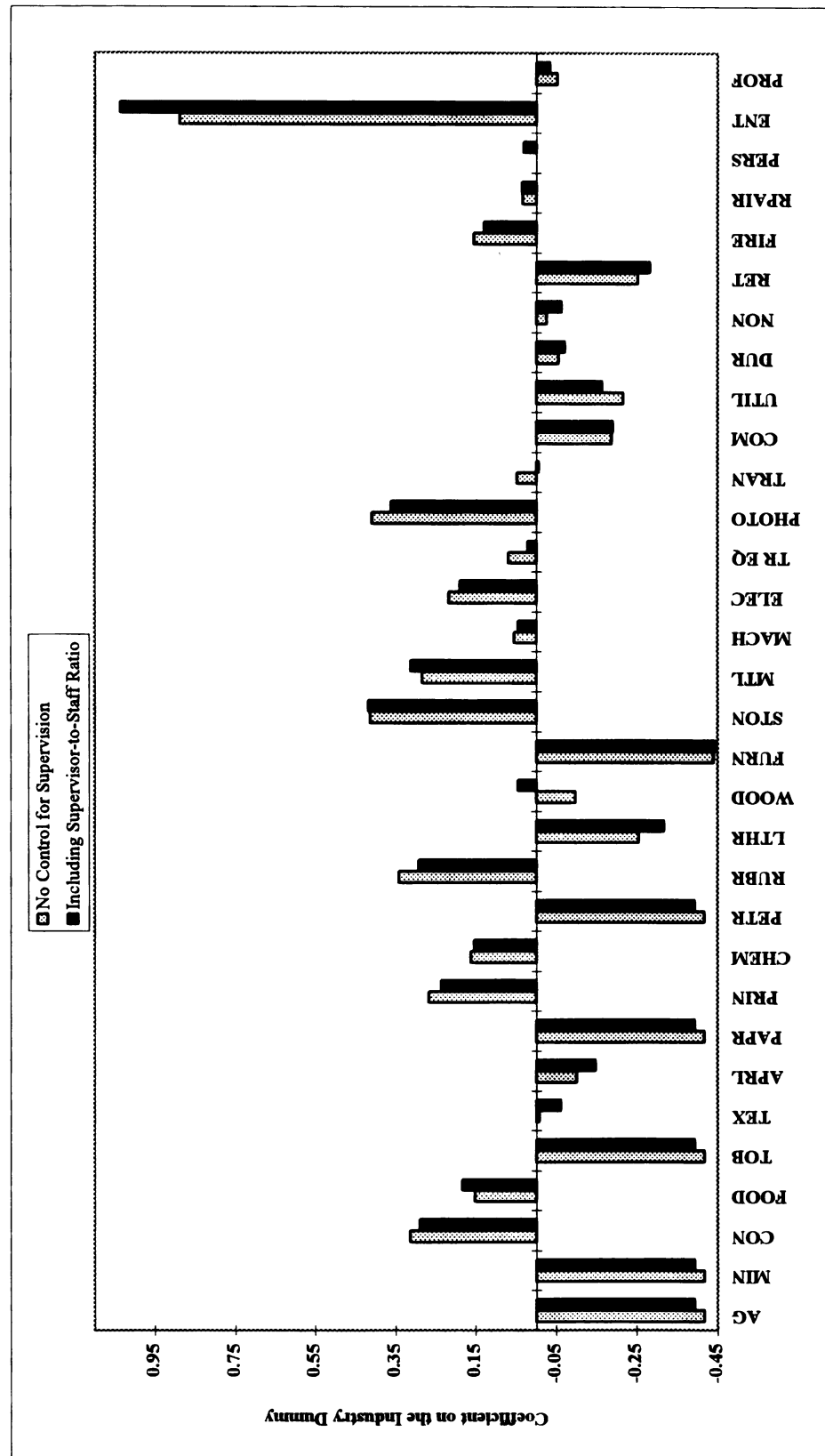
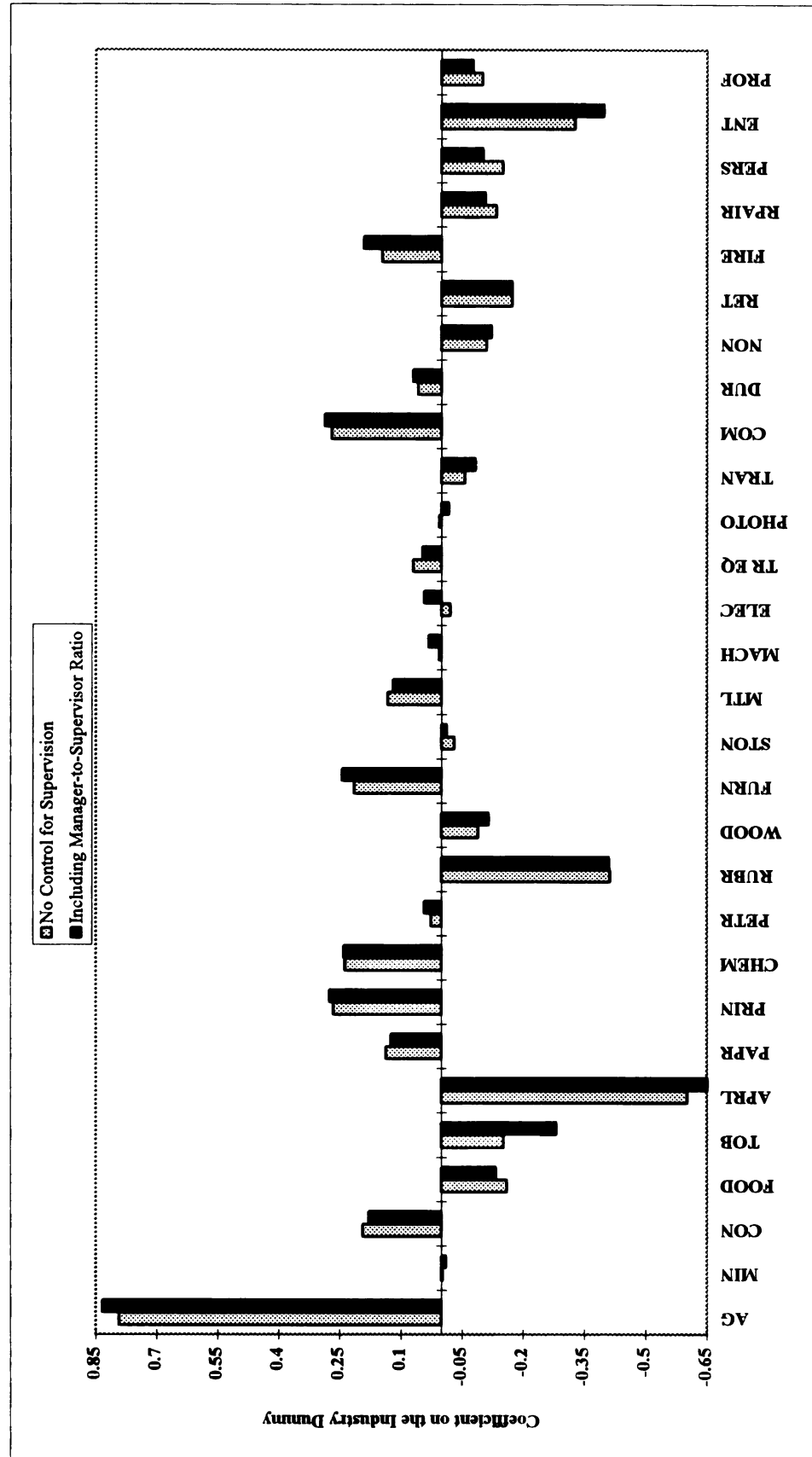


Figure 3
Inter-Industry Wage Differences and Supervision
for the Sample of Supervisors Identified by Occupation Codes



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APPENDICES

Appendix A

The broadest sample of supervisors is composed of those responding “yes” to:

“(Do/Did) you supervise the work of other employees, or tell them what work to do, on a day-to-day basis?”

The supervisor-to-staff ratio for the supervisor samples is derived from the follow-up question:

“About how many people (do/did) you supervise on a day-to-day basis?”

The second sample of supervisors is composed of those responding “yes” to another follow-up question:

“(Are/Were) you responsible for deciding their rate of pay or promotion?”

In 1988 and 1989, the possible responses are “Yes” and “No.” In 1990, the responses are expanded to: “Yes - Full Responsibility,” “Yes - Partial Responsibility,” and “No.” The two forms of “Yes” are combined for the purpose of this study. The results are not affected if only the 1988 and 1989 data are used.

All of the above questions appear in the survey from 1988 through 1990.

The supervisor-to-staff ratio for the worker samples, and the manager-to-supervisor ratio for the supervisor samples is taken from the question:

“Besides yourself, for how many other people (does/did) your supervisor also serve as the immediate supervisor on a day-to-day basis?”

This question is only asked in 1990.

Appendix B
1980 Census of Population Occupation Codes Used to Identify Supervisors

243 Supervisors and proprietors, sales occupations
303 Supervisors, general office
304 Supervisors, computer equipment operators
305 Supervisors, financial records processing
307 Supervisors; distribution, scheduling, and adjusting clerks
413 Supervisors, firefighting and fire prevention occupations
414 Supervisors, police and detectives
415 Supervisors, guards
433 Supervisors, food preparation and service occupations
448 Supervisors, cleaning and building service workers
456 Supervisors, personal service occupations
477 Supervisors, farm workers
485 Supervisors, related agricultural occupations
494 Supervisors, forestry, and logging workers
503 Supervisors, mechanics and repairers
553 Supervisors; brickmasons, stonemasons, and tile setters
554 Supervisors; carpenters and related workers
555 Supervisors; electricians and power transmission installers
556 Supervisors; painters, paperhangers, and plasterers
557 Supervisors; plumbers, pipefitters, and steamfitters
558 Supervisors; n.e.c.
613 Supervisors, extractive occupations
633 Supervisors, production occupations
803 Supervisors, motor vehicle operators
843 Supervisors, material moving equipment operators
863 Supervisors, handlers, equipment cleaners, and laborers, n.e.c.

Appendix C

Two-Digit Industries Used in the Figures and Regressions

AG	Agriculture, Forestry, and Fisheries
MIN	Mining
CON	Construction

Manufacturing, Nondurable Goods

FOOD	Food and kindred products
TOB	Tobacco manufactures
TEX	Textile mill products
APRL	Apparel and other finished textile products
PAPR	Paper and allied products
PRIN	Printing, publishing, and allied products
CHEM	Chemicals and allied products
PETR	Petroleum and coal products
RUBR	Rubber and miscellaneous plastics products
LTHR	Leather and leather products

Manufacturing, Durable Goods

WOOD	Lumber and wood products, except furniture
FURN	Furniture and fixtures
STON	Stone, clay, glass, and concrete products
MTL	Metal industries
MACH	Machinery, except electrical
ELEC	Electrical machinery, equipment, and supplies
TR EQ	Transportation equipment
PHOTO	Professional and photographic equipment, and watches

Transportation, Communications, and Other Public Utilities

TRAN	Transportation
COM	Communications
UTIL	Utilities and sanitary services

Wholesale Trade

DUR	Durable goods
NON	Nondurable goods

RET	Retail Trade
FIRE	Finance, Insurance, and Real Estate
RPAIR	Business and Repair Services
PERS	Personal Services
ENT	Entertainment and Recreation Services
PROF	Professional and Related Services